

RE: ACLU Matter vs. - REF# 1340012232  
Analysis of Coded ISR Narratives  
January-June 2016  
For Input to  
Hon. Arlander Keys' (Ret.)  
First Period Report  
  
**REVISED FINAL Technical Report**

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## 2 INTRODUCTION TO REVISED VERSION

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Comments by the Parties and their experts on the initial version of this report led to modifications that appear in this version. The major modifications include the following.

1. Clarifying the key question tested by each analysis.
2. Adding a descriptive table showing the average score on each outcome for each of the three ethnoracial groups examined using appropriately weighted data. These weighted descriptive results reflect the full set of ISRs capturing investigatory stops for the reporting period.
3. Clarifying the three levels of scrutiny that can be applied to ethnoracial differences on each outcome: gross impact, net impact, and statistically significant net impact of a race or ethnicity predictor.
4. Clarifying how geographic variation on each outcome was presented.
5. Discussing the partialling fallacy as a potential limitation when interpreting net impacts of race or ethnicity variables.

## 3 FOR THE NON-TECHNICAL READER: FAQ

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This section asks and answers questions that the non-technical reader might have about this report. It simultaneously guides the non-technical reader to findings that might be of most interest to him or her. Even technical readers might benefit from scanning the questions and answers listed here.

### 3.1 PURPOSE

Q: What is the **purpose** of this report?

A: This report looks at two features of stops: the *legal basis* for the investigatory stop itself, and the *legal basis* for a pat down, if a pat down occurred. For each of these features, the key question is whether civilian race or ethnicity had an impact on that basis.

### 3.2 SCORING STOPS AND PROTECTIVE PAT DOWNS

Q: How was that **legal basis determined**?

A: The Consultant made that judgment. He has legal expertise making similar decisions as a federal judge. For each sampled investigatory stop report (ISR) he independently reviewed narratives written by the officer and other key fields in the report. He determined in each case whether the narratives adequately articulated facts supporting the idea that the police officer's decision to stop the subject of the ISR was based on "reasonable articulable suspicion" (RAS) to suspect that a crime had been, or was about to be committed (stop legal basis). If the Consultant determined that the police officer articulated RAS for the stop, he coded the stop as "good" and assigned a corresponding numeric code. If he determined that the officer failed to articulate RAS for the stop, it was coded as "bad" and a different numeric code was assigned. These two numeric codes became the outcome variable that statistical models sought to link to race and ethnicity.



The same process was used for assessing the protective pat down, when applicable. The Consultant reviewed narratives for an independent set of facts giving the police officer an additional “reasonable suspicion” that the subject possessed or had access to a weapon or firearm, creating danger to the officer or bystanders nearby (protective pat down legal basis).

Again, pat downs like stops could be coded as “good” or “bad,” with corresponding numerical codes.

In short, the legal bases for stops and pat downs involved a coding process in which the Consultant independently reviewed the factual content, documented by the police officer, in the narrative remarks section of the submitted ISR, with due consideration given to the boxes checked by the officers in each ISR reviewed.

If the stop involved a search, the Consultant used a different legal standard to gauge whether that search was “good” or “bad.” This report, however, will not analyze the legal basis of searches because the number of “bad” searches was extremely small.

### 3.3 USING SAMPLES

Q: Were *all* investigatory stop records for the period January-June, 2016 examined by the Consultant?

A: No. We asked the computer to draw a random sample of reports from each of three groups: stops involving a White non-Hispanic civilian, a Hispanic civilian, and a Black non-Hispanic civilian. We asked for the same number of sampled reports from each of the three groups.

Q: Is it important that the **samples** you drew were **random**?

A: Yes. Random samples have sampling error in them. They do not perfectly capture the full set of records for each of the three groups because we have just a subset of the records for each group. But because the sampling error is random it will not distort the picture we have of each group. It will give us an unbiased picture of each group.

Q: Can the results from these three samples, when put together, **reflect the full set of stop records** for these three groups altogether?

A: Yes, if we take two things into account. First, we must remember that when we sampled we sampled a bigger fraction of some group’s records (White non-Hispanics), and a smaller fraction of other group’s records (Black non-Hispanic). So when we put all three groups in the sample back together to reflect the full set of stops we count a group’s records more if we took a smaller fraction from that group in the first place, and less if we took a bigger fraction. That way when we put all three groups together the proportional contribution of each group roughly matches what we find in the full set of records investigatory stop records. Second, we remember that the samples have error so we take that into account when inferring back to the full set of records.

### 3.4 RACE AND ETHNICITY IMPACTS ON STOP BASIS

Q: What do you mean by race or ethnicity "had an **impact**"?

A: We, the authors, are thinking about "influence," or "impact" *using a social science framework. That's because we're social scientists, not legal scholars.* In this report, we think

about impact in three ways: a gross impact of race or ethnicity, a net impact of race or ethnicity, and whether that net impact is or is not statistically significant.

### 3.4.1 Gross impact

Q: What you mean by **gross impact**?

A: Gross impact refers to different groups having different average scores on an outcome. So a gross impact of race or ethnicity refers to these three groups having different average scores on either the judgements about legal basis of stops or of pat downs. No other factors beyond race/ethnicity and the outcome in question are considered. This is pure *description*.

Q: **Where** in the technical, statistical reports are these gross impacts described?

A: If you are interested in the gross impact of race or ethnicity on the legal basis of the stop, you can find the relevant percentages showing both absolute differences<sup>1</sup> and relative differences<sup>2</sup> described as ratios in Table 11. These are for weighted data and refer to the full set of investigatory stops from which the examined records were sampled.

More specifically, to look at absolute differences we start with absolute percentages. For example, look at in the bottom third of Table 11 under the section labeled "66 bicycle/sidewalk excluded" under the subsection "percent", under the row "improper (zero)." You see the numbers 3.52, 4.83, 8.21. This means that 3.52 percent of white non-Hispanic stops, 4.83 percent of Hispanic stops, and 8.21 percent of black non-Hispanic stops were improperly premised.<sup>3</sup> The absolute differences in these percentage describe gross impact of ethnoracial category on this outcome, that is, the differences across the three groups.

For example you can say that stopped Black non-Hispanic civilians had the highest rate of "bad" stops (8.21percent) because their percentage is higher than the percentage for either of the other groups. You also could say that the percent of bad stops involving Black non-Hispanic civilians was (8.21-3.52) 4.69 percent higher than the percent of bad stops involving White non-Hispanic civilians.

To look at relative differences compare the ratio of two absolute percentages. You are asking: how many times higher or lower was the bad stop rate for Black as compared to White non-Hispanics?

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<sup>1</sup> An absolute difference is just a difference in the proportion or percentage of a group that has an attribute. Say you have two groups of 10 people each, group A and group B. Five out of 10 or 50 percent of those in group A have tuberculosis. Four out of 10 or 40 percent of those in group B have tuberculosis. The absolute difference is  $50 - 40 = 10$  percent. The gross impact of being in Group A or B on tuberculosis is 10 percent.

<sup>2</sup> Relative differences are created when the percentage for one group is expressed relative to the percentage for another group. Go back to the two groups of 10, A with a 50 percent tuberculosis rate and B with a 40 percent tuberculosis. The relative difference in the disease rate between the two groups can be expressed in two ways. If you want to talk about the disease rate in group A relative to group B you would take the ratio 50:40; alternatively you could say the rate in A was 20 percent higher. If you want to talk about the disease rate in group B relative to group A you would take the ratio 40:50; alternatively you could say the disease rate in B was 20 percent lower.

<sup>3</sup> The term "improperly premised" means, here, that the Consultant determined that this percentage of the coded legal narratives failed to satisfy the RAS standard.

The ratio  $8.21:3.52 = 2.33$ . You can say the Black “bad” stop rate was 2.33 *times* the White “bad” stop rate. This is also the same as saying that the Black “bad” stop rate was 133 *percent more* than the white stop rate.<sup>4</sup> These relative differences are also shown in Figure 1.

Q: Where do I find the **gross impact** of race and ethnicity on **pat down basis**?

A: See Table 20.

### 3.4.2 Net impact and other factors

Q: What do you mean by **net impact** of ethnicity or race?

A: The net impact of the race or ethnicity variable refers to the size of the connection between that factor and the outcome *after taking into account other factors*. It is a part of the connection that is unrelated to these other factors.

For example, if you are interested in the impact of being Black non-Hispanic on being in a bad vs. good stop. You start by describing the gross impact, the percent of Hispanics (4.83) vs. the percent White non-Hispanics (3.52) vs. the percent Black non-Hispanics (8.21) involved in bad stops.

*After taking into account other factors* means that the influence of each of these other factors (see below) on the Black variable, and on the outcome variable, has been removed. The link now reflects *only* the portions of each of these two variables – Black non-Hispanic and good vs. bad stop, that are *unrelated* to these other factors. Thus, “net impacts” are a part of the observed influence of the predictor on the outcome, which is unrelated to the other “controlled for” factors.

Q: What **other factors** do you take into account?

A: The police district in which the stop took place, and the gender and age of the civilian as well. Reasons for including these specific other factors appear in section 8.2.

Q: What are the implications of removing these other factors to examine net race impacts?

A: There are two broad implications. On the one hand, it helps focus on *only* the influence (effect/impact) of the isolated factor of race or ethnicity on the outcome being studied. This is *recommended social science best practice* in situations like this. On the other hand, race connects to these other factors so by removing these other factors we might be removing a substantial part of the race influence on the outcome. The analyses conducted here took steps to address this latter concern.<sup>5</sup> But more importantly, at least for the race impact on stop basis, the size of the net impact is comparable to the size of the gross impact. This suggests the latter issue was not a concern.

Q: Suppose your model had **expanded** the set of **other factors** that you took into account? Could that have changed the results shown here?

A: Yes it could. Statistical results shown here (see below on “statistical significance”) are specific to the predictors used in these models. Different models with different predictors could

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<sup>4</sup> When switching from one number times another to one number as a percent of another, we subtract 100 percent because if a number is 100 percent of another number it is the same number.

<sup>5</sup> More specifically, models were tested to see if the race variable depended on (interacted with) other possible demographic combinations (being young and black, being black and male, being young and black and male). Those models allowing race impacts on the outcome to depend on these other factors were not noticeably better. Further, geography, in the form of district differences, was examined and described.

have resulted in a statistically significant race effect shown here in some models (Table 16 for example) become non-significant.

Q: How **big was the net impact of race** on stop basis?

A: It was between four and six percent. In other words, after controlling for these other factors, the percentage of properly premised stops for Black non-Hispanic civilians was about four to six percent lower when compared to the percentage for White non-Hispanic civilians stopped. This is shown by the line in each panel in Figure 4. These numbers are for investigatory (coded based on the presence or absence of RAS) stops only. (Generally, stops coded as probable cause stops were dropped. Some analyses did include a particular type of probable cause stop, bicycle sidewalk violations, to see how that affected results.)

### 3.5 GEOGRAPHY AND STOP BASIS

Q: You took police district into account. Does this mean you **threw away** all the **geographic variation** in the outcome?

A: Not at all. That geographic variation was just put into a separate compartment, and the geographic compartment was split into two sub-compartments.

Q: Can you explain these two geographic sub-compartments?

A: One geographic sub-compartment shows the influence of geography that *relates* to age, gender, race and ethnicity of the stopped civilians, that is, is the factors we used to model the outcome (stop properly or improperly premised. The other sub-compartment is the part of the geographic variation that is *unrelated* to the factors used in our models.

Q: Where do I find the gross impact of geography that was due to the civilian factors you mentioned?

A: If you are interested in the results with just investigatory stops, look at Figure 8. The length of each bar refers to the portion of stops in that district that were predicted to be improperly premised, given the impacts of age, gender, race and ethnicity, and location on stop premise.

Q: In Figure 8, which district was predicted to be the best?

A: Districts 19 and 24 had fewer than three percent of their stops predicted to be improperly premised.

Q: In Figure 8, which district was predicted to be the worst?

A: District 3, where over ten percent of their stops were predicted to be improperly premised.

Q: Where do I find the gross impacts of geography that were not explained by the civilian factors you mentioned?

A: The geographic portion of the outcome not explained by model factors appears in Figure 9. Each district has a filled in circle. If the filled in circle for a district is **below the red line** it means that in that district, even after taking civilian age, race, ethnicity and gender into account, the **proportion of proper stops** in that district was **lower than overall**. If the filled in circle for a district is **above the red line** it means that in that district, even after taking civilian age, race, ethnicity and gender into account, the **proportion of proper stops** in that district was **higher than overall**.

Q: Why does each filled in circle have lines coming out of it?

A: Those lines take sampling error into account. After we consider that error, our best guess is that in the full set of investigatory stop records the true mean score for that district on that district is somewhere between where the upper line ends and the lower line ends.

Q: Are any of these district differences in Figure 9 meaningful?

A: They may be. Look at the two left-most district means, which are for Districts 10 and 3. The top end of their lines do not cross the red zero line. This means that if we were to repeat this sampling and analysis 100 times with 100 independent samples, 95 times out of 100 these two districts would have lower-than-average fractions of properly premised stops.

Q: So are you saying there may be something going on in Districts 10 and 3, based on Figure 9, that is unrelated to the civilian factors you used, that is resulting lower fractions of good investigatory stops in those districts?

A: We are.

Q: Do you know what is responsible?

A: We do not. It could be something about the district organization itself, something about the mix of people encountered on the street walking or driving, something about the mix of land uses or public transit in these districts, or some other factor. We just don't know.

### 3.6 STATISTICAL SIGNIFICANCE

Q: So you've explained a gross impact of race, and a net impact of race; what does a **statistically significant impact of race** mean?

A: It means that the net impact of race is not due to chance alone. Stated differently, when we infer from the sample finding, back to the full set of investigatory stops, and take sampling error into account, if a net race impact is statistically significant we are confident that the impact of race, after taking other factors into account, in the full set of records, is not zero.

Q: How confident are you?

A: We are confident that if we repeated these analyses with 100 independent random samples, and all sampling and analytic steps were the same, 95 times out of 100 our sample estimate of net race impact after taking sampling error into account would encompass only *non-zero* net impacts of race in the full set of records.

Q: So it sounds like you are thinking in three increasingly restrictive ways about impacts of race on the outcomes: any connection, any connection after taking other factors into account, and a connection after taking other factors into account that may be "true" in the full set of records.

A: Yes.

Q: If race has a statistically significant impact on stop basis, like it does here, does this mean that the race of the civilian encountered by the officer is *causing* the outcome?

A: In a social science framework, not necessarily. In social science, correlation does not always mean causation. Figuring out whether the impact might be causal, wholly or in part, requires additional social science steps not undertaken here.

### 3.7 PAT DOWN BASIS AND RACE

Q: Does civilian race link to whether the civilian experienced in improperly premised or “bad” pat down?

A: There is no statistically significant impact of civilian race on whether a bad pat down took place. But there seems to be a **noticeable gross** geographic connection between race and this outcome. See Figure 13. The predicted chances that the stop would involve a bad pat down are higher, about 3 percent rather than 2 percent, in districts where higher proportions of stopped civilians are Non-Hispanic Black. There are too few districts to allow for a meaningful test of a net connection at the district level.

### 3.8 LEGAL QUESTIONS

Q: How do these ways of thinking about race impacts connect to legal ideas about disparate race impact and disparate race treatment?

A: We don’t know. Those are legal determinations. We leave that to those with legal training, such as the Consultant, who will consider our findings along with all other relevant features of the data and the broader context of these assessments.

### 3.9 BOTTOM LINE

Q: What are the most important take away lessons?

A: In the authors’ view, there are three. *First*, the majority of investigative stops, somewhere around 90 percent, appear to be sufficiently premised or “good” stops. *Second*, stops of non-Hispanic Black civilians, compared to those of non-Hispanic White civilians, were less likely to be “good” stops. Thus, even though the fraction of “bad” stops is relatively small, there is racial patterning within that fraction. That is, there is a statistically significant difference by race on this outcome after controlling for other factors. *But bear in mind* that the significant net race effect depends on the type of model used and the set of stops included in that model. *Third*, “good” stops seem less likely in a couple of districts, Districts 3 and 10, for reasons that are not clear at this time.

## 4 EXECUTIVE SUMMARY

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This report analyzes a sample of ISR data from the period January through June 2016. These records were coded to determine the legal sufficiency of the stop itself, the legal sufficiency of a pat down if it occurred, and the legal sufficiency of a search if it occurred. The sample of records coded included equal numbers of non-Hispanic Black stopped civilians, Hispanic stopped civilians, and white non-Hispanic stopped civilians. The following factors were used in a statistical model predicting whether or not the stop itself was properly premised on reasonable articulable suspicion: race, ethnicity, age, gender, and district context. Stop premising was considered three different ways: with bicycle sidewalk violations meeting the probable cause standard excluded; with those same violations included but classified as improperly premised because the stop was based on probable cause rather than reasonable articulable suspicion relevant to investigation; and finally, with those same violations included but classified as properly premised because there was a reason for the stop even though that reason was not



investigatory. Except for the bicycle sidewalk violations as noted above, all other stops based on probable cause were excluded.

These models sought to learn whether race, unrelated to its links with other factors; and ethnicity, unrelated to its links with other factors, had significant **net** impacts on the outcomes in question. Those outcomes in question were: properly or improperly premised stop basis; properly or improperly premised pat downs; and properly or improperly premised searches. It turned out, however, that the third outcome presented such a small number of improper searches that models were not run.

**Proper or improper stop premises.** Finding a significant net impact of race depended on how the aforementioned bicycle sidewalk violations meeting the probable cause standard were treated. Results showed a statistically significant net impact of race ( $p < .05$ ) on stop premise with bicycle sidewalk violations excluded, and when those violations were included but classified as properly premised. But if probable cause bicycle sidewalk violations were treated as improperly premised investigatory stops no significant net impact of race appeared. To see how sturdy the significant net race impact was, models with the significant race effect were repeated using a different type of analytic approach. The significant net impact of race, however, failed to replicate with this different type of analytic model.

In models where there was a significant net race impact, gross race impacts on the outcomes were examined as well. These examinations do not seek to isolate the impacts **solely** associated with race or ethnicity or gender. These showed that stops which had the highest average predicted probability of being improperly premised were stops involving Black males.

Turning to geographic variation in the models with significant race impacts, some districts had a significantly higher portion of improperly premised stops after taking model factors into account (Districts 10, 3).

**Pat down premises.** Another outcome of interest was the sufficiency of the reasons given for a pat down, if such a pat down occurred. At the level of individual records, a net effect of gender surfaced. This suggested that although women were less likely to be patted down at all, if women were patted down their chances were higher than men's chances of being in an improperly premised pat down. This finding should not be leaned on too heavily, however, since it was based only on seven improperly premised pat downs of women. Descriptively, at the district level, a gross relationship between race and pat down premise appeared. Districts that had higher average predicted probabilities of improperly premised pat downs also had higher fractions of stopped civilians who were non-Hispanic Black. This is a very small difference, but noticeable.

**Search basis.** After removing custodial searches, there were too few searches lacking probable cause to allow any analysis of multiple factors determining whether searches were properly premised.

**Overall.** Results suggest the following

- A significant net impact of race on stop premising surfaces in some models.
- But whether this impact is significant or not depends on how the subset of probable cause stops examined here, bicycle sidewalk violations, are classified in terms of stop premising, and the type of analysis used.

- Models showing a significant net race impact align with the descriptive pattern of gross race impacts. Descriptive patterns based on these models showed that stopped non-Hispanic Black civilians were predicted by the models to have the highest chances of being in an improperly premised stop. When predicted probabilities, based on age, gender, race, ethnicity and district context are considered rather than raw data, the group predicted to be most likely involved in an improperly premised stop were Non-Hispanic Black civilians, especially if they were male (Figure 3).
- There may be an ecological link between good or bad pat downs and race. Districts with higher fractions of stopped civilians who were Black Non-Hispanic were districts where a higher fraction of stops involved bad pat downs.
- There are three important points of context. First, this significant net race effect seen on stop premising in two out of the three models occurred in a context where roughly 90 percent of stops appeared properly premised. Some might think this makes the net race impact small. Second, others might think it a testament to the race link that it occurred *even though* such a small fraction of stops were improperly premised. Third, the net race impact failed to prove significant when alternate single-level rather than multilevel analytics were used.
- A significant net impact of gender on proper pat down premising surfaced, with women more likely to be involved in an improper pat down. But this finding is built on only seven improperly premised pat downs, and thus should be interpreted with extreme caution.



## 5 PURPOSE

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This report examines a sample of investigatory stop reports (ISRs) generated by the Chicago Police Department during the period January-June 2016. In this sample, three different races/ethnicities are equally represented: Black Non-Hispanic, White Non-Hispanic, and Hispanic stopped civilians. The narrative fields in these stops have been coded to reflect the propriety of the legal premises of three actions: the stop taking place; a pat down, if it took place; and a search, if it took place. The first two outcomes are legitimate as investigatory procedures if based on reasonable articulable suspicions, as specified in the narrative fields of the ISRs completed by the officers. The third is legitimate if based on probable cause, as specified in the same way. So for each outcome, this report examines the rate at which each of these three outcomes was properly premised, or improperly premised, given legal considerations.

At the outset probable cause stops were excluded because they were not investigatory stops, and the focus of The Agreement is on police investigative stop protocols. Nevertheless, to explore the implications of bounding and classifying such probable cause stops, and how that bounding and classification might affect the outcome, as an illustration one specific type of stop was treated in different ways. Those stops involved officers notifying civilians over the age of 12 riding bicycles on sidewalks that doing so was a municipal violation. There were 10 stops involving bicycles on sidewalks that were investigative in nature, but there were 66 that were probable cause stops to notify civilians of their lawbreaking. So analyses were done three ways. Bicycle sidewalk violations could be included but classified as improperly premised because they were not investigatory in nature. Or, they could be included but classified as properly premised because the officers had a reason for making some type of stop. Or they could be excluded.

If models indicated significant net race impacts of race or ethnicity on the propriety of stop premises surfaced under one of these three bicycle coding arrangements, further explorations were conducted with that model. More specifically, geographical residual variation was explored to learn whether some districts, even after taking account of the determinants of stop premising had been taken into account, deviated significantly; and, predicted gross differences on the outcome, depending on ethnoracial and gender combinations, were described.

This report only addresses how race/ethnicity connect with the legal premises of the actions examined. This report will not address how race/ethnicity connect to the *occurrences* of stops, or pat downs, or searches. Those linkages receive attention in a separate report on post stop outcomes.

## 6 METHODOLOGY

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### 6.1 SAMPLING STRATEGY

From the full set of ISRs for the period, three random samples were pulled: one for each of the three key racial/ethnic groups. Simple random sampling was used. Each group was sampled at a rate to provide 1,800 sampled records for each group for the period January-April. When data

became available for May-June, additional records for each race/ethnicity were sampled, using the same sampling ratios as were used in January-March.

For each race/ethnicity, the random sample was further sampled, taking a random 50 percent sample of each. These 50 percent subsamples were then joined together so that records for all three races/ethnicities could be analyzed.

Given the samples drawn, none of the results here apply to any other racial/ethnic groups not examined (e.g., differences between Asian and White Non-Hispanic stopped civilians).

## 6.2 IMPLICATIONS FOR WEIGHTED AND UNWEIGHTED ANALYSES

The equal size subsamples for each race/ethnicity maximize the statistical power of analyses examining race/ethnicity differences. Descriptive information is usually presented for unweighted data, with roughly equal numbers of stops in each of the three racial/ethnic groups. Statistical models are conducted with weighted data. Therefore, patterns of statistical significance from the models indicate whether an impact observed with the sample likely applies as well to the full population of records.

## 6.3 CODING

The 50 percent subsamples were coded. Coding categories appear in Appendix A.

## 6.4 A PRIORI POWER ANALYSES

A priori statistical power analyses showed that with at least 1,800 records, a difference in proportions of five percent would have slightly better than 80 percent statistical power. This is considered an acceptable level of statistical power in many fields (Cohen, 1992). Statistical power analyses specific to the multivariate and mixed effects models conducted here were not estimated.

## 6.5 OUTCOMES

The three outcomes examined are:

- Was the stop properly premised on reasonable articulable suspicion (RAS) factors?
- Was the pat down, if it occurred, properly premised on reasonable articulable suspicion (RAS) factors?
- Was the search, if it occurred, properly premised on probable cause?

# 7 DESCRIPTIVE STATISTICS

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Descriptive statistics for predictor variables and binary outcomes appear in Table 1. Descriptive statistics for the pat down outcome appear in Table 2. Search outcome descriptive statistics appear in Table 3. Specific outcome variables are explained later as they are introduced.

Table 1. Descriptive statistics: Predictors and binary outcome variables

7.1.1 Predictor variables		N	Min	Max	Mean	SD	Median
Black	dblack	3,376	0	1	0.709	0.454	1
Hispanic	dhispanic	3,376	0	1	0.214	0.410	0
Male	dmale	3,376	0	1	0.869	0.337	1
Age	age2	3,376	7	100	28.823	13.276	24
Age (Centered)	c_age2	3,376	-22.550	70.450	-0.727	13.276	-5.550
District 1	dist01	3,376	0	1	0.016	0.125	0
District 2	dist02	3,376	0	1	0.042	0.201	0
District 3	dist03	3,376	0	1	0.071	0.257	0
District 4	dist04	3,376	0	1	0.076	0.265	0
District 5	dist05	3,376	0	1	0.034	0.182	0
District 6	dist06	3,376	0	1	0.039	0.193	0
District 7	dist07	3,376	0	1	0.076	0.265	0
District 8	dist08	3,376	0	1	0.064	0.245	0
District 9	dist09	3,376	0	1	0.078	0.269	0
District 10	dist10	3,376	0	1	0.076	0.266	0
District 11	dist11	3,376	0	1	0.110	0.312	0
District 12	dist12	3,376	0	1	0.037	0.189	0
District 14	dist14	3,376	0	1	0.014	0.119	0
District 15	dist15	3,376	0	1	0.059	0.235	0
District 16	dist16	3,376	0	1	0.023	0.151	0
District 17	dist17	3,376	0	1	0.016	0.127	0
District 18	dist18	3,376	0	1	0.019	0.136	0
District 19	dist19	3,376	0	1	0.023	0.151	0
District 20	dist20	3,376	0	1	0.019	0.136	0
District 22	dist22	3,376	0	1	0.017	0.129	0
District 24	dist24	3,376	0	1	0.042	0.201	0
District 25	dist25	3,376	0	1	0.048	0.214	0
Binary outcome variables							
Stop properly premised v. 1	stopsuff3	3,376	0	1	0.927	.261	1
Stop properly premised v. 2	stopsuff4	3,376	0	1	0.930	0.255	1
Additional information							
Pat down occurred	dpat	3,376	0	1	0.347	0.476	0
Search occurred	dsearch	3,376	0	1	0.150	0.357	0

Note. Stop premise v. 1 treats bicycle sidewalk probable cause violations (n=66) as improperly premised stops; v. 2 treats those as properly premised stops. Both versions treat stops lacking RAS factors as improperly premised. Unweighted data. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 2 Descriptive statistics: Pat down premising (pat\_ras3) with bicycle sidewalk violations included

Code	Category	N	Percent
1	Properly premised	955	28.29
2	Improperly premised	80	2.37
3	No pat down	2341	69.34
Total		3376	100

Note. Unweighted data. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 3 Descriptive statistics: Search sufficiency basis

Code	Category	N	Percent
0	Properly premised	218	6.46
1	Improperly premised	16	0.47
2	Custodial	385	11.4
.	No search	2,743	81.25
.i	Insufficient information	14	0.41
Total		3,376	100

Note. Unweighted data. Source: Jan.-Jun. 2016 legal narratives equal race sample.

## 8 RESULTS: STOP PREMISING

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This section considers the determinants of whether the stop was properly premised on reasonable articulable suspicion factors (RAS), or not. The approach starts just by examining the connection between the outcome and racial/ethnic combinations. These provide clues to gross race and ethnicity connections with the outcome without taking additional factors into account. Later models then introduce those additional factors so the *net* impact of race, ethnicity, and gender can be gauged.

Table 4 below shows the distribution of race/ethnicity for the coded ISRs. 17 inappropriately duplicated sampled ISRs have been removed.

Table 4 Numbers and percent in sample by race/ethnicity

White NH	1,416	33.45
Hispanic	1,394	32.93
Black NH	1,423	33.62
<b>Total</b>	<b>4,233</b>	<b>100.00</b>

Note. White NH = White Non-Hispanic; Black NH = Black Non-Hispanic. Source: Jan.-Jun. 2016 legal narratives equal race sample. All sampled and coded records included. Unweighted data.

The table shows the number of ISRs for each of the three racial/ethnic groups: White Non-Hispanic, Hispanic, and Black Non-Hispanic. Each group contributed, as planned with the sampling design, about a third of the coded records.

In these 4,233 records there were 857 stops (20.2 percent) classified as probable cause stops. These were dropped so the focus could be exclusively on investigatory stops, except as noted below.

Close examination was made of stopping civilians for sidewalk bicycle riding. 76 stops included both “BICYCLE” and “SIDEWALK” in the narrative fields. Almost all of these involved a person over 12 years of age riding a bicycle on a sidewalk. One involved private contractors removing a bicycle rack on the sidewalk. Of these 76, 66 were probable cause stops and 10 were investigatory stops. As explained above, analyses were repeated treating these bicycle sidewalk violations three different ways.

The distribution on race/ethnicity is shown in Table 5 with probable cause stops, save the 66 bicycle sidewalk violations, excluded.

Table 5 Numbers and percent in sample by race/ethnicity: Investigatory stops only

Racial/ethnic combination	N	Percent
White NH	1,134	33.59
Hispanic	1,142	33.83
Black NH	1,100	32.58
<b>Total</b>	<b>3,376</b>	<b>100</b>

Note. Unweighted data. White NH = White Non-Hispanic; Black NH = Black Non-Hispanic. Probable cause stops dropped. Bicycle on sidewalk stops included. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 6 shows the distribution of cases to RAS codes with probable cause stops, save the 66 bicycle sidewalk violations, excluded.

In the sample, 92.6 percent (3,128/3,376) of the ISR stops were properly premised on reasonable articulable suspicions (code 0).

The next largest group of records were those 99 (2.9 percent) where the narratives captured insufficient information to make a determination about stop basis (code=7). Another four records had a different insufficiency code, .i, making the total number of records in this group 103 (3.1 percent).

The next largest group of 66 records (1.95 percent) were instances of police stopping individuals who were over the age of 12 but riding bicycles on public sidewalks in violation of municipal code.

The next largest group were records where no criminal activity appeared to be underway or planned (“afoot”); 57 records (1.7 percent) were classified improperly premised on these grounds.

All of the other codes, individually, were applied to less than one percent of the reviewed records. Among these, the only code applied to more than ten records was the judgement (code=11, 13 cases) that there was no basis whatsoever for a Terry investigatory stop.

*Table 6. Assessment of stop premises: Investigatory stops only*

	N	Percent
0. RAS sufficient	3,128	92.65
1. Bicycle on sidewalk	66	1.95
2. Time/distance too attenuated	2	0.06
4. Hunch not personal observation	5	0.15
7. Not enough facts	99	2.93
8. Fleeing or avoidant subject only	2	0.06
9. No criminal activity afoot	57	1.69
11. No basis for Terry or PC stop	13	0.39
.i (insufficient information)	4	0.12
Total	3,376	100

Note. Unweighted data. Bicycle on sidewalk stops included. Probable cause stops dropped.  
Source: Jan.-Jun. 2016 legal narratives equal race sample.

According to the municipal code in Chicago, individuals over the age of 12 are not permitted to ride bicycles on sidewalks. These cases here include one instance where a bicycle rider hit a pedestrian (ISR 6174) and another instance (ISR 82496) where a person riding a bicycle on a public sidewalk was “approaching several unknown subjects and engaging in short conversations at a location that has been subjected to numerous civilian complaints regarding narcotic activity and multiple arrests pertaining to such.”

If these bicycle sidewalk violation stops are classified as properly premised because they meet a higher probable cause standard, being a clear violation of municipal code, the percent of stops in the sample that were properly premised rises to 94.6 percent. See Table 7. If the bicycle sidewalk violations are classified as improperly premised because they are not properly premised as *investigative* stops, the percent of stops properly premised is, as shown in Table 6, 92.6 percent.

Table 7 Assessment of stop premises: Investigatory stops only, bicycle sidewalk violations included and coded as RAS sufficient

	N	Percent
0. RAS sufficient	3,194	94.61
2. Time/distance too attenuated	2	0.06
4. Hunch not personal observation	5	0.15
7. Not enough facts	99	2.93
8. Fleeing or avoidant subject only	2	0.06
9. No criminal activity afoot	57	1.69
11. No basis for Terry or PC stop	13	0.39
.i	4	0.12
Total	3,376	100

Note. Unweighted data. Bicycle on sidewalk stops included. Probable cause stops dropped.  
Source: Jan.-Jun. 2016 legal narratives equal race sample.

If the 66 bicycle on sidewalk violations that met the probable cause standard are removed altogether, 3,310 records remain. For these records, the distribution on stop premises appears in Table 8.

Table 8 Assessment of stop premises: Investigatory stops only, 66 bicycle sidewalk violations removed

	N	Percent
0. RAS sufficient	3,128	94.5
2. Time/distance too attenuated	2	0.06
4. Hunch not personal observation	5	0.15
7. Not enough facts	99	2.99
8. Fleeing or avoidant subject only	2	0.06
9. No criminal activity afoot	57	1.72
11. No basis for Terry or PC stop	13	0.39
.i	4	0.12
Total	3,310	100

Note. Unweighted data. Bicycle on sidewalk stops excluded. Probable cause stops dropped.  
Source: Jan.-Jun. 2016 legal narratives equal race sample.

In order to present this outcome in a more condensed format, a summary stop premise variable (**stopsuff4**) was constructed with just two values, 0 and 1. A value of 1 means that the stop was properly premised (code = 0 in Table 6); the narrative revealed reasonable articulable suspicion. A value of 0 collapses all the other codes (Table 6, or Table 7 or Table 8 depending on the analysis) suggesting the stop was improperly premised. The 66 bicycle sidewalk violations rising to the probable cause standard are classified here as RAS sufficient.

So here

- 1 = stop had sufficient RAS (code 0 in Table 6)
- 0 = stop premised neither on reasonable suspicion nor on probable cause (codes 2 and higher in Table 6 or Table 7 or Table 8 depending on the analysis)

Another version of this variable (**stopsuff3**) was the same as the preceding variable, except that here the 66 bicycle sidewalk violations were classified as *improperly premised* because they were not investigatory stops.

The relationship between these two versions of the stop premise outcome variable appears in Table 9.



Table 9 Propriety of stop premises: relationship between two coding approaches to outcome

		Propriety of stop premises: bicycle sidewalk violations included and treated as proper (stopsuff4)		Total
		Improperly premised (0)	Properly premised (1)	
Propriety of stop premises: bicycle sidewalk violations included and treated as improper (stopsuff3)	Improperly premised (0)	182	66	248
	Properly premised (1)	0	3,128	3,128
Total		182	3,194	3,376

Note. Unweighted data. Probable cause stops dropped. Source: Jan.-Jun. 2016 legal narratives equal race sample.

### 8.1 RACE AND ETHNICITY: DESCRIPTIVE PATTERNS OF GROSS IMPACTS

Table 10 shows the counts and proportions of properly premised stops, and improperly premised stops, by race/ethnicity combinations. These appear under three arrangements. In the top portion of the table bicycle sidewalk violations meeting the probable cause standard are included but classified as improperly premised because they are not investigatory stops. In the middle portion of the table those same bicycle sidewalk violations are included but are now treated as properly premised because the officers had a reason, albeit not an investigatory one, for making the stop. In the bottom portion of the table bicycle sidewalk violations meeting the probable cause standard (n=66) are removed from the calculations.

*Please note that the figures in the total column, on the right hand side of the table, apply only to the equally weighted sample. We describe below the totals that apply to the entire set of stops, with each of the three groups weighted proportional to their contribution to the total set of stop records for these groups.*

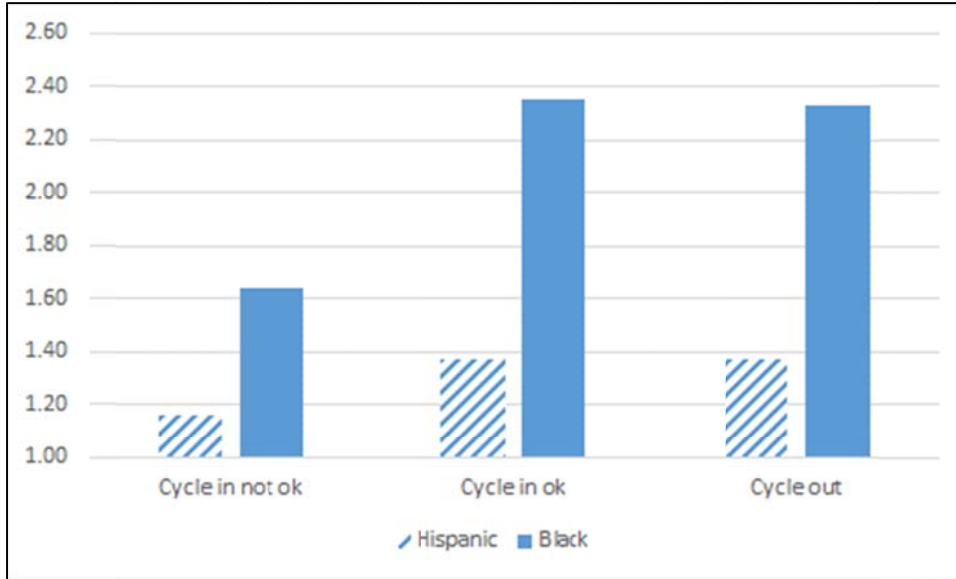
The differentials in improperly premised stop rates also are depicted graphically in Figure 1. Two points are clear

- **How the probable cause sidewalk violations are handled has a noticeable impact on the size of the disparities across ethnoracial groups.** The disparities relative to the white improperly premised rate are clearly lower if bicycle sidewalk violations get included as improperly premised, and higher if those same stops are included as properly premised, or excluded.

- **In addition, the disparities relative to the white stop properly premised rate is greater when Black Non-Hispanic civilians are contrasted with White Non-Hispanic civilians than it is when Hispanics are contrasted with Whites.**

The relevant detailed numbers appear in Table 10.

*Figure 1 Improperly premised stop rate, relative to white improperly premised stop rate, for Hispanic and Black Non-Hispanic civilians, under three treatments of bicycle sidewalk violations.*



Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 10 Comparing counts and proportions of improperly premised stops across three race/ethnic groups

66 Bicycle/sidewalk in but improper premises for investigatory purposes

	White NH	Hispanic	Black NH	Total
N				
Improper (0)	66	77	105	248
Proper (1)	1,068	1,065	995	3,128
Total	1,134	1,142	1,100	3,376
Percent				
Improper (0)	5.82%	6.74%	9.55%	7.35%
Proper (1)	94.18%	93.26%	90.45%	92.65%
Total	100.0%	100.0%	100.0%	100.0%

Ratio Improperly premised percent relative to White percent Improperly premised

	1.16	1.64
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66 Bicycle/sidewalk in & properly premised

Stop premises	White NH	Hispanic	Black NH	Total
N				
Improper (0)	39	54	89	182
Proper (1)	1,095	1,088	1,011	3,194
Total	1,134	1,142	1,100	3,376
Percent				
Improper (0)	3.4%	4.7%	8.1%	5.4%
Proper (1)	96.6%	95.3%	91.9%	94.6%
Total	100.0%	100.0%	100.0%	100.0%

Ratio Improperly premised percent relative to White percent Improperly premised

	1.37	2.35
--	------	------

66 Bicycle/sidewalk excluded

Stop premises	White NH	Hispanic	Black NH	Total
N				
Improper (0)	39	54	89	182
Proper (1)	1,068	1,065	995	3,128
Total	1,107	1,119	1,084	3,310
Percent				
Improper (0)	3.5%	4.8%	8.2%	5.5%
Proper (1)	96.5%	95.2%	91.8%	94.5%
Total	100.0%	100.0%	100.0%	100.0%

Ratio Improperly premised percent relative to White percent Improperly premised

	1.37	2.33
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Note: Do NOT interpret total column as representative of entire population of non-probable cause stops. See Table 11. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Of course the results in Table 10 do not reflect the full population of non-probable cause (investigatory) stops. This is because the relative representation of each of the three ethn racial groups in the sample does not match the proportions of each of those groups in the full set of investigatory stops. In order to apply results with the sample to the full population of investigatory stops, the cases in the sample need to be appropriately weighted so that each group is proportionally represented.

In effect this means “counting” each Black non-Hispanic record in the sample as more than one record because this group’s relative representation in the sample, just a third, is far smaller than its representation in the full set of investigatory stops, which is about 71 percent. So each Black non-Hispanic record counts for 2.15 records in the full set of investigatory records.

The reverse is the situation for White non-Hispanics. In the sample they are a third, but they make up about eight percent in the full set of records. So each White non-Hispanic record is counted as equivalent to about a quarter of a record (weight=.23) in the full set of investigatory records.

Hispanics, like White non-Hispanics, are over counted in the equal race sample. Making up a third of the equal race sample, they are only about 21 percent of the full set of investigatory stops. So each Hispanic record is counted as equivalent to about two thirds (weight=.62) of a record in the full set of investigatory stops.

Once these weights are “turned on,” we can estimate the fraction of properly and improperly premised stops in the full set of investigatory stops.

One additional point merits mention before getting to the bottom line. When extrapolating from the sample back to the full set of records, uncertainty must be added in because of sampling error. Sampling error is captured with the upper and lower bounds of the confidence interval. These intervals inform us that, according to sampling theory, if the entire sampling procedure were to be independently replicated 100 times with comparable data, 95 times out of 100 we estimate that the “real” mean or proportion for the full set of records would lie within that interval.

Looking at the last two columns of Table 11 reveals the following. Although the numbers vary depending on which of the three scenarios are investigated,

- **The estimated proportion of properly premised non-probable cause stops ranges from 90 percent to 94 percent.**
- **The estimated proportion of properly premised investigatory stops ranges from 6 to 10 percent.**
- **Although the estimated proportion of improperly premised stops varies somewhat depending on the inclusion and coding scenario used, the confidence intervals overlap meaning the estimates are essentially equivalent.**
- **The same holds for the estimated proportion of properly premised stops.**

Here are the details.

If bicycle/sidewalk violations are included but viewed as improper for investigative purposes, the percent of good, properly premised stops using weighted data is **91.3 percent**. Taking sampling error into account our best guess is that the true percent, when considering all investigatory stops, is between **90.4 and 92.3 percent**.

The percent for bad or improperly premised stops is **8.7 percent**, and our best estimate is between **7.7 and 9.6 percent**.

In the middle portion of the table, if bicycle/sidewalk violations are included and treated as properly premised stops, the percentage of properly premised or good stops using weighted sample data is **93 percent**. Taking sampling error into account our best guess for the “true” percentage of good stops for all investigatory stops is between **92.1 and 93.9 percent**.

The proportion of bad or improperly premised stops is **7 percent** and the best estimate taking sampling error into account is between **6.1 and 7.9 percent**.

In the bottom portion of the table with bicycle/sidewalk violations excluded, the weighted sample percentage is **92.9 percent** and the best guess for the “true” percent of good or properly premised stops in the full set of investigatory records is between **92 and 93.8 percent**.

For bad or improperly premised stops the weighted mean is **7.1 percent**. The best estimate for percent bad or improperly premised stops after taking sampling error into account is between **6.3 and 8 percent**.

Table 11 Comparing counts and proportions of improperly premised stops across three race/ethnic groups: Weighted sample

Stop premises	66 Bicycle/sidewalk in but improper premises for investigatory purposes				95% LCL	95% UCL
	WHITE NH	HISPANIC	BLACK NH	TOTAL		
	N					
Improper (0)	15.24	48.61	228.44	292.29		
Proper (1)	246.65	672.37	2164.70	3083.71		
Total	261.89	720.98	2393.13	3376.00		
	Percent					
Improper (0)	5.82	6.74	9.55	8.66	7.71	9.61
Proper (1)	94.18	93.26	90.45	91.34	90.39	92.29
Total	100	100	100	100		
	Ratio Improperly premised percent relative to White percent Improperly premised					
		1.16	1.64			
	66 Bicycle/sidewalk in & properly premised					
	N					
Improper (0)	9.01	34.09	193.63	236.72		
Proper (1)	252.88	686.89	2199.51	3139.28		
Total	261.89	720.98	2393.13	3376.00		
	Percent					
Improper (0)	3.44	4.73	8.09	7.01	6.15	7.87
Proper (1)	96.56	95.27	91.91	92.99	92.13	93.85
Total	100	100	100	100		
	Ratio Improperly premised percent relative to White percent Improperly premised					
		1.38	2.35			
	66 Bicycle/sidewalk excluded					
	N					
Improper (0)	8.98	33.98	193.02	235.98		
Proper (1)	245.87	670.25	2157.89	3074.02		
Total	254.85	704.24	2350.91	3310.00		
	Percent					
Improper (0)	3.52	4.83	8.21	7.13	6.25	8.01
Proper (1)	96.48	95.17	91.79	92.87	91.99	93.75
Total	100	100	100	100		
	Ratio Improperly premised percent relative to White percent Improperly premised					
		1.37	2.33			

Note. Results based on weighted data, so the proportion of records for each of the three ethn racial categories matches their proportions in the sample with probable cause stops removed. Last two columns show the upper and lower bounds of the 95 percent confidence interval. Numbers of cases are not integers because these are weighted counts. Source: Jan-Jun 2016 legal narratives equal race sample.

## 8.2 WHICH ADDITIONAL FACTORS BEYOND RACE AND ETHNICITY AND GENDER SHOULD BE TAKEN INTO ACCOUNT?

1. District context. Each district presents its own complex of crimes, disorder problems, and populations using the streets in the district. Within one police department, partly in response to the above, district cultures can vary (Klinger, 1997).
2. Gender requires attention. (a) The agreement directs attention to gender, and females do get stopped. In the sampled investigatory stops, the bulk of stopped civilians (2,840 or 84 percent) are male, but the sample includes 536 women (16 percent). (b) Further, gender is linked to race/ethnicity.<sup>6</sup> Whereas 21 percent of White Non-Hispanic investigatory stops were of women, the corresponding percentage was 14.8 for Hispanic investigatory stops and 11.7 for Black Non-Hispanic investigatory stops. To get at the net race/ethnicity connection with stop premise disadvantage, gender must be taken into account. (c) Further, gender has an overall relationship with stop premise sufficiency.<sup>7</sup> Whereas 7.8 percent of investigatory stops of males were improperly premised, the corresponding percentage for females was 5.0 percent. (d) Finally, gender may be relevant to the outcome in a particular combination. Given intersectionality theory (Fader & Traylor, 2015), one might expect Black Non-Hispanic women to be at particular risk of an improperly premised stop.
3. Given results in other jurisdictions finding younger Black males more at risk of discretionary searches (Rosenfeld, Rojek, & Decker, 2012), one might anticipate that younger Black males are more at risk of improperly premised investigatory stops.
4. Civilian age is relevant. Given the age-crime curve (Laub & Sampson, 2003), officers might pay closer attention to younger civilians on the street. Alternatively, older people might stand out as more suspicious at certain places and times.

## 8.3 MODELING APPROACH

Mixed effects logit models (melogit) conducted in Stata 15 control for district context (Rabe-Hesketh & Skrondal, 2012). Given recent concerns about mixed models with small numbers of level 2 units (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016), the final model will be repeated with a single level logit model, controlling for district using fixed effects with District 1 (The Loop) as the reference string.

## 8.4 DECIDING WHICH SPECIFIC MODEL IS THE “BEST” MODEL

Considerable discussion among scholars and activists raises the possibility that particular combinations of demographic factors prove influential for the outcomes under consideration in this report. For example on the basis of intersectionality theory (Fader & Traylor, 2015) one could argue that Black women are particularly at risk. On the basis of driving while black literature one could argue that young black males are particularly at risk.

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<sup>6</sup> LR  $\chi^2$  (df=2) = 36.87; p < .001.

<sup>7</sup> LR  $\chi^2$  (df=1) = 5.5; p < .05.

Following up on this suggestion requires examining models which add, after taking into account the main effects of age, gender, race, ethnicity, and the random effects of district context, additional interaction effects.

Therefore, for this outcome of stop sufficiency, the following series of models were run.<sup>8</sup>

- A. A null or ANOVA model determines whether average scores on the outcome differ significantly across districts. Does district context matter?
- B. Age and gender main effects are added jointly.
- C. Race (black=1) and ethnicity (Hispanic=1) are added to learn whether race and ethnicity result in a markedly better fitting model will controlling for model complexity.
- D. A two way interaction of female x Black indicates whether this race/gender combination links to the outcome.
- E. To set up for testing the three way interaction (Black and young and male), a model with the constituent two way interactions is run.
- F. The three way interaction is added to see if fit while controlling for complexity improves markedly.

When comparing models against one another, the Bayesian Information Criterion (BIC) is the preferred metric for choosing a “better” model (Raftery, 1995). A substantially lower BIC suggests that the model with the lower BIC provides a better fit to the data, while simultaneously controlling for model complexity. Drops of at least 2, 6, and 10 provided, respectively, positive, strong, and very strong evidence of a better model.

Results from the ANOVA model appear in Table 12. The significant likelihood ratio chi squared values confirm that district context should be taken into account.

Results comparing other models in the series are shown in Table 13. The following points emerge. The models adding a specific two way interaction of gender and race, either the male X Black interaction or the female X Black interaction, worsened fit while controlling for model complexity. BIC values went up substantially. Similarly, a model with all two way interactions relevant to the young x Black x male interaction also produced less fit while controlling for complexity, compared to the main effects model. BIC values went up quite substantially. Finally, the three way young x Black x male interaction does not improve model fit compared to model with the constituent main effects and two way interactions already included.

The upshot is simple. The model with only main effects for age, gender, race, and ethnicity, and controlling for district context will be used.<sup>9</sup> This is true for all three treatments of bicycle sidewalk violations

Each model in the series was run with weighted data. In each case models were run three times.

- Once with bicycle sidewalk violations included but considered properly premised.
- Once with bicycle sidewalk violations included but considered improperly premised.
- Once with bicycle sidewalk violations excluded.

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<sup>8</sup> Time did not permit examining the interaction question with other outcomes.

<sup>9</sup> Although these same tests were not conducted for the other outcomes, main effects models are used there as well for consistency in modeling across different outcomes.



Table 12. Null mixed effects logit model: Stop sufficiency

**Weighted data, bicycle sidewalk violations included and treated as IMproperly premised**

	B	SE	OR	95% CI of OR	
				LCL	UCL
<b>Fixed effects</b>					
Constant	2.696	0.142	14.827	11.226	19.584
Proportions			0.937	0.918	0.951
<b>Random effects</b>					
	Variance	SE of variance			
District	0.258	0.138			

LR  $\chi^2$ (df=1) vs. logistic model: = 19.65; p < .001  
n = 3,376

**Weighted data, bicycle sidewalk violations included and treated as properly premised**

	B	SE	OR	95% CI of OR	
				LCL	UCL
<b>Fixed effects</b>					
Constant	2.766	0.140	15.892	12.088	20.892
Proportions			0.941	0.924	0.954
<b>Random effects</b>					
	Variance	SE of variance			
District	0.212	0.123			

LR  $\chi^2$ (df=1) vs. logistic model: = 16.05; p < .001  
n = 3,376

Table 12, continued

Weighted data, bicycle sidewalk probable cause violations included

	B	SE	OR	95% CI of OR	
				LCL	UCL
Fixed effects	2.751	0.141	15.665	11.890	20.637
Constant			0.940	0.922	0.954
Proportions					
Random effects					
	Variance	SE of variance			
District	0.218	0.125			
LR $\chi^2$ (df=1) vs. logistic model: = 16.42; p < .001					

n = 3,310

Note. Outcome: 0 = stop improperly premised; 1 = stop properly premised on reasonable articulable suspicion. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 13 BIC values different models

Model	Bicycle sidewalk violations					
	Included – NOT ok		Included-ok		Excluded	
	BIC	BIC Δ	BIC	BIC Δ	BIC	BIC Δ
Null (random effects for districts only)	1,964.4		1,696.53		1,688.05	
+ race and ethnicity	1,974.0		1,699.40		1,691.31	
+ age and gender (Full main effects model)	1,972.9		1,710.48		1,701.48	
+ interaction (male x Black )	1,980.9	8.0	1,718.49	8.01	1,709.47	7.99
Full main effects model + 2 way interactions (young, male, Black )	1,993.6	20.7	1733.125	22.65	1723.87	22.39
Above + 3 way interaction	1,996.0	2.4	1,735.47	2.35	1,726.27	2.40

Note run=117. Weighted data. Source: Jan.-Jun. 2016 legal narratives equal race sample.

## 8.5 PREDICTED PROBABILITIES BASED ON MODEL RESULTS

Once the statistical model is run, each case in the sample has a predicted probability that *that* stop, based on the factors used in the model, is properly premised. This indicates the *predicted* likelihood, between 0 and 1, that the stop in question was properly premised. Each case's score

on the predictors in the model, combined with the parameters from the model, generate these predicted probabilities. A *higher* predicted probability means, according to model results, a *greater* likelihood that the investigatory stop was properly premised.

*To repeat, these differences in predicted probabilities by race, ethnic groups and gender inform us of overall or **gross** race, gender, and ethnicity effects based on the contributions of all the factors considered by the model. A later investigation of marginal probabilities illuminates **net** racial, gender and ethnic effects, controlling for other factors.*

Since there are only two outcomes, one minus these predicted probabilities reflects the predicted chances that stops were *improperly premised*. Of interest will be the differences, between gender-and-race/ethnicity-based groups, in these predicted probabilities of an improperly premised investigatory stop.

**The predicted probabilities based on model results will be presented only under the bicycle scenarios that result in a significant net impact of race. If the results show no net significant impact of race, predicted probabilities are not pursued.**

#### 8.5.1 Net race impacts

Net race impacts get presented under the three different bicycle sidewalk violation scenarios: included and treated as improperly premised investigatory stops; included and treated as properly premised stops; and excluded.

**Whether a significant net race impact shows depends on which of the bicycle scenarios are being examined.**

##### 8.5.1.1 *Bicycle sidewalk violations included, treated as improperly premised*

Table 14 shows the results of a model with only main effects. Bicycle sidewalk violations are included as improperly premised. The odds ratio for race suggests that controlling for other factors, Black non-Hispanic civilians' expected odds of having a [*properly* premised stop vs. an improperly premised stop] are about  $(1-.639)=.361$  lower, or 36.1 percent *lower*.

This impact of race, however, is not statistically significant either with a two tailed or even a more generous one tailed test; in both cases  $p > .05$ . Nor is it significant in the model with only race and ethnicity entered as predictors (results not shown). With this set of investigatory stops, there is no suggestion of a net race impact on stop premise sufficiency after taking district context into account.

Table 14. Main effects model predicting sufficient stop premise: Bicycles on sidewalk included and treated as IMproperly premised

Variable	Variable name	B	SE	OR	z	p <	OR: 95 % confidence interval	
							LCL	UCL
<b>Fixed effects</b>								
Black non-Hispanic (= 1; white non-Hispanic = 0)	dblack	-0.448	0.292	0.639	-1.53	ns	0.361	1.133
Hispanic (= 1; white non-Hispanic = 0)	dhispanic	-0.099	0.318	0.906	-0.31	ns	0.486	1.688
Female (=1; 0 = male)	dfemale	0.560	0.231	1.751	2.43	.05	1.115	2.752
Age (centered by sample mean)	c_age2	-0.014	0.004	0.986	-3.2	.01	0.977	0.994
	Constant	2.825	0.296	16.859			9.444	30.097
<b>Random effects</b>								
	District		0.204	0.115			0.067	0.616

LR  $\chi^2(df=1)$  vs. logistic model: = 18.40;  $p < .001$

Note. Outcome = stop sufficiently premised (=1) or not (=0). Weighted data. Bicycles on sidewalk included.  $n = 3,376$ . Source: Jan.-Jun. 2016 legal narratives equal race sample. Probabilities are two tailed. Results from mixed effects logit model, investigatory stops grouped by police districts.

Age and gender each significantly influence stop premise sufficiency. Women are *more* likely to be in a *sufficiently* premised stop ( $p < .05$ ), but *older* stopped civilians are *less* likely ( $p < .01$ ).<sup>10</sup> The suggestion is that gender and age each influence stop premise sufficiency in the full sample when bicycle sidewalk violations are included.

Table 15 shows the predicted probabilities based on the factors shown in the above table, and including district context. Figure 2 shows the differences graphically.

<sup>10</sup> Since age goes up to 100, this model was repeated with only 79 or younger, and again with only those 69 or younger. The significance pattern for age did not change, and the OR for age was unchanged for the first two decimal places.

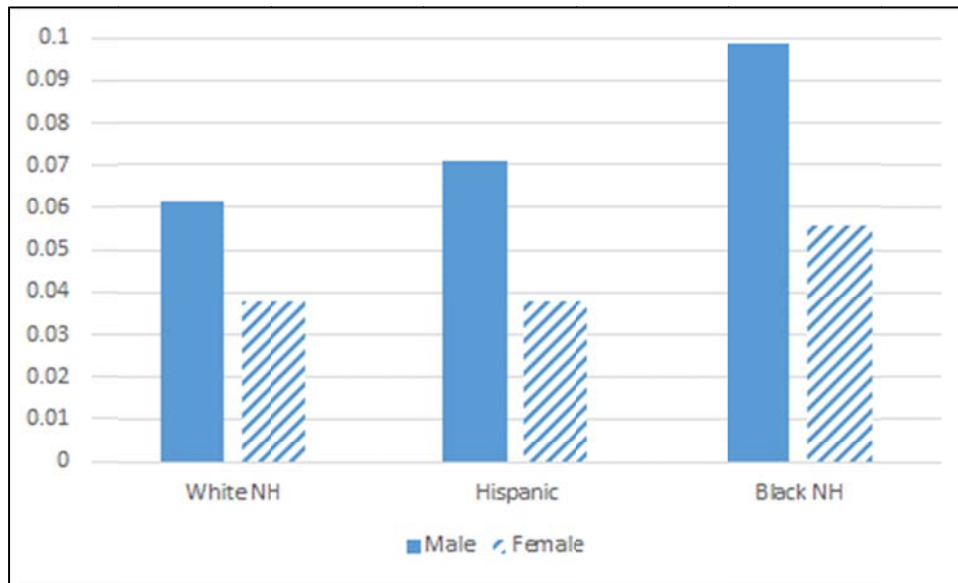
Table 15 Predicted probabilities for stop premises by gender and race/ethnicity: Bicycle sidewalk violations included and treated as IMPROPERLY premised

Predicted probability stop properly premised				
Gender	White NH	Hispanic	Black NH	Total
Male	0.9386	0.9292	0.9012	0.9226
Female	0.9624	0.9623	0.9443	0.958
Total	0.9436	0.9341	0.9062	0.9282

Predicted probability stop improperly premised				
	White NH	Hispanic	Black NH	Total
Male	0.0614	0.0708	0.0988	0.0774
Female	0.0376	0.0377	0.0557	0.042
Total	0.0564	0.0659	0.0938	0.0718

Note. N = 3,376. Predicted probabilities from model with main effects with bicycle sidewalk violations INCLUDED and treated as IMPROPERLY premised. Higher probability means greater likelihood that stop was properly premised on reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Figure 2 Graphical depiction of predicted probabilities for stop premises by gender and race/ethnicity: Bicycle sidewalk violations included and treated as IMPROPERLY premised



Note. N = 3,376. Predicted probabilities from model with main effects. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample.

This picture shifts, however, if bicycle sidewalk violations are excluded as investigatory stops. The picture also shifts if bicycle sidewalk violations are treated as proper stops.

### 8.5.1.2 *Bicycle sidewalk violations included, treated as properly premised*

Table 16 shows the results with bicycle sidewalk probable cause violations included and treated as properly premised. Race significantly affects the chances of being in a properly premised stop. Non-Hispanic Black stopped civilians’ predicted chances were significantly lower ( $p < .05$ ) than the chances of Non-Hispanic Whites. **This is a significant net impact of race.** Stopped Black civilians’ odds of being in a [sufficiently vs. improperly premised] stop were predicted to be (1-.448=) 55 percent lower than the corresponding chances for stopped White Non-Hispanic civilians.

Table 16 Main effects model predicting sufficient stop premise: Bicycles on sidewalk Included and treated as properly premised

		B	SE	z	OR	p <	OR: 95 % confidence interval	
							LCL	UCL
Fixed effects								
Black non-Hispanic (= 1; White Non-Hispanic = 0)	dblack	-0.802	0.363	-2.208	0.448	.05	0.220	0.914
Hispanic (= 1; White Non-Hispanic = 0)	dhispanic	-0.207	0.395	-0.525	0.813	ns	0.375	1.763
Female (=1; 0 = male)	dfemale	0.388	0.240	1.615	1.474	ns	0.920	2.358
Age (centered by sample mean)	c_age2	-0.007	0.005	-1.460	0.993	ns	0.983	1.003
	Constant	3.333	0.362					
Random effects								
	District	Variance	SE of variance					
		0.180	0.108					

LR  $\chi^2(df=1)$  vs. logistic model: = 13.35 (df=1);  $p < .001$

Note. Outcome = stop properly premised (=1) or not (=0). Weighted data. Bicycles on sidewalk excluded.  $n = 3,376$ .

Source: Jan.-Jun. 2016 legal narratives equal race sample. Probabilities are two tailed. Results from mixed effects logit model, stops grouped by police districts (run=117)

### 8.5.1.3 *Bicycle sidewalk violations excluded*

Weighted results for the main effects model when treating stopped bicyclists on public sidewalks as probable cause rather than investigatory stops, and thus removing them, also yielded a statistically significant impact of race. Results of this model appear in Table 17.

Table 17 Main effects model predicting sufficient stop premise: Bicycles on sidewalk EXcluded.

		B	SE	OR	z	p <	OR: 95 % confidence interval	
							LCL	UCL
<b>Fixed effects</b>								
Black non-Hispanic (= 1; white non-Hispanic = 0)	dblack	-0.791	0.363	0.454	-2.18	.05	0.223	0.924
Hispanic (= 1; white non- Hispanic = 0)	dhispanic	-0.209	0.395	.0811	-0.53	ns	0.374	1.760
Female (=1; 0 = male)	dfemale	0.408	0.240	1.503	1.7	ns	0.939	2.406
Age (centered by sample mean)	c_age2	-0.009	0.005	0.991	-1.69	ns	0.982	1.001
	Constant	3.306	.363	27.270			13.390	55.538
<b>Random effects</b>								
	District	Variance	SE of variance					
		0.186	0.111				0.057	0.599

LR  $\chi^2$ (df=1) vs. logistic model: = 13.71; p < .001

Note. Outcome = stop sufficiently premised (=1) or not (=0). Weighted data. Bicycles on sidewalk excluded. n = 3,300. Source: Jan.-Jun. 2016 legal narratives equal race sample. Probabilities are two tailed. Results from mixed effects logit model, stops grouped by police districts

#### 8.5.1.4 Short aside on bicycle sidewalk violations

To learn a bit more about the impact of how bicycle violations on between-group disparities on observed probabilities that a stop was properly premised or not, the observed proportion of properly vs. improperly premised stops was gauged under two scenarios: with the 66 bicycle sidewalk violations excluded, and with them included but coded as **improperly** premised. This descriptive exploration helps us understand why the net race impact is significant under one option and not under the other.

If the 66 bicycle PC stops are excluded, the percentage of White non-Hispanic stops that were bad was 3.5 (39 out of 1,107 White non-Hispanic stops; 3,310 total). Adding in the 66 PC bicycle stops and coding them as bad jumped the percentage of White non-Hispanic stops that were bad sizably, up to 5.8 percent (66 out of 1,134 white stops; 3,376 total). In contrast the percent of bad black stops was unaffected in these two situations. If the 66 stops are excluded, the percentage of Black non-Hispanic stops that were bad was 8.2 percent (89 out of 1,084 Black non-Hispanic stops; 3,310 total). If the 66 stops are included and coded as bad, the percent of Black non-Hispanic bad stops remains virtually the same at 8.1 percent (89 out of 1,100 Black non-Hispanic stops; 3,376 total). So the difference between the two groups in their respective percentages of bad stops has diminished markedly (8.2-3.5=4.7 percent difference; down to 8.1-5.8=2.3 percent difference). Indeed, the difference in percent bad stops between these two race groups, White vs. Black non-Hispanic individuals, has been cut in half.

### 8.5.2 Understanding the net statistical impacts of race

To better understand the significant race effect the pattern of marginal race effects over age and gender are examined.

These indicate the “partial change in the probability” of the outcome when race shifts from one group to another and other factors are held constant (Long, 1997: 71).<sup>11</sup> Stated more simply, these are about **just** the **net** impacts of race. The race impact is shown for stopped civilians of different age and gender combinations. Only non-Hispanic civilians are considered. Figure 3 shows the marginal probabilities for non-Hispanic stopped civilians when bicycle probable cause violations are included. Figure 4 shows the same effects from the same main effects model when bicycle sidewalk violations that were probable cause were excluded.

Males appear on the left in each figure, and females on the right. The line shows the estimated marginal net impact of race for persons of different ages.

For example, looking at Figure 3, and considering 15 year old males, the model says the following. The probability that a 15 year old male would be involved in a properly premised stop goes down about five percent if that individual is Black and Non-Hispanic instead of White and non-Hispanic. This predicted impact is due **just** to race **after controlling for** other factors in the model.

The figure also shows that the net race effect becomes somewhat larger as stopped civilian age increases. For example if a stopped male non-Hispanic civilian aged 45 rather than 15, and is Black rather than white, his probability of being in a properly premised stop goes down about six percent rather than five percent.

One more point about the left hand panel in the figure. The lines extending up and down from the net race impact line represent the upper and lower bounds of the 95 percent confidence interval. In the case of males, these intervals do not cross the zero value. This means that in the **full population** of stop records from which this sample was drawn, there is likely to be a **significant net race effect** for **males** regardless of age.

That is not true for females. There, the confidence intervals touch or barely cross zero. So there appears to be **no significant net** race effect in the **full population** for non-Hispanic **females**.

In short,

- The results show a five to six percent probability penalty for males who are Black rather than white. Their chances of being involved in a properly premised stop go down by that amount according to the model. This **net** race impact probably applies to the entire population of stops of non-Hispanic civilians.
- The results show a smaller probability penalty for females who are Black rather than White, as compared to males who are Black rather than White. Females’ chances of being involved in a properly premised stop go down by about four percent if they are Black rather than White. The net race impact for females may **not** apply to the population of

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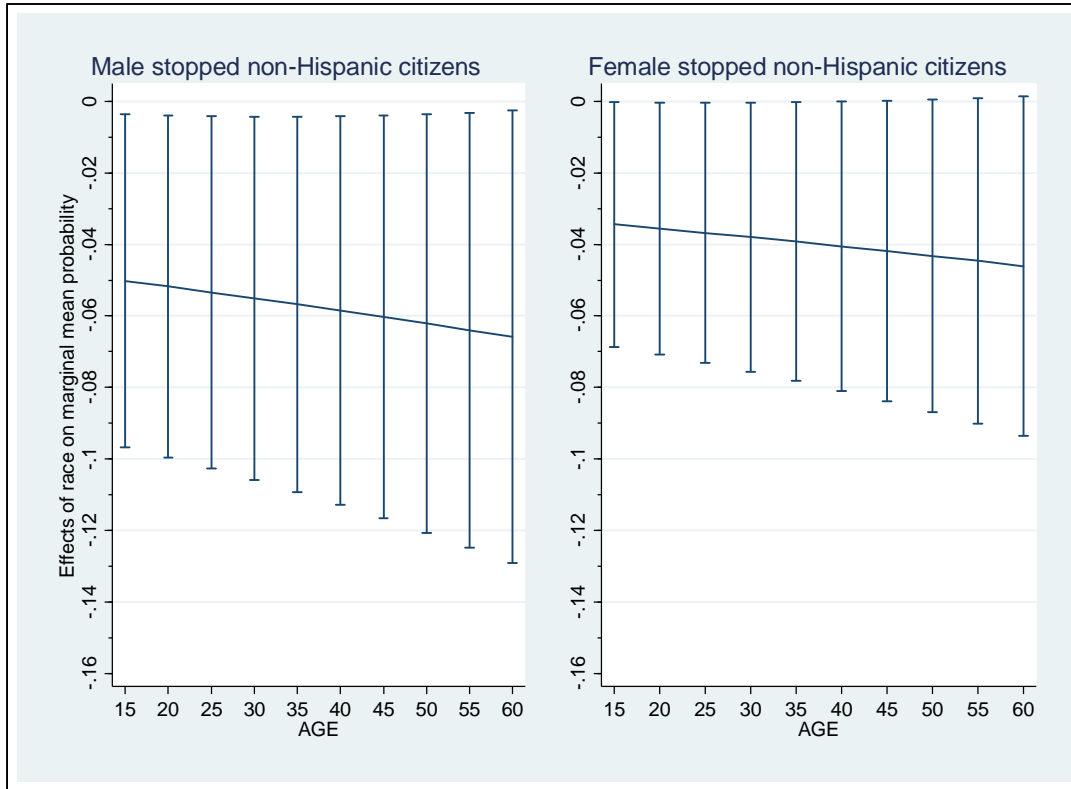
<sup>11</sup> In Stata, these are generated using the `dydx` option in `marginsplot`.



female non-Hispanic records from which the sample was drawn because the confidence intervals for women cross zero. So in the population the estimated “true” net race impact might be zero for non-Hispanic women.

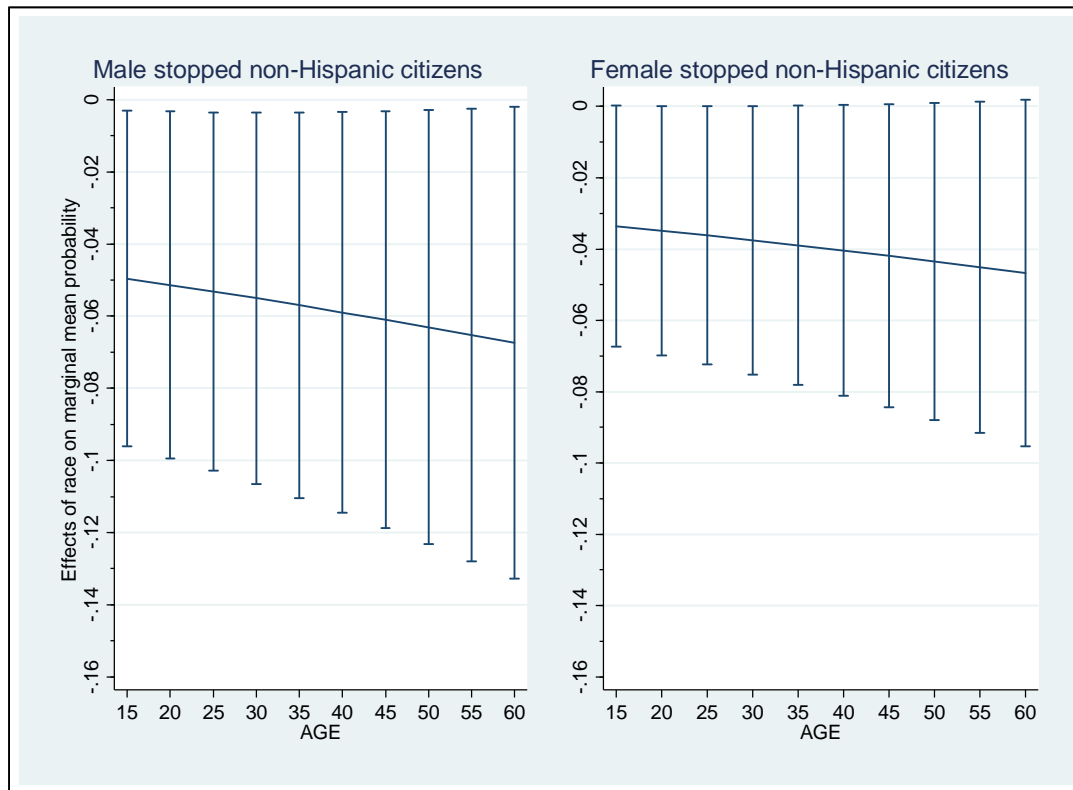
Figure 4 shows the same information when probable cause bicycle sidewalk violations are excluded. The pattern is identical to that already described.

Figure 3. Partial change in probability of a properly premised stop due to race variable: Net impact, bicycles included



Note. Sidewalk bicycle violations included and treated as properly premised (n=3,376). Margins and margin plot generated from full model of main effects, weighted data. 95% upper and lower confidence limits shown. Hispanic stopped civilians excluded. Each data point reflects a predicted impact of switching from a White stopped civilian to a Black stopped civilian on the probability that the stop is properly premised. For males, none of the upper confidence limits cross zero, this is a significant race impact for all the ages shown, for males. Some of the 95 % confidence interval limits appear to cross zero, suggesting the predicted race effect may not be significant for females of all ages.

Figure 4. Partial change in probability of a properly premised stop due to race variable: Net impact, bicycles excluded

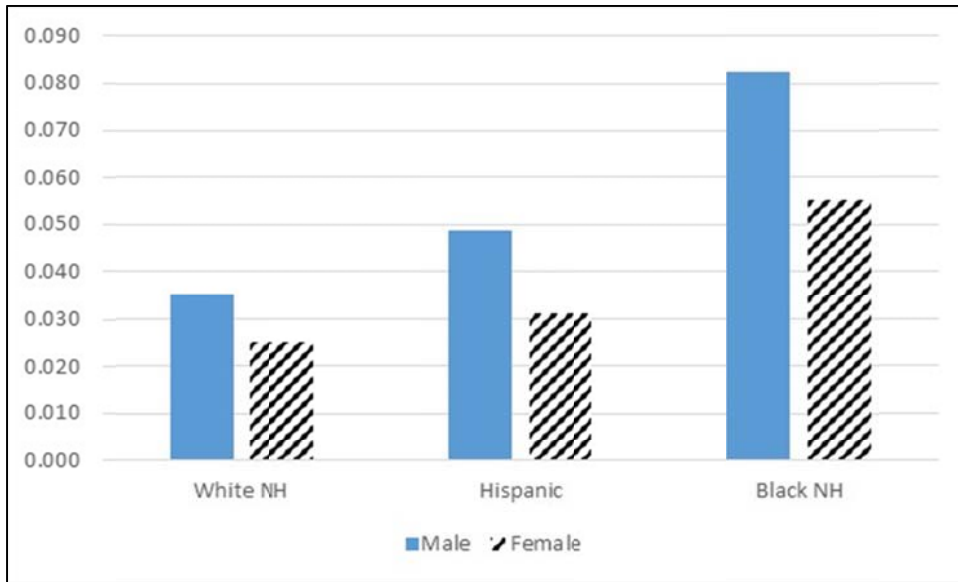


Note. Sidewalk bicycle violations excluded (n=3,310). Margins and margin plot generated from full model of main effects, weighted data. 95% upper and lower confidence limits shown. Hispanic stopped civilians excluded. Each data point reflects a predicted impact of switching from a White stopped civilian to a Black stopped civilian on the probability that the stop is properly premised. Because none of the upper confidence limits cross zero, this is a significant race impact for all the ages shown, for both males and females.

### 8.5.3 Modeled gross race/ethnicity and gender impacts: Description using predicted probabilities

One can gain a closer appreciation of these patterns of **modeled gross** impacts by examining predicted probabilities from the full model separately for different race, ethnicity and gender combinations. These predicted probabilities, with bicycle sidewalk violations included and treated as properly premised, expressed as the chances that the stop **lacked** reasonable articulable suspicion factors are displayed graphically in Figure 5 and numerically in Table 18. With bicycle sidewalk violations excluded, those patterns are displayed graphically in Figure 6 and numerically in Table 19.

Figure 5 Predicted probabilities stop improperly premised by gender and race/ethnicity: Bicycle sidewalk violations included and treated as properly premised



Note. N = 3,376. Predicted probabilities from model with main effects. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 18 Predicted probabilities for stop premises by gender and race/ethnicity: Bicycle sidewalk violations included and treated as properly premised

Bicycle sidewalk violations included and treated as properly premised

Predicted probability stop Properly premised

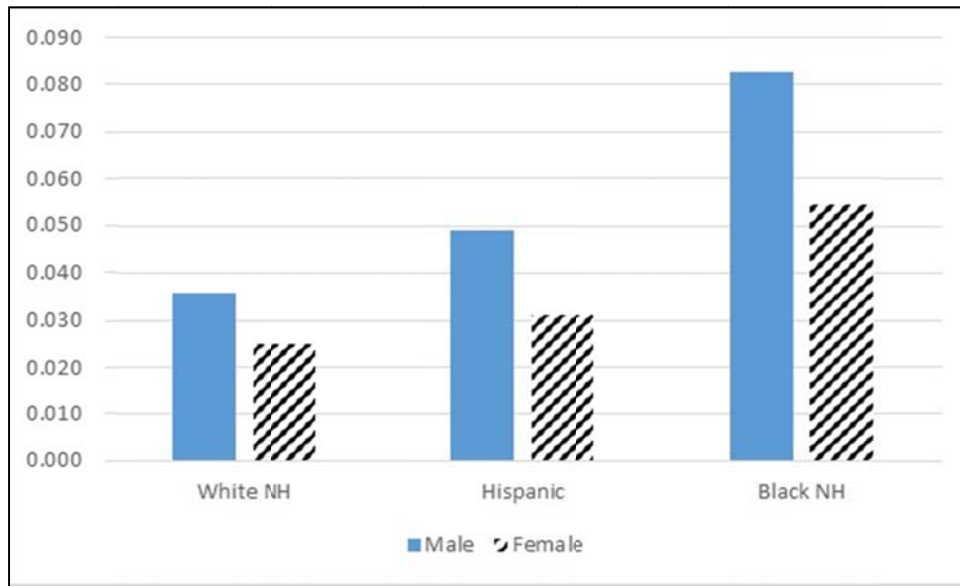
Gender	White NH	Hispanic	Black NH	Total
Male	0.965	0.951	0.918	0.944
Female	0.975	0.969	0.945	0.966
Total	0.967	0.954	0.921	0.948

Predicted probability stop Improperly premised

	White NH	Hispanic	Black NH	Total
Male	0.035	0.049	0.083	0.056
Female	0.025	0.031	0.055	0.034
Total	0.033	0.046	0.079	0.053

Note. N = 3,376. Predicted probabilities from model with main effects. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Figure 6 Predicted probabilities stop improperly premised by gender and race/ethnicity: Bicycle sidewalk violations EXcluded



Note. N = 3,310. Predicted probabilities from model with main effects. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Table 19 Predicted probabilities for stop premises by gender and race/ethnicity: Bicycle sidewalk violations EXcluded

Bicycle sidewalk violations Excluded

Predicted probability stop Properly premised

Gender	White NH	Hispanic	Black NH	Total
Male	0.964	0.951	0.917	0.944
Female	0.975	0.969	0.946	0.966
Total	0.966	0.954	0.921	0.947

Predicted probability stop Improperly premised

	White NH	Hispanic	Black NH	Total
Male	0.036	0.049	0.083	0.057
Female	0.025	0.031	0.054	0.034
Total	0.034	0.046	0.079	0.053

Note. N = 3,310. Predicted probabilities from model with main effects. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample.

These displays show the following.

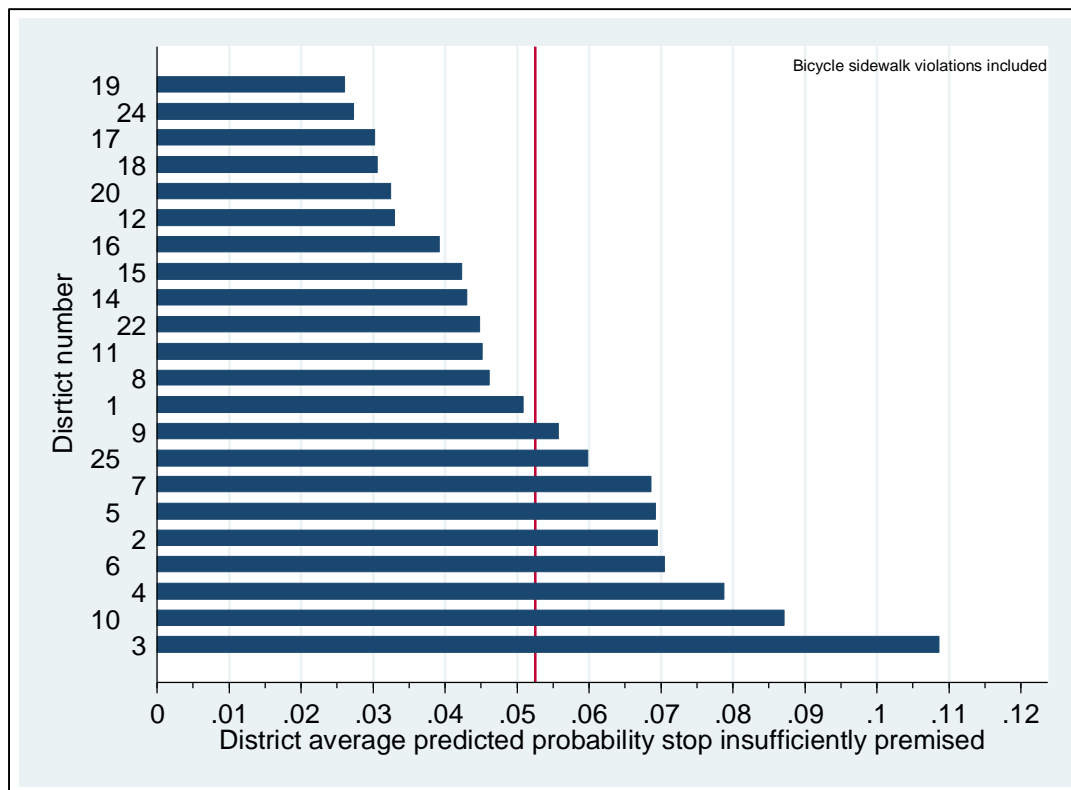
- The pattern proves consistent regardless of whether bicycle sidewalk violations presenting probable cause (n=66) are included or excluded.
- As a group, stopped Black Non-Hispanic civilians’ predicted chances of being in an improperly premised stop on average were more than twice the average predicted chances of stopped White Non-Hispanic civilians. This held for both males and females.
- Further, especially for Non-Hispanic Black stopped civilians, average chances of being in an improperly premised stop appeared higher for males than females.

These are gross race and ethnicity impacts which means factors associated with race have not been controlled, nor has district context.

#### 8.5.4 Describing overall geographic patterns in predicted probabilities

District variation in predicted chances that a stop would lack reasonable articulable suspicion factors appears in Figure 7 with bicycle violations included, and in Figure 8 with those records removed. Districts ranged from predicted insufficiency rates that were about half the average predicted insufficiency rate (Districts 19, 24), to those that were about twice the average predicted insufficiency rate (District 3). The pattern was essentially equivalent regardless of how bicycle sidewalk violations were treated.

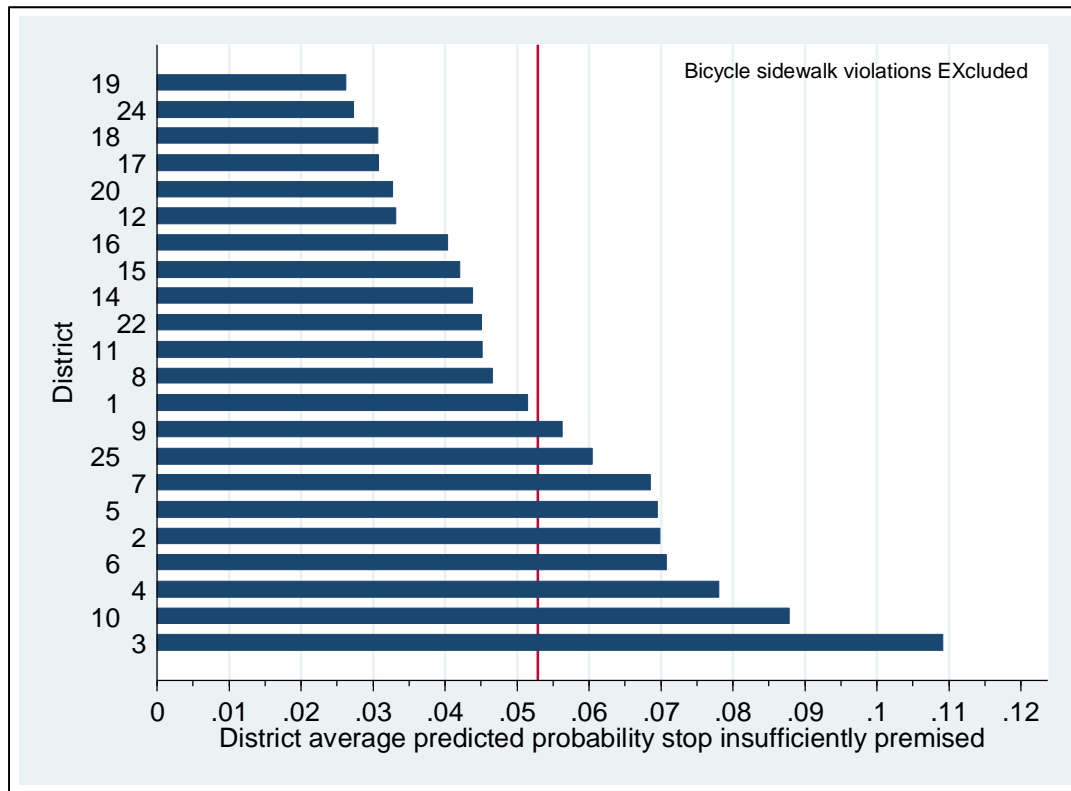
Figure 7 District level average predicted probabilities stops improperly premised: Bicycle sidewalk violations included and treated as properly premised



Note. N = 3,376. Predicted probabilities from model with main effects and weighted data. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors.

Source: Jan.-Jun. 2016 legal narratives equal race sample. Vertical reference line represents overall average predicted probability.

Figure 8 District level average predicted probabilities stops improperly premised: Bicycle sidewalk violations EXcluded



Note. N = 3,310. Predicted probabilities from model with main effects and weighted data. Higher probability means greater likelihood that stop lacked reasonable articulable suspicion factors. Source: Jan.-Jun. 2016 legal narratives equal race sample. Vertical reference line represents overall average predicted probability.

The above figures, in essence, display how model predictions play out across different districts, given the factors taken into account by the model: race, ethnicity, age, gender, and district context. They do not indicate what is responsible for these variations. These figures merely describe the variations.

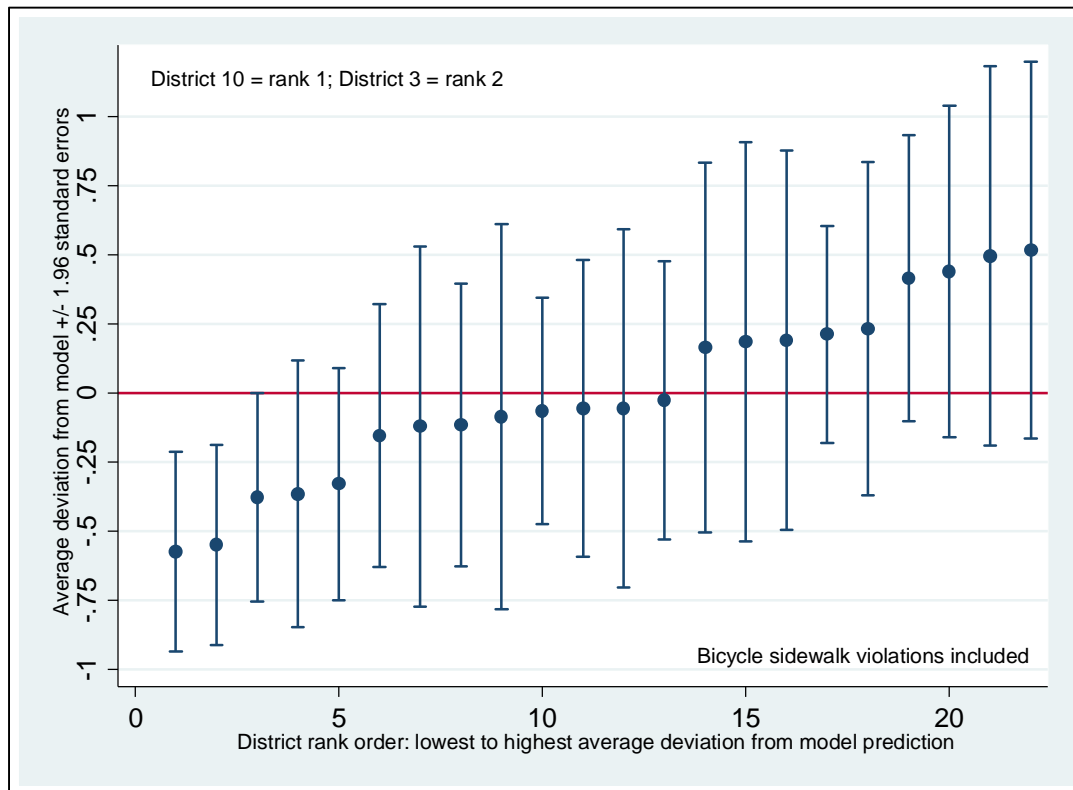
### 8.5.5 Geographic unexplained variation

The discrepancies between what the model **predicted** should happen with stop basis, and what **actually** happened, are called residuals. These are generated on a case by case basis. These residuals represent deviations from the model prediction. They can be averaged at the district level to capture the district-level average discrepancy from model predictions.

$$\text{Residuals} = [\text{observed score (0 or 1)}] - [\text{predicted score (predicted probability)}]$$

Since the outcome was scored zero if a stop was **improperly premised**, a **negative** average residual at the district level suggests that in that district there was a **higher** proportion of stops **lacking** RAS factors.

Figure 9 District-level discrepancies from model predictions of stop sufficiency: Bicycle sidewalk violations included and treated as properly premised



Note. N = 3,376. Residuals capture deviations from predicted probabilities from model with main effects. *Lower* average residual represents *higher* fraction of stops *improperly premised*. Bars capture 95 percent confidence interval around each district’s average residual. Horizontal line at zero represents average residual. Confidence intervals not crossing the reference line are significantly different from the average. Source: Jan.-Jun. 2016 legal narratives equal race sample.

District average residuals with bicycle sidewalk violations included appear in Figure 9. The average discrepancy for both District 10 and District 3 was significantly below average. **This means that improperly premised stops in these districts occurred more frequently than expected by the model.**

Potential reasons for the discrepancies are numerous. Only one was examined here: the proportion of stopped civilians who were non-Hispanic Black. That factor at the district level **did not** correlate with these discrepancies.

With bicycle sidewalk violations excluded, the caterpillar plot showed the exact same pattern of significance; Districts 10 and 3 each had an average deviation from the model that fell significantly below zero (results not shown).

#### 8.5.5.1 Robustness tests

The mixed effects models presented here are potentially problematic given the low number of higher level units; there are only 22 districts (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016). Therefore, as a robustness test of the main race effects, single level logit models were run with dummy variables entered for all districts save District 1. These models were problematic in that stops from two districts (18, 19) were dropped because there was no variation on the outcome there.<sup>12</sup>

**With the single level models the previously significant net impacts of race ( $p < .05$ ) became only marginally significant ( $p < .10$ ) (detailed results not shown). The size of the net race effect was closely comparable to what was seen earlier:**

- Black OR = .501;  $z = -1.83$ ;  $p = .067$  (two tailed) with bicycles included and treated as properly premised
- Black OR = .507;  $z = -1.80$ ;  $p = .072$  (two tailed) with bicycles Excluded

But the impact was no longer significant using a conventional two tailed hypothesis test.

## 8.6 SUMMARY AND LIMITATIONS: STOP PROPERLY OR IMPROPERLY PREMISED

These analyses of stop premises suggest the following:

1. The likelihood that a stop is properly premised on RAS factors varies significantly across districts (Table 12).
2. A statistically significant ( $p < .05$ ) net impact of race on stop premise sufficiency emerges. BUT:
3. This significant net race impact on stop premise sufficiency **depends on how probable cause bicycle violation stops are treated**. If they are seen as properly premised, then the net race impact is observed. If they are seen as improperly premised **investigatory** stops, then there is **no significant net race impact**.
4. The significant net effect of race appears, based on the marginal plots, to depend somewhat on gender. Statistically significant net race impacts on the outcome surface regardless of age for males, but not for females (Figure 3, Figure 4).
5. The marginal plots display the **size** of the marginal net race impact, holding other factors constant. **It is a difference of about 4 or 5 percent in the predicted probability that the stop is sufficiently premised. The practical implications of this sized net impact deserve careful discussion. That discussion should take into account the overall rates of properly and improperly premised stops.**
6. Switching from net to gross impacts, average predicted probabilities that a stop lacked sufficient grounds depend on both gender and ethnoracial combinations (Figure 5, Figure 6).

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<sup>12</sup> Alternate modeling leaving district 18 as the reference string, and thereby losing only 64 observations had no effect on the net race impact.



7. The group with the highest average model prediction that their stops would be improperly premised are Black Non-Hispanic males (Figure 5, Figure 6).
8. The average model prediction that stops would be improperly premised varies markedly across districts (Figure 7, Figure 8)
9. Two districts (10 and 3) have a higher than expected fraction of improperly premised stops, even after taking into account the factors used by the model (Figure 9).

These analyses have limitations, so results should be interpreted with caution.

10. Most importantly, analyses rely mainly on predicted probabilities and those predicted probabilities rely on a specific model with a specific set of predictors and random effects for districts. Different results could appear with different predictors.
11. **The significant net race impact failed to replicate if we controlled for district context a different way.** Instead of treating districts as random effects, an alternate model entered dummies for each district. This resulted in losing observations from at least one district. It is not clear if the alternative analytics, or the lost observations were the cause of the different result pattern.
12. Mixed effects models presume that random effects of the higher level units represent a normal distribution of effects. This assumption may not be warranted with a relatively small number of higher level units such as we have here. Future analyses probably should be conducted at the beat-within-district level as the geographic unit of analysis.
13. Some might object that by controlling for geography, gender and age we committed the partialling fallacy (Gordon, 1968). The factors controlled for, some might argue, especially geography, were standing in as proxies for race. We don't think this applies for the stop premise outcome because the gross impact of race is about comparable in size to the net impact of race.

## 9 RESULTS: REASONABLE ARTICULABLE SUSPICION FOR A PAT DOWN

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This section examines the relationship between pat down basis and race/ethnicity, before and after controlling for civilian age and gender, and district context. Because some stopped civilians were selected to receive a pat down, and others were not, analyses of the pat down basis need to take that into account. Whether a pat down occurred depended on CPD officers' checking the appropriate box.

There are three possible outcomes:

- A. Stopped civilian receives a properly premised pat down.
- B. Stopped civilian receives an improperly premised pat down.
- C. Stopped civilian does not receive a pat down.

Each model run will generate, for each stopped civilian, a predicted probability, based on the factors in the model, for each of these three outcomes. For each stopped civilian, the three probabilities necessarily sum to 1 or 100 in percent terms. Of greatest interest here are effects of

race/ethnicity, controlling for age and gender, on the predicted probability the stopped civilian was subjected to an improperly premised search.

For each specific model, these three outcomes lead to two predictions:

- The chances of B vs. A: The relative risk of receiving an [improperly premised vs. properly premised pat down]. This is called Contrast 1.
- The changes of C vs. A: The relative risk of receiving [no pat down vs. a properly premised pat down]. This is called Contrast 2.

Because only a sample of stops were coded, and because the full report on post stop outcomes analyzes in detail whether a pat down occurs or not, discussion here centers on Contrast 1.<sup>13</sup>

Further, district context also must be taken into account if the proportion of properly premised pat downs varies across districts. As will be seen, it does. The appropriate type of model, therefore, is a multilevel multinomial model with the data weighted so that results reflect the overall population of stops (Rabe-Hesketh & Skrondal, 2012). This is carried out using generalized structural equation models.

## 9.1 DESCRIPTIVE PATTERN

Using weighted data, but excluding probable cause stops including bicycle sidewalk “on view” violations, Table 20 shows differences across the three ethnoracial groups on this outcome.

The group with the highest percentage of records involved an improperly premised pat down was Black non-Hispanic civilians. In the weighted data, 75 out of 2,327 stops in this group or 3.2 percent involved an improperly premised pat down lacking reasonable articulable suspicion. This contrasts with 2.4 percent of the stops of White non-Hispanics that involved a pat down lacking RAS. Least likely to be involved in an improper pat down were Hispanic civilians where only 1.5 percent of their stops involved an improper pat down.

Overall, the weighted data suggest that about 2.8 percent of all investigatory stops of members of these three groups involved an improper pat down. (Of course, there is sampling error around this overall percentage, but it is not shown here.)

---

<sup>13</sup> Nevertheless, all three outcomes need to be considered simultaneously in one model rather than two models of pairwise comparisons. Otherwise different civilians are in different analyses, and predicted probabilities across the three outcomes for a civilian may not total to 100 percent. (Long, 1997: 151).

Table 20 Descriptive differences, pat down premise

Pat down and basis		Ethnoracial category			
		White NH	Hispanic	Black NH	Total
Pat down, RAS	Weighted N	51	210	794	1,055
	Percent	20.23	30.05	34.14	32.19
Pat down, no RAS	Weighted N	6	11	75	92
	Percent	2.35	1.52	3.24	2.8
No pat down	Weighted N	196	478	1,457	2,131
	Percent	77.42	68.43	62.63	65
Total	Weighted N	253	698	2,327	3,278
	Percent	100	100	100	100

Note. Equal race sample, Jan-June 2016, weighted data. Investigatory stops only; probable cause stops excluded. NH = non-Hispanic. Percentages shown are column percentages for each group.

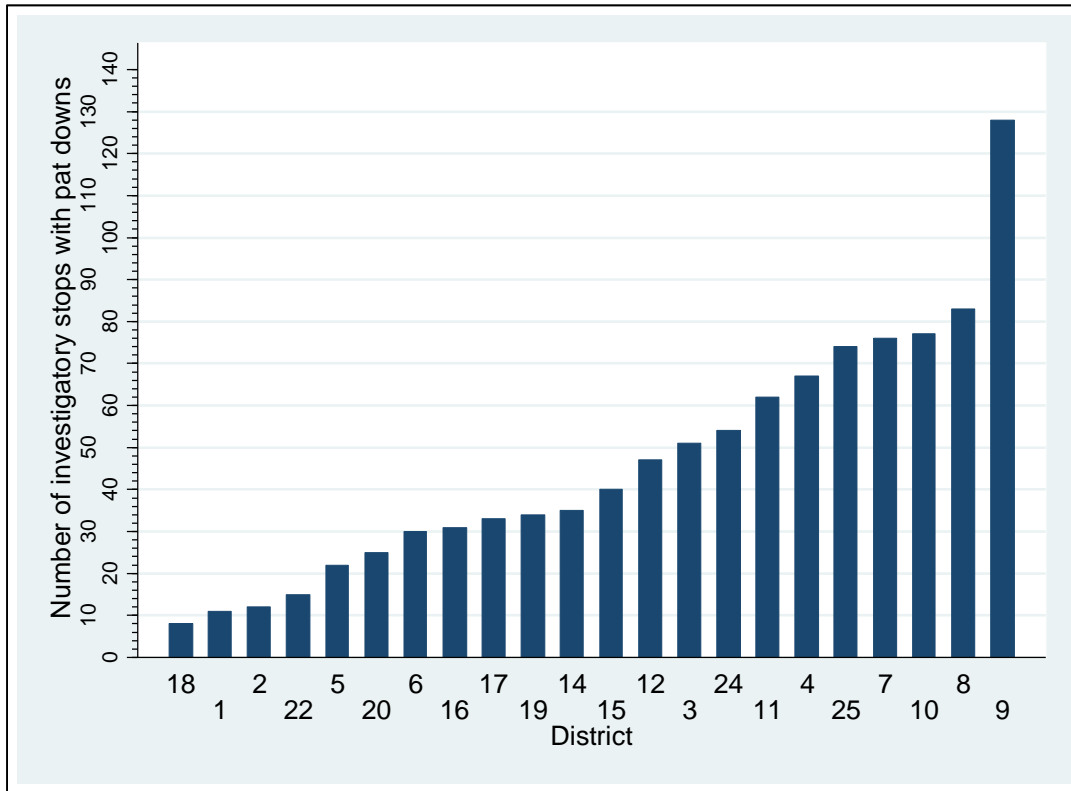
## 9.2 PATTERNS OF PAT DOWNS AND PAT DOWN BASIS ACROSS DISTRICTS

The number of ISRs varies across districts from 44 to 383 with bicycle sidewalk violations included and 44 to 375 with bicycle sidewalk violations excluded. Counts of pat downs across districts appear in Figure 10 with bicycle sidewalk violations included. In the sample, the number of pat downs per district, like the number of stops per district, varies widely across the city.<sup>14</sup> The numbers range from around 10 (Districts 1, 2, 18), to over 100 (District 9).

The number of stops that included an improperly premised pat down in each district appears in Figure 11. The numbers range from zero (Districts 5, 22) to eight (Districts 9, 11, 19).

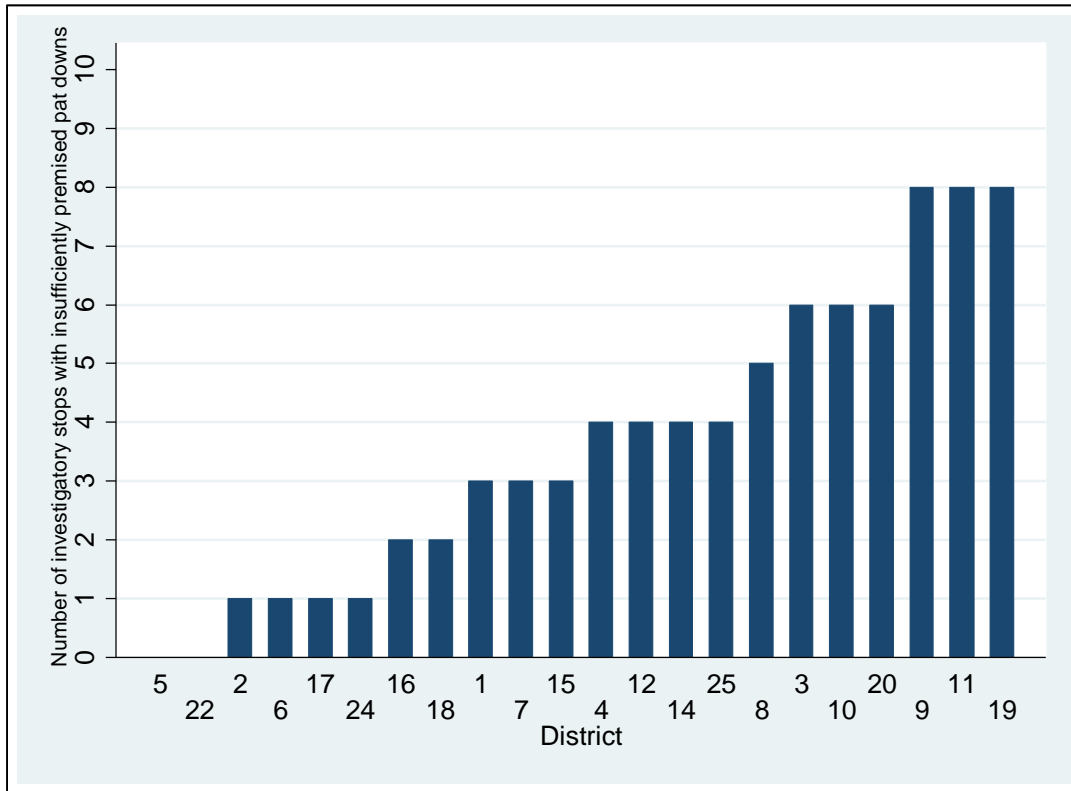
<sup>14</sup> Whether a pat down occurs at all is predicted in the post stop outcomes report.

Figure 10 Number of pat downs per district in sample



Note. Bicycle sidewalk violations included. Source: Jan.-Jun. 2016 legal narratives equal race sample.

Figure 11 Number of improperly premised pat downs per district in sample



Note. Bicycle sidewalk violations included. Source: Jan.-Jun. 2016 legal narratives equal race sample.

### 9.3 STOP-LEVEL PREDICTORS OF PAT DOWN BASIS

As mentioned earlier, with the multinomial model and three groups there are two contrasts.

Contrast 1: Those receiving an improperly premised pat down vs. a properly premised one

Contrast 2: Those receiving no pat down vs. a properly premised one

For both contrasts, receiving a properly premised pat down is the base category.

Results for Contrast 1 appear in Table 21. There is no significant impact for ethnicity or age. There is, however, a significant impact for gender.

Table 21 Net impacts of race, ethnicity and gender of likelihood of receiving an improperly vs. properly premised pat down, while controlling for district context.

Predictor	B	SE	Z	p <	LCL (95%)	UCL (95%)
Bicycle sidewalk violations included						
Black	0.0308	0.4572	0.07	ns	-0.8653	0.9270
Hispanic	-0.7727	0.5422	-1.43	ns	-1.8354	0.2900
Female	0.8075	0.3679	2.2	.05	0.0865	1.5285
c_age2	-0.0061	0.0099	-0.62	ns	-0.0255	0.0132
_cons	-2.3369	0.4489				
-2 x log likelihood	-2318.67					
Bicycle sidewalk violations excluded						
Black	0.0334	0.4575	0.07	ns	-0.8632	0.9300
Hispanic	-0.7710	0.5425	-1.42	ns	-1.8344	0.2923
Female	0.8071	0.3679	2.19	.05	0.0861	1.5281
c_age2	-0.0057	0.0099	-0.57	ns	-0.0251	0.0137
-2 x log likelihood	-2285.88					

Note. Results from stop-level multilevel multinomial model with stops nested within districts. Results shown only for Contrast 1. Results for contrast 2 not shown. N = 3,372 with bicycle sidewalk violations included, and 3,296 with bicycle sidewalk violations excluded. These numbers of cases differ from other tables because of missing values on this outcome. C\_age2 = age centered on sample average (29.55 years). Source: Jan.-Jun. 2016 legal narratives equal race sample

### 9.3.1 Gender

#### 9.3.1.1 Net impact

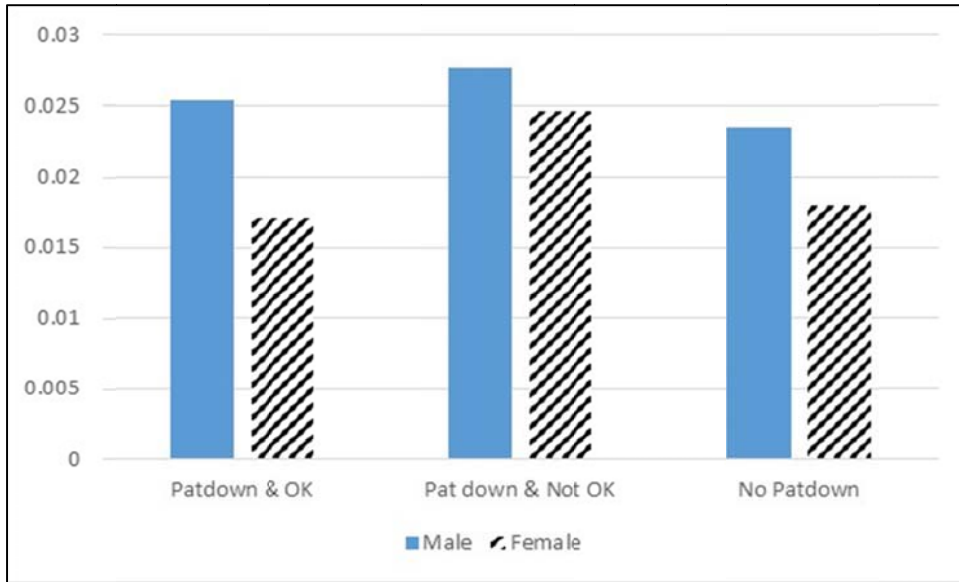
Stopped women are less likely to be patted down than men. But if a stopped civilian of either gender is patted down, the pat down is significantly ( $p < .05$ ) more likely to be improperly premised if the stopped civilian is female. **The chances are better than 95 out of a 100 that in the full set of investigatory stops there is a net gender impact on whether a pat down is properly premised. Pat downs of women are significantly more likely to be improperly premised compared to pat downs of men, controlling for other factors.**

Figure 12 helps clarify. The chart shows the average predicted probability for six groups. Gender is crossed with what actually happened: no pat down, a good pat down, or a bad pat down. The chart shows how gross gender impacts play out in predicted probabilities.

Note the disparity between the two left most bars for females. When *actually* in a *good* pat down, women's predicted probabilities of being in a bad pat down were quite low, about 1.7 percent. But when *actually* in a *bad* pat down, their predicted chances of being in a bad pat down were markedly higher, averaging almost 2.5 percent. By contrast, males' predicted chances of being in a bad pat down were quite similar, regardless of whether they actually were or were not in a good or a bad pat down.

This net gender impact, controlling for civilian race, ethnicity, age, and district context, appears regardless of whether bicycle sidewalk violations are included as investigatory stops (results not shown).

Figure 12 Full model predicted probabilities of a problematic pat down



Note. Actual outcome shown on horizontal axis. Bicycle sidewalk violations included (n=3,376). Predicted probabilities from multilevel multinomial model with stops nested within districts and main effects for race, ethnicity, age, and gender. Source: Jan.-Jun. 2016 legal narratives equal race sample.

**9.3.1.2 CAUTION: Descriptive background**

More descriptive background may help better contextualize this impact.

Counts by gender for each outcome category appear in Table 22. There are seven improperly premised pat downs of women and 71 of men.

Expressing those numbers as percentages: 1.3 percent of investigatory stops involving women include an improperly premised pat down compared to 2.5 percent of the investigatory stops of men.

The important point here is that **the significant net gender impact noted in the above table is based only on seven properly premised pat downs of women.**

Table 22 pat down occurrence and premises by gender: Counts, percentages and relevant risks

	Male	Female	
Counts			
Outcome			
pat down: Properly premised (G)	865	68	
pat down: Improperly premised (B)	71	7	
No pat down (N)	1,900	461	
Total	2,836	536	3,372
Percentages of total			
Outcome			
pat down: Properly premised (G)	0.305	0.127	
pat down: Improperly premised (B)	0.025	0.013	
No pat down (N)	0.670	0.860	
	1.000	1.000	

Note. Bicycle sidewalk violations included. Source: Jan.-Jun. 2016 legal narratives equal race sample. Unweighted data. G = “good”; B = “bad”; N = no pat down.

9.3.2 Predicted probability of an improperly premised pat down, geographic context, and race  
 Although the individual level race variable does not link to the odds that a pat down experienced was [improperly premised vs. properly premised], **descriptively** at the **district level** there does appear to be a relationship with race. See Figure 13.

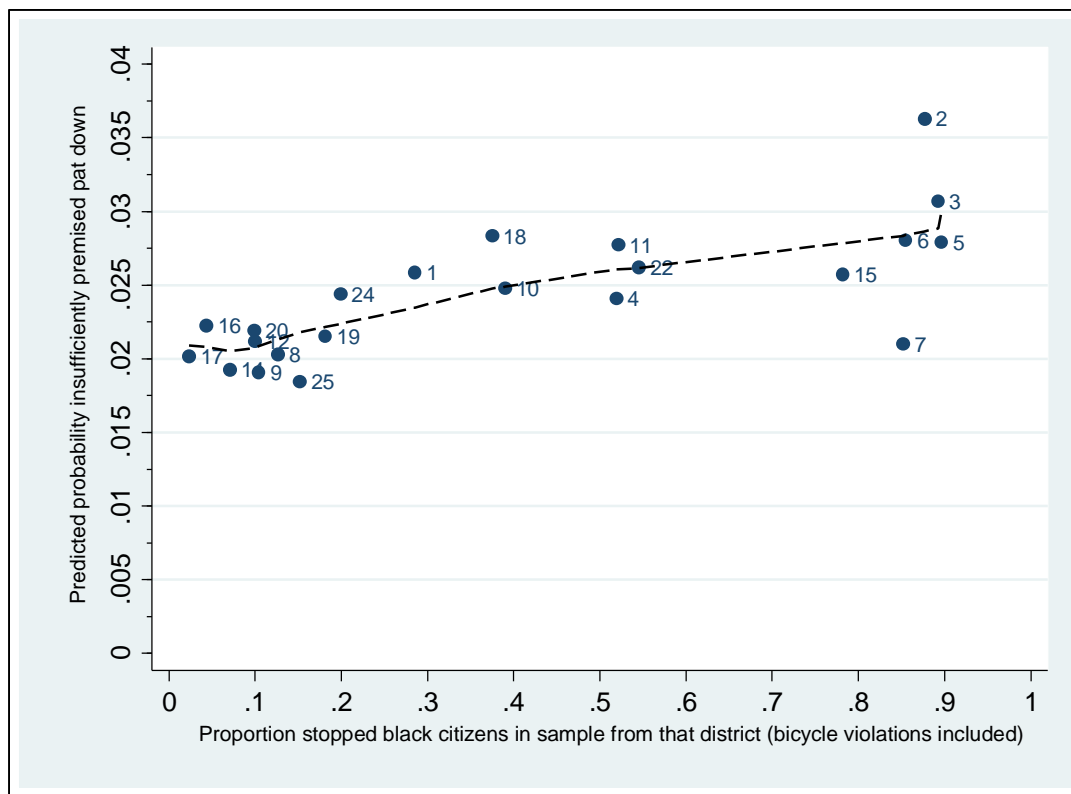
This figure shows the district average predicted probabilities that the pat downs occurring in the district are improperly premised. These range from a little less than two percent to more than four percent.

These district average predicted probabilities are organized by the percent of stopped civilians in the district who were non-Hispanic Black.

The pattern **descriptively suggests a district level gross rather than net connection** between the chances of a bad pat down and the racial composition of stopped civilians. The predicted probabilities that pat downs occurring would be poorly premised were higher in districts with a higher proportion of stopped Black non-Hispanic civilians.



Figure 13. District average predicted probabilities pat down improperly premised, and district percent stopped civilians who are black



Note. District average predicted probabilities based on multilevel full model with main effects for race, gender, age and ethnicity, random effect for district (weighted data). Bicycle sidewalk violations included. Line shown is a locally weighted smoothed regression line (Cleveland, 1979). Source: Jan.-Jun. 2016 legal narratives equal race sample.

#### 9.4 SUMMARY AND LIMITATIONS: PAT DOWN BASIS

These analyses of pat down premise suggest the following points.

1. Regardless of whether bicycle sidewalk violations are included or excluded, women, although they have a lower chance of being patted down at all, if they are patted down, are significantly more likely than men to receive an improperly premised pat down. This is a net gender effect, controlling for district, race, ethnicity, and age. Although it is statistically significant, practically speaking it relies heavily on just seven unwarranted pat downs of women.
2. Further, although there is no stop-level relationship between civilian race and pat down basis.
3. There may, however, be something going on at the district level between pat down premising and race of stopped civilians. At the district level the average predicted probability of an improperly premised pat down does link positively with the fraction of stopped civilians in the district who are Non-Hispanic Black. This is a descriptive gross relationship at an ecological level.

The pat down basis analysis have limitations as well.

4. Most importantly, analyses rely mainly on predicted probabilities and those predicted probabilities rely on a specific model with a specific set of predictors and random effects for districts. Different results could appear with different predictors.
5. Mixed effects models presume that random effects of the higher level units represent a normal distribution of effects. This assumption may not be warranted with a relatively small number of higher level units such as we have here. Future analyses probably should be conducted at the beat-within-district level as the geographic unit of analysis.
6. Some might object that by controlling for geography, gender and age we committed the partialling fallacy (Gordon, 1968). The factors controlled for, some might argue, especially geography, might be standing in as proxies for race. It is just because of this concern that we examine predicted probabilities of a bad pat down by the racial composition of those stopped in the district. That examination suggests pat down basis is more likely to be inadequate in districts with higher proportions of stopped civilians who are Black and non-Hispanic. Because there are so few districts it is not possible to do a meaningful statistical test of this link. In order to better address this limitation for future periods analyses are planned at the beat-within-district level.

## 10 RESULTS: PROBABLE CAUSE FOR A SEARCH

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Attention turns to whether a conducted search during an investigatory stop was properly premised on probable cause, given the descriptions provided in the narratives.

Many investigatory stops resulted in searches that were incident to arrest or transport. In these instances a search was mandated. Therefore, whether the search was premised on probable cause or not was irrelevant in these instances.

### 10.1 SEARCH FREQUENCY AND BASIS

Table 23 provides information about searches. In the unweighted sample (n=3,310) excluding probable cause bicycle sidewalk violations, searches were conducted for 15.5 percent of the stops (n=512).

Of the 512, focusing just on investigatory stops, and only on records where CPD officers checked the search box, 343 searches (67 percent of the 512) were incident to arrest (n=316) or transport (n=27). In these instances the question of the search being properly premised was irrelevant.

In an additional 44 (8.6 percent) investigatory stops where CPD officers checked the search box, the narratives on the ISR forms provided insufficient information to gauge whether the search was premised on probable cause.

That left 125 (24.4 percent of the 512) searches during investigatory stops, where CPD officers checked the search box, where search premise could be gauged.

In 120 of these 125 (96 percent), the narratives indicated the searches were properly premised on probable cause.

In only 5 instances (4 percent of 125) were the searches deemed improperly premised on probable cause. (With weighted data the number of improperly premised searches was 3.)

### 10.2 SEARCH BASIS AND RACE/ETHNICITY

Given the extremely low number of searches improperly premised on probable cause in the sample (n=5), it is not possible to conduct a meaningful analysis examining the relationship between race/ethnicity and this search premise variable.

### 10.3 SEARCHES AND PAT DOWNS

In the sample using unweighted data, the search outcome links to the previously analyzed pat down outcome. Of the 1,011 investigatory stops resulting in a pat down, in 18.8 percent of them a search also took place (n=190). This contrasts, in the stops without a pat down (n=2,299), where only 14 percent of those stops also involved a search (n=322).

Table 23

Search probable cause basis	Did a search take place? (police check box)		
	No	Yes	Total
0. Sufficient probable cause articulated	0	120	120
1. Sufficient probable cause NOT articulated	0	5	5
Custodial search	0	343	343
INAP (no search) / Insufficient information (search)	2,798	44	2,842
<b>Total</b>	<b>2,798</b>	<b>512</b>	<b>3,310</b>

Note. Unweighted data. Bicycle sidewalk violations excluded.

## 11 APPENDIX A: CODES FOR STOP RAS, PAT DOWN RAS, AND SEARCH PC

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NARRATIVE CODED RESULT stop RAS	Freq.	Percent	Cum.
0. RAS sufficient	3,128	73.90	73.90
1. PC stop no RAS needed	923	21.80	95.70
2. time/distance too attenuated	2	0.05	95.75
4. hunch not personal observation	5	0.12	95.87
7. not enough facts	99	2.34	98.20
8. fleeing or avoidant subject only	2	0.05	98.25
9. no crim activity afoot	57	1.35	99.60
11. no basis for terry or PC stop	13	0.31	99.91
.i	4	0.09	100.00
Total	4,233	100.00	

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RE: ACLU Matter vs. - REF# 1340012232

Analysis of Chicago Police Department  
Post-stop Outcomes during Investigatory  
Stops January through June 2016:  
Input to Hon. Arlander Keys' (Ret.)  
First Year Report

REVISED FINAL TECHNICAL REPORT

Ralph B. Taylor & Lallen T. Johnson

DATE: 20170321

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## 2 INTRODUCTION TO REVISED VERSION

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Comments by the Parties and their experts on the initial version of this report led to modifications that appear in this version. The major modifications include the following.

1. Pointing out to readers, in section 3, where they can find how the different ethn racial groups scored on the outcomes of interest here. Those tables also show how scores on each outcome varied by district, and varied within district by ethn racial category.
2. Clarifying the four levels of scrutiny be applied to ethn racial differences on each outcome: gross impact, net impact, statistically significant net impact, and statistically significant net impact that may be causal rather than just correlational.
3. Section 3 also explains the specific social science meaning of the term “cause” and “causal impact” as it is used in this report.
4. Section 3 further highlights the series of questions that each analysis is designed to answer.
5. Clarifying how geographic variation in the multivariate analyses was reported and presented.
6. Clarifying that the main model applied to each outcome here **as requested by the Parties experts and as agreed, are multiple regression models**. They have some improvements over garden-variety ordinary least squares single-level multiple regression models, but they are multiple regression models at heart. Further, the improvements they incorporate are in line with current best social science scholarly practices in this area.
7. Clarifying that stops with searches associated with arrests were dropped only in analyses of the search outcome, not when other outcomes were considered, and addressing the under-excluding/over-excluding question when dropping searches associated with arrests.
8. Discussing the partialling fallacy as a potential limitation when interpreting net impacts of race or ethnicity variables.

## 3 FOR THE NON-TECHNICAL READER: FAQ

---

This section asks and answers frequently asked questions the non-technical reader might have about this report. It simultaneously guides the non-technical reader to findings and interpretations that might be of most interest to him or her. Even technical readers might benefit from scanning the questions and answers listed here.

### 3.1 OUTCOMES OF INTEREST AND DATA SOURCES

Q: What is this report about?

A: This report describes what happens to civilians stopped by Chicago Police Department officers during the first six months of 2016. Of special interest is how what happens may depend on the race or ethnicity, that is, the ethnoracial category, of the stopped civilian.

Q: What kinds of things can happen to a stopped civilian during a police encounter?

A: Many things can happen, but only a few of those are considered here. The post stop outcomes investigated include: whether the civilian is patted down or not; whether the pat down resulted in a weapon or firearm being recovered; whether the civilian was searched or not; and, among those civilian stops that resulted in no enforcement action being delivered, if race or ethnicity link to whether or not a pat down took place.

In some jurisdictions a pat down is also known as a frisk.

Q: What data source does this report rely on?

A: The report analyzes records from the Chicago Police Department investigatory stop reports (ISR) database. The database provides a wealth of information, only some of which is used here.

### 3.2 HOW TO THINK ABOUT ETHNORACIAL DIFFERENCES

Q: How do you decide if an outcome depends on the race or ethnicity of the stopped civilian?

A: This report frames the question of ethnoracial differences on each outcome in multiple ways. From a social science perspective, those ways range from less restrictive to more restrictive.

Records in the CPD stop database record the race and ethnicity of the stopped civilian. The three numerically largest groups stopped, in terms of their race and ethnicity, were: Black non-Hispanic civilians, Hispanic civilians, and White non-Hispanic civilians. It is the differences between Black vs. White non-Hispanics, and Hispanics vs. White non-Hispanics, on each outcome, that we investigate here.

Q: Can you explain what you mean by less restrictive vs. more restrictive view on race or ethnicity impacts?

A: Yes. See below.

#### 3.2.1 Least restrictive: Gross impact of race or ethnicity

The least restrictive way to think about these ethnoracial differences is to look at group mean differences on each outcome across the three groups. Simple differences in the average score of

each group on an outcome describe a **gross impact** of ethnoracial category on the outcome. It is called a gross impact because no other factors are taken into account. That is, the difference on the outcome across the ethnoracial groups has nothing removed from it. A gross impact is usually just described rather than tested using statistical analysis. Nevertheless, gross impacts may prove important in the discussion of these results.

Although gross impacts can prove important for many purposes, from a social science perspective we want to know more. We recognize that many other factors link to race, and/or the outcome in question. So we seek an estimate that takes those other factors into account, and then re-examines the connection between race or ethnicity and the outcome after doing that.

### 3.2.2 More restrictive: Net impact of race or ethnicity

Statistical analyses remove the impacts of these other factors from both the race or ethnicity variable, and the outcome in question. After this removal what remains is a **net impact** of ethnoracial category on the outcome. It is called a net impact because it is the amount of connection that remains between ethnoracial categories and the outcome *after* removing the influences of these other factors.

If the analyses work as they are supposed to, the net impact is made up of the connection between two quantities: the portions of the race and ethnicity variables that are unrelated to any of the other factors that have been considered; and the portion of the outcome variable that is unrelated to any of those other factors as well.

Oftentimes, but not always, net impacts of ethnicity or race will be smaller in size – that is, reflect less of an impact – than the gross impacts. There is a tradeoff. The (e.g.) race impact might be smaller in size after taking other factors into consideration. But, depending on the circumstances and one’s perspective, and what statistical model diagnoses reveal, one may be more assured that the link is telling you about more about (e.g.) race per se.

### 3.2.3 Even more restrictive: Statistically significant net impact of race or ethnicity

A third and even more restrictive way to think about these ethnoracial differences is to gauge whether a net impact of ethnoracial differences on an outcome represents something more than just noise in the data or a chance connection.

We rely on the statistical probability associated with a net impact to decide if it is indeed more than just noise or chance. If, given certain assumptions, the statistical probability associated with the net impact in question is very low -- usually this means we would see a result like this due just to chance alone fewer than five times in 100 -- we are more confident that the net impact in question is meaningful in a statistical sense.

Putting aside the specific type of statistical analyses done, in social science investigations of potential disparities in policing, this guidepost – is the net ethnicity or race difference on the outcome statistically significant? – is what is routinely relied upon by those using such studies as part of their inquiry into potential disparities.

### 3.2.4 Most restrictive: Is the statistically significant net impact causal or just correlational?

The fourth and most restrictive way to think about these ethnoracial differences is to test the statistical models we have done, to “look under the hood” if you will, and conduct additional

statistical models. The hope is to learn whether the net impact examined should be interpreted as causal or correlational.

You may have heard the phrase “correlation does not necessarily imply causation” or more simply “correlation is not causation.”

Even though a statistical model might tell us that a predictor like an ethnoracial difference has a statistically significant net impact on an outcome like whether the stopped civilian is patted down or not, we're not sure that it is the race or ethnicity difference *per se* that is responsible for that net impact. Even though we have tried to control for other factors that we have data on in the database, those other factors could still be playing a role. Further, there might be factors outside the variables used in the statistical models that could be playing a role.

So we put the statistical models we have run under the microscope, and conduct additional statistical models, to try to learn whether other factors in our models, or other factors outside our models, could still be playing a role in generating the statistically significant net impact of ethnoracial category that we have observed.

In almost every instance these additional diagnostics suggest the net connections observed here are not assuredly causal in nature, suggesting a correlational interpretation of the link may be the more prudent interpretation. It is not at all unusual when analyzing data sources that do *not* come from a *true* scientific experiment to have doubts about whether the impacts seen are causal.

### 3.2.5 Clarifying causal

As social scientists, when we say that an impact could be causal, we are saying that the impact appears to be related to the predictor alone, and is not influenced by other factors inside or outside the model, or by selection dynamics. In social science it is extremely difficult to prove a causal claim unless a very particular type of study is done: a randomized controlled trial. These are often done in in medicine and public health as well as many other areas.

But the data here are from ongoing operations of the Chicago Police Department, not a randomized controlled trial. Police are more or less likely to encounter civilians of particular races and ethnicities at certain times in certain locations with certain surrounding circumstances based on a whole range of factors. Separating race and ethnicity differences from those other factors in a situation like this is extremely challenging.

This challenge is *not* something specific to the outcomes being investigated here or the database used or the location. This is a *general* challenge that crops up in *almost all non-experimental data*.

### 3.2.6 Gross impacts versus net impacts and the importance question

Q: Because estimates of net impacts attempt to remove influences of other factors, does that mean that these estimates of net impacts of ethnoracial category are more important than the estimates of gross impacts of ethnoracial category?

A: It depends on your point of view.

One could argue, depending upon the policy or practice in question and one's viewpoint, that the simple difference between the experiences of the civilians in the three different ethnoracial groups considered is of primary importance.

Alternatively, one could argue from a social science perspective that the net impacts are more important because they may be more likely to inform the reader about the impacts associated with the key variable in question. The social science goal is to test for the impact of individual factors, like being Black non-Hispanic vs. White non-Hispanic, and attach to that difference the impact that seems to be associated with *just* that difference. That goal is not always met as we see here from additional diagnostics of models, but that is the goal.

Q: But when you control for these other factors, don't you run the risk of *underestimating* the remaining racial and ethnic differences?

A: You could run that risk –social scientists call it the partialling fallacy – depending on how you set up some details in your analyses, what those other factors are in your model, and what your *theory* says about the links between racial or ethnic differences and those other factors.

For a number of technical reasons – multicollinearity assessments, selecting other factors based on comparable other studies, how geography is handled in the models, and the underlying theoretical frames – we would argue that these analyses do not commit the partialling fallacy. But that can be a point for vigorous debate and we recognize that others may disagree with us on this point. Scholars argue as well about the partialling fallacy among themselves.

### 3.2.7 Gross and net impacts and disparate impact and treatment

Q: I do not see anything in your report about legal standards like disparate impact and disparate treatment. Why not?

A: For two reasons. First, the authors are social scientists, not legal scholars. From a social science perspective, the purpose of the analysis is to gauge gross impacts of race or ethnicity differences, or net impacts of race or ethnicity differences, on stop activity, where net impacts are defined in progressively stricter ways. Second, for the outcomes in question here, the authors are not aware of a widely accepted mapping of gross or net impacts as defined in a social science framework onto disparate impact or disparate treatment standards.

We have described four ways, with varying levels of restrictiveness, for gauging impacts of racial or ethnic differences on outcomes of interest. Should all four of those ways be interpreted as relevant to disparate impact? Should some of those ways be interpreted as relevant to disparate treatment? We don't know, but think that the cross-referencing question, and how the cross-referencing might depend on broader features of context, merits conversation between the legal scholars and social scientists.

## 3.3 DESCRIBING ETHNORACIAL DIFFERENCES: LOCATING GROSS IMPACTS OF RACE AND ETHNICITY

Q: I am interested in seeing the average scores of the civilians in each of the three different ethnoracial groups for each post stop outcome you examine. Where do I find that information?

A:

Table 1 below tells you where to find the average score of each of the three ethnoracial groups on each outcome. These differences describe the gross impact of ethnoracial grouping on the outcome in question.



Table 1 Where to find gross impacts of ethnoracial category for each outcome

Post-stop outcome	Ethnoracial group differences found in:
Pat down conducted	Table 10
Weapons/firearms discovered through pat down	Table 12
Search conducted	Table 14, Table 15
Weapons/firearms discovered through search	Table 16, Table 17, Table 18
Weapons/firearms or contraband discovered through search	Table 19
Stopped civilian receives any enforcement action	Table 21
Stopped civilian receives pat down but no enforcement action	Table 22, Table 24

Note. Average scores for each ethnoracial group usually found in the last row of the listed table. All the outcomes listed in this table are scored 0 if the outcome did not happen and 1 if it did. Thus the average score for each group represents the proportion of that group that did experience that outcome.

### 3.4 SELECTING THE OTHER FACTORS

Q: How do you decide what the other factors are that you're going to take into account?

A: We looked carefully at other studies where researchers have investigated questions like the ones being considered here. That, along with a general concern about taking into consideration outcome variation that can be due to time of day, time of the week, or season; and outcome variation that can be due to geographic differences, led to the final selection of other factors.

Q: So what are the other specific factors to take into account in your models?

A: You will find them in Table 26. The variables that do not have a star are used in almost all the models that took multiple factors into account. In addition to race and ethnicity, other variables included gender, age categories, time of day categories, whether the stop happened on the weekend, and whether it was a vehicle as opposed to a pedestrian stop.

In addition to these specific features of individual stops, geographic variation also was taken into account. Geography was taken into account by allowing each police district to have its own average score on the outcome variable being considered.

### 3.5 DESCRIBING GEOGRAPHIC DIFFERENCES

#### 3.5.1 Locating basic geographic differences on outcome variables

Q: I am interested in seeing how scores on each outcome examined vary across police districts. Where can I find that information?

A: Each of the tables listed in

Table 1 above also show differences on the outcome score by police districts. All these tables have a separate row for each police district. The number that appears at the end of each row indicates the average outcome score in that particular district.

### 3.5.2 Geography as an important source of differences on the outcome

Q: Is the geographic variation on outcome scores important?

A: Yes. As you can see from the different proportions for each district for each outcome in the tables noted above, each outcome is more likely to happen in some places and less likely to happen in others. All of the statistical models taking multiple factors into account confirm that the geographic variation in each outcome is more than just chance or noise in the data.

### 3.5.3 Important “left over” geographic differences even after taking model factors into account

But geography matters in a second way as well. For some of the outcomes we examined what was left over, that is, the portion of each outcome that is not predicted by the factors used in the model.

It turns out that for some outcomes that remaining geographic variation suggest “something going on” in some districts. By "something going on" we mean something that is statistically discrepant from the overall picture, and is unrelated to the factors that we used in our models.

For example, take a look at Figure 6. This shows results from analyzing the first random sample. Each district has a filled in circle. If the filled in circle for a district is **below the horizontal line** it means that in that district, even after taking into account ethnoracial category and all the other factors used when predicting whether or not a pat down took place, and even after allowing each district to have its own (adjusted) average score on the outcome, the **proportion of stops resulting in a pat down** in that district is **lower than overall**. If the filled in circle for a district is **above the horizontal line** it means that in that district, even after taking the same factors into account, the **proportion of stops resulting in a pat down** in that district is **higher than overall**.

Q: Why does each filled in circle have lines coming out of it?

A: Those lines take sampling error into account. After we consider that error, our best guess is that the true mean score for that district on that outcome is somewhere between where the upper line ends and the lower line ends.

Q: Are any of these district differences in Figure 6 meaningful?

A: They may be.

Look at the left-most district mean. This is for District 16. Because the lines coming out of the circle do not cross the horizontal line this means that after taking predictors into account stops in this district are still significantly less likely to result in a pat down compared to the overall average across all the districts.

The line coming out of the fourth circle from the left corresponds to District 2. Here too the proportion of stops resulting in a pat down is significantly lower than the overall average, even after taking all factors into account.

Take a look at the two right most circles with vertical lines coming out of them. These correspond to **Districts 6 and 7**. In these two districts, **even after** taking other factors into

account, the proportion of stops here resulting in a pat down in each of these two districts is significantly **higher than the overall average**.

Q: So are you saying there may be something going on in Districts 6 and 7, based on Figure 6, that is unrelated to the factors you used, that is resulting in significantly more stops involving pat downs compared to the overall average across all the districts?

A: We are.

Q: Do Districts 6 and 7 stand out this way when you analyze your second random sample?

A: They do. See Figure 8.

### 3.5.4 Source of significant geographic discrepancies not currently clear

Q: Do you know what is responsible?

A: We do not. In each case it could be something about the district organization itself, something about the mix of people encountered on the street walking or driving, something about the mix of land uses or public transit in these districts, or some other factor(s). We just don't know for certain.

But we did tentatively explore the connection between these district deviations from expected patterns. See Figure 7 and Figure 9.

In each figure, the vertical axis shows the district mean deviation, after taking model factors into account, on proportion stopped civilians patted down. On the horizontal axis is the percent of stopped civilians who were Black and non-Hispanic.

The curvy shows the locally-weighted relationship between these two factors. For both the first and second random samples, it looks like districts where probabilities of a pat down taking place are higher, even after taking model factors into account, are also districts where the proportion of those stopped was more predominantly Black and non-Hispanic.

But this is just an **exploratory descriptive** examination, **with no tests for statistical significance** and is **not definitive**. We just cannot say anything definitive about what this “something going on” is that results in some districts having higher fractions of stops with pat downs than the model expects.

## 3.6 TAKEAWAY LESSONS

Q: What are your most important findings?

A: “Most important” is in the eye of the beholder. From our social science vantage, however, we would focus most attention on those statistically significant net impacts of a Black vs. White difference or Hispanic vs. non-Hispanic White difference that:

- Appear with both random samples using the primary analysis model;
- Appear with both random samples using an alternative analysis model; and where
- There was a low degree of concern about other observed or unobserved factors interfering with the race or ethnic impact observed.

Table 55 organizes the findings using these considerations. Given these considerations, in our view the **strongest** findings were as follows. ***This does not mean we think any of the other***

*findings are necessarily unimportant.* It is just that these highlighted findings seem the most durable, at least from an analytic perspective.

### 3.6.1 Pat downs

A: Significant net differences between White non-Hispanic and Black non-Hispanic stopped civilians appeared on the pat down outcome in both random samples using both the main statistical analysis and the alternative statistical analysis.

The **gross** difference between these two groups in both samples was about 11% or 12%; about 34% to 35% of stopped Black non-Hispanic civilians got patted down as compared to about 23% to 24% of stopped White non-Hispanic civilians. The size of the **net** impact can be expressed in the how odds of this happening versus that happening – the odds of [a pat down happening versus not happening] were higher or lower depending on the group in question. These odds, which reflect net impacts, were anywhere from 19% to 32% higher for Black non-Hispanic compared to White non-Hispanic stopped civilians, depending on the sample and the model. This race differential was always **statistically significant** meaning it was not due to chance or noise in the data. Given model diagnostics this link is probably best interpreted as correlational rather than causal.

### 3.6.2 Pat downs during a stop in which no enforcement action is delivered

A: This outcome is also about pat downs, but only in situations where officers deliver no enforcement action. Procedural justice scholars suggest that getting patted down is intrusive, and if it happens in a stop where no other actions are taken against the civilian he or she may perceive such actions as unwarranted.

In stops where police officers delivered no enforcement action, *both* significant net race and net ethnicity impacts appeared. Black non-Hispanic stopped civilians got patted down significantly more often than White non-Hispanic stopped civilians in these situations, as did Hispanic stopped civilians. The gross difference between White non-Hispanics and the other two groups in these stops was about 12% to 15%; see Table 25. This result is highlighted here because the significant net impact replicated across two random samples and across alternate analytics.

### 3.6.3 Searches and ethnicity

A: The most stringent analyses conducted found that Hispanic stopped citizens, as compared to non-Hispanic stopped citizens, were more likely to be searched.

## 3.7 LIMITATIONS

Q: Does your study have limitations?

A: It has many. These are described in a section of the discussion. Most importantly, though, the results seen here could change if the models we used had taken into account a different set of factors than the ones we used. In addition, there were things we wanted to do either in terms of different types of analytics, or additional diagnostics of the models we used, that we have not yet had time to complete.



## 4 EXECUTIVE SUMMARY

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This report analyzes investigative stop report data from the Chicago Police Department for the period 1/1/2016-6/30/2016. Five different outcomes from these stops are analyzed. Simple differences on *each* of these outcomes across the three ethnoracial groups of interest – stopped Black non-Hispanic civilians, stopped White non-Hispanic civilians, and stopped Hispanic civilians – are also displayed in a series of tables (see Table 1 in the FAQ section above). These allow the reader to examine differences by race and ethnicity as well as differences by location, and race and ethnicity differences within locations. These simple differences, which we also call **gross impacts** of different ethnoracial categories, are of interest in their own right.

Beyond these descriptive differences, of key interest is whether, after taking into account other factors, there are differences on a post stop outcome associated with civilian race, civilian ethnicity, or civilian gender. A **net impact** of an ethnoracial category difference refers to these associations observed after controlling for other factors.

Of even more interest are net impacts of ethnoracial category that prove statistically significant. This means the net link observed is likely not due to chance or noise in the data.

Further, if a statistically significant net impact is found, statistical models are diagnosed to learn whether that connection is best interpreted as causal or correlational. If the interpretation is correlational only, that is because other factors, or selection dynamics, may play roles in “driving” the net connection between the ethnicity or race variable and the outcome.

Finally, we conduct alternative statistical models to learn if statistically significant net impacts of race or ethnicity can be repeated using models that make different assumptions.

This executive summary focuses on key findings for race and ethnicity and suggested interpretations. Much of this section is repeated in the final Key Findings section at the end. One also can find there a summary table (Table 55).

**Pat downs.** The strongest pattern revealed by these analyses are net connections between race and whether a pat down occurred, and between ethnicity and this outcome. Both analytic approaches yielded statistically significant net connections in both samples.

Diagnostics of both types of pat down models, however, suggested a moderate level of potential concern about observed and unobserved selection biases. Stated differently, there were other things going on that were not handled sufficiently by the analytics. Given that, the net race and ethnicity impacts are probably best interpreted as correlational. Nonetheless, the links were there, after controlling for other factors, and for district context. As compared to White non-Hispanic civilians, Black non-Hispanic and Hispanic civilians were more likely subjected to a pat down.

**Pat downs leading to weapons.** Previous work on pat down and search hit rates suggested that pat downs of Black and Hispanic civilians would be less likely to lead to recovered weapons. This turned out to be true when examining weapons produced from pat downs, after controlling for other factors and district context. It held for Black as compared to White civilians. Hit rates were significantly lower in both random samples in the regression analyses for Black as compared to White non-Hispanic civilians.

The significant net race effect did not resurface, however, using more stringent analytics. Again, diagnostics suggested some concerns. The conclusion seems to be that there is a net race effect, but it is probably correlational and was just not quite strong enough to be robust across alternate analytics.

**Searches.** The search outcome results showed no significant net race effects. But significant net ethnicity links appeared, for both samples, using the more stringent alternative analytics. Hispanic civilians were more likely to be searched than non-Hispanic White civilians after controlling for other factors. Diagnostics suggested some level of concern, so the conclusion about ethnicity and the search outcome is that the link is probably correlational, but not robust across different approaches.

Reviewers have raised some worthwhile points with regard to the search outcome. In essence they argue that removing stops where searches and arrests happened may have inappropriately dropped a large number of stops, and were those included a different picture of net race impacts might appear. Future work will address this concern.

**Any enforcement action delivered.** The enforcement outcome yielded robust net ethnicity links across both samples and both analytic approaches.

Stopped Hispanic civilians, as compared to stopped Black non-Hispanic civilians, were significantly more likely to be subjected to some type of enforcement action during the stop. The gross impact of ethnicity was as follows (Table 21): whereas 28 percent of stopped White non-Hispanic civilians received some type of enforcement action, 31 percent of stopped Hispanic civilians received such an action.

Net race links surfaced only with one analytic approach. The conclusion seems to be, in light of diagnostics, that for both race and ethnicity there is a net connection with this outcome, that for both it is probably best considered correlational, and that for race it is not robust across alternative analytic approaches.

**Pat down and no enforcement.** The last outcome examined contrasted two types of stops, no enforcement action and no pat down vs. no enforcement action and receiving a pat down. Analyses included both a main and an alternate approach. No diagnostics of either analytic model have yet been completed.

Across both analytic approaches, significant net race and ethnicity effects surfaced. After controlling for other factors and district context, in stops where no enforcement actions were taken by police, Black non-Hispanic and Hispanic stopped civilians had much higher odds of being patted down than did stopped White non-Hispanic civilians. Given the potentially corrosive nature of police interactions such as this, this would seem to be an important pattern to address.

These net race and ethnicity links should be considered correlational only at this time, since no diagnostics have been completed.

The gross impact was as follows. In 38 percent of the Black non-Hispanic stops with no enforcement, and in 36 percent of the Hispanic stops with no enforcement, a pat down was delivered. The corresponding percent for stopped White non-Hispanics in stops with no enforcement was 24 percent.

## 5 SCOPE

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This report analyzes investigatory stop report (ISR) records generated by the Chicago Police Department (CPD) during the period January 1, 2016- June 30, 2016.

Analyses consider multiple post-stop outcomes.

The unit of analysis is the individual stop.

The focus is on understanding the connections between civilian race, ethnicity, and gender differences and each of these outcomes.

The connections are considered in a number of different ways.

First, the connections are considered on their own, without taking other factors into account. These represent gross impacts of race or ethnicity differences on the outcome.

The connections are also considered using progressively stricter criteria.

So the second examination asks: Does the difference persist after controlling for other factors? We refer to these as net impacts.

The third examination asks: Is the net impact statistically significant, that is, unlikely to be due to chance alone?

And finally, after reviewing model diagnostics, and perhaps conducting alternative analytics, the fourth examination asks: is a statistically significant net impact more appropriately interpreted as causal or correlational?

### 5.1 OUTCOMES OF INTEREST

What happens after a stop has been initiated, has important practical and policy repercussions. This report considers the racial and ethnic patterning of post-stop outcomes. Questions of who is stopped where is addressed in a different ecological report.

The following specific post-stop outcomes receive attention here:

- A. Is a pat down conducted or not?
- B. If a pat down is conducted, is a weapon found?
- C. Is a search conducted or not?
- D. If a search is conducted, is a weapon found?
- E. Is any enforcement action delivered or not?
- F. What are the chances that the stopped civilian experienced a pat down combined with no enforcement action vs. no pat down and no enforcement action?



## 5.2 QUESTIONS ADDRESSED

### 5.2.1 Descriptive

To provide descriptive context, simple race and ethnicity differences, and district differences, are portrayed for these outcomes.

Although statistical tests are often not applied to these differences, these descriptive differences between ethnoracial categories represent an important part of the examination.

### 5.2.2 Involving statistical inference

For each outcome, the question is the same:

**The race question.** Controlling for observed covariates, i.e., other relevant factors, is there a statistically significant net difference on outcome scores between non-Hispanic Black civilians and non-Hispanic White civilians?; and

**The ethnicity question.** Controlling for observed covariates, is there a statistically significant net difference on outcome scores between Hispanic civilians and non-Hispanic White civilians?

Stated differently, each model tests a null hypothesis of no difference between non-Hispanic White civilians and either non-Hispanic Black civilians or Hispanic civilians after controlling for observed covariates and district context.

Potential net gender links with each outcome are of interest as well.

## 6 BACKGROUND: POLICE POST STOP OUTCOMES

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### 6.1 GENERAL

At the time of the current study, researchers have been investigating questions of racially or ethnically biased policing, for civilians on foot and in cars stopped by police, for well over two decades (Banks, 2003; Beckett, Nyrop, & Pflingst, 2006; Brunson & Miller, 2006; Engel, 2008; Engel & Calnon, 2004; Engel, Calnon, & Bernard, 2002; Engel & Tillyer, 2008; Fagan, 2002; Fagan & Braga, 2015; Fagan, Geller, Davies, & West, 2009; Fridell, 2005; Gelman, Fagan, & Kiss, 2007; Grogger & Ridgeway, 2006; Harris, 1997; Jernigan, 2000; Lundman & Kaufman, 2003; MacDonald, Stokes, Ridgeway, & Riley, 2007; Meares, 2014; Ridgeway, 2006, 2007a, 2007b, 2009; Ridgeway & MacDonald, 2010; Ridgeway & MacDonald, 2009; Ridgeway & Riley, 2007; Rojek, Rosenfeld, & Decker, 2012; R. Tillyer, Engel, & Cherkauskas, 2010; Rob Tillyer, Klahm, & Engel, 2012; Tyler, Fagan, & Geller, 2014; Walker, 2001).

At the broadest level, for social scientists investigating potential racial or ethnic disparities for a particular post-stop outcome, there are two broad challenges for the analyst: separation and selection. These are described below.

**Separation** refers to separating out three different sources that could be contributing to a racial or ethnic differences – or any other group based difference -- in police recorded behaviors (Ridgeway, 2009; Walker, 2001). (1) The race or ethnicity linked police differential could arise

from differences across groups or across locations in the amounts of recorded or reported criminal/disorderly behaviors drawing attention from officers. (2) The different groups might experience differential exposure to patrolling officers. If some neighborhoods are more heavily policed because of crime or calls for service differences, and if there are racial and or ethnic differences in who is found walking or driving in those neighborhoods, the racial or ethnic differential in exposure could lead to differences in police stop or post stop outcome rates. (3) The third possibility is that police are treating members of different groups in disparate ways. Research has underscored the many problems with finding indicators that can reliably be used to estimate sources (1) and (2), and remove that variation (Ridgeway & MacDonald, 2010), so that the size of (3) can be gauged.

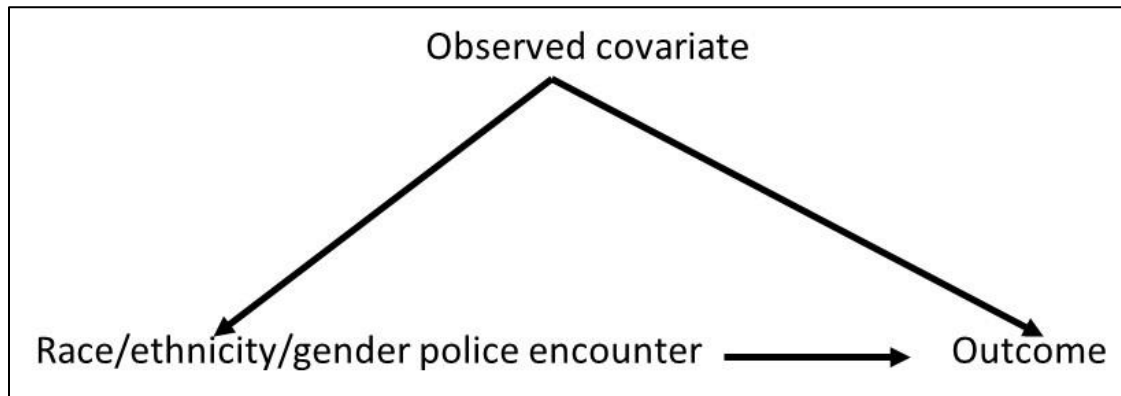
**Selection** is used here to refer to three distinct but related dynamics. Researchers also seek to gauge the size of these three dynamics. If these three different dynamics can be either estimated or ruled out, then the researcher can make a stronger case that the connection between the race or ethnicity indicator, and the outcome, if such a connection is observed, arises from causal rather than correlational processes. The literature on these matters refers to treatment and control groups. For example, the researcher might be interested in comparing intensive supervision probationers to regular supervision probationers (Petersilia & Turner, 1990).

The data considered here are observational not experimental data. “Observational data generally create challenges in estimating causal effects” (Imbens & Wooldridge, 2009: 7).

Here, selection dynamics refer to differences between stopped Non-Hispanic Black civilians vs. stopped White civilians, or between stopped Hispanic vs. Non-Hispanic White stopped civilians, or between stopped men vs. stopped women, rather than treatment and control groups. This shift in conceptual frame is at some level problematic. The race and ethnicity of civilians encountered by police link to so many aspects of where people live (Peterson & Krivo, 2010). Further, race, ethnicity and gender link to so many features of people’s interactions with others (Delgado & Stefanic, 2012; Reskin, 2012). Consequently, the challenge of disentangling or un-confounding impacts on police recorded behavior of the race or ethnicity or gender of the civilians they encounter, from relevant other attributes, seems Herculean. Nevertheless, attempts to disentangle proceed.

In the situations examined here, the selection problem has three aspects: selection on observables (Figure 1), selection on unobservables (Figure 2), and sequential selection (Figure 3). Each is explained in turn.

Figure 1. The Problem of selection on observed covariates

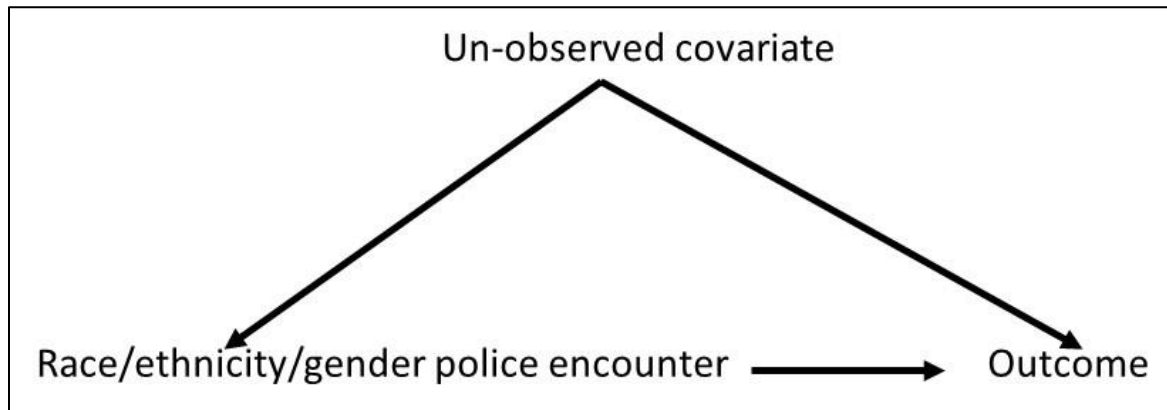


*Selection on observables.* This type of selection, also known as “unconfoundedness, exogeneity, [and] ignorability,” represents an assumption that must be satisfied if one is to support causal interpretations for the impact of a treatment, or, here, the race, or ethnicity or gender variable, on an outcome (Imbens & Wooldridge, 2009: 7). “All these labels refer to some form of the assumption that adjusting treatment and control groups for differences in observed covariates, or pretreatment variables, remove all biases in comparisons between treated and control units ... Without unconfoundedness, there is no general approach to estimating treatment effects” (Imbens & Wooldridge, 2009: 7). Therefore, patterns arising from different diagnostics associated with different models merit scrutiny to learn whether the selection on observables can be ruled out. If it can, a causal interpretation of racial or ethnic or gender impacts receives more support. If it cannot, a correlational interpretation receives more support.

For example, in the current work, age of stopped civilians is known, so this is an observed covariate. So its influence can be controlled, and patterning between this covariate and outcome residuals can be examined to see if connections remain.

*Selection on unobserved covariates.* With observational data, key differences between two groups of interest could be present but not detected. That is there could be “differences due to unobserved covariates” and this “should be addressed ... using models for sensitivity analyses” (Rubin, 2001: 173). “Unobserved covariates” refers to factors outside of those used in the model.

Figure 2 The Problem of selection on un-observed covariates



So the challenge here is estimating the potential impacts of *factors not included in the models analyzed* that might be linked to either race or ethnicity.

Different diagnostics for different models help gauge whether selection on unobserved covariates is a sizable concern. If it is a sizable concern, then a causal interpretation of an observed race or ethnicity or gender impact is unwarranted; rather, the interpretation should remain correlational.

*Sequential selection.* This refers to a well-known problem in economics, sociology, criminology, criminal justice and other social and hard sciences (Babu & Jang, 2006; Berk, 1983; Bushway, Johnson, & Slocum, 2007; Bushway & Reuter, 2008; Fu, Winship, & Mare, 2004; Heckman, 1979). The problem surfaces if the data for an outcome variable gets collected only if something else happens prior to that. In criminal justice, for example, whether or not a defendant found guilty is sentenced to one or more years of prison, or to a less severe sentence, depends on the defendant being found guilty in the first place. A researcher studying the determinants of sentence severity would want to take into account and control for the determinants of the prior outcome, obtaining a guilty verdict. Should the researcher fail to model those prior selection dynamics, answers she obtains to her main question of interest, the determinants of more vs. less severe sentences, could be misleading.

Sequential selection surfaces as a concern in research on race or ethnicity or gender and police activities such as driver stops, pedestrian stops, frisks of stopped civilians, or searches of stopped civilians. Ideally, and this has been done in some of the driving while Black research (Grogger & Ridgeway, 2006), one wants to control for or at least neutralize the factors associated with one group being more likely to be stopped in the first place.

Of most relevance here, and separate from being selected for a stop in the first place, are other sequential selection concerns if a post stop outcome depends upon the prior occurrence of an earlier post stop outcome. Whether a pat down results in weapons being located requires that a pat down occur in the first place. Whether a search results in weapons being located requires that a search occur in the first place.

The sequential selection analytic concern aligns with a broader theoretical assumption about policing behavior prior to and during civilian stops: that officers are making a series of decisions prior to and during a stop. For example, Fallik and Novak (2012: 148) discuss three decision points in the case of automobile stops:

Racial profiling within automobile stops has focused on three distinct officer-initiated decision-making points that can measure the presence of racial and/or ethnic non neutrality ... The first is the officer's decision to initiate a stop. This decision-making point typically considers the propensity of racial and ethnic minorities to be stopped or whether Blacks or Hispanics are stopped at a higher rate than their community representation ... The second officer-initiated decision making point considers an officer's application of formal sanctions or the exercise of coercion. Research from this decision-making point considers the propensity of racial and ethnic minorities to be warned, cited, arrested, and have force used against them ... The third decision-making point considers minority representation in searches ... An extension of this line of inquiry involves analyzing the contraband hit rate or outcome test during searches[.]

Backing up this idea of sequential decision-making points during stops are numerous studies of how police respond to ongoing civilian actions and civilian demeanor during stops (Mastrofski, Reisig, & McCluskey, 2002; Reisig, McCluskey, Mastrofski, & Terrill, 2004; Terrill & Mastrofski, 2002).

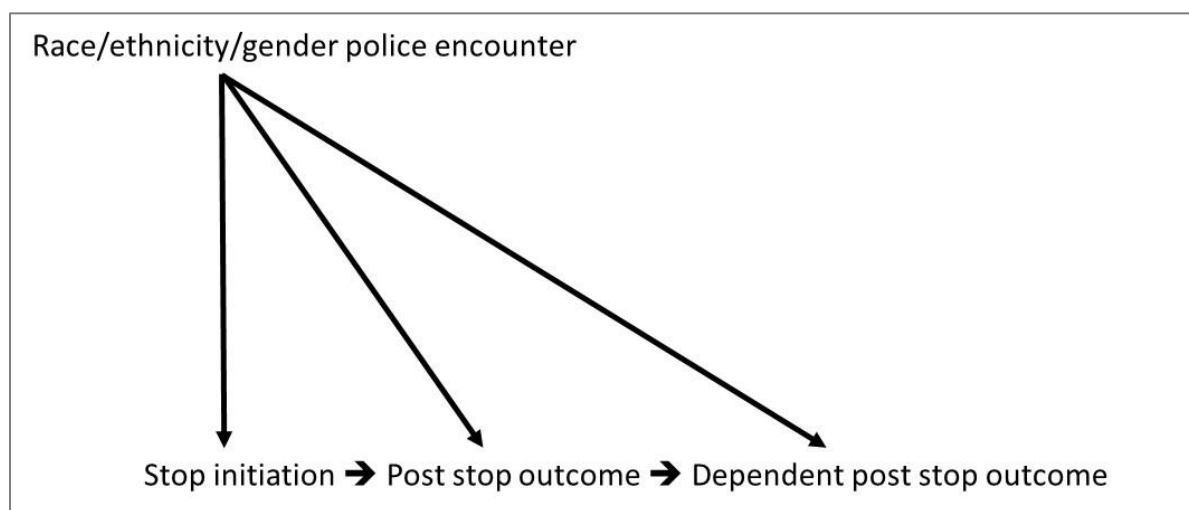
No specific view is promoted at this point about what the specific sets of distinct decision-making or action-selection dynamics are, or about what the temporal relationship might be between the different sets of dynamics, or how one set of dynamics might condition the other. Those are important theoretical questions to be addressed by others. But the general idea of officer sequential decision making prior to and during a stop does lead to the following points that are relevant here.

- (1) There are at least two distinct but certainly related sets of dynamics: those leading to the stop, and those involving whether certain actions are initiated after the stop. The degree of relatedness or overlap across these dynamics is not known. The degree to which **each** of these dynamics is racially or ethnically linked represent important and distinct questions.
- (2) The fact that information, associated with officers selecting or not selecting a civilian for a stop, is not available here, places an important limitation on this report. It means that **all the outcomes examined in this report fail to report for the first stage of sequential selection.** The extent to which this omission is problematic cannot be estimated with the information available. That said, this omission similarly plagues a large number of other studies of police/civilian post stop outcomes.
- (3) Once a stop is underway, if the outcome being examined depends on an action the officer took subsequent to initiating the stop but before this outcome is known, then there is a third set of dynamics which may be racially/ethnically/gender linked. Consider pat down weapon hit rates for example. Race or ethnicity may link to this outcome. Those racial or ethnic links to that outcome could be different than the race or ethnicity links to whether the civilian gets selected for a pat down in the first place.
- (4) In each post stop dynamic, race or ethnicity or gender may play distinguishable roles. For example, race or ethnicity could be involved only in pat down selection; race or ethnicity

could be involved only in determining pat down outcomes; or, race or ethnicity could be involved in both pat down selection and in pat down outcomes.

- (5) As far as these authors understand, researchers in this field have yet to make a clear case about why race or ethnicity should be involved in some of these post stop dynamics and not others. Therefore, analyses of outcomes that depend on the officer doing something else first, while the stop is underway, need to gauge all possible ways race or ethnicity could contribute to each of these dynamics. That is, for these outcomes sequential selection needs to be modeled. Studies investigating post stop outcomes that *fail* to explicitly also model the selection dynamic (see for example Carroll (2014)) may be generating misleading results.

Figure 3 The Problem of sequential selection



So the race or ethnicity or gender of the person encountered, could have contributed in three separate ways to the chain of events leading to a weapon being produced or not produced from a pat down. Any one of these, or all of these, could have affected the chances that the civilian would be stopped. Any or all of them could have affected the chances that the stopped civilian would be patted down. And, finally, any or all of them could have affected the chances that the pat down would produce a weapon.<sup>1</sup>

In short there are three different ways race or ethnicity or gender could affect officers' discretionary decision making. There are three processes, in sequence, that lead in each case to some person or some action being selected. Consequently, the experts sought to disentangle some of processes. That is, in the case of this outcome, they wanted to estimate the impacts of

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<sup>1</sup> The reason for a potential race or ethnicity link to the last outcome arises from what researchers call the subgroup validity problem. In non-technical terms, members of one group may engage more frequently in verbal or nonverbal behaviors that the officers' training suggest are clues to acting suspiciously or having something to hide. But the higher rate of doing those things may just be a group difference, not a clue to something suspicious. So a Black and a White stopped civilian may both be engaging in the same set of behaviors indicating something to hide, but the Black civilian may in fact actually be less likely to be hiding something.

race or ethnicity or gender on the pat down outcome – was a weapon found? -- *separate from* the impacts of each of these on the pat down occurring in the first place. They had no way of separating out these sequential selection dynamics from the selection factors associated with stop initiation in the first place.<sup>2</sup>

These selection dynamics reflect officers' discretion. Figuring out when highly discretionary decision making shades into racially- or ethnically- or gender-*biased* decision making is a tough call. Research on criminal justice decision making does suggest that more highly discretionary decision points have greater chances of being influenced by decision makers' biases (Gottfredson & Gottfredson, 1988). But for now the goal is just to learn about how race or ethnicity or gender link to each of these decision points or outcomes, while taking what happened earlier in the stop into account.

## 6.2 COMMENTS ON SPECIFIC OUTCOMES

### 6.2.1 Hit rate outcomes

Search hit rates have drawn particular interest in the driving stop and pedestrian stop literatures. An example hit rate would be: in the case of police stops on a major interstate, what fraction of vehicles searched produced drugs? In the civilian pedestrian stop context, if the purpose of the police stop strategy is to interdict those carrying weapons who are in high crime locations at high crime times, one can ask: is the fraction of searches of Black pedestrians producing a weapon lower than the same fraction for stopped and searched White pedestrians? Economists, making certain assumptions, have provided the conceptual underpinnings for the hit rate analysis (Knowles, Persico, & Todd, 2001; Persico & Todd, 2008).

Other researchers question the assumptions behind this model (Barnes, 2005; Ridgeway & MacDonald, 2010: 22 [online]). The potential subgroup validity problem (Ayres, 2002) seems to be the biggest concern. Simply put, the kinds of verbal and nonverbal factors police are trained to use during a stop to gauge civilian suspiciousness happen at different base rates in different racial/ethnic groups. "The subgroup validity problem remains a concern for the application of the outcome test to police searches ... verbal and non-verbal behavioral cues to suspicion and deception are not racially neutral. Thus the accuracy of suspicion cues will likely differ across racial/ethnic groups. Conclusions of racial bias cannot be made using the outcome test" (Engel, 2008: 24). Controversy about the outcome test continues (Engel & Tillyer, 2008; Persico & Todd, 2008).

### 6.2.2 Frisk or pat down and release

An outcome not previously examined in stop, question and frisk research is introduced here: civilians being patted down and released vs. released without a pat down. Two arguments warrant its examination.

First, situated accounts of police civilian interactions highlight that pat down and release does occur and that it does bother civilians (McArdle & Erzin, 2001; Simon & Burns, 1997). Such

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<sup>2</sup> This is because only data on stopped individuals were available. There is no information about persons in comparable situations but not stopped by CPD officers.



interactions contribute to tension between inner city Black residents and police (Brunson, 2006, 2007a; Brunson & Gau, 2011; Gau & Brunson, 2010). To be patted down and released may strike many residents of color as simply being hassled by police (McArdle & Erzin, 2001).

Further, this outcome seems particularly relevant given a procedural justice perspective (Sunshine & Tyler, 2003; Tyler, 1988, 1997, 2001, 2003; Tyler & Huo, 2002; Tyler & Lind, 2001). The outcome reflects a component of the construct "degree of police intrusion during... stops" (Tyler et al., 2014: abstract), an outcome recently introduced by procedural justice scholars.

Tyler, Fagan, and Geller (2014: 763) used telephone survey data of young men living in New York City to learn about impacts of their contacts with police on both their views of police legitimacy and their willingness to cooperate with police and courts. In describing "general neighborhood experiences with police" participants reported on the "degree of intrusion during those stops" happening near where they lived. Several survey items contributed to a broader index reflecting intrusion. One of the items in this index was "did the police... 'Frisk or pat you down'" (Tyler et al., 2014: 784).

Would most agree that a stop ending with a pat down and release is more intrusive than a stop and no pat down and release? This certainly seems to be the implication of the work by Tyler, Fagan, and Geller (2014). Those authors observed significant impacts of police intrusiveness on respondents' willingness to cooperate with police (Table 6). This aligns with much of the ethnographic work on urban Black residents and police agrees that unwarranted frisks are intrusive and affects residents' views of police (Brunson, 2005, 2006, 2007b).

That said, no inferences are drawn about the fraction of frisk-and-release stops where police had grounds for a much more intrusive stop such as for example a frisk-and-cite or frisk-and-search stop, or a frisk-search-and-arrest stop. Nor are any inferences made about the fraction of no-frisk-and-release stops where police similarly might have had grounds for more intrusive actions.

## 6.3 ANALYTIC CONCERNS

### 6.3.1 Internal replication across independent samples

Two representative random samples of data were available after sampling. Tests of statistical significance were then conducted on both samples. If a key statistically significant finding surfaced with one sample also reappears as significant in the second sample, then the statistical finding has been internally replicated. Internally replicated significant findings inspire more confidence. They suggest the findings are robust across independent random samples. They suggest that the linkage observed does not depend on something about the particular mix of records found in one sample but not the other.

### 6.3.2 Internal replication across alternative analytic approaches

The main statistical analysis used throughout is **multiple regression**. This is used in many different studies examining potential racial or ethnic disparities in policing. For example, the agreed upon statistical benchmarks as a result of the consent decree emerging from *Bailey et al. v. City of Philadelphia* use multiple regression models.



**Such models are used here, with some minor improvements. The improvements are in line with the current best practices for scholarship in this area.** First, if the outcome is binary it is modeled as binary rather than normally distributed. Second, mixed effects models separate random variation by district on each outcome, and allow for correlated errors within districts. They also make Empirical Bayes adjustments to district-level means.

In one case the outcome is categorical so the model used rather than logistic multiple regression is multinomial multiple regression.

But, in addition to these main multiple regression models, we employed **for every outcome an alternate analytic strategy**. Doing so allows us to learn whether a particular statistically significant net impact of a race or ethnicity difference is robust across different models with may make different assumptions and/or use the data in different ways.

So this allows for a different type of internal replication to see if results are robust across different statistical approaches.

### 6.3.3 Clustered data

The data here represent stops taking place within a specific police district. That clustering has numerous statistical and analytic implications (Snijder & Bosker, 2012). It is taken into account in different ways with the different models used.

### 6.3.4 Statistical power

A priori power analyses were run (see below) and used to guide selection of the alpha level.

### 6.3.5 Multiple correlated outcomes

This report analyzes multiple outcomes. They do not correlate sizably with one another; all correlations are well below .10. We do not think there is an inflated experiment-wise error rate (Aickin & Gensler, 1996). But if the reader was still concerned, he or she could make his/her own internal Bonferroni adjustment by only considering effects that are significant at  $p < .01$  rather than  $p < .05$ .

## 7 METHODOLOGY

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### 7.1 DATA SOURCES

Chicago Police Department (CPD) personnel made available monthly csv files containing the final version of each Investigatory Stop Report (ISR) available during the period. Each record represented an individual stop report. The files contained data relevant to each field in the ISR form adopted by CPD in January, 2016.

### 7.2 TERMS

An individual **stop** references one particular stopped civilian whose information was recorded by officers during any type of interaction recorded in the ISR database. These include vehicle stops, pedestrian stops, and gang enforcement. An **event** refers to stops which are grouped together. For over 99 percent of the records here, that grouping of stops was based on unique CPD event numbers [field = **event\_no**]. For the remaining less than one percent of the cases stops were

grouped if they shared the same date, the same district, the same beat, the same starting hour and minute, and the same first officer star number.

### 7.3 DATA PROCESSING

CPD sent monthly csv files. Data processing included the following steps. Date and time variables were checked for out of range values and recoded to missing as needed. Numeric variables were created from string variables as needed. Age values below 7 were recoded to 7 and ages above 90 were recoded to missing given the ambiguity in some of the values (was 115 15 or 11?)

Data were de-duplicated so there was only one record with each individual ISR number.

Authors understand from the CPD that in January sometimes different ISRs were generated for the same stop. Those are not removed here.<sup>3</sup>

CPD uses a field for event number (**event\_no**) to keep track of different events. This was missing for 424 of 54,701 records (0.78 percent). For these records a proxy event number was generated based on different records taking place in the same district on the same day at the same time and with the same responding first officer. A dummy variable (eventmis) was included in analyses to control for the fact that for some number of records a proxy event number was used.

Using assigned and proxy event numbers permitted gauging the number of stops per event. The distribution appears in Table 2. The number of stops per event ranged from one to 21. Over half of the stops involved three or fewer stops per event.

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<sup>3</sup> One way to resolve that matter would have been to randomly sample one ISR number per event number. That was not done, given the importance attached to analyzing all the stops taking place.

Table 2 Number of individual stops per event

N of stops per event (variable = event_n3)	N	Percent	Cumulative Percent
1	10,435	19.08	19.08
2	11,520	21.06	40.14
3	9,936	18.16	58.3
4	7,416	13.56	71.86
5	5,510	10.07	81.93
6	3,408	6.23	88.16
7	2,569	4.7	92.86
8	1,456	2.66	95.52
9	999	1.83	97.35
10	620	1.13	98.48
11	308	0.56	99.04
12	204	0.37	99.42
13	130	0.24	99.65
14	42	0.08	99.73
15	60	0.11	99.84
16	32	0.06	99.9
17	17	0.03	99.93
18	18	0.03	99.96
21	21	0.04	100

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 Total            54,701            100  
 Source: Jan-June 2016 ISR data, CPD

Indicator (or dummy) variables where 1 = quantity present and 0 = quantity not present were created for gender, race, and ethnicity, various times of day, days of the week, months, and age ranges.

The original distribution of race/ethnicity codes used by CPD personnel in the field **RACE\_CODE\_CD** appears in Table 3. This report will focus on three racial/ethnic groups: White non-Hispanics, White Hispanics, and Black non-Hispanics. Stops associated with other races or ethnicities are dropped from the analysis.<sup>4</sup> This permits a clean focus on three mutually exclusive racial/ethnic groups most prevalent in Chicago. These three groups represent 54,116 out of 54,701 cases and 98.9 percent of ISR records for the period.

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<sup>4</sup> The small number of Black Hispanics in the data are dropped so that the three groups of interest are completely exclusive of one another.

Table 3 Counts and categories for CPD variable for race and ethnicity (RACE\_CODE\_CD)

Description	Code	N	Percent
Asian Pacific Islander	API	417	0.76
Black	BLK	38,361	70.13
American Indian / Alaskan Native	I	98	0.18
Undocumented code	P	67	0.12
Black Hispanic	WBH	3	0.01
White	WHI	35	0.06
White	WHT	4,163	7.61
White Hispanic	WWH	11,557	21.13
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	Total	54,701	100
Three group sub-total: White non-Hispanic, White Hispanic, Black non-Hispanic	Sub-Total	54,116	98.93

Note. Period = January-June, 2016. Source: CPD ISR data

The distribution across districts of the three predominant racial/ethnic groups among stopped civilians appear in Table 4. Among these three groups, Black non-Hispanic civilians are the group most frequently stopped, making up almost 71 percent of the stops of members of these three groups. Hispanic civilians comprised 21 percent of those stopped in these three groups. And White non-Hispanic stopped civilians occurred least frequently, appearing in about eight percent of the stops.

That said, the racial/ethnic mix often varied markedly by district. Stopped Black non-Hispanic civilians contributed 98 percent of the stops in district 3, but only sixteen percent among the stops in district 17. Stopped Hispanic civilians made up 61 percent of those stopped of these three groups in district 14, but less than one percent in district 3.

Table 4 Number of stopped civilians by district and race/ethnicity, and district percent by race/ethnicity

District	White NH		Black NH		Hispanic		District total	
	N	Percent	N	Percent	N	Percent	N	Percent
1	126	15.27	640	77.58	59	7.15	825	100
2	37	1.61	2,237	97.43	22	0.96	2,296	100
3	34	1.11	3,027	98.47	13	0.42	3,074	100
4	85	2.37	2,900	80.85	602	16.78	3,587	100
5	25	1.3	1,867	97.29	27	1.41	1,919	100
6	37	1.49	2,417	97.3	30	1.21	2,484	100
7	59	1.23	4,694	97.65	54	1.12	4,807	100
8	332	9.49	1,552	44.34	1,616	46.17	3,500	100
9	380	8.85	1,572	36.6	2,343	54.55	4,295	100
10	110	2.76	2,683	67.36	1,190	29.88	3,983	100
11	341	5.91	5,113	88.66	313	5.43	5,767	100
12	256	11.07	1,041	45.01	1,016	43.93	2,313	100
14	126	13.98	224	24.86	551	61.15	901	100
15	68	2.13	3,033	95.2	85	2.67	3,186	100
16	612	47.3	330	25.5	352	27.2	1,294	100
17	303	27.03	181	16.15	637	56.82	1,121	100
18	125	13.31	705	75.08	109	11.61	939	100
19	279	21.83	717	56.1	282	22.07	1,278	100
20	179	22.1	288	35.56	343	42.35	810	100
22	65	5.37	1,120	92.56	25	2.07	1,210	100
24	381	18.82	1,081	53.41	562	27.77	2,024	100
25	238	9.51	939	37.51	1,326	52.98	2,503	100
Total	4,198	7.76	38,361	70.89	11,557	21.36	54,116	100

Note. NH = non-Hispanic. Period = January-June, 2016. Source: CPD ISR data. Counts only shown for the three most predominant racial/ethnic combinations among stopped civilians. Percentages shown are the district share associated with each racial/ethnic combination.

## 7.4 SAMPLING

The data for the period were separated into two independent 50 percent random samples. Random numbers between 0 and 1 were generated for each record. The numbers followed a uniform distribution. A median split on the random numbers generated two independent samples.

## 7.5 UNITS OF ANALYSIS

The unit of analysis is the individual (person) stopped, that is, each individual stop.

## 7.6 CLUSTERING

Multiple stops can and do occur within a single event. Further, events are nested within districts. Attempts to model these three levels – stops within events within districts – failed to converge. Therefore models presented control only for the clustering of stops within districts. Mixed effects models with stops at Level 1 and districts at Level 2 are used.

Future models will attempt to simultaneously control for the clustering of stops within events, and events within districts.

## 7.7 GEOGRAPHIES AND IMPLICATIONS FOR ANALYSES

The current report uses Chicago Police Department districts as the geographic unit of clustering. Since there were only a small number of these this creates some analytic limitations (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016). Given the limitations associated with only 22 grouping units, these analyses do not incorporate specific district-level predictors. They simply allow the outcome to differ across districts, and incorporate district-to-district differences as random effects in these models.

There is one instance where a stop feature, the district-level proportion of stopped Black civilians, is used as part of diagnostic routines. It is probably advisable in the future to move to smaller within-district units of analysis such as beats within districts.

## 7.8 OUTCOME VARIABLES

### 7.8.1 Overall descriptive statistics

Descriptive statistics on the binary outcome variables appear in Table 5. Details on the levels and patterns for each variable are described further below.<sup>5</sup>

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<sup>5</sup> Earlier circulated analysis plans, and discussions with the City's and ACLU's experts referenced additional outcomes beyond those mentioned here. Those additional outcomes included any pat down hits where the latter were defined as either weapons or contraband, a pat down hits based only on contraband, any search hits resulting in either weapons or contraband, and any search hits resulting in contraband. Time did not permit including models of those other outcomes. Given the policy salience of weapons and weapons recovery, hit rate analyses here focused only on weapons.

Table 5 Descriptive statistics, binary outcome variables

Variable	Variable name	N	Min.	Max.	Mean	SD	Median	Sum
pat down conducted	dpat	54,116	0	1	0.339	0.473	0	18,364
pat down→weapon (*)	pathit_w2	18,364	0	1	0.025	0.157	0	465
Search conducted	dsearch	54,116	0	1	0.177	0.382	0	9,595
Search →weapon v. 1 (*) <sup>a</sup>	se_hit_w	54,116	0	1	0.006	0.076	0	313
Search→weapon v. 2 (*) <sup>b</sup>	se_hit_w2	9,595	0	1	0.027	0.163	0	263
Search→weapon v. 3 (*) <sup>c</sup>	se_hit_w3	2,640	0	1	0.009	0.095	0	24
Enforcement action taken	denforce2	54,116	0	1	0.322	0.467	0	17,425

Note. For all binary outcomes, 1 = outcome occurred, 0 = did not occur

Note. For each outcome variable 1 = yes; 0 = no.

Source: Data from January-June 2016 ISR reports, CPD. MED. = median

(\*) = dependent variable depends on selection through another dependent variable.

<sup>a</sup> On this version of the “search hits on weapon” variable, a hit counted as recovering either a firearm or another type of weapon or, as happened in ten instances, both.

<sup>b</sup> On this version of the “search hits on weapon” variable, a hit was as defined above in v. 1, but 0 was recoded to missing if officers did not check the search box on the ISR form. The discrepancy between v. 1 and 2 of the search weapon hit variable summary count indicates 50 instances where officers indicated a weapon or firearm was recovered from a search but the search box was checked “N”. This was verified directly.

<sup>c</sup> On this version of the “search hits on weapon” variable, a hit was as defined above in v. 2, but 0 was recoded to missing if an arrest took place during the stop. This removed from the variable all weapons found incident to custodial searches conducted while taking a civilian into custody.

Some outcomes are dependent upon another particular post stop outcome taking place and are marked accordingly in the table (\*). The pat down weapon hit variable, and three versions of the search weapon hit variable are all in this group. This means that models capturing sequential selection, as described above, are preferred.

The search weapons hit variable was constructed three different ways, resulting in three different totals for numbers of weapons recovered (Table 5).

With no restrictions, searches surfaced 313 weapons (version 1). If this variable is considered valid only if officers also checked the search box, then searches surfaced 50 fewer weapons, a total of 263 (version 2). If weapons found during searches are removed from consideration if the stop resulted in an arrest, then only 24 weapons surfaced (version 3). **The searches removed with this version could be searches incident to taking the civilian into custody. They also could be searches that led to discovering something that in turn led to an arrest. Because narrative fields were not analyzed for all records, we do not know how many of the search/arrest stops were searches incident to taking into custody vs. searches leading to an arrest.** We comment later on this exclusion when we get to the search outcome.

Descriptive statistics for the one categorical outcome analyzed appear in Table 6. The analyses of this outcome will consider all four possible combinations of outcomes when enforcement and pat down actions are jointly considered, but attention will center on the determinants outcome category 2 vs. outcome category 1. Among those experiencing no enforcement action during the stop, what was associated with receiving a pat down or not receiving a pat down?

Table 6 Descriptive statistics: Categorical outcome variable, pat down and enforcement combination

	Category	N	Percent
No pat down delivered, no enforcement action	1	23,236	42.94
pat down delivered, no enforcement action taken	2	13,444	24.84
No pat down delivered, enforcement action taken	3	12,508	23.11
pat down delivered and enforcement action taken	4	4,917	9.09
Missing	.	11	0.02
	Total	54,116	100.00

Note. There were 11 ISRs where the police checkbox “Enforcement action taken yes/no” was checked “no” but officers did indicate some type of enforcement action (10 instances, other, 1 instance, PSC). In cases where the data were internally in conflict, the variable shown here, which depends in part on whether an enforcement action was taken, was coded to missing.

### 7.8.2 Pat downs: Across groups and districts

In about a third of the stops – 18,364/54,116 or 33.9 percent – the officer delivered a pat down to the stopped civilian.

The number of pat downs in each district, for each of the three racial/ethnic groups, appears in Table 9. The number of pat downs ranged from a high of 2,377 in District 7, to a low of 162 in District 1 (the Loop).

Within each district, the proportion of each racial/ethnic group receiving a pat down appears in Table 10.

Looking at the overall numbers in the bottom of the table, the chances that a stopped civilian would be patted down does appear to depend on the race/ethnicity of the stopped civilian. Whereas about a third of stopped non-Hispanic Black civilians (34.9 percent) or stopped Hispanic civilians (34.7 percent) received a pat down, only about a quarter of stopped non-Hispanic White civilians received the same (23.3 percent).

To give the reader a sense of odds ratios that get presented in later models consider the following.

The odds of [getting patted down vs. not patted down] for each group are derived by taking the [proportion patted down / not patted down] for each group. This is shown below in Table 7.

Table 7. Patted down vs. not patted down: Proportions and odds

Group	Proportion patted down vs. not patted down	Odds of being patted down vs. not patted down
White NH	0.233/(1-0.233)	0.304
Black NH	.349/(1-.349)	0.536
Hispanic	.347/(1-.347)	0.531

For example, White non-Hispanics **odds** of being [patted down vs. not patted down] are derived by taking the proportion patted down and dividing it by the proportion not patted down. That



creates odds of [pat down vs. no pat down] of .34. One could say: White non-Hispanics chances of getting a pat down versus not getting one were about 3 out of 10.

Odds are always about the chances of [this versus that]. **Odds are different from proportions** because proportions are just about the chances of this.

The reader can see that Black non-Hispanics' odds of [getting vs. not getting a pat down] were higher: their odds were .536. One could say: Black non-Hispanics' chances of getting a pat down vs. not getting one were around 5 in 10.

So Black non-Hispanics' odds of [getting vs. not getting a pat down] were higher than White non-Hispanics' odds. How much higher.

To find out one takes the ratio of the two odds, making an **odds ratio**. The odds ratio tells you how much higher or lower one group's odds were relative to the odds of the other group.

So to find the odds ratio of White NH/Black NH – the difference in the odds between the two groups – one divides the two odds.

$$\frac{\text{Odds of Black NH [getting vs. not getting pat down]}}{\text{Odds of White NH [getting vs. not getting pat down]}} = \text{Odds Ratio of [Black NH vs. White NH] [getting vs. not getting pat down]}$$

So for

$$\text{Black NH /White NH OR} = .536/.304 = 1.765$$

That is, Black non-Hispanics' odds of [getting vs. not getting patted down] were **76 percent higher** than the odds for White non-Hispanics of [getting vs. not getting patted down].

$$\text{The odds ratio for being Hispanic vs. White non-Hispanic} = .531/.304 = 1.749$$

When you have an odds ratio close to 1 it means the two groups have about equal chances of [this vs. that] happening. Take the odds ratio for [getting vs. not getting patted down] for

$$\text{Hispanic vs. Black non-Hispanic} = .531/.536 = 0.991$$

*Table 8 Odds ratios depicting ethnoracial differences in odds of getting vs. not getting patted down*

Comparison of odds	OR
Black NH vs. White NH	1.765
Hispanic vs. White NH	1.749
Hispanic vs. Black NH	0.991

**Odds ratios will be the main metric used to describe net impacts of racial or ethnic differences in analyses gauging net impacts.**

Going back to Table 10, the last column in the table demonstrates that the chances of receiving a pat down depended on district context. In several districts (16, 18) police patted down around one out of six or one out of seven stopped civilians. In some districts that proportion was around one out of three (e.g., 3, 4, and 9). In a small number of districts that proportion hovered around one out of two (6, 7).

*Table 9 Counts of pat downs by district and race/ethnicity*

District	White NH	Black NH	Hispanic	Total
1	15	129	18	162
2	4	503	9	516
3	12	1,074	5	1,091
4	29	1,101	220	1,350
5	5	792	16	813
6	19	1,197	10	1,226
7	25	2,327	25	2,377
8	78	427	465	970
9	117	557	823	1,497
10	45	754	513	1,312
11	60	1,339	78	1,477
12	68	263	267	598
14	30	82	227	339
15	13	1,074	33	1,120
16	79	34	86	199
17	70	44	205	319
18	19	115	43	177
19	63	267	65	395
20	36	77	100	213
22	13	451	8	472
24	111	384	213	708
25	66	386	581	1,033
Total	977	13,377	4,010	18,364

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

Table 10 Proportion of stopped civilians patted down, by district and race/ethnicity

District	White NH	Black NH	Hispanic	Total
1	0.119	0.202	0.305	0.196
2	0.108	0.225	0.409	0.225
3	0.353	0.355	0.385	0.355
4	0.341	0.38	0.365	0.376
5	0.2	0.424	0.593	0.424
6	0.514	0.495	0.333	0.494
7	0.424	0.496	0.463	0.494
8	0.235	0.275	0.288	0.277
9	0.308	0.354	0.351	0.349
10	0.409	0.281	0.431	0.329
11	0.176	0.262	0.249	0.256
12	0.266	0.253	0.263	0.259
14	0.238	0.366	0.412	0.376
15	0.191	0.354	0.388	0.352
16	0.129	0.103	0.244	0.154
17	0.231	0.243	0.322	0.285
18	0.152	0.163	0.394	0.188
19	0.226	0.372	0.23	0.309
20	0.201	0.267	0.292	0.263
22	0.2	0.403	0.32	0.39
24	0.291	0.355	0.379	0.35
25	0.277	0.411	0.438	0.413
Total	0.233	0.349	0.347	0.339

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

### 7.8.3 If a pat down is conducted, are any weapons/firearms recovered?

How many actual weapons or firearms were recovered as a result of officers patting down stopped civilians? The counts appear in Table 11. For the period, the recovered weapons totaled 465. The number of recovered firearms/weapons varies from a low of 2 in District 20 to a high of 59 in District 7.

Table 11 Counts of weapons/firearms recovered from pat downs, by district, race/ethnicity

District	White NH	Black NH	Hispanic	Total
1	0	4	1	5
2	0	13	0	13
3	0	25	2	27
4	0	19	7	26
5	0	18	0	18
6	0	28	1	29
7	5	54	0	59
8	2	17	11	30
9	4	15	22	41
10	0	17	13	30
11	3	33	4	40
12	2	6	6	14
14	3	0	8	11
15	2	21	2	25
16	3	0	6	9
17	3	2	5	10
18	1	3	0	4
19	3	7	3	13
20	2	0	0	2
22	0	16	0	16
24	5	1	8	14
25	2	9	18	29
Total	40	308	117	465

Note. NH = non-Hispanic. Only weapons and firearms recovered in course of a pat down listed. Source: January-June 2016 ISRs, CPD.

The corresponding proportions appear in Table 12. Over all groups and over all districts about 2 1/2 percent of the pat downs yielded a weapon or a firearm. The weapon/firearm yield appeared somewhat higher for White non-Hispanics – around four percent – as compared to Black non-Hispanics – a little over two percent. The yield for Hispanic stopped and patted down civilians was between these two.

Table 12 Proportion of pat downs yielding a weapon/firearm by district and race/ethnicity

District	White NH	Black NH	Hispanic	Total
1	0	0.031	0.056	0.031
2	0	0.026	0	0.025
3	0	0.023	0.4	0.025
4	0	0.017	0.032	0.019
5	0	0.023	0	0.022
6	0	0.023	0.1	0.024
7	0.2	0.023	0	0.025
8	0.026	0.04	0.024	0.031
9	0.034	0.027	0.027	0.027
10	0	0.023	0.025	0.023
11	0.05	0.025	0.051	0.027
12	0.029	0.023	0.022	0.023
14	0.1	0	0.035	0.032
15	0.154	0.02	0.061	0.022
16	0.038	0	0.07	0.045
17	0.043	0.045	0.024	0.031
18	0.053	0.026	0	0.023
19	0.048	0.026	0.046	0.033
20	0.056	0	0	0.009
22	0	0.035	0	0.034
24	0.045	0.003	0.038	0.02
25	0.03	0.023	0.031	0.028
Total	0.041	0.023	0.029	0.025

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

#### 7.8.4 Is a search conducted or not?

During the period, officers conducted 9,595 searches of stopped civilians who were in these three racial/ethnic groups.<sup>6</sup> This amounted to one search for every five to six stops. The numbers of searches by racial/ethnic group, and district, appear in Table 13. The largest number of searches of stopped Black non-Hispanic civilians took place in District 11, where there were over 1,000 searches during the first six months of 2016. The largest number of searches of stopped Hispanic civilians took place in District 9, where there were 318. In many districts the number of searches for a specific racial/ethnic group were quite low. This means the ethnoracial proportions of stopped civilians who were searched should be interpreted with caution in these instances.

<sup>6</sup> There were 173 cases where the search checkbox completed by police indicated that no search took place, but police also indicated that some type of contraband was recovered as part of a search. Regardless of search hit variables, if no search check box was checked no search was coded.

Table 13 Number of searches by district, by racial/ethnic group

District	White NH	Black NH	Hispanic	Total
1	18	90	10	118
2	3	268	5	276
3	8	464	4	476
4	16	447	81	544
5	4	400	3	407
6	13	513	9	535
7	9	896	15	920
8	26	168	148	342
9	63	226	318	607
10	21	472	201	694
11	76	1145	92	1313
12	39	207	133	379
14	25	34	96	155
15	12	598	13	623
16	101	65	100	266
17	57	30	134	221
18	10	71	14	95
19	55	147	65	267
20	24	44	60	128
22	8	235	5	248
24	86	219	137	442
25	53	200	286	539
Total	727	6939	1929	9595

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

The proportions appear in Table 14. Those proportions across all districts are roughly the same for all three different racial/ethnic groups. Across the entire city for each of the three groups of stopped civilians about one in five or one in six were searched.

Table 14 Proportion of stopped civilians who were searched, by racial/ethnic group and by district

District	White NH	Black NH	Hispanic	Total
1	0.143	0.141	0.169	0.143
2	0.081	0.12	0.227	0.12
3	0.235	0.153	0.308	0.155
4	0.188	0.154	0.135	0.152
5	0.16	0.214	0.111	0.212
6	0.351	0.212	0.3	0.215
7	0.153	0.191	0.278	0.191
8	0.078	0.108	0.092	0.098
9	0.166	0.144	0.136	0.141
10	0.191	0.176	0.169	0.174
11	0.223	0.224	0.294	0.228
12	0.152	0.199	0.131	0.164
14	0.198	0.152	0.174	0.172
15	0.176	0.197	0.153	0.196
16	0.165	0.197	0.284	0.206
17	0.188	0.166	0.21	0.197
18	0.08	0.101	0.128	0.101
19	0.197	0.205	0.23	0.209
20	0.134	0.153	0.175	0.158
22	0.123	0.21	0.2	0.205
24	0.226	0.203	0.244	0.218
25	0.223	0.213	0.216	0.215
Total	0.173	0.181	0.167	0.177

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

Because officers arresting or transporting a civilian are required to conduct custodial search before taking the stop civilian into custody, the numbers and proportions searched were re-run after excluding stops that resulted in an arrest. As discussed further below, we do not know if all these searches were custodial searches, or if some of them were searches *which led to* an arrest. Nonetheless, the numbers of those searched after excluding stops resulting in an arrest, and the proportion of non-arrested civilians in each racial/ethnic group, in each district, who were searched, appear in Table 15. Focusing only on those not arrested, these figures suggest that searches were conducted in about one out of 20 stops, and this proportion looked roughly comparable across the three different racial/ethnic groupings.

Table 15 Count and Proportion searched, by racial/ethnic group, by district: Stops leading to arrest excluded

District	Count: Searched				Proportion within Each Group Searched			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	11	22	4	37	0.096	0.039	0.075	0.051
2	1	107	4	112	0.029	0.052	0.19	0.053
3	3	167	3	173	0.103	0.062	0.25	0.063
4	5	116	20	141	0.068	0.046	0.038	0.045
5	0	120	1	121	0	0.078	0.042	0.076
6	5	191	5	201	0.179	0.093	0.192	0.095
7	1	239	6	246	0.02	0.06	0.136	0.061
8	7	54	64	125	0.023	0.038	0.043	0.039
9	22	94	131	247	0.066	0.067	0.062	0.064
10	2	92	51	145	0.023	0.041	0.05	0.043
11	7	206	19	232	0.026	0.051	0.083	0.051
12	3	30	50	83	0.014	0.036	0.054	0.042
14	6	17	28	51	0.058	0.085	0.06	0.066
15	1	118	6	125	0.018	0.047	0.079	0.048
16	21	5	20	46	0.041	0.019	0.078	0.045
17	19	6	35	60	0.074	0.039	0.068	0.065
18	1	15	7	23	0.009	0.024	0.071	0.027
19	11	31	5	47	0.051	0.054	0.026	0.047
20	11	13	19	43	0.068	0.051	0.065	0.061
22	0	67	2	69	0	0.074	0.095	0.07
24	39	90	52	181	0.117	0.096	0.11	0.104
25	8	58	66	132	0.042	0.074	0.061	0.064
Total	184	1858	598	2640	0.052	0.057	0.06	0.057

### 7.8.5 If a search is conducted, are any weapons recovered?

The counts and proportions of each racial group within each district producing a weapon or firearm or both as a result of a search appear in the following three tables.

In Table 16 counts and proportions, by district and by racial/ethnic group, are shown for all records for these three racial/ethnic groups. Note that the variable equals 0 if no weapons or firearms are found as a result of the search, and 1 if a weapon, or a firearm, or both, are found as a result of the search. Because some searches (10) resulted in both a weapon and a firearm, the number of weapons recovered is greater than the number of search “hits” for weapons or firearms.

For simplicity’s sake, if the term firearm is not mentioned, the term weapon applies to either firearm or non-firearm weapons.



Table 16 Searches resulting in weapons or firearms or both: No exclusions

District	Count				Proportion within Each Group Yielding Weapons Hit			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	0	3	0	3	0	0.005	0	0.004
2	0	13	0	13	0	0.006	0	0.006
3	0	13	1	14	0	0.004	0.077	0.005
4	2	15	5	22	0.024	0.005	0.008	0.006
5	0	25	0	25	0	0.013	0	0.013
6	0	14	1	15	0	0.006	0.033	0.006
7	3	45	0	48	0.051	0.01	0	0.01
8	0	5	3	8	0	0.003	0.002	0.002
9	2	6	12	20	0.005	0.004	0.005	0.005
10	1	7	3	11	0.009	0.003	0.003	0.003
11	0	29	2	31	0	0.006	0.006	0.005
12	1	5	2	8	0.004	0.005	0.002	0.003
14	0	1	7	8	0	0.004	0.013	0.009
15	0	18	0	18	0	0.006	0	0.006
16	4	1	4	9	0.007	0.003	0.011	0.007
17	0	1	4	5	0	0.006	0.006	0.004
18	1	2	0	3	0.008	0.003	0	0.003
19	1	4	1	6	0.004	0.006	0.004	0.005
20	1	0	1	2	0.006	0	0.003	0.002
22	0	13	0	13	0	0.012	0	0.011
24	3	6	3	12	0.008	0.006	0.005	0.006
25	2	4	13	19	0.008	0.004	0.01	0.008
Total	21	230	62	313	0.005	0.006	0.005	0.006

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

The total number of times a search resulted in a weapons “hit”, as shown in Table 16, was 313. This translated to searches generating weapons, a weapons “hit rate,” of 6/10<sup>ths</sup> of a percent. Descriptively speaking, that hit rate seemed closely comparable across the three racial/ethnic groups: 5/10<sup>ths</sup> of a percent for White Non-Hispanic stopped civilians and Hispanic stopped civilians, and 6/10<sup>ths</sup> of a percent for Black Non-Hispanic civilians.

Table 17 Searches resulting in weapons or firearms or both: Records included only if search check box also checked

District	Count				Proportion within Each Group Yielding Weapons Hit			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	0	2	0	2	0	0.022	0	0.017
2	0	10	0	10	0	0.037	0	0.036
3	0	11	1	12	0	0.024	0.25	0.025
4	2	14	5	21	0.125	0.031	0.062	0.039
5	0	21	0	21	0	0.052	0	0.052
6	0	10	1	11	0	0.019	0.111	0.021
7	2	41	0	43	0.222	0.046	0	0.047
8	0	4	3	7	0	0.024	0.02	0.02
9	1	6	8	15	0.016	0.027	0.025	0.025
10	1	5	3	9	0.048	0.011	0.015	0.013
11	0	23	2	25	0	0.02	0.022	0.019
12	1	5	2	8	0.026	0.024	0.015	0.021
14	0	1	6	7	0	0.029	0.063	0.045
15	0	16	0	16	0	0.027	0	0.026
16	4	1	4	9	0.04	0.015	0.04	0.034
17	0	1	3	4	0	0.033	0.022	0.018
18	1	2	0	3	0.1	0.028	0	0.032
19	1	2	1	4	0.018	0.014	0.015	0.015
20	1	0	1	2	0.042	0	0.017	0.016
22	0	10	0	10	0	0.043	0	0.04
24	2	5	2	9	0.023	0.023	0.015	0.02
25	2	3	10	15	0.038	0.015	0.035	0.028
Total	18	193	52	263	0.025	0.028	0.027	0.027

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

The picture shifts if search weapons hits are calculated only on records where officers also recorded that a search had taken place. As seen in Table 17, this reduced the number of searches generating a weapons hit to 263.

It also increased the search weapons hit rate to between two and three percent: 2.7 percent overall. Further, the weapons hit rate for the three different ethnic/racial groups, speaking descriptively, looked similar: non-Hispanic Black civilians generated a search weapons hit rate of 2.8 percent, slightly above the overall average, while White non-Hispanic civilians generated a search weapons hit rate slightly below the overall average, at 2.5 percent.

Table 18 Searches resulting in weapons or firearms or both: Records included only if search check box also checked **and** no arrest associated with the stop

District	Count				Proportion within Each Group Yielding Weapons Hit			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	0	0	0	0	0	0	0	0
2	0	0	0	0	0	0	0	0
3	0	0	1	1	0	0	0.333	0.006
4	0	2	0	2	0	0.017	0	0.014
5	0	0	0	0		0	0	0
6	0	2	1	3	0	0.01	0.2	0.015
7	0	5	0	5	0	0.021	0	0.02
8	0	1	0	1	0	0.019	0	0.008
9	0	1	0	1	0	0.011	0	0.004
10	0	0	0	0	0	0	0	0
11	0	1	0	1	0	0.005	0	0.004
12	0	0	0	0	0	0	0	0
14	0	1	0	1	0	0.059	0	0.02
15	0	2	0	2	0	0.017	0	0.016
16	0	0	0	0	0	0	0	0
17	0	0	0	0	0	0	0	0
18	0	1	0	1	0	0.067	0	0.043
19	0	0	0	0	0	0	0	0
20	0	0	0	0	0	0	0	0
22	0	0	0	0		0	0	0
24	0	0	0	0	0	0	0	0
25	1	2	3	6	0.125	0.034	0.045	0.045
Total	1	18	5	24	0.005	0.01	0.008	0.009

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

The picture shifts again if stops resulting in an arrest are removed, as shown in Table 18. Again, as in Table 17, only records where officers also checked the search box are considered. The removal of stops associated with an arrest is undertaken because officers are required to conduct a search prior to taking the arrested civilian into custody. Of course, this also may inappropriately remove some searches that *led to* an arrest.

Now the overall search weapons hit rate is slightly below one percent: 9/10<sup>ths</sup> of a percent. (The reader can find the number of searches taking place for stops with no arrest in Table 15.) This is based on 24 searches generating a weapons hit out of 2,640 searches for stops with no arrests.

### 7.8.6 Quick aside: Search hits on weapons or contraband

Although this outcome is not analyzed statistically, for further descriptive context Table 19 shows search hit rates if a hit is widened to include **either** weapons **or** drug contraband. The numbers below are for all searches, with no restrictions.

Table 19 Search hit rates: Any weapons or contraband, by district and race/ethnicity

District	White NH	Black NH	Hispanic	Total
1	0.111	0.167	0.1	0.153
2	0	0.235	0	0.228
3	0.5	0.157	0.5	0.166
4	0.375	0.221	0.247	0.23
5	0.5	0.22	0.333	0.224
6	0	0.216	0.111	0.209
7	0.222	0.234	0.267	0.235
8	0.269	0.107	0.162	0.143
9	0.238	0.186	0.255	0.227
10	0.286	0.208	0.149	0.193
11	0.303	0.244	0.25	0.248
12	0.077	0.256	0.15	0.201
14	0.16	0.118	0.146	0.142
15	0.417	0.281	0.385	0.286
16	0.158	0.062	0.25	0.169
17	0.263	0.233	0.306	0.285
18	0.7	0.141	0.143	0.2
19	0.091	0.218	0.2	0.187
20	0.167	0.114	0.133	0.133
22	0.25	0.26	0	0.254
24	0.267	0.215	0.19	0.217
25	0.264	0.185	0.178	0.189
Total	0.227	0.22	0.203	0.217

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

For all three racial/ethnic groups, in roughly about one out of four or one out of five searches, weapons or contraband were discovered. The hit rates varied by district from about one out of four (district 11, district 22) to around one out of seven (district 8).

### 7.8.7 Is any enforcement action delivered or not?

CPD recorded four types of enforcement actions.

The numbers of each type appear in Table 20. Some type of enforcement action was delivered in 17,436 stops; out of 54,116 stops this means an enforcement action was delivered in 32.2 percent of these stops.<sup>7</sup>

<sup>7</sup> In seven instances, the recording of a specific enforcement action conflicted with the overall indicator completed by officers indicating whether any enforcement action was taken. In the analyses of any enforcement action taken (see section 10.5) the outcome analyzed aligned with the overall indicator completed by officers, not the recording of a specific enforcement action.

Table 20 Frequencies of different enforcement actions

Types of enforcement actions	N	Percent
ANOV (administrative notice of violation)	5,141	29.48
ARR (arrest)	8,037	46.09
OTH (other)	3,386	19.43
PSC (personal service citation)	861	4.94
Total	17,425	100.00

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD. This descriptive total **excludes** 11 stops where a specific enforcement action was checked but the overall “any enforcement action taken” box was **not** checked. In ten of those instances the action was OTH and in one instance it was PSC. In statistical models using this outcome, or this outcome combined with a pat down, these 11 cases were set to missing on the outcome.

Counts of enforcement action of any type appear by district and race/ethnicity combination in Table 21. Police engaged in fewest enforcement actions in district 20, and the most in district 11.

Proportions of stops receiving any enforcement action, by district and race/ethnicity combination, also appear in Table 21. Stopped civilians in district 1 (the Loop) were the most likely to be targeted for enforcement; about 42 percent of stops in that district resulted in some kind of enforcement action by police. Overall, slightly over a quarter of stopped White non-Hispanic civilians received some type of enforcement action by the stopping officer. The corresponding proportion for stopped Black non-Hispanic civilians was around a third. The proportion for stopped Hispanic civilians was between these two.

Table 21 Counts and proportions of stopped civilians receiving any enforcement action, by district and race/ethnicity

District	Count: Any enforcement action				Proportion: Any enforcement action			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	56	271	25	352	0.444	0.423	0.424	0.427
2	7	586	7	600	0.189	0.262	0.318	0.261
3	13	911	2	926	0.382	0.301	0.154	0.301
4	25	731	174	930	0.294	0.252	0.289	0.259
5	6	579	7	592	0.24	0.31	0.259	0.308
6	20	904	13	937	0.541	0.374	0.433	0.377
7	17	1655	13	1685	0.288	0.353	0.241	0.351
8	81	521	610	1212	0.244	0.336	0.377	0.346
9	86	418	600	1104	0.226	0.266	0.256	0.257
10	35	939	347	1321	0.318	0.35	0.292	0.332
11	94	2021	123	2238	0.276	0.395	0.393	0.388
12	63	351	229	643	0.246	0.337	0.225	0.278
14	34	48	153	235	0.27	0.214	0.278	0.261
15	19	1113	16	1148	0.279	0.367	0.188	0.36
16	183	89	137	409	0.299	0.27	0.389	0.316
17	87	63	277	427	0.287	0.348	0.435	0.381
18	30	194	37	261	0.24	0.275	0.339	0.278
19	104	207	126	437	0.373	0.289	0.447	0.342
20	58	83	98	239	0.324	0.288	0.286	0.295
22	24	463	6	493	0.369	0.413	0.24	0.407
24	84	223	135	442	0.22	0.206	0.24	0.218
25	63	310	421	794	0.265	0.33	0.317	0.317
Total	1189	12680	3556	17425	0.283	0.331	0.308	0.322

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

### 7.8.8 Pat down but no enforcement action

As described above, this outcome emerges from the procedural justice literature, and considers the relative likelihood of two joint outcomes.

In simultaneously considering whether the stopped civilian is patted down, and whether the stopped civilian receives any enforcement action, there are four possible sets of outcomes

1. Citizen is not patted down, nor does he/she receive any enforcement action.
2. Citizen *is* patted down, but no enforcement action taken.
3. No pat down, but enforcement action taken.
4. Pat down and enforcement action both taken.

The analyses reported here simultaneously contrasted option 1 above with each of the other three in a multinomial model. But the reporting of results focuses only on the contrast of 1 vs. 2.<sup>8</sup> Stated differently, does race or ethnicity affect the stopped civilians' odds of experiencing:

[A pat down but no enforcement action (2) vs. no pat down and no enforcement action (1)]?

Counts of stops where during a stop civilians were patted down by police but did not receive any enforcement action from the officer appear in

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<sup>8</sup> The focus on this contrast of 1 vs. 2 emerges from the procedural justice literature. **Of course the other contrasts are important, and race or ethnicity differences can and do prove important in those other contrasts.** For example, impacts of ethnoracial differences on 1 vs. 3 are worthy of exploration. Those impacts, however, do not align with the procedural justice frame which is our conceptual starting point when considering this outcome.

Table 22, organized both by district and by race/ethnicity. This happened a total of 13,444 times during the timeframe. It occurred over thousand times each in districts 4, 7, 9, and 11. The number of times this occurred with stopped Black non-Hispanic civilians – 9,828 – was more than 10 times the corresponding number for stopped White non-Hispanic civilians.

Recall that the unit of analysis here is the stop. Therefore, there is no way of knowing how many times the **same** civilian was in a stop with a pat down but no enforcement.

Focusing just on stops where no enforcement action occurred, Table 24 indicates the fraction of *those* stops where a pat down occurred. So in essence, if no enforcement action took place what were the chances that a pat down simultaneously occurred?



Table 22 Counts and proportions of stops where civilians receiving pat down but no enforcement action, by district and race/ethnicity

DISTRICT	Count				Proportion			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	10	88	6	104	0.079	0.138	0.102	0.126
2	4	352	3	359	0.108	0.157	0.136	0.156
3	8	798	3	809	0.235	0.264	0.231	0.263
4	22	882	169	1073	0.259	0.304	0.281	0.299
5	5	598	12	615	0.2	0.32	0.444	0.32
6	8	837	8	853	0.216	0.346	0.267	0.343
7	18	1762	20	1800	0.305	0.375	0.37	0.374
8	53	312	345	710	0.16	0.201	0.213	0.203
9	93	441	642	1176	0.245	0.281	0.274	0.274
10	34	543	361	938	0.309	0.202	0.303	0.236
11	51	930	52	1033	0.15	0.182	0.166	0.179
12	51	190	204	445	0.199	0.183	0.201	0.192
14	16	62	160	238	0.127	0.277	0.29	0.264
15	11	800	26	837	0.162	0.264	0.306	0.263
16	54	19	47	120	0.088	0.058	0.134	0.093
17	52	27	122	201	0.172	0.149	0.192	0.179
18	11	67	28	106	0.088	0.095	0.257	0.113
19	40	186	45	271	0.143	0.259	0.16	0.212
20	20	52	72	144	0.112	0.181	0.21	0.178
22	6	282	4	292	0.092	0.252	0.16	0.241
24	88	303	161	552	0.231	0.28	0.286	0.273
25	51	297	420	768	0.214	0.316	0.317	0.307
Total	706	9828	2910	13444	0.168	0.256	0.252	0.248

Note: The counts in the columns at left reflect the total number of stops where **both** of the following took place: the civilian was patted down **and** no enforcement action was recorded. The proportions in the right most columns express those counts as fractions of all stops.

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

Overall, the proportion of non-enforcement delivered stops where a pat down occurred appears larger for stopped Black non-Hispanic civilians (.383) than for stopped White non-Hispanic civilians (.235). The corresponding proportion for stops with Hispanic civilians (.364) seems quite close to the stops with Black civilians' proportion.

How do these proportions align with the overall representation of the three ethnoracial groups in all the stops, that is, their respective overall stop shares? Table 23 compares the proportions of each ethnoracial group when overall representation in all stops is contrasted with representation in stops with pat downs and no enforcement. That comparison appears in the last column of the table. If the three groups were represented, proportionally, the same way in all stops, and in stops with pat downs but no enforcement, the ratios for each group in the last column would be 1. If a group was *under* represented in stops with pat downs but no enforcement, given their share of all stops, the ratio of the two proportions in the last column would go *below* 1.0. If a group was *over* represented in stops with pat downs but no enforcement, given their share of all stops, the ratio of the two proportions in the last column would go *above* 1.0.

Results show that White Non-Hispanic civilians are under-represented in stops with pat downs but no enforcement, given their overall share of all stops. Whereas this group contributed 7.76 percent of all stops they contributed only 5.25 percent of stops with pat downs but no enforcement. Their chances of being in this type of stop were about 28 percent less than their overall stop share.

By contrast, Black Non-Hispanic civilians were somewhat over-represented in stops with pat downs but no enforcement (73 percent), given their overall stop share (71 percent). Their chances of being in this type of stop were about three percent higher than their overall stop share.

Table 23. Proportional representation, three ethnoracial groups: All stops vs. stops with (pat down and no enforcement action)

Racial / ethnic group	N: All stops	Proportional representation: all stops	N: PD+NEA	Proportional representation: PD+NEA	Ratio: [PR (Pat + NEA) / PR(All)]
White NH	4,198	7.76	706	5.25	0.68
Black NH	38,361	70.89	9,828	73.10	1.03
Hispanic	11,557	21.36	2,910	21.65	1.01
Total	54,116		13,444	100	

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD. PD = pat down; NEA = no enforcement action taken. PD+NEA = stops where civilian was patted down but no enforcement actions were taken.

These descriptive results suggest that proportional representation in stops with pat downs and no enforcement may not be comparable across the three ethnoracial groups considered. Statistical models presented later seek to learn whether that disproportionality can be linked exclusively to race or ethnicity.

These suggested disproportionalities between White Non-Hispanic and Black Non-Hispanics appear to loom larger if the focus drills down to consider just stops where no enforcement actions took place. In Table 24 the left hand columns are the same numbers as seen in

Table 22. But the proportions in the right hand columns differ because the (pat down + no enforcement) stop count is now being divided by *only* the total number of stops where no enforcement action took place. So the numbers on the left translate to higher proportions.

Table 24 Focusing *ONLY* on stops where no enforcement actions occurred: Counts and proportions of stops where civilians receiving pat down but no enforcement action, by district and race/ethnicity

District	Count				Proportion			
	White NH	Black NH	Hispanic	Total	White NH	Black NH	Hispanic	Total
1	10	88	6	104	0.143	0.238	0.176	0.22
2	4	352	3	359	0.133	0.213	0.2	0.212
3	8	798	3	809	0.381	0.377	0.273	0.377
4	22	882	169	1073	0.367	0.407	0.395	0.404
5	5	598	12	615	0.263	0.464	0.6	0.463
6	8	837	8	853	0.471	0.553	0.471	0.551
7	18	1762	20	1800	0.429	0.58	0.488	0.577
8	53	312	345	710	0.211	0.303	0.343	0.31
9	93	441	642	1176	0.316	0.382	0.368	0.369
10	34	543	361	938	0.453	0.311	0.428	0.352
11	51	930	52	1033	0.206	0.301	0.274	0.293
12	51	190	204	445	0.264	0.275	0.259	0.266
14	16	62	160	238	0.174	0.352	0.402	0.357
15	11	800	26	837	0.224	0.417	0.377	0.411
16	54	19	47	120	0.126	0.079	0.219	0.136
17	52	27	122	201	0.241	0.229	0.339	0.29
18	11	67	28	106	0.116	0.131	0.389	0.156
19	40	186	45	271	0.229	0.365	0.288	0.322
20	20	52	72	144	0.165	0.254	0.294	0.252
22	6	282	4	292	0.146	0.429	0.211	0.407
24	88	303	161	552	0.296	0.353	0.377	0.349
25	51	297	420	768	0.291	0.472	0.464	0.449
Total	706	9828	2910	13444	0.235	0.383	0.364	0.366

Note: The counts in the columns at left reflect the total number of stops where **both** of the following took place: the civilian was patted down **and** no enforcement action was recorded. The proportions in the right most columns express those counts as fractions of JUST stops where no enforcement actions occurred. Stops where any enforcement actions occurred are dropped from the entire table.

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD.

The fraction of non-enforcement action stops where a pat down is delivered varies across districts. The proportion is over half in Districts 6 and 7. It is around the fifth in Districts 1 and 2.

The discussion can be further specified if the focus shifts to proportional representation, for each ethnracial group, in two sets of stops: all stops with no enforcement actions (NEA), and, of the

latter, the subset that also included pat downs (PD + NEA). When each group’s relative contribution to the latter (PD + NEA) is contrasted with its contribution to the former (NEA), one can learn whether, among stops with no enforcement action, certain groups of civilians were more or less likely to be patted down. See Table 25. The ratio of the two proportions, for each group, is shown in the right most column. As was seen before when all stops were considered in Table 23, White Non-Hispanic civilians were under-represented in the pat down stops, and Black Non-Hispanic civilians were somewhat over-represented. White Non-Hispanics’ representation in the subset of no action stops with pat downs is one third less than their proportional representation in the set of all no enforcement action stops (ratio = .64). Black Non-Hispanics’ representation was about four percent higher (ratio = 1.04) in the non enforcement stops with pat downs than it was in the set of all non enforcement stops.

*Table 25 Focusing ONLY on stops where no enforcement actions occurred: Proportional representation, three ethnoraical groups: All non enforcement stops vs. non enforcement stops with pat down*

	N: NEA	PR: NEA	N: PD + NEA	PR: PD + NEA	Ratio
White NH	3,009	8.2	706	5.25	0.64
Black NH	25,672	69.99	9828	73.10	1.04
Hispanic	7,999	21.81	2910	21.65	0.99
Total	36,680		13,444	100.00	

Note. NH = non-Hispanic. Source: January-June 2016 ISRs, CPD. PD = pat down; NEA = no enforcement action taken. PD+NEA = stops where civilian was patted down but no enforcement actions were taken. Ratio in right most column compares, for each group: [(proportional representation in PD + NEA stops) / (proportional representation in NEA stops)]

## 7.9 INDEPENDENT VARIABLES

Descriptive statistics for independent variables appear in Table 26. Some variables listed there are not used in the analyses but provide more detail about features of the data being examined.

Table 26 Descriptive statistics: Independent variables

	Variable	N	MIN	MAX	MEAN	SD	MED.
Black non-Hispanic civilian	dblack	54116	0	1	0.709	0.454	1
Hispanic civilian (d)	dhispanic	54116	0	1	0.214	0.410	0
White civilian (*) (d)	dwhite	54116	0	1	0.078	0.268	0
Male civilian (d)	dmale	54116	0	1	0.866	0.341	1
Age in years (*)	age2	54112	7	89	29.568	13.355	25
Age in years (centered) (*)	c_age2	54112	-22.568	59.432	0.000	13.355	-4.568
Age 10-17 (*) (d)	age1017	54116	0	1	0.168	0.374	0
Age 18-25 (d)	age1825	54116	0	1	0.345	0.475	0
Age 25-35 (d)	age2635	54116	0	1	0.210	0.408	0
Age 36-45 (d)	age3645	54116	0	1	0.116	0.320	0
Age 46 and up (d)	age46pl	54116	0	1	0.161	0.368	0
District 1 (*) (d)	dist01	54116	0	1	0.015	0.123	0
District 2 (*) (d)	dist02	54116	0	1	0.042	0.202	0
District 3 (*) (d)	dist03	54116	0	1	0.057	0.231	0
District 4 (*) (d)	dist04	54116	0	1	0.066	0.249	0
District 5 (*) (d)	dist05	54116	0	1	0.035	0.185	0
District 6 (*) (d)	dist06	54116	0	1	0.046	0.209	0
District 7 (*) (d)	dist07	54116	0	1	0.089	0.284	0
District 8 (*) (d)	dist08	54116	0	1	0.065	0.246	0
District 9 (*) (d)	dist09	54116	0	1	0.079	0.270	0
District 10 (*) (d)	dist10	54116	0	1	0.074	0.261	0
District 11 (*) (d)	dist11	54116	0	1	0.107	0.309	0
District 12 (*) (d)	dist12	54116	0	1	0.043	0.202	0
District 14 (*) (d)	dist14	54116	0	1	0.017	0.128	0
District 15 (*) (d)	dist15	54116	0	1	0.059	0.235	0
District 16 (*) (d)	dist16	54116	0	1	0.024	0.153	0
District 17 (*) (d)	dist17	54116	0	1	0.021	0.142	0
District 18 (*) (d)	dist18	54116	0	1	0.017	0.131	0
District 19 (*) (d)	dist19	54116	0	1	0.024	0.152	0
District 20 (*) (d)	dist20	54116	0	1	0.015	0.121	0
District 22 (*) (d)	dist22	54116	0	1	0.022	0.148	0
District 24 (*) (d)	dist24	54116	0	1	0.037	0.190	0
District 25 (*) (d)	dist25	54116	0	1	0.046	0.210	0
Weekend (Sat, Sun) (d)	wknddum	54116	0	1	0.266	0.442	0
Midnight to 3 AM (*) (d)	dhr0003	54116	0	1	0.080	0.271	0
3 AM – 6 AM (d)	dhr0306	54116	0	1	0.018	0.133	0
6 AM – 9 AM (d)	dhr0609	54116	0	1	0.042	0.200	0
9 AM – noon (d)	dhr0912	54116	0	1	0.141	0.348	0
Noon – 3 PM (d)	dhr1215	54116	0	1	0.164	0.370	0
3 PM – 6 PM (d)	dhr1518	54116	0	1	0.132	0.339	0
6 PM – 9 PM (d)	dhr1821	54116	0	1	0.231	0.421	0
9 PM – 11:59 (d)	dhr2123	54116	0	1	0.193	0.394	0
Vehicle stop (d)	dvehstop	54116	0	1	0.074	0.261	0
ISR missing event no. (d)	eventmis	54116	0	1	0.008	0.088	0

Note. (d) = binary variable; 1 corresponds to variable name, 0 otherwise. (\*) = variable not used in multivariate analyses. MED = median. Source: January-June 2016 ISRs, CPD.

## 7.10 ANALYTIC SEQUENCE: RATIONALE AND DETAILS

The specific analytic sequence depends in part on the specific outcome being examined. Nonetheless, the following broad outlines may be helpful.

Each random sample was analyzed separately. As mentioned above, this allowed learning whether crucial statistically significant impacts could be internally replicated across the two samples. If they could, that would suggest more confidence in findings.

### 7.10.1 Outcomes where there is no necessary selection process

Non-conditioned outcomes, that is outcomes where a prior selection process is not logically needed, included:

- whether a pat down took place;
- whether a search took place;
- whether any enforcement action was delivered; and
- whether a pat down occurred in a stop in which no enforcement action took place

For the first three of these outcomes the analytic sequence is as follows.

(1) A series of mixed effects logit models determine (a) whether there is significant variation in the outcome across districts; (b) the gross impacts of race and ethnicity on the outcome in question; (c) the net impacts of race and ethnicity after controlling for other covariates. All these mixed effects models control for the district context as a random effect.

As noted earlier **these are at heart multiple regression models**, incorporating necessary improvements to align with the clustered nature of the data and the binary or categorical nature of the outcomes.

(2) Results from the net impact model are subjected to diagnostics. These seek to gauge the extent to which observed or unobserved selection is potentially problematic. These diagnostics shape whether the interpretation of any observed net race or ethnicity effects should be along correlational or causal lines.

(3) Propensity score matching models are built separately for two contrasts: White non-Hispanics versus Black non-Hispanics; Hispanics versus White non-Hispanics.

**Propensity score matching models use the exact same set of predictors used in the multiple regression models**, except that race or ethnicity necessarily gets treated differently.

The steps of the propensity score matching models were as follows.

(a) For each contrast a mixed effects logit model using the same covariates that appeared in the regression models predict the race or ethnicity contrast. These models generate a propensity score for each stopped civilian included in that contrast – for example the propensity of the stopped civilian to be Black and non-Hispanic instead of White and non-Hispanic.

(b) One-to-one propensity score matching is carried out, and nonmatched cases are dropped. The matching is done with various caliper restrictions: within .10 or .07 or .06 of a standard deviation on the propensity score. Most models use just the most stringent matching threshold, .06.

(c) "Treatment effects", that is the impacts of being Black and non-Hispanic versus White and non-Hispanic, or being Hispanic versus White and non-Hispanic, are estimated for each outcome using just the matched cases. Again, mixed effects logit models with random effects for districts are used for this estimation.

(4) Results from the propensity score matching models are subjected to diagnostics to learn whether selection on observed covariates is potentially problematic. If there is an observed selection problem then the interpretation of any effects seen in the propensity models should be along correlational rather than causal lines.

(5) Results from the propensity score matching models are subjected to a sensitivity analysis to estimate the extent to which unobserved selection is potentially problematic. If the results of the sensitivity analysis indicate that this could be a concern, interpretation of any effects seen in these models should be along correlational rather than causal lines.

For the last of these outcomes, a series of multinomial mixed effects logit models indicate whether the outcome of interest varies across districts, the size of the gross race and ethnicity impacts, and net race and ethnicity impacts after controlling for other covariates.

The alternative analysis applied to the multinomial outcome was a discriminant function analysis.

#### 7.10.2 Outcomes where there is sequential selection

Several outcomes are observed only if something prior takes place. This brings up the problem of sequential selection mentioned earlier.

Two complementary approaches get applied to these outcomes. The first approach employs mixed effects logit models with districts as random effects as was done previously. But these models will include as an additional predictor the probability of being selected for the outcome. For example if the outcome is whether or not a pat down resulted in a weapon being discovered, the predicted probability that a pat down would take place is included as a predictor.

The second approach uses a single level model with error terms clustered by district: a Heckman selection model for a binary (probit) outcome (Baum, 2006).<sup>9</sup>

## 8 A PRIORI STATISTICAL POWER CALCULATIONS

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Statistical power calculations were carried out using GPower software (Faul, Erdfelder, Buchner, & Lang, 2009).<sup>10</sup>

Although preferences differ depending upon the discipline in question, in psychology an acceptable level of statistical power is usually considered to be .80 or higher (Cohen, 1988, 1992). One minus the level of statistical power represents the Type II error rate, that is, the chances that a significant difference will be overlooked.

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<sup>9</sup> In Stata this is heckprobit.

<sup>10</sup> These calculations ignore the clustered nature of the data. Power calculations will be replicated at a later time using simulation software that recognizes such clustering in the data (Browne, Lahi, & Parker, 2009). The OD power estimation program for hierarchical models is inappropriate here because it explicitly assumes an experimental rather than a nonexperimental set up (Spybrook, Raudenbush, Congdong, & Martinez, 2009).



Detailed power calculations were conducted for the first outcome, whether or not the stopped civilian received a pat down. This outcome was selected for detailed power analysis because it is relevant to all stopped civilians. Power analysis considers whether a more stringent alpha level, for example .01 or .001 instead of .05, was desirable given the large number of stops being examined.

Power, with a focus on the impact of the binary variable for Black vs. non-black stopped civilian was estimated for a multiple logistic regression model with 26,000 cases, roughly the number of stops in each 50 percent random sample. The power analyses were further tuned to reflect the mean on the outcome, and the overlap between being patted down and being a Black civilian. Power curves were estimated for an odds ratio associated with the Black variable that ranged from 1.05 to 1.30 in .05 increments. For each specific odds ratio, different power curves were run assuming either 10 percent, 20 percent, or 30 percent of the outcome variation was explained by other predictors. For each specific combination of the above, power curves were run for two tailed alpha levels of .05, .01, and .001. Results from these power curves are summarized in Table 27. Entries at or exceeding the recommended power level of .80 appear in **bold**. A sample power curve appears in Figure 4.

Table 27 A Priori statistical power estimates for pat down outcome

		Alpha level (two tailed)								
		0.05			0.01			0.001		
		R squared other			R squared other			R squared other		
		0.1	0.2	0.3	0.1	0.2	0.3	0.1	0.2	0.3
OR										
1.05		0.37	0.33	0.3	0.17	0.15	0.13	0.05	0.04	0.03
Race impact		<b>0.89</b>	<b>0.85</b>	<b>0.8</b>	0.73	0.66	0.59	0.46	0.38	0.31
expressed as		<b>1</b>	<b>0.99</b>	<b>0.99</b>	<b>0.98</b>	<b>0.97</b>	<b>0.94</b>	<b>0.92</b>	<b>0.87</b>	<b>0.8</b>
an odds ratio		<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>0.99</b>	<b>0.98</b>
(OR)		<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>
1.25		<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>
1.3		<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>	<b>1</b>

The summary table suggests that with an a priori alpha level of .05 an odds ratio associated with the race variable of 1.1 or higher has an 80 percent chance or better of being detected, regardless of how much of the outcome is explained by the other variables in the model.

If an a priori alpha level of .01 or .001 is adopted, power is estimated to be acceptable if the odds ratio associated with the race variable is 1.15 or higher, regardless of how much of the outcome is explained by the other variables.

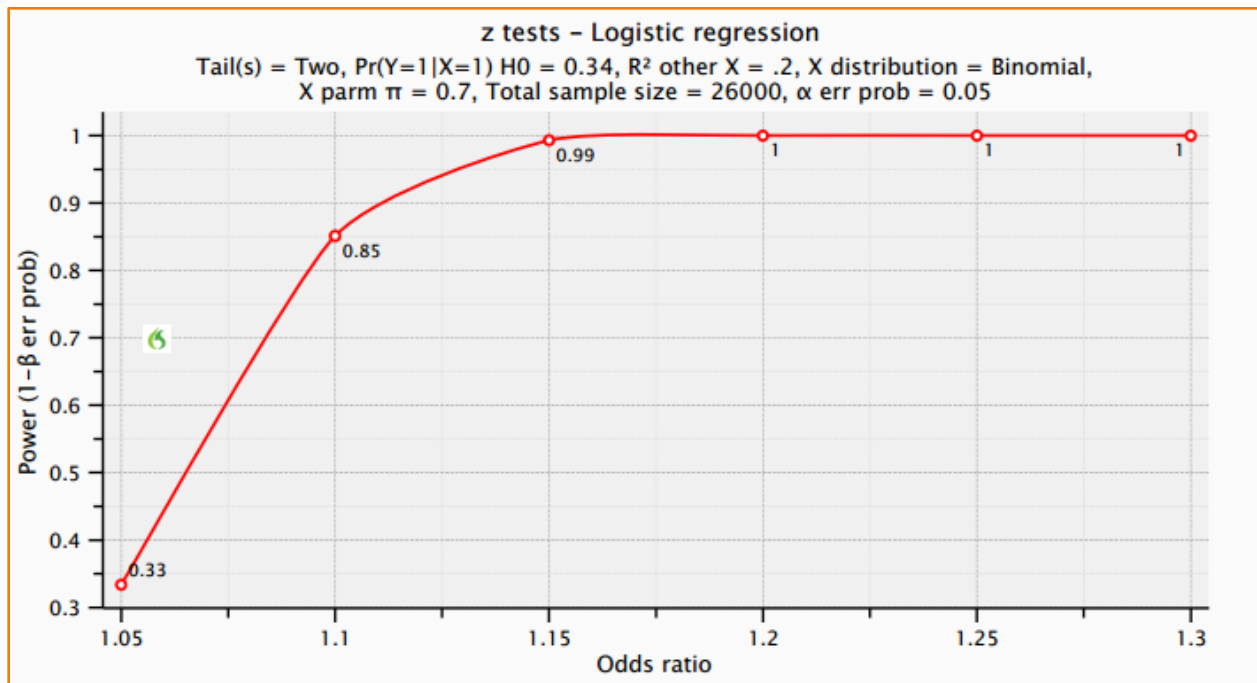
It bears repeating that what is in question here is the odds ratio associated with the race variable, not a percentage difference on the outcome. The race- or ethnicity-linked percentage difference associated with a specific odds ratio depends upon the mean score on the outcome for the group against which stopped Black civilians are being contrasted.

In light of these power estimations, despite the large number of cases being analyzed, the authors decided to use a conventional two tailed alpha level of .05.

These power analyses conducted here are just for single specific outcomes. If multiple outcomes correlate strongly with one another then the experiment-wise alpha level could inflate to something higher than .05.

In fact, save for one exception, correlations across outcomes are below  $|\cdot 04|$ . The one exception is getting or not getting an enforcement action, and being searched (Kendall's tau = .18 in a randomly sampled 50 percent of the records).

Figure 4 Sample power curve for race impact from power analysis of pat down outcome



## 9 BACKGROUND ON ANALYTIC CHOICES

### 9.1 DIAGNOSTICS AND RATIONALE

#### 9.1.1 Regression Diagnostics

In the regression models several types of diagnostics are undertaken. Sequence includes the following:

(a) Model fit is gauged by "comparing predicted probabilities to a moving average of the proportion of cases that are one [on the outcome]" (Long & Freese, 2006: 156).

Several features of residuals are examined. Throughout, the Anscombe residuals are used. These are "usually close to the standardized deviance" (Hilbe, 2009: 279).

- (b) The distribution of residuals is examined for normality and outliers.
- (c) Predicted scores on the X axis are plotted against residuals on the Y axis using a LOWESS smoothed scatterplot line (Charpentier, 2013; Cleveland, 1979).
- (d) Geographic residuals at the district level and their 95 percent confidence intervals indicate whether there is a geographically patterned lack of fit for the model.
- (e) The relationship between residuals and a non-randomly selected covariate is examined to learn whether residuals appear correlated with this covariate.

### 9.1.2 Propensity models: Assessing selection on observables

Following the matched propensity score models, the sequence of balance diagnostics suggested by Austin (2009) are undertaken. Overall balance statistics suggested by Rubin are considered as well (Rubin, 2001).

### 9.1.3 Propensity models: Assessing selection on unobservables

Sensitivity analyses are undertaken to address the potential problem of selection on unobserved factors. Sensitivity tests of propensity score models gauge the impact of unobserved factors that might be simultaneously influencing both race and the outcome, or ethnicity and the outcome (Aakvik, 2001; Becker & Calaiendo, 2007; Guo & Fraser, 2015: 358-359; Mantel & Haenszel, 1959). “A sensitivity analysis in an observational study addresses this possibility: it asks what the unmeasured covariate would have to be like to alter the conclusions of the study” (Rosenbaum, 2005: 1809). “It is not possible to estimate the magnitude of selection bias with nonexperimental data. We rather calculate upper and lower bounds on the test-statistics to test the null hypothesis” of no race or ethnicity impact on the outcome (Aakvik, 2001: 129).

The sensitivity test starts by assuming “no unobserved selection bias” and setting  $e^{\gamma} = 1$  (Aakvik, 2001: 130). This parameter,  $e^{\gamma}$  or  $\Gamma$  (gamma) in the program, is then adjusted upward, in increments of .05, to 2. “If  $e^{\gamma}$  close to 1 changes the inference about the training effect, then estimated training affects are said to be sensitive to unobserved selection bias. However, if a large value of  $e^{\gamma}$  does not alter inferences about the training effect the study is not sensitive to selection bias” (Aakvik, 2001: 130). For the situation here, substitute race or ethnicity effect for “training effect.”

## 9.2 MULTICOLLINEARITY IN REGRESSION MODELS

The degree of multicollinearity in regression models was gauged by examining variance inflation factors. Inclusion of dummy variables capturing specific districts created significant multicollinearity; that is, VIFs were **substantially** above 4.0. To avoid this problem districts were treated as random effects in a mixed effects model. All predictors had VIFs below 4.0.

Multicollinearity was reassessed when a predicted scores’ inclusion was mandated given a conditioned outcome.

## 9.3 CLUSTERED DATA

Events were clustered within districts, and, if there were multiple stops per event, stops were clustered within events. Such clustered data require mixed effects models for a number of

reasons (Snijder & Bosker, 2012). Modeling efforts recognizing both levels of clustering fail to converge. Therefore, the regression models reported here are two level mixed effects models recognizing only the clustering of stops within districts, and ignoring the clustering of stops within events. The implications of ignoring the clustering of stops within events is not known at this time. Nevertheless, given the considerable community criminology research on neighborhood effects (Sampson, Morenoff, & Gannon-Rowley, 2002) and policing work on the ecology of policing behavior across districts (Klinger, 1997), the geographic clustering was judged the more important of the two clustering sources.

## 9.4 GEOGRAPHY

Crime and delinquency patterns, that is the levels of each and the mix within each, vary geographically. Over a century of work establishes this point (Bursik & Grasmick, 1993; Taylor, 2015). At the same time, ecological models predicting crime and delinquency rates can never completely explain all of this variation (Pratt & Cullen, 2005). Excluding geography results in a theoretically under-specified model. Stated more simply, such a model leaves out causes of the outcome that we already know are important.

Geography is also important from a police perspective. Recent ecological theorizing on policing suggests (Klinger, 1997), and research supports the idea (Taniguchi, 2010), that within a single police department, police district-level norms exist about how to respond to crimes and calls for service of varying seriousness.

In the mixed effects models district context is always included as a random effect. This means that the mean score on the outcome varies across districts. As noted above, due to multicollinearity concerns it was not possible to include dummies for district variables. So geography was modeled as a random effect.

What is left over geographically proves interesting and potentially important. On the pat down outcome we observe **significant district level discrepancies from predicted pat down outcomes for a small number of districts**. These discrepancies may be important and may warrant further investigation.

# 10 RESULTS

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## 10.1 DID A PAT DOWN OCCUR?

### 10.1.1 Regression

#### 10.1.1.1 Results

Results appear in Table 29. In both random samples stopped Black non-Hispanic civilians were about 24 to 25 percent more likely to be [patted down versus not patting down] compared to stopped White non-Hispanic civilians ( $p < .001$ ). This suggests a net race impact on the outcome controlling for the other covariates and for district context.

A similarly sized and similarly significant ( $p < .001$ ) net ethnicity impact appeared as well. Stopped Hispanic civilians were also about 23 to 28 percent more likely to be [patted down versus not patting down] after controlling for the other covariates and for district context.

Gender proved significant ( $p < .001$ ) as well. Males were about three times more likely to be [patted down versus not patting down].

For age, the reference group was those younger than 18. Compared to that reference age group, those aged 18 to 25 were significantly more likely ( $p < .001$ ) to be patting down. Stopped civilians older than 36 were significantly less likely to be patting down compared to the youngest reference group ( $p < .001$ ).

The odds of being [patted down versus not patting down] seemed to wane in the later months in the series. Compared to the reference month of January, those odds were about 15 percent lower in April ( $p < .05$  or  $p < .001$ , depending on sample), about 22 percent lower in May ( $p < .001$ ), and about 30 percent lower in June ( $p < .001$ ).

If a civilian was stopped on the weekend, his or her chances of being patting down were about 10 to 13 percent higher ( $p < .001$ ).

The reference time used was stops between midnight and 3 AM. Compared to that timeframe, pat downs were significantly more likely between 3 and 6 AM ( $p < .05$  or  $P < .001$ , depending on sample), but significantly less likely at all other times ( $p < .01$  or  $p < .001$ , depending on sample and specific time block).

If the stop was flagged as a vehicle stop, the odds of a pat down were significantly higher, anywhere from 37 to 51 percent higher depending on the sample ( $p < .001$ ).

In the first random sample but not the second random sample those stops missing an event number were significantly more likely to include a pat down ( $p < .05$ ).

These results suggest a significant impact of both race and ethnicity on the likelihood of a pat down taking place during the stop. This appears as a net impact because it persists after controlling for other covariates and for district context.

Diagnostics suggest, however, that it might be unwise to interpret this net connection as anything more than correlational. Details appear below in the next section.

Table 28. Gross ethnoraical impacts on predicted probabilities of receiving a pat down

First random sample

Ethnoracial category	Predicted proportion patted down	Standard error of proportion
White NH	0.232	0.003
Black NH	0.347	0.001
Hispanic NH	0.35	0.002

Second random sample

Ethnoracial category	Predicted proportion patted down	Standard error of proportion
White NH	0.233	0.003
Black NH	0.35	0.001
Hispanic NH	0.344	0.002

Note. 2016, Jan.-June ISR data. White NH predicted proportion is significantly lower

The modeled results can be used to describe **gross ethnoraical impacts** on chances of getting a pat down as well. See

Table 28. It shows the predicted probability of receiving a pat down, based on all the factors used in the model, for the first and second random samples. The standard error around each proportion is shown as well. If two proportions are farther apart than two standard errors from each other then they are significantly different in statistical terms. The table shows that, in both random samples stopped White non-Hispanic civilians are predicted to be significantly ( $p < .001$ ) less likely to receive a pat down compared to the other two groups, Black non-Hispanic and Hispanic stopped civilians. In both samples the predicted probabilities for a pat down are at least 10 percent lower for White non-Hispanic stopped civilians.

Table 29. Predicting pat down occurrence: Mixed effects logit models

Fixed effects		First random sample		Second random sample	
		b	OR	b	OR
Black civilian	dblack	0.227***	1.255***	0.214***	1.239***
Hispanic civilian	dhispc	0.244***	1.277***	0.210***	1.234***
Male	dmale	1.136***	3.114***	1.175***	3.240***
Age 18-25	age1825	0.196***	1.217***	0.136***	1.146***
Age 26-35	age2635	-0.0661	0.936	-0.0722	0.93
Age 36-45	age3645	-0.662***	0.516***	-0.524***	0.592***
Age 46 and up	age46pl	-1.166***	0.311***	-1.191***	0.304***
February	dfeb	0.0178	1.018	0.0418	1.043
March	dmar	-0.00820	0.992	0.0474	1.049
April	dapr	-0.185***	0.831***	-0.122*	0.885*
May	dmay	-0.284***	0.753***	-0.218***	0.804***
June	djun	-0.407***	0.666***	-0.329***	0.720***
Weekend	wknddum	0.128***	1.136***	0.0977**	1.103**
3 - 6 AM	dhr0306	0.246*	1.278*	0.418***	1.519***
6 - 9 AM	dhr0609	-0.932***	0.394***	-0.792***	0.453***
9 - 12 AM	dhr0912	-0.647***	0.523***	-0.697***	0.498***
12 - PM	dhr1215	-0.474***	0.622***	-0.447***	0.639***
3 - 6 PM	dhr1518	-0.311***	0.733***	-0.244***	0.784***
6 - 9 PM	dhr1821	-0.346***	0.708***	-0.312***	0.732***
9 - 12 PM	dhr2123	-0.268***	0.765***	-0.180**	0.835**
Vehicle stop	dvehstop	0.319***	1.376***	0.412***	1.510***
Missing event no.	eventmis	0.316*	1.371*	0.241	1.273
Constant		-1.347	0.26	-1.443	0.236
Random effects					
	District variance	0.134**		0.140**	
	Observations	27,058		27,058	
BIC		31699		31817	
	Number of groups	22		22	

Note. \*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Source: January – June 2016 ISR data, CPD

Note. For sample 1: Null model: LR  $\chi^2$  test vs. logistic model = 823.76; p < .001; BIC = 33,848.69

For sample 2: Null model: LR  $\chi^2$  test vs. logistic model = 810.58; p < .001; BIC = 33,891.18



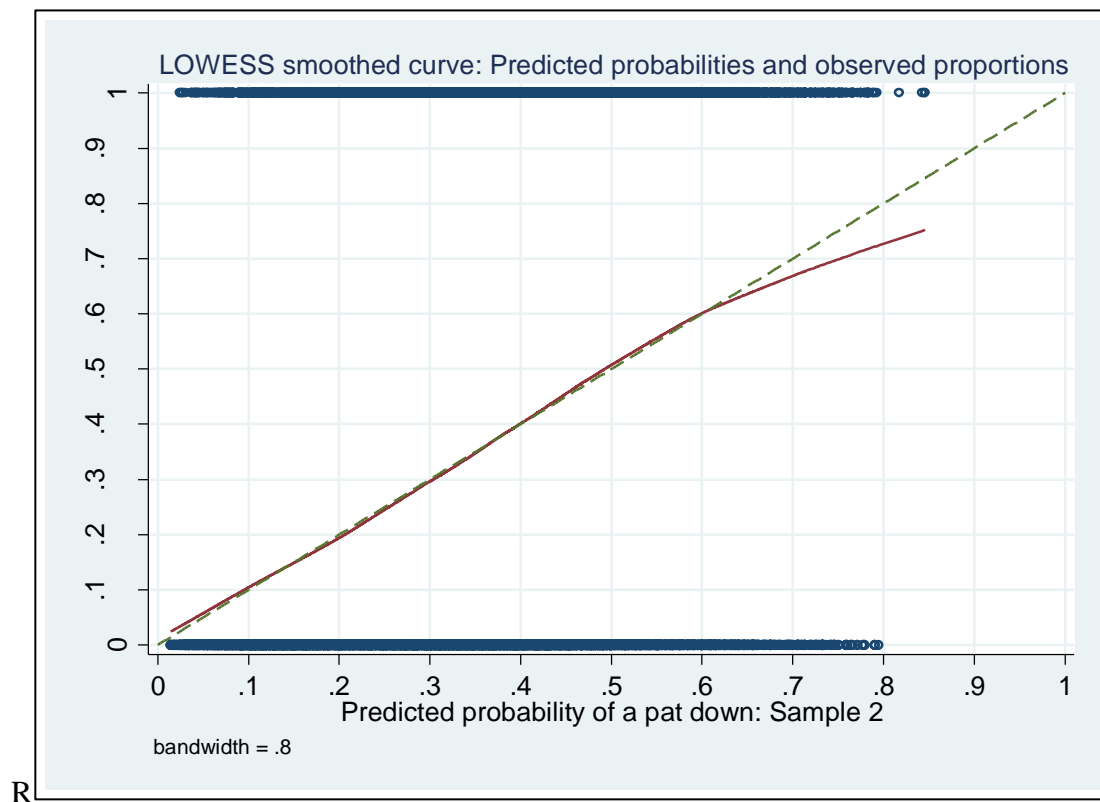
### 10.1.1.2 Model diagnostics

#### 10.1.1.2.1 Patterns

Diagnostics of both the predicted scores and residuals revealed areas of concern.

Starting with predicted scores, LOWESS smooth curves linking predicted probabilities with observed outcome scores showed a significant lack of fit above predicted probabilities of around .7. This occurred in both random samples. The relationship for the second sample appears in Figure 5. It shows that predicted probabilities that a pat down would occur started to be markedly lower than the observed probabilities as the observed proportion patted down climbed above .70.

Figure 5 Predicted probabilities fit to observed proportions: pat down outcome, sample 2



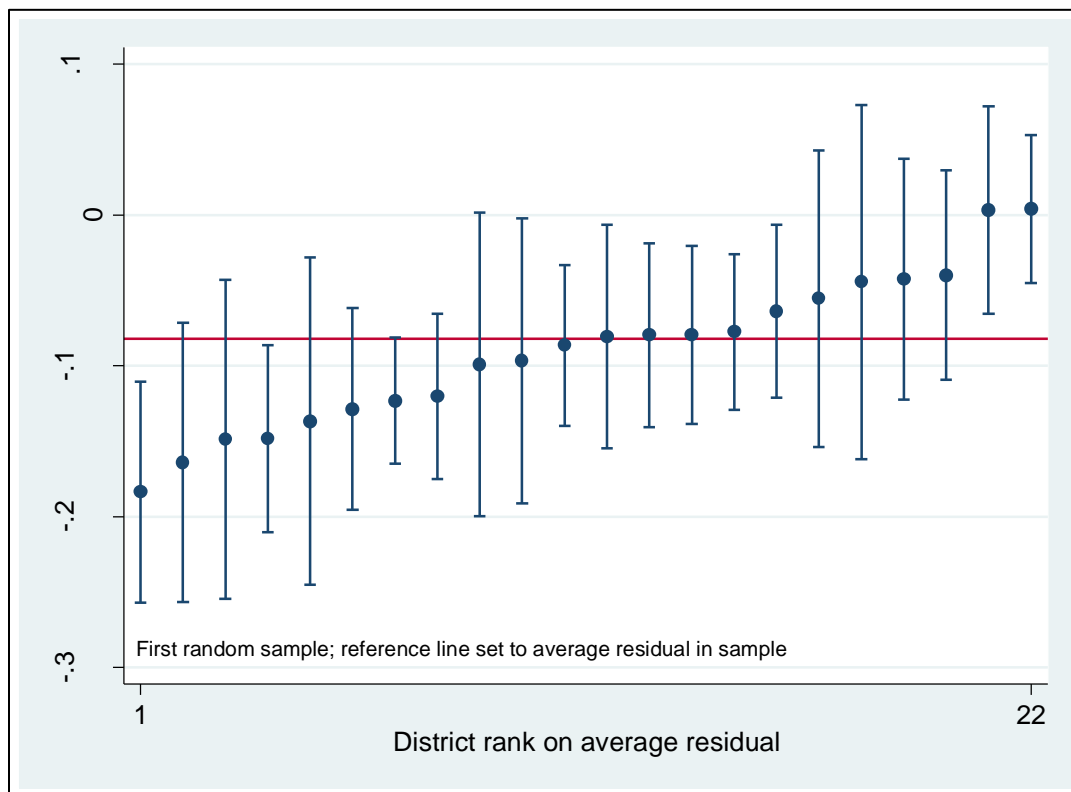
Plots of predicted scores against residuals with a superimposed LOWESS smoothed curve showed no relationship between the two (results not shown).

Residuals appeared to be potentially problematic in two ways: geographically, and in relationship to at least one covariate.

The average district-level residuals for the first sample appear in Figure 6. The reference line shown corresponds to the overall residual.<sup>11</sup> Starting on the left-hand side of the figure, the first district (district 16) and fourth district (district 2) had residuals significantly ( $p < .05$ ) below the average. This means that after taking the predictors into account, stops in these districts were predicted to be significantly less likely to result in a pat down. In district 16, 26 percent of the stopped pedestrians were non-Hispanic Black civilians, and in district 2 97 percent were in the same group.

In districts, 6 and 7, the average residual was significantly above the average. In both districts approximately 98 percent of the stopped civilians were Black non-Hispanics. Because this is a positive average residual, it suggests that a significantly higher fraction of stopped civilians were patted down than factors in the model led us to expect.

Figure 6. Average pat down residuals, by district, first random sample



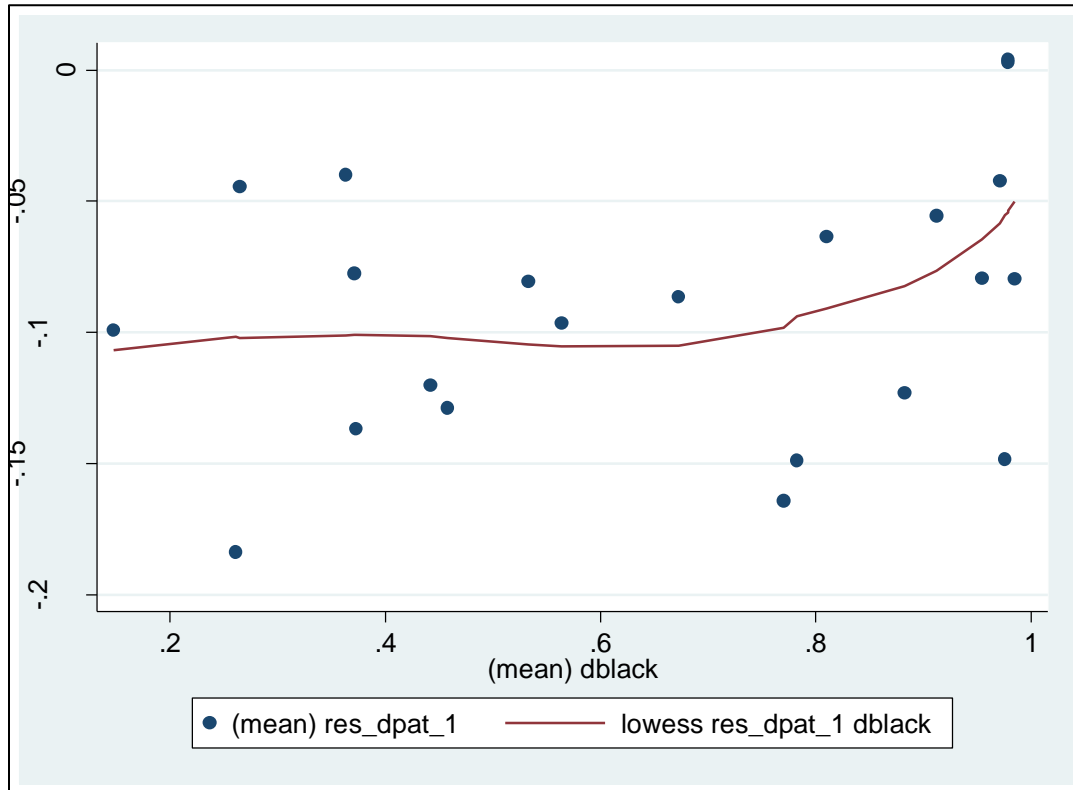
Note. January – June 2016 CPD ISR stop data. 95 percent confidence intervals shown

The relationship between the district residuals and the percent of stopped civilians who were Black non-Hispanic in that district appears in Figure 7. The smoothed LOWESS curve suggests that district level residuals trended upward if more than about 80 percent of the stopped civilians in the district were Black non-Hispanic. This suggests that non-modeled factors associated with the racial mix of stopped civilians in these districts were contributing to higher fractions of stops

<sup>11</sup> The overall residual is not zero because more stopped civilians were not patted down than were. If the outcome was not patted down, scored a zero, the residual is automatically a negative number.

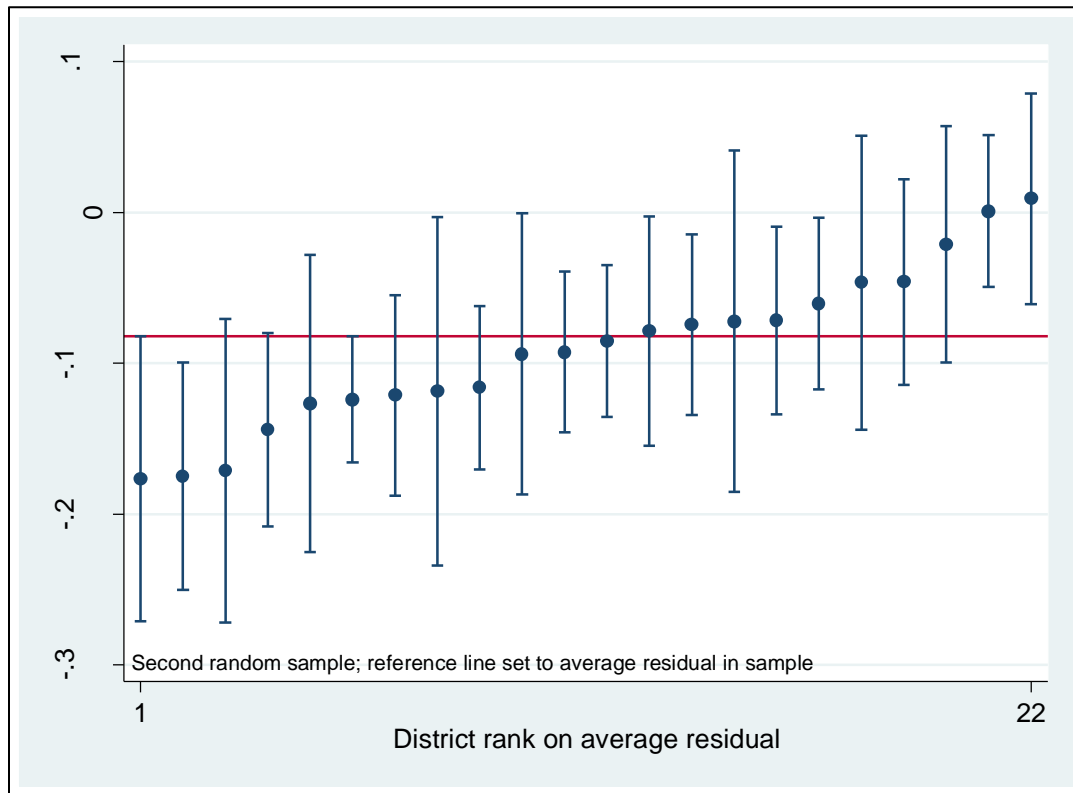
resulting in pat downs. This depicted relationship uses a district level covariate, and as pointed out previously, there are problems with using district factors given the low number of districts. **So this pattern should be considered exploratory only.**

Figure 7 First random sample: District pat down residual and percent stopped civilians who are Black



The caterpillar plot of district residuals for the second sample appears in Figure 8. Starting again on the left hand side of the plot, the district second from left, district 16, had an average residual significantly below the mean for the sample. This meant that fewer stopped civilians were patted down in this district than expected given the features in the model and the behavior of the other districts. This same departure from normality was noted with the results from the first random sample. In this second random sample, 25 percent of stopped civilians in this location were non-Hispanic Black.

Figure 8 Second random sample: District pat down residual and percent stopped civilians who are Black

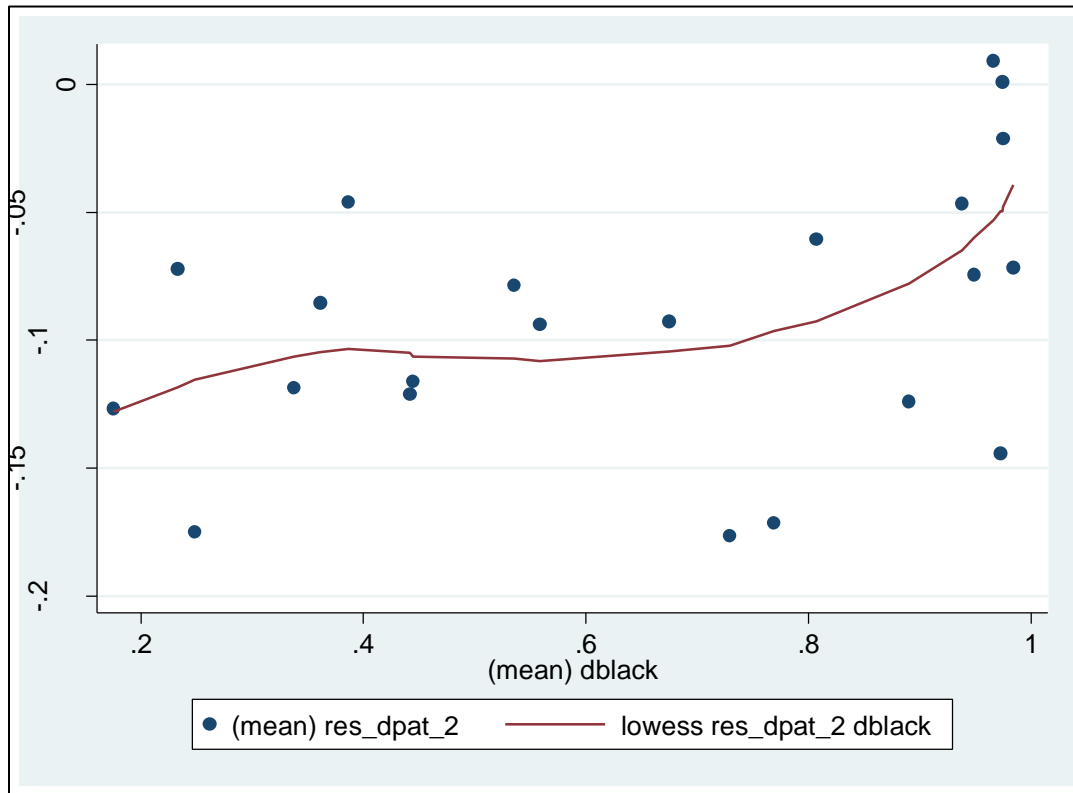


Note. January – June 2016 CPD ISR stop data. 95 percent confidence intervals shown

On the right-hand side of Figure 8, two districts have an average residual significantly above the overall average. These are Districts (left to right) 7 and 6. In both these districts approximately 97 percent of stopped civilians were Black non-Hispanic. These two districts also surfaced in the results from the first random sample as locations with significantly higher than average residuals. Again, the implication is that more pat downs were occurring in these locations than were anticipated by the features included in the model.

The connection for the second random sample between these district residuals and the percentage of stopped civilians in the district who were Black is displayed in Figure 9.

Figure 9 Second random sample: District pat down residual and percent stopped civilians who are Black



Again, as was seen with the first random sample, the smoothed LOWESS curve suggests that residuals were trending upward when more than about 80 percent of stopped civilians in a district were Black. **This depicted relationship should be considered exploratory only.**

Results from both samples suggest there is one district, 16, where significantly fewer persons are patted down than expected, and two districts, 6 and 7, where more stopped civilians are patted down than the model expects.

For the first sample, the relationship between pat down residuals and civilian age was examined, separately for each outcome group (Figure 10). If residuals are well patterned there should be no relationship between scores on the predictor and residual scores. That does not appear to be the case here. In both groups average residuals appear somewhat dependent on age.

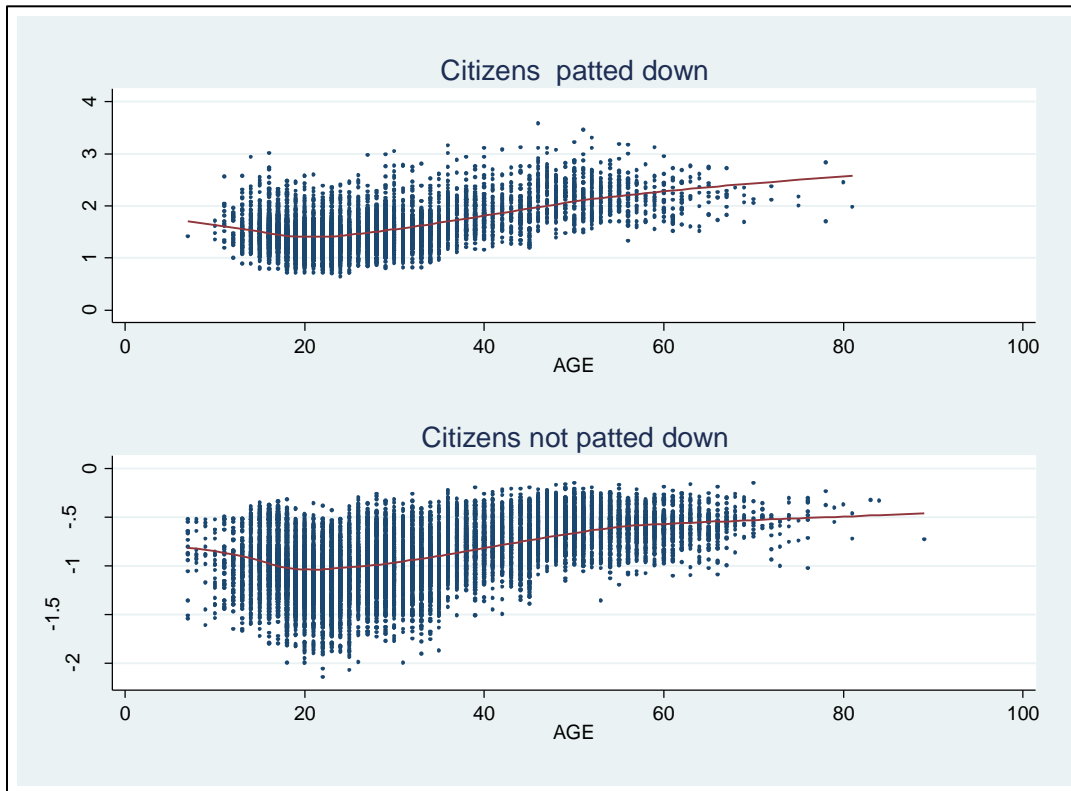
This analysis uses continuous age, whereas the model used categorical age. Despite this limitation, a pattern seen suggests that one or more unobserved covariates linked with age were perhaps affecting the outcome.

#### 10.1.1.2.2 Conclusion

These diagnostics, considered in total, argue against a causal interpretation of the impacts of race and ethnicity from these regression models. Some of the diagnostics suggest that selection on unobserved covariates may be affecting the outcome. The safest conclusion at this juncture is that civilian race and ethnicity correlate with the outcome examined, but race or ethnicity *per se*

may not be playing causal roles. Rather or ethnicity linked factors, factors not modeled here, cannot be ruled out.

Figure 10 First random sample: Relationship between stopped civilian age and pat down residual



### 10.1.2 Caliper matched propensity score models: Non-Hispanic Black vs. White civilians

#### 10.1.2.1 Steps

Separately for each sample, all the covariates used in the regression model, save race or ethnicity, were used to predict the stopped civilian being Black non-Hispanic versus White non-Hispanic (Hispanics excluded from these models). As with the regression models, these also were mixed effects logit models with random effects for districts. An initial model first confirmed that the race or ethnicity variable being predicted varied significantly across districts.

Following the prediction of race or ethnicity using observed covariates and district context, the predicted score on the race or ethnicity outcome was saved.<sup>12</sup> This predicted score was treated as the propensity score in the matching program.<sup>13</sup> Caliper matching within 1/10th of a standard deviation was specified. Models were run again specifying an even tighter caliper match, within .07 of a standard deviation on the propensity scores. (Models for later outcomes used even tighter caliper matching requirements, .06 of a standard deviation.)

<sup>12</sup> The Stata option mu was used here; this incorporated both the fixed and random effects in the prediction model.

<sup>13</sup> psmatch2 in Stata.

### 10.1.2.2 Results

Results from propensity score caliper matched models appear in Table 30. These analyses use only matched pairs of Black and White non-Hispanic stopped civilians. In each pair, the propensity scores of members of the pair are the most closely matching propensity scores of the non-matched cases remaining.

In the first random sample using the caliper match of  $1/10^{\text{th}}$  of a standard deviation, 1,875 Black stopped civilians were matched with 2,087 stopped White civilians (total = 3,962). If the caliper match is tightened to within .07 hundredths of the standard deviation, the corresponding numbers are 1,873 matched Black civilians and 2,087 matched White civilians (total = 3,960). The numbers of White and Black civilians are not exactly equal because multiple civilians might have exactly the same score on the propensity-to-be-black variable.

Impacts of the contrast between stopped Black non-Hispanic and stopped White non-Hispanic civilians appear for both random samples and for caliper matches within  $1/10^{\text{th}}$  of a standard deviation on the propensity score and again within 7 hundredths of a standard deviation on the propensity scores. In all analyses non-matched cases are dropped. The model shown, as recommended (Guo & Fraser, 2015: 384), are mixed effects models with random effects for districts.

Regardless of which sample is examined, and regardless of the restrictions set on caliper matching, in all instances stopped Black non-Hispanic civilians appear more likely to be patted down than matched stopped White non-Hispanic civilians. Black civilians' odds of being [patted down versus not patted down] were anywhere from 19 to 31 percent higher depending on the caliper match specified in the sample. The statistical significance associated with the race variable ranged from  $p < .05$  to  $p < .001$  depending on the caliper match and the sample. These results confirm a net association, seen in the mixed effects logit models, between race and the likelihood of receiving a pat down.

That said, diagnostics suggested this link should be interpreted as correlational only and not causal. Details appear in the next sections.

Table 30 Propensity score model estimates of impact of Black vs. White civilian race on pat down outcome (non-Hispanics only)

	B	SE	Z	P =	LCL	UCL	OR	OR-LCL	OR-UCL
Caliper = .10									
Sample 1									
Black non-Hispanic	0.277***	0.0743	3.722	0.0002	0.131	0.422	1.319***	1.140	1.526
Constant	-1.106						0.331		
District variance (se)	0.200	0.076							
Observations	3,962								
Number of groups	22								
LR chi square test	83.8 (p < .001)								
Sample 2									
Black non-Hispanic	0.177*	0.0744	2.380	0.0173	0.0312	0.323	1.194*	1.032	1.381
Constant	-1.045						0.352		
District variance (se)	0.193	0.071							
Observations	4,034								
Number of groups	22								
LR chi square test	91.66 (p < .001)								
Caliper = .07									
Sample 1									
Sample 1									
Black non-Hispanic	0.218**	0.0747	2.915	0.0036	0.0713	0.364	1.243**	1.074	1.439
Constant	-1.129						0.324		
District variance (se)	0.135	0.055							
Observations	3,960								
Number of groups	22								
LR chi square test	57.14 (p < .001)								
Sample 2									
Black non-Hispanic	0.231**	0.0737	3.136	0.00171	0.0867	0.375	1.260**	1.091	1.456
Constant	-1.109						0.33		
District variance (se)	0.170	0.066							
Observations	4,026								
Number of groups	22								
LR chi square test	80.03 (p < .001)								

Note. January – June 2016 CPD ISR stop data. 95 percent confidence intervals shown.

\* = p < .05; \*\* = p < .01; \*\*\* = p < .001



### 10.1.2.3 Diagnostics: Covariate balancing and observed selection

Austin (2009) recommends balance diagnostics between the treatment (black) and control (white) groups that examine each covariate, and consider mean differences as well as variance differences. If these balance diagnostics fail, then observed selection cannot be ruled out. This means that even after matching, Black and White stopped civilians still differ on these other factors, and these other factors could be simultaneously affecting both race and the outcome. There is an observed selection problem.

For both samples, regardless of the caliper matching level used, balance diagnostics failed (results not shown). In all instances, there were multiple mean differences on covariates, and the treatment to control variance ratios were outside acceptable limits.

Rubin (2001: 177) has suggested some overall balance statistics that simultaneously take all covariates into account. Rubin’s *B* is “the standardized difference in the means of the propensity scores” between the two groups being compared. The suggested limits are within a half a standard deviation (p. 174), which translates in the program used to a value lower than 25. Rubin’s *R* is “the ratio of the variances of the propensity scores” (p. 177) of the two groups. The suggested limits are between .75 and 1.25 (p. 177).

Rubin’s summary statistics appear in Table 31. These show that Black and White non-Hispanic civilians were *not* sufficiently balanced on covariates after matching, as shown by the Rubin’s *B* values above 25, in sample 1. The two groups, on this summary measure, do appear sufficiently balanced on covariates in sample 2. Ratios of variances (Rubin’s *R*) appear acceptable in both samples at both matching levels.

Table 31 Summary covariate balancing statistics after matching Black and White respondents for propensity score model of pat downs

		Rubin's <i>B</i>	Rubin's <i>R</i>
Caliper match = .10	Sample 1	26.6*	1.11
	Sample 2	21.8	1.07
Caliper match = .07	Sample 1	25.1*	1.04
	Sample 2	20.7	1.06

### 10.1.2.4 Diagnostics: Sensitivity to unobserved selection

The results of the sensitivity analysis to gauge the impacts unobserved selection might have on the race impact appear in Table 32. In three out of the four scenarios, if two individuals who are similar on the observed covariates differ in their odds of being Black and non-Hispanic versus White and non-Hispanic by only about 15 percent, then there is no significant impact of race on

the pat down outcome. Given that this value of gamma ( $\Gamma$ ) is relatively close to 1.0, the significant race impact seen is "sensitive to unobserved selection bias" (Aakvik, 2001: 30)

Table 32 Sensitivity analysis, propensity score models, pat down outcome, Black vs. White non-Hispanic civilians

	Gamma ( $\Gamma$ ) value where race impact becomes non-significant
Caliper match = .10	
Sample 1	1.25
Sample 2	1.15
Caliper match = .07	
Sample 1	1.15
Sample 2	1.15

#### 10.1.2.5 Limitations and conclusion

Propensity score matching models can run afoul of a wide variety of problems (Guo & Fraser, 2015: 381-386). The resulting propensity score matching models here should be considered preliminary until additional analyses using a different matching protocol such as the Mahalanobis nearest neighbor approach can be completed. That said, results seen here are robust in the following ways: they replicate across two independent random samples, and they replicate using different caliper matching restrictions. But bear in mind, as noted below, the covariates are **not** balanced.

Excluding all Hispanic stopped civilians, results showed that stopped Black civilians experience significantly higher chances of being patted down compared to matched White civilians. Blacks' odds were anywhere from 19 to 31 percent higher for being [patted down versus not patted down].

Two features of diagnostics suggest, however, that this link should be interpreted as correlational and not causal. Diagnostics suggest that observed selection bias – that is differences between Blacks and matched Whites on the covariates used – was a problem in one of the samples if we just look at summary statistics on covariate balancing, and in both of the samples if we look at the covariate-by-covariate results. Although matching dramatically reduces differences between Blacks and Whites on the observed covariates, troubling discrepancies remained.

Sensitivity analyses also suggested that selection bias on unobserved factors was potentially problematic. If civilians similarly situated on the covariates differ in their odds of being [black and non-Hispanic versus White and non-Hispanic] by as little as 15 percent, then the significant impact of race on the outcome would probably disappear.

### 10.1.3 Caliper matched propensity score models: Hispanic vs. White non-Hispanic civilians

#### 10.1.3.1 Steps

The procedures paralleled what was done to learn about different outcomes between White non-Hispanic civilians versus Black non-Hispanic civilians. Whereas that analysis dropped all

stopped Hispanics, this analysis dropped all Black non-Hispanic stopped civilians, leaving the focus on contrasting Hispanics versus White non-Hispanics who were stopped.

As with the Black versus White contrast, models were done with two levels of caliper matching. The first level of matching created a match if the non-Hispanic stopped civilian was within  $1/10^{\text{th}}$  of a standard deviation of the corresponding Hispanic member of the pair on the propensity score. The second level of matching was slightly tighter here, using .06 of a standard deviation on the propensity score rather than .07 as was done with a Black versus White contrast.

#### **10.1.3.2 Results**

Propensity score model results for impacts of ethnicity appear in Table 33. Stopped Hispanic civilians, compared to matched White non-Hispanic stopped civilians, had odds of [being patted down versus not patted down] that were anywhere from 35 to 45 percent higher, depending on the sample and the caliper restriction. All of these differences were highly significant statistically ( $p < .001$ ).

To help better understand the results, we use the sample 1 predicted probabilities from the model requiring a caliper match of .06 of a standard deviation or better on the propensity score. These predicted probabilities show that whereas a matched stopped White non-Hispanic civilian in a typical district had predicted chances of being patted down that were about 23.9 percent, the corresponding predicted chances for a Hispanic stopped civilian of being patted down were 31.3 percent.

#### **10.1.3.3 Diagnostics**

Diagnostics on whether the covariates are balanced between the Hispanic and the White non-Hispanic groups suggest that observed selection was not a problem (Table 34). Both summary balancing statistics were within acceptable ranges. Although the ratios of the variances contrasting the two groups on individual covariates routinely seem quite different, in most cases means on covariates were not significantly different.

Sensitivity analyses indicated, however, that selection on unobserved covariates probably cannot be dismissed as an important potential confound. Gamma ( $\Gamma$ ) values as little as 1.15 rendered the ethnicity impact on the outcome nonsignificant. The interpretation of this ethnicity impact probably should remain correlational rather than causal.

Table 33 Propensity score model estimates of impact of Hispanic vs. White civilian ethnicity on pat down outcome (black non-Hispanics excluded)

	B	SE	Z	p	LCL	UCL	OR	OR-UCL	OR-LCL
Caliper = .10									
Sample 1									
Hispanic	0.300***	0.0743	4.036	< .0001	0.154	0.446	1.350***	1.167	1.562
Constant	-1.157						0.314		
District variance (se)	0.0831	0.0385							
Observations	3,879								
Number of groups	22								
LR chi square test	33.28 (p < .001)								
Sample 2									
Hispanic	0.317***	0.0744	4.262	< .0001	0.171	0.463	1.373***	1.187	1.589
Constant	-1.116						0.328		
District variance (se)	0.122	0.0582							
Observations	3,881								
Number of groups	22								
LR chi square test	42.19 (p < .001)								
Caliper = .06									
Sample 1									
Hispanic	0.371***	0.0740	5.014	< .0001	0.226	0.516	1.449***	1.253	1.675
Constant	-1.156						0.315		
District variance (se)	0.0901	0.0401							
Observations	3,853								
Number of groups	22								
LR chi square test	39.41 (p < .001)								
Sample 2									
Hispanic	0.330***	0.0745	4.424	< .0001	0.184	0.476	1.390***	1.202	1.609
Constant	-1.107***						0.33		
District variance (se)	0.134	0.0628							
Observations	3,859								
Number of groups	22								
LR chi square test	49.08 (p < .001)								

Note. January – June 2016 CPD ISR stop data. 95 percent confidence intervals shown.  
 \*\*\* = p < .001

Table 34 Summary covariate balancing statistics after matching Hispanic and White non-Hispanic respondents for propensity score model of pat downs

	Rubin's B	Rubin's R
Caliper match = .10		
Sample 1	14.7	1.08
Sample 2	20.8	1.08
Caliper match = .06		
Sample 1	15.8	1.12
Sample 2	17.2	0.99

Table 35 Sensitivity analysis, propensity score models, pat down outcome, Hispanic vs. White non-Hispanic civilians

	Gamma ( $\Gamma$ ) value where ethnicity impact becomes non-significant
Caliper match = .10	
Sample 1	1.15
Sample 2	1.15
Caliper match = .06	
Sample 1	1.2
Sample 2	1.15

## 10.2 DID THE PAT DOWN RESULT IN A WEAPON/FIREARM BEING DISCOVERED?

Officers sometimes recovered weapons including firearms when they conducted pat downs of stopped civilians. Table 36 reports the number of pat downs and the number of recovered weapons for each of the two random samples of data. In each random sample around 8,900 pat downs took place resulting in roughly 240 recovered weapons. If a recovered weapon and only a recovered weapon counts as a hit, then the hit rate for pat downs in each sample was quite close to 2.5 percent.

This outcome depends on a prior officer action during the stop. Recovering a weapon or failing to recover a weapon requires that the officers initiate a pat down. Therefore, this outcome depends on an officer selection process. Statistical modeling must consider that process.

Put another way, and as described above in discussing sequential selection, race or ethnicity or gender can matter twice once a stop is underway. With this particular outcome, race or ethnicity can affect the likelihood of the civilian being selected for a pat down after controlling for other covariates. In addition, race or ethnicity can affect the likelihood that the pat down leads to a

recovered weapon. Modeling seeks to separately estimate these two race/ethnicity post-stop dynamics.

Regrettably, theories of officer behavior initiating stops and officer behavior during stops provide no clear theoretical guidance on which civilian, stop, or context features, i.e., covariates, are associated with which specific set of dynamics. Therefore, the models used here make some untested assumptions which will be explained as we go along.

In the first set of analyses, multiple regression mixed effects logit models were run that included an additional predictor intended to take into account dynamics leading to a stopped civilian being selected for a pat down. That additional predictor was the predicted probability from the mixed effects logit models that a stop would result in a pat down. See 10.1.1.1.

Table 36 Frequency of pat downs resulting in firearm/weapon recovered: Samples 1 and 2

Sample 1

	Pat down?		Total
	No (0)	Yes (1)	
Weapon found?			
No (0)	0	8,942	8,942
Yes (1)	0	229	229
(not applicable)	17,887	0	17,887
Total	17,887	9,171	27,058

Sample 2

	Pat down?		Total
	No (0)	Yes (1)	
Weapon found?			
No (0)	0	8,957	8,957
Yes (1)	0	236	236
(not applicable)	17,865	0	17,865
Total	17,865	9,193	27,058

Source: January-June 2016 ISR data from CPD.

Following those models, a different type of model formulated specifically to address the selection issue, was run.<sup>14</sup> This is the Heckman selection model (Heckman, 1979) for a binary outcome (Baum, 2006).

For the multiple logistic regression models, an initial mixed effects null model with each sample confirmed a **lack** of significant district-to-district variation in this outcome. A second single level logit model with all the dummy variables for districts, save the Loop, also revealed no significant

<sup>14</sup> This is heckprobit in Stata.

impacts of any district on the weapon recovered outcome. Therefore all the models run were single level models logit and did not include dummy variables for districts. Whether this lack of geographic variation on this pat down “hit” outcome was a function merely of the low base rate of weapon recovery, or something else, is not clear.

Although these are single level models they took clustering within district into account by allowing for clustered errors at the district level using the Huber-White sandwich estimator (White, 1982), despite some recent concerns about these adjustments (Freedman, 2006).

### 10.2.1 Multiple logistic regression models with predicted probabilities of a pat down

#### 10.2.1.1 Results

Multiple logistic regression models for sample 1 appear in Table 37, and results for sample 2 appear in Table 38. Each table shows the results for three models.

- Model A included just race and ethnicity.
- Model B added in the predicted probability that the stop would result in a pat down. This predicted probability was intended to control for the pat down selection process.
- Model C then added in all the other covariates used in previous models. Tables report both the coefficient (B) and the associated odds ratio (OR) for each predictor.

Hosmer-Lemeshow (2000) tests with both 10 groups and 50 groups generated nonsignificant results, suggesting some degree of overall fit (results not shown).

In short, for both samples Model C with all factors entered yielded a significant impact of race, in the expected negative direction, on the likelihood of a weapon being recovered as a result of the pat down ( $p < .05$ ). In the first sample patted down Black non-Hispanic civilians as compared to patted down White civilians had odds of [the pat down producing a weapon versus not producing a weapon] that were about 31 percent lower (1-.689). The corresponding figure in the second sample was odds that were about 38 percent lower (1-.617). Both of these demonstrated a negative net impact of race on the likelihood of recovering the weapon from a pat down.

Table 37 Multiple logistic regression models of pat down weapon recovered: Sample 1

Predictors	Reporting: Variable name	Model A		Model B		Model C	
		B	OR	B	OR	B	OR
Black civilian	dblack	-0.439	0.645	-0.246	0.782	-0.372*	0.689*
Hispanic civilian	dhispc	-0.179	0.836	-0.00136	0.999	-0.0523	0.949
Predicted: pat down	pre_dpat_1			-1.562***	0.210***	-0.698	0.498
Male	dmale					0.768**	2.156**
Age 18-25	age1825					-0.0172	0.983
Age 26-35	age2635					0.126	1.134
Age 36-45	age3645					0.525**	1.690**
Age 46 and up	age46pl					0.607**	1.835**
February	dfeb					0.613**	1.846**
March	dmar					0.0250	1.025
April	dapr					0.457*	1.580*
May	dmay					0.634**	1.885**
June	djun					0.549*	1.732*
Weekend	wknndum					0.00404	1.004
3 - 6 AM	dhr0306					-0.431	0.650
6 - 9 AM	dhr0609					-0.257	0.773
9 - 12 AM	dhr0912					0.0565	1.058
12 - PM	dhr1215					0.0516	1.053
3 - 6 PM	dhr1518					0.438	1.550
6 - 9 PM	dhr1821					0.108	1.114
9 - 12 PM	dhr2123					0.335	1.398
Vehicle stop	dvehstop					-0.309	0.734
Missing event no.	eventmis					0.371	1.449
Constant		-3.315***	0.0363***	-2.871***	0.0566***	-4.527***	0.0108***
N		9,171		9,171		9,171	

\*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Source: January-June 2016 ISR data from CPD.



Table 38 Multiple logistic regression models of pat down weapon recovered: Sample 2

Predictors	Reporting: Variable name	Model A		Model B		Model C	
		B	OR	B	OR	B	OR
Black civilian	dblack	-0.727**	0.484**	-0.518*	0.596*	-0.660*	0.517*
Hispanic civilian	dhisp	-0.501*	0.606*	-0.33	0.719	-0.326	0.722
Predicted: pat down	pre_dpat_2			-1.758**	0.172**	0.0148	1.015
Male	dmale					-0.151	0.86
Age 18-25	age1825					0.105	1.11
Age 26-35	age2635					0.305	1.357
Age 36-45	age3645					0.984***	2.674***
Age 46 and up	age46pl					1.172***	3.228***
February	dfeb					0.46	1.584
March	dmar					0.605	1.831
April	dapr					0.887***	2.427***
May	dmay					0.784***	2.190***
June	djun					0.726**	2.066**
Weekend	wknddum					0.343	1.409
3 - 6 AM	dhr0306					1.086**	2.961**
6 - 9 AM	dhr0609					0.137	1.147
9 - 12 AM	dhr0912					-0.0782	0.925
12 - PM	dhr1215					0.427	1.533
3 - 6 PM	dhr1518					0.504	1.655
6 - 9 PM	dhr1821					0.444*	1.558*
9 - 12 PM	dhr2123					0.297	1.345
Vehicle stop	dvehstop					-0.59	0.554
Missing event no.	eventmis					-	-
Constant		-3.015***	0.0490***	-2.508***	0.0814***	-4.360***	0.0128***
N		9,193		9,193		9,109	

\*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Source: January-June 2016 ISR data from CPD.

The predicted probabilities that a stop would result in a recovered weapon, presented separately depending on race, ethnicity and gender, appear in Table 39. In both samples, predicted recovery rates for stopped Black non-Hispanic civilians were in the two and a half percent range, in contrast to predicted probabilities in the three and a half percent to four and a half percent range, depending on the sample, for stopped White non-Hispanic civilians. Gender discrepancies in predicted weapon recovery rates via pat down within race/ethnicity groups proved sample dependent. For each of the three race/ethnicity groups predicted recovery rates were lower for women than men in sample 1. But in sample 2, pat downs of women linked to higher predicted probabilities of recovery for Black non-Hispanic and Hispanic women. The differences described here are descriptive only, and not statistically significant.<sup>15</sup>

<sup>15</sup> Models adding a gender x race interaction resulted in a BIC value that was almost equal to the BIC value of the model with no interaction (results not shown).

Table 39 Predicted probabilities of weapon recovery as a result of a pat down: Single level logit model

Sample		Racial/ethnic Group			Total
1		White NH	Black NH	Hispanic	
	Female	0.0202	0.0122	0.0151	0.0136
	Male	0.0371	0.0236	0.0304	0.0258
	Total	0.0351	0.0229	0.0295	0.025
2	Female	0.0426	0.0263	0.0335	0.0296
	Male	0.0482	0.0232	0.0289	0.0257
	Total	0.0474	0.0234	0.0291	0.0259

Note NH= non-Hispanic. Results from single level logit model with covariates and predicted probability that a pat down would take place. Source: Jan-June 2016 ISR reports from CPD.

### 10.2.1.2 Diagnostics

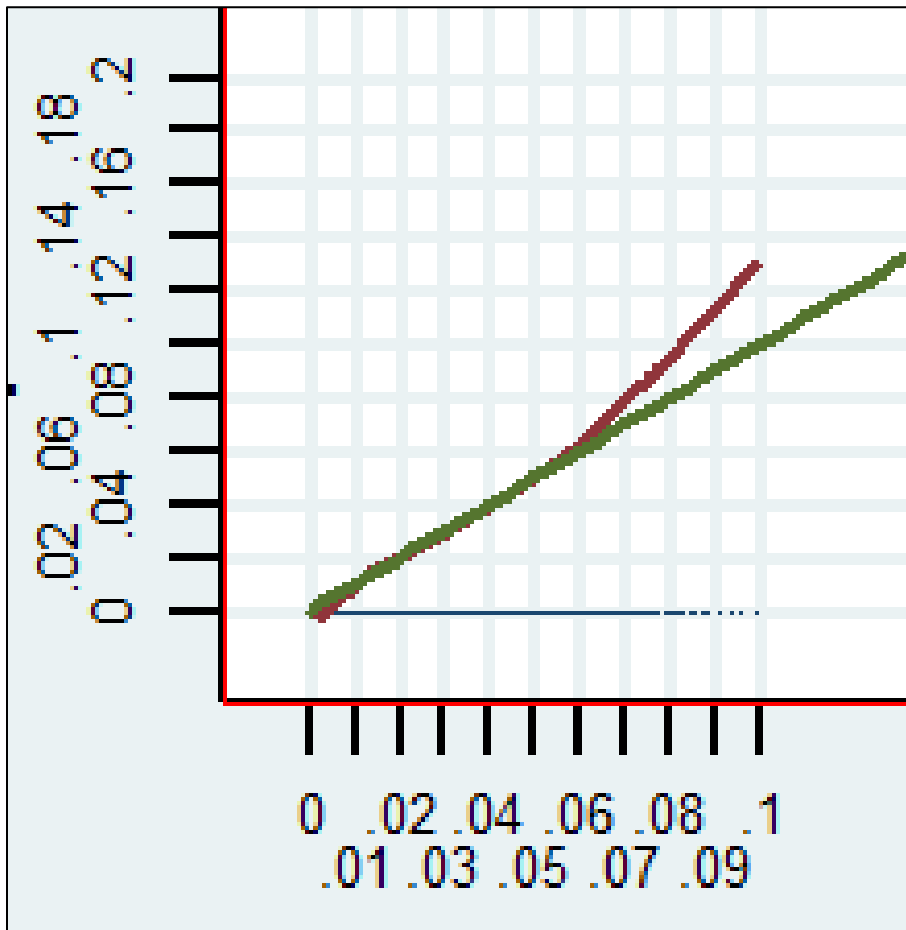
Despite the acceptable Hosmer-Lemeshow fit statistics, other diagnostics suggested some potential concerns about these models. For sample 1, Figure 11 plots predicted probabilities against the moving average of the proportion of pat downs yielding a weapon.

- The green [straight] line = the “moving average of the proportion of cases that equal one [weapon discovered];”
- The red [curved] line = the “fraction of observed cases that equal 1 [weapon discovered] at each level of the model’s predicted probability of observing a 1 [weapon discovered]” (Long & Freese, 2006: 156-157).

The red line uses local LOWESS smoothing. The X axis stops at .10 because that was the maximum predicted probability. Figure 12 shows the same information for sample 2.

For sample 1, results suggested the “model fail[ed] in predicting” the higher probabilities of a weapon being discovered where “the fraction of observed cases exceed[ed] the predicted probabilities” (Long & Freese, 2006: 157). This is because at values of predicted probabilities greater than about .06, the predicted red line began to diverge upward from the green line. This divergence suggested a lack of fit in this range of predicted probabilities.

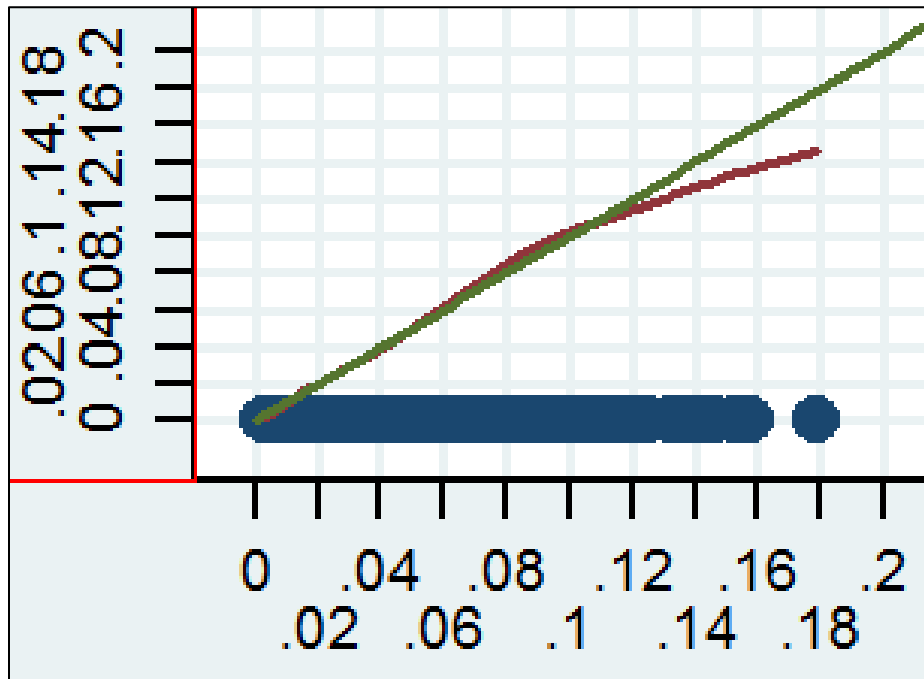
Figure 11 pat down results in a weapon, sample 1: LOWESS smoothed curve of observed (Y axis) and predicted (X axis) outcome



Note. Sample 1. Y (vertical) axis = observed outcome; X (horizontal) axis = predicted outcome. Red line shows local relationship between these two features. Divergence from straight green line suggests lack of fit.

In sample 2, shown in Figure 12, predicted probabilities ranged roughly twice as far as they did in sample 1. Here, predicted probabilities went up to almost .18. Again, as in sample 1, the diagnostics suggested a lack of fit at higher predicted probability values. The divergence here, however, went in the opposite direction. Starting at predicted probabilities of around .14 and going to higher values, the fraction of observed cases was lower than would be predicted from the model results. For example, at a predicted probability of weapons recovery of around .18, the observed proportion of weapons recovered was only .14.

Figure 12 pat down results in a weapon, sample 1: LOWESS smoothed curve of observed (Y axis) and predicted (X axis) outcome



Note. Sample 2. Y (vertical) axis = observed outcome; X (horizontal) axis = predicted outcome. Red line shows local relationship between these two features. Divergence from straight green line suggests lack of fit.

The suggested lack of fit between data and model at higher predicted probabilities, however, should be contextualized. It was in these observed and predicted probability ranges that observations were, in relative terms, somewhat sparse.

At this juncture, further diagnostics to determine the source of the model lack of fit in each sample were not undertaken given time constraints. All that can be said at this point is that there appears to be potential concern that the model, for reasons not yet known, appears to get off-track at higher predicted probabilities, and the way it gets off track depends on the specific random sample. Future diagnostics could more closely examine case level diagnostics such as influence and leverage.

## 10.2.2 Heckman probit selection models

### 10.2.2.1 Results

Attention turns now to models specifically designed to incorporate selection dynamics. These models worked as follows. They simultaneously estimated two independent equations: factors affecting the initial outcome, whether or not a pat down occurred; and factors affecting whether a weapon was recovered from a pat down. Each equation simultaneously took into account the other, including the degree of relatedness between the different dynamics represented by the two equations.

Because these analytics were specifically designed for this particular type of selection problem, they provided a more stringent test. Compared to the foregoing multiple logistic regression models, the results of this type of model will present a more “conservative” estimate of the impacts of race or ethnicity on weapon recovery following a pat down.

Although these are single level models they took clustering within district into account by allowing for clustered errors at the district level using the Huber-White sandwich estimator (White, 1982), despite some recent concerns about these adjustments (Freedman, 2006).

In these models the full set of predictors used in the multiple logistic regressions just reported, minus the predicted probability variable, predicted whether a pat down took place. Model A employed three variables to predict weapons recovered: whether the civilian was non-Hispanic Black, whether the civilian was Hispanic, and whether the civilian was male. Model B added age variables to the above to predict weapons recovery. Two different sets of models with different predictors of weapons recovery reflected the theoretical uncertainty mentioned earlier about the factors relevant to different elements of the post stop process. Table 40 presents results from sample 1 and Table 41 for sample 2. Although these tables present the selection equations, results will discuss just the outcome equation, that is, the determinants of whether a weapon was recovered during a pat down. Select features from the outcome equations appear in Table 42. Predicted probabilities of weapons recovered appear in Table 43.

Table 40. Sample 1: Determinants of pat down recovery of weapon controlling for pat down selection (heckprobit model)

SAMPLE 1 variable name	MODEL A Outcome equation: Weapon recovered				Selection equation: pat down occurs				MODEL B Outcome equation: Weapon recovered				Selection equation: pat down occurs			
	b	se	t	OR	b	se	t	OR	b	se	t	OR	b	se	t	OR
dblack	-0.0835	(0.0592)	-1.410	0.920	0.282***	(0.0724)	3.901	1.326	-0.0965	(0.0650)	-1.484	0.908	0.282***	(0.0724)	3.904	1.326
dhispanic	0.0177	(0.0703)	0.252	1.018	0.190**	(0.0665)	2.864	1.210	0.0121	(0.0698)	0.174	1.012	0.191**	(0.0664)	2.868	1.210
dmale	0.465***	(0.0925)	5.028	1.592	0.647***	(0.0344)	18.84	1.910	0.439***	(0.0892)	4.926	1.551	0.647***	(0.0344)	18.82	1.910
age1825					0.125*	(0.0524)	2.395	1.134	-0.0026	(0.0806)	-0.0329	0.997	0.125*	(0.0525)	2.389	1.134
age2635					-0.0320	(0.0419)	-0.762	0.969	0.0212	(0.0946)	0.224	1.021	-0.0315	(0.0413)	-0.764	0.969
age3645					-0.385***	(0.0562)	-6.861	0.680	0.113	(0.114)	0.990	1.119	-0.383***	(0.0572)	-6.685	0.682
age46pl					-0.690***	(0.0589)	-11.72	0.502	0.0584	(0.131)	0.446	1.060	-0.690***	(0.0595)	-11.60	0.502
dfeb					0.000114	(0.0346)	0.00331	1.000					0.00105	(0.0347)	0.0302	1.001
dmar					0.00982	(0.0495)	0.198	1.010					0.00988	(0.0495)	0.200	1.010
dapr					-0.0872	(0.0586)	-1.487	0.917					-0.0866	(0.0590)	-1.469	0.917
dmay					-0.160*	(0.0678)	-2.364	0.852					-0.159*	(0.0679)	-2.348	0.853
djun					-0.240***	(0.0660)	-3.634	0.787					-0.239***	(0.0660)	-3.625	0.787
wknddum					0.0779**	(0.0242)	3.224	1.081					0.0780**	(0.0242)	3.215	1.081
dhr0306					0.133	(0.101)	1.317	1.142					0.133	(0.101)	1.315	1.142
dhr0609					-0.604***	(0.150)	-4.028	0.547					-0.606***	(0.151)	-4.002	0.546
dhr0912					-0.436***	(0.0652)	-6.687	0.647					-0.437***	(0.0654)	-6.677	0.646
dhr1215					-0.323***	(0.0663)	-4.862	0.724					-0.323***	(0.0664)	-4.867	0.724
dhr1518					-0.194***	(0.0532)	-3.641	0.824					-0.193***	(0.0531)	-3.631	0.824
dhr1821					-0.219***	(0.0527)	-4.153	0.803					-0.219***	(0.0529)	-4.137	0.803
dhr2123					-0.173***	(0.0414)	-4.174	0.841					-0.172***	(0.0414)	-4.165	0.842
dvehstop					0.248***	(0.0533)	4.664	1.282					0.249***	(0.0533)	4.665	1.282
eventmis					0.187	(0.275)	0.682	1.206					0.189	(0.275)	0.686	1.208
Constant	-2.712			0.0664	-0.845***			0.430	-2.658***			0.0701	-0.846***			0.429
athrho	0.630***	(0.124)	5.096						0.485	(0.250)	1.94					
rho	0.558								0.4505							
Wald test	chi squared (df=1) = 25.97; p < .001								chi squared (df=1) = 3.76; p = .052							
BIC	34,350								34,359							
Observations	27,058															

Robust standard errors in parentheses. \*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Table 41 Sample 2: Determinants of pat down recovery of weapon controlling for pat down selection (heckprobit model)

SAMPLE 2 variable name	MODEL A Outcome equation: Weapon recovered				MODEL A Selection equation: pat down occurs				MODEL B Outcome equation: Weapon recovered				MODEL B Selection equation: pat down occurs			
	b	se	t	OR	b	se	t	OR	b	se	t	OR	b	se	t	OR
dblack	-0.147	(0.0892)	-1.653	0.863	0.280***	(0.074)	3.812	1.323	-0.223	(0.117)	-1.902	0.8	0.280***	(0.073)	3.839	1.324
dhisp	-0.0678	(0.104)	-0.653	0.934	0.163**	(0.056)	2.9	1.177	-0.099	(0.122)	-0.815	0.905	0.164**	(0.056)	2.926	1.178
dmale	0.231*	(0.0961)	2.398	1.259	0.660***	(0.034)	19.57	1.934	0.0647	(0.190)	0.341	1.067	0.659***	(0.034)	19.58	1.933
age1825					0.0954**	(0.034)	2.787	1.1	0.0431	(0.109)	0.395	1.044	0.0962**	(0.034)	2.87	1.101
age2635					-0.0373	(0.039)	-0.947	0.963	0.1	(0.087)	1.154	1.105	-0.0357	(0.039)	-0.926	0.965
age3645					-0.314***	(0.044)	-7.1	0.73	0.338**	(0.107)	3.155	1.402	-0.304***	(0.044)	-6.94	0.738
age46pl					-0.702***	(0.042)	-16.84	0.496	0.354*	(0.154)	2.294	1.424	-0.698***	(0.042)	-16.61	0.497
dfeb					0.0183	(0.036)	0.511	1.018					0.0219	(0.037)	0.599	1.022
dmar					0.0298	(0.056)	0.53	1.03					0.0346	(0.057)	0.611	1.035
dapr					-0.0699	(0.060)	-1.159	0.932					-0.0623	(0.062)	-1.003	0.94
dmay					-0.138	(0.073)	-1.897	0.871					-0.133	(0.075)	-1.778	0.876
djun					-0.196**	(0.071)	-2.751	0.822					-0.193**	(0.073)	-2.656	0.824
wknddum					0.0595*	(0.024)	2.45	1.061					0.0624**	(0.024)	2.628	1.064
dhr0306					0.218*	(0.086)	2.54	1.244					0.233*	(0.092)	2.528	1.262
dhr0609					-0.533***	(0.142)	-3.748	0.587					-0.539***	(0.142)	-3.794	0.584
dhr0912					-0.449***	(0.055)	-8.178	0.638					-0.455***	(0.055)	-8.312	0.634
dhr1215					-0.298***	(0.051)	-5.84	0.743					-0.299***	(0.052)	-5.753	0.742
dhr1518					-0.150*	(0.069)	-2.195	0.86					-0.148*	(0.069)	-2.15	0.863
dhr1821					-0.209***	(0.051)	-4.082	0.811					-0.208***	(0.052)	-4.016	0.812
dhr2123					-0.117***	(0.032)	-3.65	0.889					-0.117***	(0.032)	-3.629	0.89
dvehstop					0.293***	(0.057)	5.132	1.34					0.292***	(0.057)	5.12	1.34
eventmis					0.174	(0.159)	1.092	1.189					0.166	(0.162)	1.022	1.18
Constant	-2.447			0.0866	-0.886			0.412	-2.13			0.119	-0.893***			0.409
athrho	0.825*	0.383	2.154						0.21	0.298	0.7					
rho	0.678	0.207							0.2065							
Wald test	chi squared (df=1) = 4.64; p < .05								chi squared (df=1)=.49							
BIC	34,523								34,503							
Observations	27,058								27,058							

Robust standard errors in parentheses; \*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Table 42 Select model features from equations predicting weapons recovery from pat downs

	Sample	1		2	
		A	B	A	B
	Variable				
	Model				
	Black	0.159	0.132	0.099	0.057
Significance level of impacts of:	Hispanic	0.801	0.862	0.514	0.415
	Male	0.000001	0.000001	0.016	0.733
	Best fitting/most parsimonious?	Yes			Yes
Significant link between selection and outcome?		Yes	No	Yes	No

Note. Two tailed significance levels associated with t statistics for race, ethnicity and gender from Heckman probit selection models. Model A included just race, ethnicity and gender in the weapons recovery equation (reference group = White non-Hispanic females). Model B also included age categories (reference group = White non-Hispanic females younger than 18) in the same equation. The best fitting/most parsimonious selection was based on sizable differences in BIC values. Source: Jan-June 2016 ISR data from CPD

Three out of the four models showed male pat downs significantly more likely to lead to a recovered weapon compared to female pat downs (Models A & B, sample 1; Model A, sample 2). For example, looking at the predicted probabilities for sample 1 (Table 43), the average predicted probability for males was in the 1.1-1.2 percent range whereas the corresponding figure for females was in the 0.3-0.4 percent range.

In both samples, both models, ethnicity was not associated with the probabilities of weapons recovery from a pat down. Table 42 shows the probabilities associated with the Hispanic variable; all of these values are highly *nonsignificant*. Hispanics and non-Hispanics clearly did not differ, according to these models.

Race results were a closer call. In sample 1, the two tailed statistical significance probabilities associated with being Black were in the .13 to .16 range. In sample 2, they ranged from .06 - .10. Using a standard two-tailed test of statistical significance, as has been done throughout, these were only marginally significant impacts of race.

### 10.2.2.2 Diagnostics

Using the model that came closest to yielding a significant race impact on weapon recovery following a pat down, the smoothed LOWESS curve capturing the relationship between predicted probabilities and observed probabilities is the red line that appears in Figure 13.



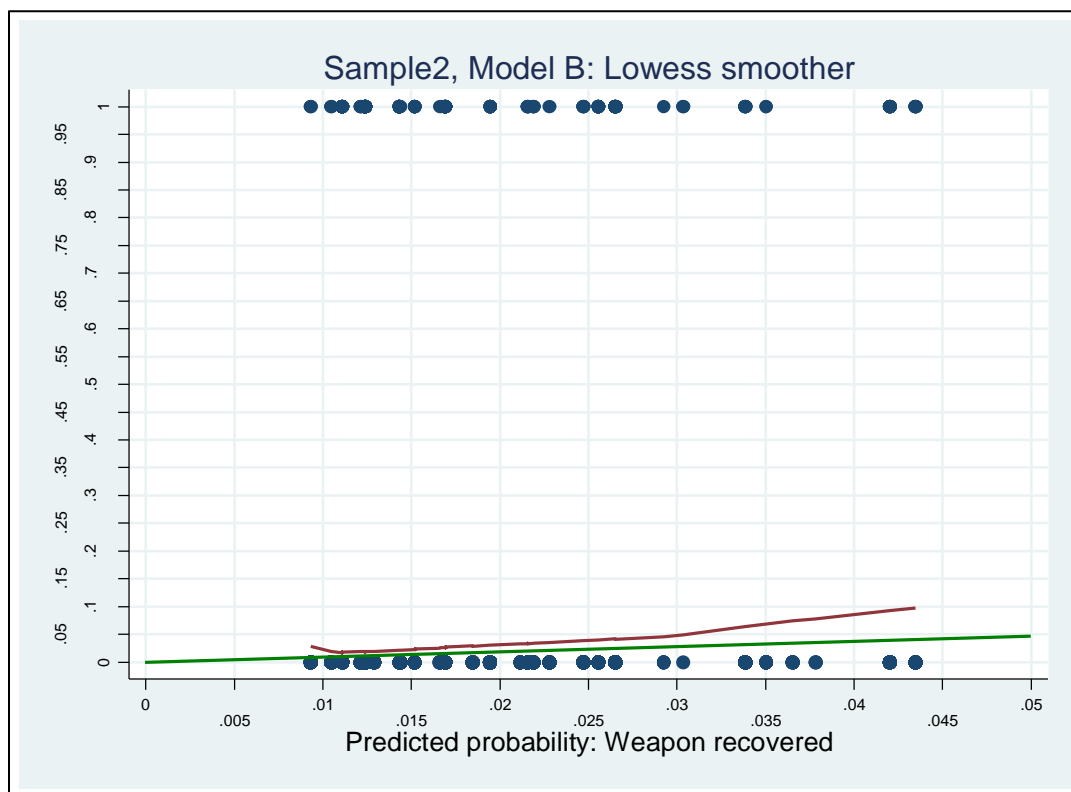
Table 43 Predicted probabilities that pat down leads to weapons recovery, by race/ethnicity/gender

Sample	Model	Racial/ethnic Group			Total	
1	A		White NH	Black NH	Hispanic	
		Female	0.0033	0.0026	0.0035	0.0029
		Male	0.0123	0.0099	0.0129	0.0107
		Total	0.0104	0.009	0.0117	0.0096
	B		White NH	Black NH	Hispanic	Total
		Female	0.0044	0.0032	0.0043	0.0036
		Male	0.0145	0.0111	0.0144	0.012
		Total	0.0123	0.0101	0.0131	0.0109
2	A		White NH	Black NH	Hispanic	
		Female	0.0072	0.0047	0.006	0.0053
		Male	0.0133	0.0091	0.0112	0.0098
		Total	0.012	0.0085	0.0105	0.0092
	B		White NH	Black NH	Hispanic	Total
		Female	0.0254	0.0144	0.0158	0.0161
		Male	0.0304	0.0167	0.0201	0.0184
		Total	0.0293	0.0164	0.0195	0.0181

Note. Predicted probabilities from Heckman probit models. Predicted probabilities generated using the default pmargin which means these represent the success (weapon found) probability

Source: Jan-June 2016 ISR data from CPD.

Figure 13 Predicted and observed probabilities of weapon recovery following pat down



Starting at predicted probability values of .035 and higher, the predicted probabilities begin to demonstrate a lack of fit. The predicted probabilities are substantially higher than the observed probabilities in that range. This lack of fit, however, is happening in a range of predicted probabilities where there are relatively few predicted scores (see Figure 14).

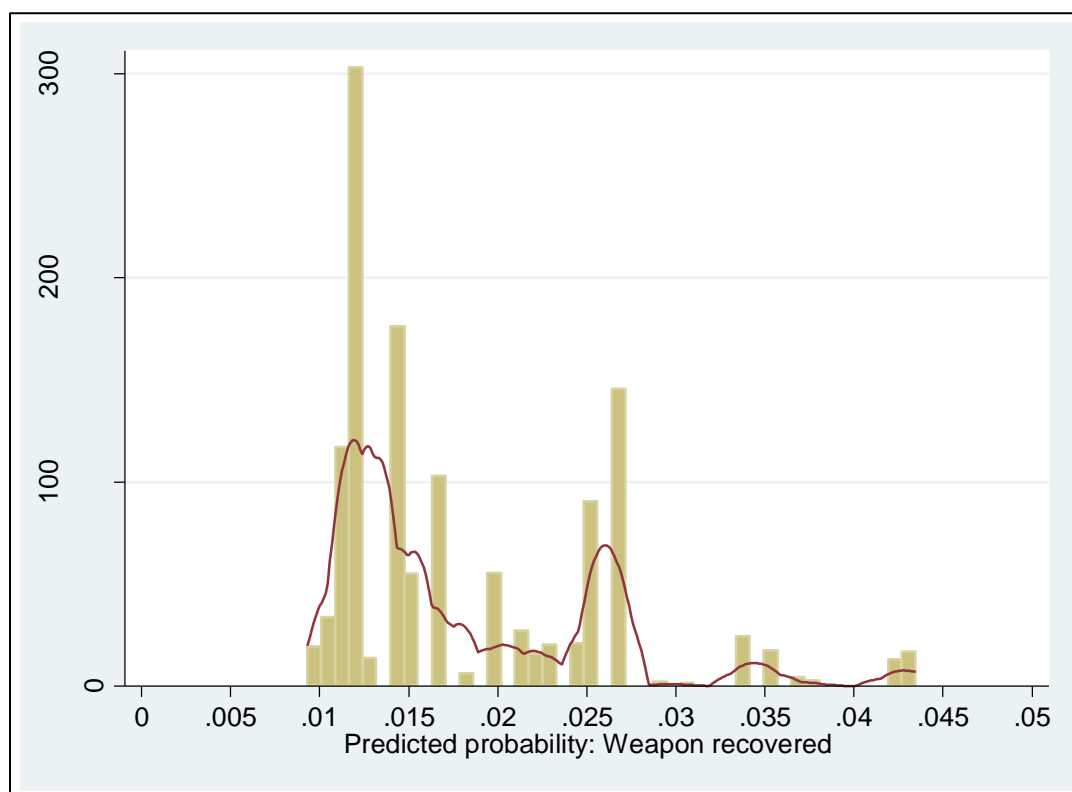
### 10.2.3 Conclusions on weapon recovery from pat downs

The clearest civilian correlate of whether a pat down yields a recovered weapon was gender. In the regression model results from one sample, and the selection model B results from both samples, male pat downs proved substantially more likely to yield weapons.

The second clearest civilian correlate was age. In Model B regression results from both samples, pat downs of civilians 36 and older proved more likely to reveal weapons. In the Heckman selection models, in sample 2 Model B results, but not in sample 1 Model B results, older civilians pat downs similarly were more likely to yield weapons. The one caution with this surprising finding that older stopped civilians who were patted down proved more likely to be armed was the failure to replicate the significant age impacts across **both** random samples in the Heckman selection models.

In sample 2, the *opposite-to-expected* age effects provided by the Heckman selection model underscored the importance of separating being patted down from a weapon being recovered. Older stopped civilians were significantly *less* likely to be patted down. But, at least in sample 2, if they were patted down they were *more* likely to have a weapon.

Figure 14 Distribution of predicted weapon recovery probabilities: Histogram and kernel density estimates



Note. Results from Model B, sample 2, Heckman probit selection model. Y axis is *density* not frequency. Source: Jan-June 2016 CPD ISR data.

The overall pattern for ethnicity link clear-cut. In the regression results ethnicity showed no significant connection with pat down weapon recovery. The Heckman selection models demonstrated the same lack of impact. But, again, this variable emphasized the importance of separating pat down and recovery dynamics. In the Heckman selection models, the null impacts of ethnicity on weapon recovery occurred in the context of a significant positive impact of ethnicity on being selected for a pat down in the first place. So two different ethnicity impacts appear to be operating once a stop is underway.

The race results prove hardest to summarize because here the results from the regression models and the Heckman selection models diverged most markedly. Regression models for both samples showed significantly lower pat down weapon yield for Black non-Hispanic as contrasted to White non-Hispanic civilians, controlling for the predicted probability that a pat down would take place.

In contrast, Heckman selection models failed to produce significant negative impacts of race on weapon recovery, although in Models A and B in sample 2 the race link had marginal significance. Underscoring yet again the importance of separating pat down and weapon recovery dynamics, the non significant or only marginally significant impacts of race on weapon

recovery occurred in the context of stopped Black non-Hispanic civilians being significantly ( $p < .001$ ) more likely to be selected for a pat down in the first place.

In short, race links to significantly lower hit rates if we follow the regression model results, but does not do so if we follow the Heckman selection model results, even though the latter come close to showing a significant race impact on pat down weapon recoveries.

If one argues that the hypothesis tests of race impacts on yield should be one tailed rather than two tailed, that is, the only possible interpretable outcome is that Black pat downs compared to non-black pat downs will be less likely to surface weapons, then the takeaway points are different. Results from both models for sample 2 using the Heckman selection model *do* yield a significant race impact: Black non-Hispanic searches failed to yield as many weapons, proportionally, as White non-Hispanic searches. But, this result *failed* to replicate across both samples, which is a remaining cause of concern.

All of these takeaway points must be contextualized in light of model diagnostics, which proved mixed as well. The regression models revealed acceptable overall fit using standard Hosmer-Lemeshow statistics. But plots of predicted and observed scores suggested divergences indicative of *lack* of model fit at the high end of the predicted probabilities of weapon recovery. Examining the Heckman selection model that came closest to producing a significant race impact on weapon recovery similarly suggested a lack of fit at higher predicted probability levels. In both cases, however, the divergence happened in a range where there were relatively few data points.

### 10.3 WAS A SEARCH CONDUCTED?

CPD officers are required to conduct a search prior to taking a civilian into custody for an arrest or transport.

#### 10.3.1 Exclusion question

***All analyses of the search outcome were conducted on records after removing those stops where an arrest took place.***

This dramatically reduced the volume of searches examined by roughly two thirds. See Table 45. In sample 1, 4,788 searches were reduced to 1,315. In sample 2, 4,807 searches were reduced to 1,325.

The reason for dropping these searches was this. We know that some fraction of these searches were *incident* to taking the arrestee into custody. Removing these is appropriate. The officer did not decide whether or not to search, rather he/she just followed department procedures in these cases. But there are other stops where the officer decided to do a search, did such a search, and based in part on what turned up in the search. One can argue that removing the latter group of searches was *inappropriate*. Such an inappropriate exclusion may render non-significant what would otherwise have been a significant net impact of race or ethnicity.

This is plausible. This concern represents a significant limitation of our search analyses, and this will be addressed in future work.

### 10.3.2 Search links to other enforcement outcomes

Aside from searches linked to arrests, how often did searches link to other enforcement outcomes? Results, combining both random samples for ease of presentation, appear in Table 44. Slightly over one third of the non arrest linked searches, 37 percent, occurred in stops where another type of enforcement action took place (n=976). But almost two times as many non-arrest-linked searches took place in stops where no enforcement action was recorded (n=1,644). This latter group made up slightly less than two thirds of the non-arrest searches (63 percent).

Table 44. Among non-arrest linked searches: N and proportion linked or not linked with other enforcement actions

N searches linked to non-arrest enforcement actions	
All non-arrest linked searches	2,640
N non-arrest searches linked to other specific enforcement actions	
ANOV	403
PSC	352
Other	221
Sum: N searches linked to non arrest enforcement actions	
	976
Proportion of non-arrest searches	0.37
N searches linked to no enforcement action	
	1,664
Proportion of non-arrest searches	0.63

Source: Jan-June 2016 CPD ISR data. Both samples combined.

### 10.3.3 Mixed effects regression models

#### 10.3.3.1 Results

Initial mixed effects logit models confirmed significant ( $p < .001$ ) variation across districts in the probability that a stop would involve a search (results not shown).

Results from the model with all covariates entered appear in Table 46. Neither civilian race nor civilian ethnicity significantly affected the odds that a search would be conducted. This was true for both samples. So for this outcome there appeared to be no net effects of either race or ethnicity.

Gender, however, did elevate the chances that officers would search civilians. But this impact appeared only for sample 2. Males' odds of [being searched versus not searched] were about 40 percent higher in that sample.

Table 45 Numbers of stops, with and without searches, before and after removing custodial searches incident to arrest

Sample			N	Percent
1	Initial			
	Search	No	22,270	82.3
		Yes	4,788	17.7
	Total		27,058	
	After removing custodial searches incident to arrest			
	Search	No	22,270	94.42
		Yes	1,315	5.58
	Total		23,585	
2	Initial			
	Search	No	22,251	82.23
		Yes	4,807	17.77
	Total		27,058	
	After removing custodial searches incident to arrest			
	Search	No	22,251	94.38
		Yes	1,325	5.62
	Total		23,576	

Source: Jan-Jun 2016 ISR data from CPD

Turning to other covariates, age mattered. In both samples stopped civilians between the ages of 18 and 35 were more likely to be searched than those below the age of 18. In addition, when the stop took place proved relevant in both samples. Compared to the reference time frame between midnight and 3 AM, searches were less likely, in both samples, between 6 AM and 9 PM. Finally, in both samples vehicle as compared to pedestrian stops had a much higher likelihood of resulting in a search. The expected odds of a search taking place were the least 200 percent higher in both samples if it was a vehicle rather than pedestrian stop.

Table 46 Predicting search occurrence: Mixed effects logit model

Fixed effects		Sample 1		Sample 2	
		b	OR	b	OR
Black civilian	dblack	0.0924	1.097	0.109	1.115
Hispanic civilian	dhis	0.0977	1.103	0.108	1.114
Male	dmale	0.164	1.178	0.343***	1.409***
Age 18-25	age1825	0.462***	1.587***	0.573***	1.774***
Age 26-35	age2635	0.409***	1.506***	0.604***	1.829***
Age 36-45	age3645	0.112	1.119	0.261*	1.298*
Age 46 and up	age46pl	-0.395**	0.674**	-0.213	0.808
February	dfeb	-0.445***	0.641***	-0.155	0.857
March	dmar	-0.193	0.825	-0.00366	0.996
April	dapr	-0.236*	0.790*	0.0849	1.089
May	dmay	-0.461***	0.631***	-0.163	0.85
June	djun	-0.399***	0.671***	-0.301**	0.740**
Weekend	wknddum	-0.0412	0.960	0.0912	1.096
3 - 6 AM	dhr0306	-0.0300	0.970	0.0532	1.055
6 - 9 AM	dhr0609	-1.326***	0.266***	-0.791***	0.453***
9 - 12 AM	dhr0912	-0.524***	0.592***	-0.281*	0.755*
12 - PM	dhr1215	-0.554***	0.575***	-0.578***	0.561***
3 - 6 PM	dhr1518	-0.403***	0.668***	-0.313*	0.731*
6 - 9 PM	dhr1821	-0.408***	0.665***	-0.259*	0.772*
9 - 12 PM	dhr2123	-0.274**	0.760**	-0.107	0.898
Vehicle stop	dvehstop	1.127***	3.087***	1.329***	3.777***
Missing event no.	eventmis	-0.333	0.717	-0.936*	0.392*
Constant		-2.739	0.0646	-3.409	0.0331
Random effects	District variance	0.0452*		0.0906**	
	Observations	23,585		23,576	
	Number of groups	22		22	
	BIC	9,874		9,841	

\*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Note. Jan.-June 2016 ISR data, CPD

For sample 1: Null model: LR  $\chi^2$  test vs. logistic model (df = 1) = 45.25; p < .001; BIC = 10,122

For sample 2: Null model: LR  $\chi^2$  test vs. logistic model (df = 1) = 80.85; p < .001; BIC = 10,142

Source: Jan-Jun 2016 ISR data from CPD

### 10.3.3.2 Diagnostics

Diagnostics generally suggested just a few concerns with these models. The LOWESS smoothed curves showing the relationship between predicted probabilities and observed proportions indicated relatively close fit of the predictions at all ranges of predicted probabilities for both samples (results not shown). Plots of residuals against predicted probabilities showed no relationship in both samples (results not shown). Only two features were potentially concerning. As seen earlier in the models predicting whether a pat down took place, here too residuals appeared correlated with age, trending lower for younger age civilians (results not shown). Finally, normal probability plots showed a higher density than expected of residuals in the second quartile for cases where a search took place (results not shown).

In contrast to the results with the pat down outcome, geographic residual variation for the search outcome showed no significant discrepancies across districts (results not shown). For each district, the confidence interval around its average residual always included the overall average residual (-.16).

In sum, these diagnostics suggested a low to moderate level of concern about observed and unobserved selection.

### 10.3.4 Propensity score models: Black vs. White non-Hispanics only

As with the pat down outcome, propensity score models with caliper matching on the propensity score were conducted. Here, only one level of caliper matching, within .06 of a standard deviation, was examined.

#### 10.3.4.1 Results

Table 47 shows impacts of race on whether a search was conducted, using a propensity score matching model. The model for sample 1 included 1,622 Black non-Hispanic civilians and 1,824 matched White non-Hispanic civilians. The model for sample 2 included 1,646 Black non-Hispanic stopped civilians and 1,831 matched White non-Hispanic civilians. Preliminary models confirmed that the chances of a stopped civilian being Black varied significantly across districts in each sample, and that the outcome varied significantly across districts in both samples when considering just the propensity matched cases (results not shown).

The table tells a simple story. No significant predicted differences on the chances of being searched appeared when contrasting matched White and Black non-Hispanic stopped civilians. For members of both groups, chances of being searched, in both samples, were right around 4.7 percent.

#### 10.3.4.2 Diagnostics

Results do not appear susceptible to selection on observed covariates based on summary measures. In both samples values for Rubin's  $B$  and Rubin's  $R$  were well within the acceptable range. In sample 1,  $B = 16.4$ ,  $R = 1.00$ . In sample 2,  $B = 15.5$  and  $R = 1.04$ .

Individual covariates, however, did suggest some slight causes for concern. There were both mean differences and variance differences. Looking at individual covariate mean differences after matching in each sample showed one significant difference (proportion of weekend stops in sample 1, proportion of males stopped in sample 2). Other covariates were mean balanced. But



the ratios of treated (black) vs. control (white) variances were all outside the acceptable range in both samples.

All of this suggests a small to moderate level of concern about selection on observed covariates.

Table 47 Propensity score model estimates of impact of Black vs. White civilians on search outcome

	B	SE	Z	p =	LCL	UCL	OR	OR-LCL	OR-UCL
Propensity score model race impact, Caliper=.06									
Sample 1									
Black non-Hispanic	-0.00437	0.160	-0.0273	0.978	-0.318	0.309	0.996	0.728	1.362
Constant	-3						0.0498		
District variance (se)	0.152	0.109							
Observations	3,446								
Number of groups	22								
LR chi square test	5.88 (p < .01)								
Sample 2									
Black non-Hispanic	0.00198	0.154	0.0129	0.99	-0.299	0.303	1.002	0.741	1.355
Constant	-3.017						0.0489		
District variance (se)	0.256*	0.127							
Observations	3,477								
Number of groups	22								
LR chi square test	26.39 (p < .001)								

\* = p < .05

Note. Impact of Black vs. White non-Hispanic stopped civilians on search outcome from propensity score model using Caliper matching within .06 of a standard deviation on the propensity score. Non-matched cases dropped. B = coefficient; SE = standard error; Z = Z test; OR = odds ratio. LCL and UCL = respectively, lower and upper bounds of 95 percent confidence interval

Source: Jan-June 2016 ISRs from CPD.

\* = p < .05

### 10.3.5 Propensity score models: White non-Hispanic vs. Hispanic only

For sample 1, propensity score matching within .06 of a standard deviation of propensity meant that the analysis focused on 1,520 Hispanics and 1,824 matched White non-Hispanics. For sample 2, the corresponding numbers were 1,831 White non-Hispanics in 1,494 Hispanics. Black civilians were excluded from this analysis.

Preliminary analyses confirmed for both samples that the probabilities of the non-black civilian being Hispanic varied significantly across districts (results not shown). They also confirmed that when analyzing just the matched cases, significant variation on the search outcome across districts persisted in both samples (results not shown).

Table 48 Propensity score model estimates of impact of Hispanic vs. White non-Hispanic civilians on search outcome

Propensity score model ethnicity impact, Caliper = .06

B	SE	Z	p =	LCL	UCL	OR	OR-LCL	OR-UCL
---	----	---	-----	-----	-----	----	--------	--------

Sample 1									
Hispanic	0.335*	0.151	2.225	0.0261	0.0399	0.630	1.398*	1.041	1.878
Constant	-2.978						0.0509		
District variance (se)	0.0298	0.0443							
LR chi squared test	0.73, ns								
Observations	3,344								
Number of groups	22								
Sample 2									
Hispanic	0.334*	0.147	2.272	0.0231	0.0458	0.621	1.396*	1.047	1.862
Constant	-2.972						0.0512		
District variance (se)	0.153	0.089							
LR chi squared test	14.42; p < .001								
Observations	3,325								
Number of groups	22								

Note. Impact of Hispanic vs. White non-Hispanic stopped civilians on search outcome from propensity score model using Caliper matching within .06 of a standard deviation on the propensity score. Non-matched cases dropped. B = coefficient; SE = standard error; Z = Z test; OR = odds ratio. LCL and UCL = respectively, lower and upper bounds of 95 percent confidence interval

Source: Jan-June 2016 ISRs from CPD.

\* = p < .05

### 10.3.5.1 Results

Results from caliper matched propensity score models with just matching cases appear in Table 48. Results from both samples indicated that Hispanic stopped civilians had about 40 percent higher predicted odds of [being searched versus not searched] compared to the predicted odds of matched non-Hispanic White stopped civilians.

In sample 1, White non-Hispanic civilians' predicted probability of being searched was .048. The corresponding figure for Hispanic civilians in the sample was .066. In sample 2 the predicted probability for White non-Hispanics was .049 and the corresponding predicted probability for Hispanic civilians of being searched was .067. In both samples these results were statistically significant (p < .05).

In short, both samples suggested that Hispanic stopped civilians' chances of being searched were significantly higher than the chances of matched White non-Hispanic civilians.

### 10.3.5.2 Diagnostics

Summary measures of covariate balancing after matching suggested selection on observed covariates was not a concern. In sample 1 Rubin's *B* = 18.8 and Rubin's *R* = 1.08. In sample 2 the corresponding numbers were 18.4 and 0.98. Examining individual covariates suggested a bit more concern about this matter. For both samples, after matching, there was at least one covariate where a significant mean difference remained. Further, the ratio comparing variances of the White cases and Hispanic cases were for each covariate outside the suggested boundaries.

Sensitivity analyses suggested extreme sensitivity to selection on unobserved covariates. In both samples, minor changes in the odds of differential “assignment” to ethnicity due to unobserved factors ( $\Gamma = 1.05$ ) resulted in the observed significant ethnicity impact disappearing.

### 10.3.6 Summing up on search outcome and race and ethnicity

The points to take away about the link between race and ethnicity of stopped civilians and whether or not they were searched include the following.

Race is not relevant. Neither the regression models nor the propensity score matching models revealed significant net differences between Black and White non-Hispanic stopped civilians on this outcome.

Ethnicity may be relevant. Although the regression models for both samples failed to find a significant ethnicity impact, the matching propensity score models for both samples did find one. That link however is probably best interpreted as correlational and not causal for the following reason. The propensity score matching models appear extremely sensitive to selection on unobserved factors.

## 10.4 DID A SEARCH RESULT IN A WEAPON BEING DISCOVERED?

After removing custodial searches incident to arrest, and considering only cases where officers also checked the search box, only an extremely low number of searches resulted in weapons being discovered. Given those extremely low numbers, and the large number of covariates involved in the models used here, this outcome was not analyzed.

In sample 1, only 10 searches produced a weapon or a firearm or both after removing searches incident to arrest. In sample 2, the number was 14 after the removal.

## 10.5 DID THE OFFICER ENGAGE IN ENFORCEMENT?

### 10.5.1 Regression results

Results from both samples reveal significant net effects of both race and ethnicity on the likelihood that any type of enforcement action would be delivered during the stop. In both samples, Black non-Hispanic stopped civilians had at least 20 percent greater odds of [receiving any enforcement action versus no enforcement action] compared to White non-Hispanic stopped civilians. The discrepancy between Hispanic and non-Hispanic White civilians was about the same magnitude. See Table 49.

Table 49 Predicting any enforcement action: Mixed effects logit models

		Sample 1		Sample 2	
Fixed effects		b	OR	b	OR
Black civilian	dblack	0.256***	1.291***	0.213***	1.237***
Hispanic civilian	dhispanic	0.239***	1.270***	0.218***	1.243***
Male	dmale	-0.105**	0.900**	-0.011	0.989
Age 18-25	age1825	-0.441***	0.643***	-0.395***	0.674***
Age 26-35	age2635	-0.201***	0.818***	-0.141**	0.868**
Age 36-45	age3645	-0.126*	0.882*	-0.0588	0.943
Age 46 and up	age46pl	0.119**	1.127**	0.170***	1.185***
February	dfeb	-0.420***	0.657***	-0.384***	0.681***
March	dmar	-0.444***	0.641***	-0.436***	0.646***
April	dapr	-0.545***	0.580***	-0.472***	0.624***
May	dmay	-0.612***	0.542***	-0.555***	0.574***
June	djun	-0.614***	0.541***	-0.635***	0.530***
Weekend	wknddum	-0.0318	0.969	0.018	1.018
3 - 6 AM	dhr0306	0.291**	1.338**	0.216*	1.242*
6 - 9 AM	dhr0609	-0.243**	0.784**	-0.0862	0.917
9 - 12 AM	dhr0912	-0.189**	0.828**	-0.118*	0.889*
12 - PM	dhr1215	-0.413***	0.661***	-0.381***	0.683***
3 - 6 PM	dhr1518	-0.484***	0.616***	-0.404***	0.668***
6 - 9 PM	dhr1821	-0.352***	0.703***	-0.355***	0.701***
9 - 12 PM	dhr2123	-0.239***	0.787***	-0.156**	0.856**
Vehicle stop	dvehstop	0.489***	1.630***	0.583***	1.792***
Missing event no.	eventmis	-2.887***	0.0558***	-1.729***	0.177***
Constant		-0.00504	0.995	-0.203*	0.816*
Random effects					
	District variance	0.0532**		0.0648**	
BIC		33,052		33,001	
Observations		27,058		27,058	
Number of groups		22		22	

Note \*\*\* p<0.001, \*\* p<0.01, \* p<0.05

Note. January – June 2016 ISR data, CPD

Note. For sample 1: Null model: LR  $\chi^2$  test vs. logistic model = 238.63; p < .001; BIC = 33,816

For sample 2: Null model: LR  $\chi^2$  test vs. logistic model = 266.12 ; p < .001; BIC = 33,728

Predicted probabilities appear in Table 50 for both samples, shown separately by race/ethnicity/gender combination. It shows, for example, in sample 1 that among stopped Black non-Hispanic males the predicted probability of receiving any enforcement action was 32.6 percent, compared to a predicted probability of 27.8 percent for White non-Hispanic males.

Table 50. Predicted probabilities, any enforcement action

Sample 1

Gender

	White NH	Black NH	Hispanic	Total
Female	0.296	0.367	0.35	0.355
Male	0.278	0.326	0.302	0.318
Total	0.282	0.331	0.308	0.323

Sample 2

Gender

	White NH	Black NH	Hispanic	Total
Female	0.283	0.348	0.321	0.334
Male	0.284	0.327	0.305	0.319
Total	0.284	0.33	0.307	0.321

Note. NH= non-Hispanic. Source: Jan-June 2016 ISRs, CPD

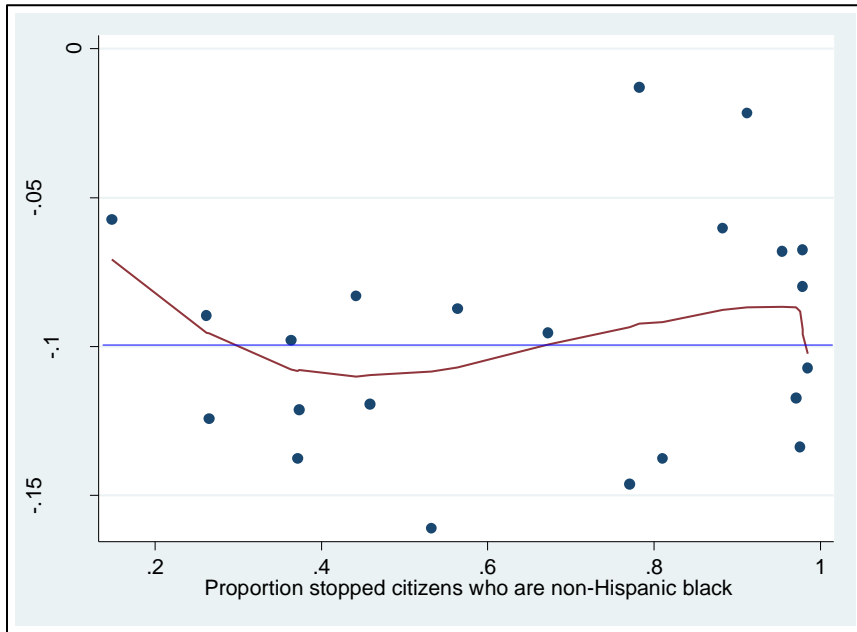
### 10.5.2 Diagnostics

Diagnostics revealed the following. In both samples predicted probabilities deviated only modestly from observed proportions receiving any enforcement action, at different predicted probabilities (results not shown). The district level residuals' 95 percent confidence intervals in all cases overlapped with the overall residual.

But, that said, two features of residuals suggested a low to moderate level of concern about potential bias due to unobserved selection. In both samples, a modest relationship between residuals and predicted scores surfaced, with residuals increasing slightly as predicted scores increased (results not shown). Further, in both samples a modest district-level relationship surfaced between the proportions of civilians stopped who were Black and non-Hispanic, and the standardized model residuals (Figure 15, Figure 16).

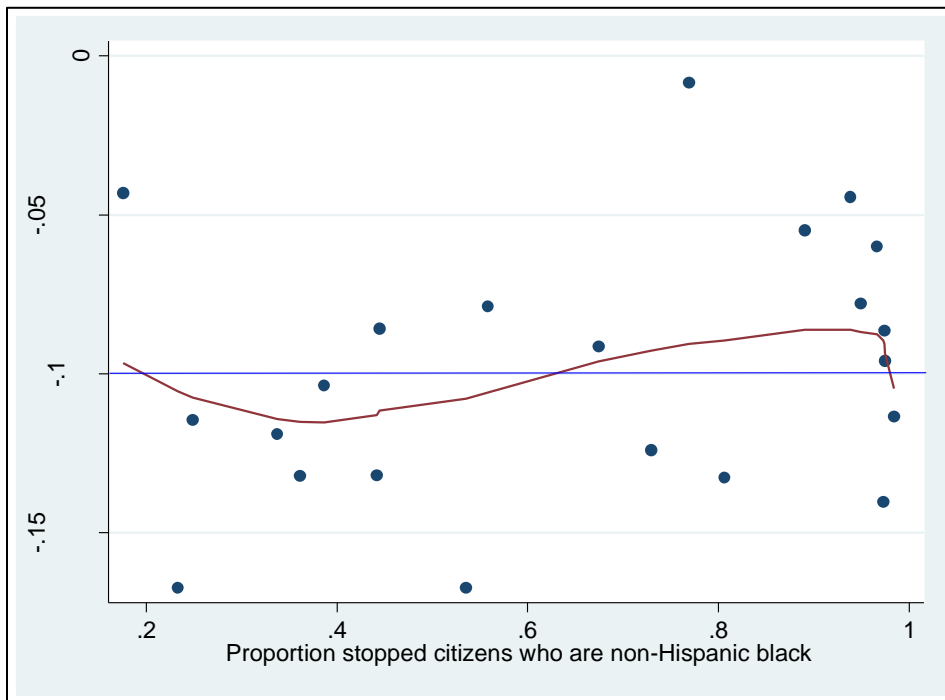
Interpreting this pattern requires a bit of background on residuals in logit models. The standardized Anscombe residuals for sample 1 appear in Figure 17.

Figure 15 Residual, any enforcement action, and proportions stopped Black civilians: Sample 1



Note. Horizontal reference line reflects average overall residual. Data shown are district level. Curved line = LOWESS smoothed curve. Source: Jan-June 2016 ISRs, CPD.

Figure 16 Residual, any enforcement action, and proportions stopped Black civilians: Sample 2

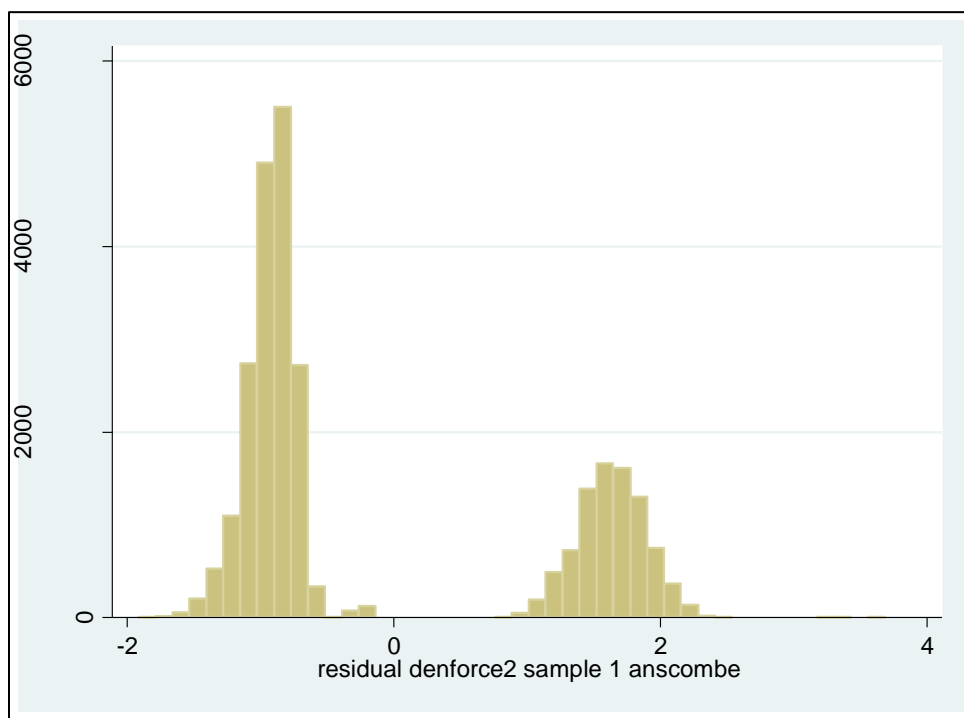


Note. Horizontal reference line reflects average overall residual. Data shown are district level. Curved line = LOWESS smoothed curve. Source: Jan-June 2016 ISRs, CPD.

A positive residual is associated with a stop where any enforcement action occurred. A negative residual is associated with a stop where an enforcement action did not occur. Standardized residual values within +/- 2 or +/- 3 are not considered outliers. There are some positive outliers indicating stops where enforcement happened despite an extremely low predicted probability that that would happen. Therefore, the point suggested by Figure 15, and Figure 16, is that in districts where a large fraction of stopped civilians were Black, there was a larger mix of stops in that district where enforcement took place despite low predicted probabilities.

Again, this patterning of residuals shifts across different predicted probabilities is only a modest pattern. But it is noticeable in both samples. That's why a low-to-moderate degree of concern about unobserved selection is suggested with these regression models.

Figure 17 Any enforcement action residuals: Sample 1



Note. Jan-June 2016 ISRs from CPD

### 10.5.3 Propensity selection model – Black non-Hispanic vs. White non-Hispanic

#### 10.5.3.1 Results

Propensity score models with matched cases within .060 of a standard deviation on the propensity score were run. Considering only matched cases, the significant race effect failed to resurface (Table 51). Black stopped civilians compared to White stopped civilians had only slightly higher odds of [receiving any enforcement action versus receiving none].

In the first random sample, Black civilians' odds were about 11 percent higher; they were about 7 percent higher in the second random sample. In neither sample, however, were these differences between Blacks and Whites statistically significant.

#### *10.5.3.2 Diagnostics*

Diagnostics suggested a low to medium level of concern about potential confounding due to observed selection. Looking at the overall balance diagnostics and sample 1, Rubin's  $B$  was above the suggested cutoff value ( $B=26.6$ ) suggesting a lack of balance on covariates. The overall balance statistics were within an acceptable range for sample 2 (Rubin's  $B = 22.1$ ; Rubin's  $R = 1.06$ ).

Looking at the individual covariate diagnostics in sample 1 showed there were a couple of covariates, such as being male and the stop taking place on the weekend, that remained significantly different between the two racial groups even after matching (sample 1). In sample 2 after matching there remained significant differences between the two groups for the stop taking place on the weekend and the stopped civilian being between the ages of 26 and 35.

#### **10.5.4 Propensity selection model – Hispanic vs. White non-Hispanic**

##### *10.5.4.1 Results*

In contrast to the race results, the ethnicity differences seen in the regression model resurfaced in the propensity score matching models (Table 52), and for both random samples. In both, Hispanics odds of being [receiving any enforcement action versus none] were about 20 percent higher than the odds for matched White non-Hispanics. The size of the ethnicity impact seen in the propensity models, expressed as an odds ratio, was closely comparable to the size of the effect seen for ethnicity in the multiple regression models. Controlling for other factors, and for district context, stopped Hispanics were more significantly likely to be on the receiving end of an enforcement action than were stopped White non-Hispanic civilians.

##### *10.5.4.2 Diagnostics*

Overall balance diagnostics suggested that selection on observed covariates was only of low concern ( $B = 15.8$ ;  $R = 1.12$ ). Individual variable diagnostics suggested somewhat more concern. There were mean differences on a couple of covariates even after matching, and variance ratios between Hispanic/non-Hispanic White groups continued to be quite dissimilar even after matching.



Table 51 Matched propensity score model results predicting any enforcement action: Black vs. White non-Hispanic

	B	SE	Z	p=	LCL	UCL	OR	OR-LCL	OR-UCL
Sample 1									
Black non-Hispanic	0.102	0.0710	1.436	0.151	-0.0372	0.241	1.107	0.963	1.273
Constant	-0.897						0.408		
District variance (se)	0.0541*	0.0270							
Observations	3,957								
Number of groups	22								
LR chi squared test; 17.51; p < .001									

Sample 2									
Black non-Hispanic	0.0665	0.0703	0.946	0.344	-0.0713	0.204	1.069	0.931	1.227
Constant	-0.871						0.418		
District variance (se)	0.0623	0.0331							
Observations	4,025								
Number of groups	22								
LR chi squared test; 17.09; p < .001									

Note. \*\*\* p<0.001, \*\* p<0.01, \* p<0.05  
 Note. January – June 2016 ISR data, CPD

Table 52 Matched propensity score model results predicting any enforcement outcome: Hispanic vs. White non-Hispanic

	B	SE	Z	p=	LCL	UCL	OR	OR-LCL	OR-UCL
Sample 1									
Hispanic	0.189**	0.0719	2.634	0.0084	0.0485	0.330	1.208**	1.050	1.391
Constant	-0.948						0.387		
District variance (se)	0.0818*	0.0370							
Observations	3,853								
Number of groups	22								
LR chi squared test (df=1)	34.22***								

Sample 2									
Hispanic	0.183*	0.0718	2.549	0.0108	0.0423	0.324	1.201*	1.043	1.382
Constant	-0.933						0.393		
District variance (se)	0.0747*	0.0371							
Observations	3,859								
Number of groups	22								
LR chi squared test (df=1)	29.11***								

Note. \*\*\* p<0.001, \*\* p<0.01, \* p<0.05  
 Note. January – June 2016 ISR data, CPD

Sensitivity tests indicated that potential selection on unobserved factors was potentially more of a problem. In sample 1, even a 10 percent shift in the odds of being Hispanic/non-Hispanic rendered the ethnic difference non-significant ( $\Gamma = 1.10$ ). In sample 2, the significant difference disappeared with a 5 percent shift ( $\Gamma = 1.05$ ).

#### 10.5.5 Overall conclusion on race/ethnicity and enforcement

In both random samples, regression results revealed significant net impacts of both race and ethnicity on the likelihood that the stopped civilian would receive any enforcement action. Black civilians and Hispanic civilians as compared to White civilians were more likely to be on the receiving end of such actions. Diagnostics suggested a low to moderate level of concern about potential confounds due to unobserved selection. The implication is that a correlational rather than causal interpretation is probably more warranted.

Race impacts failed to re-appear in the propensity matching models.

Ethnicity impacts, however, did resurface with the propensity matching models, and their size was closely comparable to that seen in the regression results. That said, low to moderate concerns about observed selection, and strong concerns about unobserved selection given the results of diagnostics, favor a correlational rather than causal interpretation of this ethnicity impact.

### 10.6 IF NO ENFORCEMENT TOOK PLACE, WHAT DETERMINED WHETHER A PAT DOWN TOOK PLACE?

The last planned analysis considered the potential roles of race and ethnicity in shaping whether the stop ended in one of two ways: a pat down was delivered but no enforcement action was taken, versus no pat down took place and no enforcement action was delivered. As discussed earlier, the procedural justice literature clearly implies that the former type of stop is more intrusive, and has more potential to reduce perceived institutional legitimacy.

#### 10.6.1 Main modeling approach

The models used here were mixed effects multinomial models with stops nested within districts. Because there are four outcome categories for all possible combinations of enforcement in pat down, propensity score matching models were not feasible. Further, model diagnostics were not undertaken. Given the lack of a cross checking analysis, and the lack of detailed diagnostics on predicted scores and residuals, the results presented here should be considered **preliminary**, and certainly should not be interpreted as more than correlational.

Both samples produced statistically significant and practically sizable race and ethnicity impacts (Table 53, Table 54). In both cases, stopped Black as compared to stopped White civilians, and stopped Hispanic as compared to stopped White civilians, had odds of being patted down but no enforcement that were at least 40 percent higher. Because of the covariates were taken into account and district context was considered, these are **net** race and ethnicity impacts.

Even more sizable impacts emerged for gender. In both samples, males' odds of experiencing [a pat down with no enforcement versus no pat down and no enforcement] were at least 250 percent higher.

Finally, the pat down with no enforcement outcome was much more likely in both samples to occur with vehicle stops.

Table 53 Predicting pat down and no enforcement vs. no pat down and no enforcement: Sample 1

Variable	Variable name	B	SE	t	p=	OR
Black civilian	dblack	0.382	0.0694	5.511	<.001	1.465
Hispanic civilian	dhispanic	0.350	0.0737	4.744	<.001	1.419
Male	dmale	1.272	0.0606	20.99	<.001	3.568
Age 18-25	age1825	0.0964	0.0476	2.027	0.0427	1.101
Age 26-35	age2635	-0.108	0.0530	-2.039	0.0415	0.898
Age 36-45	age3645	-0.737	0.0660	-11.17	<.001	0.479
Age 46 and up	age46pl	-1.159	0.0656	-17.68	<.001	0.314
February	dfeb	-0.0779	0.0656	-1.189	0.234	0.925
March	dmar	-0.0607	0.0610	-0.994	0.320	0.941
April	dapr	-0.330	0.0611	-5.411	<.001	0.719
May	dmay	-0.433	0.0599	-7.237	<.001	0.649
June	djun	-0.587	0.0613	-9.579	<.001	0.556
Weekend	wknddum	0.144	0.0363	3.962	<.001	1.155
3 - 6 AM	dhr0306	0.597	0.141	4.245	<.001	1.817
6 - 9 AM	dhr0609	-1.131	0.115	-9.800	<.001	0.323
9 - 12 AM	dhr0912	-0.772	0.0743	-10.40	<.001	0.462
12 - PM	dhr1215	-0.575	0.0697	-8.259	<.001	0.563
3 - 6 PM	dhr1518	-0.449	0.0715	-6.290	<.001	0.638
6 - 9 PM	dhr1821	-0.387	0.0656	-5.902	<.001	0.679
9 - 12 PM	dhr2123	-0.302	0.0670	-4.513	<.001	0.739
Vehicle stop	dvehstop	0.561	0.0767	7.311	<.001	1.752
Missing event no.	eventmis	0.108	0.155	0.702	0.483	1.114
	M1[district]	1				
	Constant	-1.245				
	District variance (se)	0.116	0.0367			
	Observations	27,051				

Note. Results from generalized multinomial structural equation model with stops nested within districts. Results only shown for one contrast: pat down and no enforcement vs. no pat down and no enforcement. Latter group was reference category.

Other multinomial contrasts were run as part of the same model, but are not shown here.

Source: Jan-June 2016 ISRs from CPD.

Seven cases dropped with discrepant scoring on any enforcement action vs. individual enforcement actions.

Table 54 Predicting pat down and no enforcement vs. no pat down and no enforcement: Sample 2

Variable	Variable name	B	SE	t	p=	OR
Black civilian	dblack	0.408	0.0695	5.879	<.001	1.504
Hispanic civilian	dhispc	0.360	0.0736	4.893	<.001	1.433
Male	dmale	1.284	0.0596	21.55	<.001	3.611
Age 18-25	age1825	0.0878	0.0475	1.848	0.0646	1.092
Age 26-35	age2635	-0.0938	0.0531	-1.765	0.0775	0.910
Age 36-45	age3645	-0.494	0.0639	-7.742	<.001	0.610
Age 46 and up	age46pl	-1.158	0.0661	-17.52	<.001	0.314
February	dfeb	-0.0576	0.0645	-0.893	0.372	0.944
March	dmar	-0.0529	0.0605	-0.873	0.383	0.948
April	dapr	-0.260	0.0602	-4.326	<.001	0.771
May	dmay	-0.395	0.0590	-6.701	<.001	0.674
June	djun	-0.474	0.0596	-7.960	<.001	0.623
Weekend	wknddum	0.0814	0.0362	2.252	0.0243	1.085
3 - 6 AM	dhr0306	0.648	0.137	4.737	<.001	1.912
6 - 9 AM	dhr0609	-1.082	0.114	-9.454	<.001	0.339
9 - 12 AM	dhr0912	-0.758	0.0748	-10.14	<.001	0.469
12 - PM	dhr1215	-0.484	0.0699	-6.920	<.001	0.616
3 - 6 PM	dhr1518	-0.330	0.0715	-4.617	<.001	0.719
6 - 9 PM	dhr1821	-0.314	0.0658	-4.772	<.001	0.731
9 - 12 PM	dhr2123	-0.167	0.0672	-2.486	0.0129	0.846
Vehicle stop	dvehstop	0.694	0.0769	9.033	<.001	2.002
Missing event no.	eventmis	-0.0140	0.157	-0.0891	0.929	0.986
	M1[district]	1				
	Constant	-1.402				
	District variance (se)	0.113	0.0360			
	Observations	27,054				

Note. Results from generalized multinomial structural equation model with stops nested within districts. Results only shown for one contrast: pat down and no enforcement vs. no pat down and no enforcement. Latter group was reference category.

Other multinomial contrasts were run as part of the same model, but are not shown here.

Source: Jan-June 2016 ISRs from CPD.

Four cases dropped with discrepant scoring on any enforcement action vs. individual enforcement actions.

### 10.6.2 Alternative models

Alternative models were run using canonical discriminant analysis.<sup>16</sup> Three orthogonal discriminant functions were generated which, collectively, sought to classify stops into one of the four groups used in this analysis. In addition to the predictors listed in the above tables, district context was controlled by adding in dummy predictors for districts 2 through 25.

Roughly, these discriminant functions, in both samples, correctly classified about 82 percent of those in the no-pat-down-no-enforcement group, and about a third of those in the pat-down-but-no-enforcement group. The multivariate F indicated the predictor variables as a set clearly distinguished between these four groups of stops ( $p < .001$  by MANOVA, details not shown).

<sup>16</sup> This is `candisc` in Stata.

More importantly, the Black variable, had a sizable standardized discriminant function coefficient on discriminant function 2 (-.23 in both samples) and, in both samples, this second discriminant function explained a sizable (27 percent in both samples ) and significant ( $p < .001$  in both samples) portion of the variation based on group membership. The Hispanic variable had a sizable (-.20) and closely comparable standardized coefficient. As in the main analytic model, gender appeared more important for this outcome.

The discriminant analysis provides less precision than the multinomial model because it cannot take account of clustering within districts like a multilevel model can, and because it is trying to discriminate all four groups at once. Nevertheless, the group mean on Black and Hispanic was clearly different between the no-pat-down-no-enforcement group and the pat-down-but-no-enforcement groups. And both the Black and Hispanic variables each had sizable standardized loadings. So the alternative analytics seem to support the main takeaway lesson from the main multinomial model: both race and ethnicity help distinguish between membership in these two groups.

## 11 DISCUSSION

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### 11.1 LIMITATIONS AND STRENGTHS

#### 11.1.1 Limitations

Numerous limitations must be kept in mind when considering the findings of this report. These include the following.

1. Analyses depended on one source of information for police behavior: investigative stop report (ISR) data compiled by the Chicago Police Department. These are administrative reports of officer behavior that have been processed by the department. Unknown at this time is how the picture painted by these data would align with other sources of information on police behavior. Policing research has a vigorous four decade tradition based on on-site assessments of police-civilian interactions (Reiss, 1971).
2. Project time constraints and other factors resulted in models leaving out additional potentially relevant covariates that could have been used and were available in the ISR data. Models with different sets of predictors have the potential to generate different patterns of statistical significance. On the other hand, we used gamma diagnostics to try and gauge how much of a difference these other factors would need to make before significant impacts disappeared.
3. Project time constraints prevented linking up the variables used here with other potentially important and relevant predictors from sources beyond ISR data. Those might include, for example, indicators either about arrests or about some classes of calls for service when those individual arrests or calls took place close in space and time to each individual stop being analyzed. In other words, a more detailed picture including additional attributes describing the context of specific stops, could have been built up given more time. From a policing perspective an argument can certainly be made that additional features of stop context related to both calls and arrests proximate in space and

time could be relevant. Again, with different predictors different patterns of statistical significance might have been observed.

4. Because there are only (exclusive of District 31) 22 police districts, this small number of geographic units argued against including district level predictors in these models for a range of technical reasons (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016). All that could be done here was to allow each police district to have its own mean score on each outcome. Including contextual predictors at the district level may have altered the impacts seen here of various stop-level predictors.
5. Project time constraints prevented including checking for spatial autocorrelation of various outcomes and, if needed, controlling for same by introducing a spatially lagged predictor as an outcome.
6. Project time constraints precluded testing additional varieties of the propensity score matching models (e.g., using Mahalanobis distance for matching).
7. Project time constraints precluded additional diagnostic assessment of the regression models. Most importantly, leverage and influence have yet to be examined.
8. Two of the outcomes examined here correlate significantly with each other. The current models and the alpha level used may be creating a slightly inflated experiment-wise alpha (Type I error) level.

### 11.1.2 Potential strengths

The above limitations should be considered in the context of several potential strengths.

1. Models examined each outcome, save the last categorical one, using at least two alternate forms of analysis. Testing links across multiple analytics provided clues to how robust any observed patterns were across modeling approaches.
2. A simple random sampling strategy divided records into two independent random samples. Doing so permitted learning whether a significant link between a predictor and an outcome, if observed, appeared in *both* random samples. If it did, that increased confidence in the durability of that link. In effect, a significant link in both samples amounts to an internal replication of a finding. More specifically, it means that the connection observed did not depend on some features of a small number of specific cases that just happened to wind up in one sample vs. another.
3. A very rough a priori statistical power analysis suggested that statistical power was ample (80 percent or better) for gauging relatively modest age and ethnicity impacts.
4. Where possible, at least some model diagnostics were completed to gauge the extent to which observed and unobserved selection were problematic.
5. For outcomes clearly involving sequential selection, appropriate selection models were used.

## 12 KEY FINDINGS

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Patterns of observed race and ethnicity net links with the outcomes, and levels of concern suggested by various diagnostics, along with implications for how to interpret, appear in Table 55.

**Pat downs.** The strongest pattern revealed by these analyses are net connections between race and whether a pat down occurred, and between ethnicity and this outcome. Both analytic approaches yielded statistically significant net connections in both samples.

Diagnostics of both types of pat down models, however, suggested a moderate level of potential concern about observed and unobserved selection biases. Stated differently, there were other things going on, correlated both with key predictors and the outcome variable, that were not handled sufficiently by the analytics. Given that, the net race and ethnicity impacts are probably best interpreted as correlational. Nonetheless, the links were there, after controlling for other factors, and for district context. As compared to White non-Hispanic civilians, Black and Hispanic civilians were more likely subjected to a pat down.

**Pat downs leading to weapons.** Previous work on pat down and search hit rates suggested that pat downs of Black and Hispanic civilians would be less likely to lead to recovered weapons. This turned out to be true when examining weapons produced from pat downs, after controlling for other factors and district context. It held for Black as compared to White civilians. Hit rates were significantly lower in both random samples in the regression analyses. The significant net race effect did not resurface using more stringent analytics, although the race effect in one sample was marginally significant. Again, diagnostics suggested some concerns. The conclusion seems to be that there is a net race effect, but it is probably correlational and was just not quite strong enough to be robust across alternate analytics.

**Searches.** The search outcome results showed no significant net race effects. But significant net ethnicity links appeared, for both samples, using the more stringent alternative analytics. Diagnostics suggested some level of concern, so the conclusion about ethnicity and the search outcome is that the link is probably correlational, but not robust across different approaches.

**Any enforcement action delivered.** The enforcement outcome yielded robust net ethnicity links across both samples and both analytic approaches. Net race links surfaced only with one analytic approach. The conclusion seems to be, in light of diagnostics, that for both race and ethnicity there is a net connection with this outcome, that for both it is probably best considered correlational, and that for race it is not robust across alternative approaches.

**Pat down and no enforcement.** The last outcome examined, contrasted two types of stops, no enforcement action and no pat down vs. no enforcement and receiving a pat down. Analyses included both a main and an alternate approach. No diagnostics of either analytic model have yet been completed.

Across both analytic approaches, significant net race and ethnicity effects surfaced. After controlling for other factors and district context, in stops where no enforcement actions were taken by police, Black and Hispanic stopped civilians had much higher odds of being patted down than did stopped White non-Hispanic civilians. Given the potentially corrosive nature of police interactions such as this, this would seem to be an important pattern to address.

These net race and ethnicity links should be considered correlational only at this time, since no diagnostics have been completed, and the patterns seen may or may not be robust across different analytic approaches.

**Gross impacts.** The above discussion concentrates on statistically significant net impacts of racial or ethnic differences. **Authors recognize that gross ethnic or racial differences represent important findings as well. That is why, as requested by the Parties experts and as agreed, we present all of these ethnoracial differences, and geographic differences, in a number of tables.** How one balances the importance of those gross differences vs. the statistically significant net differences we leave up to the individual readers.



Table 55 Summary of results patterns, and implications, for post stop outcomes

		pat down	Pat==>Weapon	Outcome Search	Enforcement	PD-No E
Result patterns						
Regression models						
	Significant net race effect observed?	Y	Y	N	Y	Y
	Significant net race effect replicated across both samples?	Y	Y	---	Y	Y
	Significant net ethnicity effect observed?	Y	N	N	Y	Y
	Significant net ethnicity effect replicated across both samples?	Y	---	---	Y	Y
Diagnostics						
	Concern level about observed selection bias	moderate	low-moderate (a)	low	low-moderate	dk
	Concern level about unobserved selection bias	moderate		low	low-moderate	dk
Alternate analytics						
	Significant net race effect observed?	Y	N (b)	N	N	Y
	Significant net race effect replicate across both samples?	Y	---	---	---	Y
	Significant net ethnicity effect observed?	Y	N	Y	Y	Y
	Significant net ethnicity effect replicated across both samples?	Y	---	Y	Y	Y
Diagnostics						
	Concern level about observed selection bias	moderate	low-moderate (a)	H: low-moderate	H: low-moderate	dk
	Concern level about unobserved selection bias	moderate		H: high	H: high	dk
Conclusion						
	Suggested interpretation of significant net race effects	correlational	CBNR	---	CBNR	correlational
	Suggested interpretation of significant net ethnicity effects	correlational		CBNR	correlational	correlational
Notes						
(a)	For this model, diagnostics did not permit discriminating between concerns about observed vs. unobserved selection bias.					
(b)	Marginally significant race impacts in sample 2 would have been statistically significant if a one tailed significance test was used.					
CBNR	correlational but not robust across different models					
PD-No E	pat down / no enforcement vs. no pat down / no enforcement					
---	not relevant because no significant net effect					
CBOUR	correlational but of unknown robustness across alternative analytics					

dk = unknown

## 13 ADDENDUM 1

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The table below organizes all stops, for the three ethn racial groups of key interest in this report, from January 1, 2016-June 30, 2016.

Stops are organized into two rows: those stops where there was no enforcement action of any kind (No Enf) and those stops where there was at least one enforcement action of any kind (Yes Enf), regardless of whether it was an arrest, a citation, an administrative action, PSC, or other.

The columns are organized into two supersets. The right hand set of columns are stops where a search took place (total = 9,595). The left hand set of columns reflect stops where no searches took place (total = 44,521).

Within each search category there are two columns, depending upon whether a pat down occurred or not. There were 14,732 pat downs when no searches took place, and 3,632 pat downs when a search also took place in the same stop. There were a total of 18,364 stops with pat downs.

The numbers in each column are broken out into two separate rows, depending on whether the stop included any enforcement action or not. In 36,691 stops no enforcement action was recorded, and in 17,425 stops some type of enforcement action was recorded.

Any Enforcement Action	No Search			Yes Search			Total
	No Pat	Yes Pat	Total	No Pat	Yes Pat	Total	
No Enf	22,611	12,414	35,025	633	1,033	1,666	36,691
Yes Enf	7,178	2,318	9,496	5,330	2,599	7,929	17,425
Total	29,789	14,732	44,521	5,963	3,632	9,595	54,116
Total pat downs						18,364	
Total searches						9,595	

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RE: ACLU Matter vs. - REF# 1340012232

## Ecological Analysis of Monthly Stop Data

January-June 2016  
For Input to  
Hon. Arlander Keys' (Ret.)  
First Period Report

### **REVISED FINAL Technical Report**

Lallen T. Johnson & Ralph B. Taylor

DATE: March 20, 2017

This is a confidential document prepared under contract for the City of Chicago, to be released only to those specifically designated by the City of Chicago, ACLU-IL, the Chicago Police Department, or the Hon. Arlander Keys (Ret.).

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## INTRODUCTION TO REVISED VERSION

Comments by the Parties and their experts on the initial version of this report led to modifications that appear in this version. The major modifications include the following.

1. Adding a less technical front end to the report, in the form of a frequently asked questions (FAQ) section, to make the report more accessible.
2. Clarifying the key question asked in these analyses.
3. Editing language throughout.
4. Responding to concerns about four different governing CPD policy periods affecting which police-civilian encounters or police actions were included in the contact card/investigatory stop report database. Key analyses were repeated for each separate policy period.
5. Correcting language in the earlier version which may have led to a mis-interpretation of findings on the part of some reviewers. More specifically, that language implied that the race and ethnicity dummy predictors were group mean centered and so captured only intra-district differences between these groups on the outcome. We did not do that. So the ethnoracial differences captured by these two predictors combine both inter- and intra-district impacts of these variables on the outcomes.
6. Further discussion of the results in terms of gross race or ethnicity impacts, net race or ethnicity impacts, and statistically significant net race or ethnicity impacts.
7. Recognizing points made by the City's experts that some of the results here may be "fragile" (their term), and acknowledging these points as potential limitations and matters to be examined in future periods of investigation.

## FOR THE NON-TECHNICAL READER: FAQ

This section asks and answers questions that the non-technical reader might have about this report. It simultaneously guides the non-technical reader to findings and interpretations that might be of most interest to him or her. Even technical readers might benefit from scanning the questions and answers listed here.

### Purpose

Q: What is the **purpose** of this report?

A: This report does three things. First, it **describes** the monthly **counts** of recorded police stops made by Chicago Police Department officers for the period January 2014 through June 2016 for all stops, and then separately for stops involving civilians of **three different ethnoracial categories**: non-Hispanic black, non-Hispanic white, and Hispanic. Differences between groups, and shifts over time are noted. This section of the report provides broader descriptive background for current discussions. These numbers are for the entire city.

Second, with a focus still on the entire city of Chicago, it **converts counts into rates** and **describes stop rates** for these **same three ethnoracial categories**. **Different types of stop rates** are created by using **different benchmark variables** to turn stop counts into stop rates. **Different benchmarking approaches** generate **different pictures** of **ethnoracial differences in**

**stop rates** city-wide. These rate differences between groups and their shifts over time provide further descriptive background.

Third, attention shifts to **monthly police-district stop rates** for the same three ethnoraical categories. **Statistical testing** procedures are applied to reveal which ethnoraical differences in stop rates are noteworthy. Noteworthy means statistically significant (see below). These statistical tests are completed before and after taking into account additional factors (see below).

### Questions

Q: Can you translate those purposes into questions?

A: Yes. First, how do the monthly counts of police stops of Black non-Hispanic, White non-Hispanic and Hispanic civilians, city wide, differ; and, how do those differences shift over time? Monthly stop counts of everyone provide part of the background context. Second, if we convert counts into rates, how do the above differences between groups shift, and do the differences between these three groups depend on *how* we convert city-wide monthly counts into monthly rates? Third, once the focus shifts to ethnoraical-specific rates at the police district level, and additional factors are taken into account, do we see noteworthy, i.e., statistically significant, differences in the stop rates between these three groups?

### Different benchmarking variables

Q: Why do you use different types of benchmarking variables to turn counts into rates?

A: First, some more background. A rate has two parts: a numerator (on top) and a denominator (on the bottom) so we can discuss how often an event (the numerator) occurs *per* some unit (the denominator). We are looking for a denominator (benchmark variable) for the numerator (the stop count) to create a rate that is x many stops *per* some unit. The benchmark variable for the denominator is also sometimes called an *exposure* variable. Because differences across ethnoraical categories are of central interest *both* the numerator and denominator need to be *specific* to the ethnoraical category being described.

There are three reasons why different types of benchmarking variables get used.

First, each benchmarking variable has its own set of problems. No one particular benchmarking variable is perfect. Scholars investigating driving stops by police and pedestrian stops by police have known about these problems for well over a decade. No one has agreed on the best way to fix these problems using available data, and it is not unusual for reports and even scholarly papers examining racial disproportionality in policing to use problematic benchmark variables.

Second, different benchmark variables create *different types of rates* that *mean different things*.

Third, because of the above – different benchmark variables problematic in different ways, different benchmark variables create rates that mean different things – different benchmark variables can *alter the ethnoraical differences observed in stop rates*. Therefore, the approach adopted here uses multiple benchmarking variables to create different types of stop rates, and reports and comments on those differences.



## Stop rates with different meanings

Q: How can different benchmark variables create rates that mean different things? Isn't the numerator, the stop count, the same?

A: Yes it is. These different meanings may become clearer if we introduce the three benchmarking variables used in these analyses.<sup>1</sup>

The first benchmarking variable, whether city wide or at the district level, was the young population, aged 15-29. The thinking was that using total population as the group exposed to being potentially stopped by police is somewhat unrealistic. Police are much less likely to encounter extremely young residents on the street or driving vehicles; similarly for extremely old residents. We also know from criminological theory that those most active are in this age range. This is the **only** benchmarking variable that is **not ethnoracial specific**. So with this benchmarking variable only the numerator is ethnoracial specific.

We used the young population in the city overall or in a district using the most recently available US Census numbers. Using this population number, and multiplying the resulting rate by 1,000 creates stop rates that mean: ***how many stops of those in an ethnoracial group did police make per 1,000 young persons "available" from ANY ethnoracial group either city-wide or in a district?***

Is this denominator better than a total population denominator? Arguably. Is it still problematic? Indeed, at the city level and even more so at the police district level. At both levels the variable is not specific to the ethnoracial group in the numerator. Therefore, the stop rate is not ethnoracial-specific.

Further, the main problem at the district level is that the volume of young people "available" to be stopped in a district depends on more than just the resident young population. Some districts, the clearest case being District 1 which includes The Loop, have many land uses and public transportation network features that draw in large numbers of outsiders.

Moreover, we don't know the exact count of people who are encountered by police and who could potentially be stopped by police as they patrol a district at different times of the day and night. No single exposure variable is going to exactly capture the quantity of civilians exposed to police and at risk of being stopped.

The second and third benchmarking variables focus on a different matter: ethnoracial-specific criminal activity as revealed through arrests. So these second and third benchmarking variables allow creating stop rates that **are ethnoracial-specific in both the numerator and the denominator**. There were two different versions of this benchmarking variable: total arrests, and violent (serious Part I crime) arrests. In each case these denominator values were from the month prior.<sup>2</sup>

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<sup>1</sup> These benchmarking variables were discussed with the City's and ACLU-IL's experts and proposed and agreed to during the second phone call between the authors and the experts.

<sup>2</sup> Using a month earlier allows police to respond, through their stop and other activities, to earlier crime concerns. Further, since the benchmarking variable can be thought of as a predictor of stop counts as

These two benchmarking variables follow a different line of thinking. With a population denominator the idea is about the resident persons “available” to be stopped. The idea with the arrest variables is that each “**indexes stop behavior to observables** about the probability of crime or guilt among different racial groups.”<sup>3</sup>

This reframes the question about ethnoracial differences in stop rates. The question becomes as follows: for each serious crime event in a district a month earlier, are police generating the same volume of stop behavior, for each ethnoracial group, a month later? Or, for some ethnoracial groups, **are police generating more stops per arrest or per violent arrest** a month later? So at the simplest level an earlier police observable (arrest or violent arrest counts) is applied to a later police observable (number of stops), separately for each ethnoracial group, as a denominator, which is a type of control variable.

### Concerns with different stop rates given the arrest benchmarks used

Q: What are the concerns with using either total arrest or violent arrest as the denominator for stops?

A: There are several, and some are more problematic at the district than the city level. First and most generally, this indexing approach makes most sense if specific types of arrests can be linked to specific types of stops. For example, violent crime arrests could be linked to later investigatory stops addressing past or suspected violent crimes. Unfortunately, the current CPD stop form does not specifically address which specific types of crime concerns led to the stop in the first place. In addition, as will be seen in the other statistical results, some stops are not investigatory but rather are probable cause stops about violations police observe such as riding a bicycle on the sidewalk. Second, some of the investigatory stops address less serious crimes such as possession of cannabis. So, given these two points right off the bat there is some degree of slippage between the types of behaviors reflected in the numerator and the denominator. Third, and particularly problematic at the district level is that extremely low numbers sometimes appear in the denominator. This is problematic for a couple of reasons.

Q: So are you saying **both** that **different stop rates using different benchmark variables mean different things** and **that all these ecological stop rates are problematic to some degree?**

A: Yes.

Q: Can you fix these problems?

A: Not now. But we will attempt to make adjustments in future analyses which will appear in future reports. For example, as suggested by the City’s experts, we may move to calendar quarters rather than monthly rates.

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well as a denominator for stop rates, it removes any potential for the outcome, the stop count, to influence the predictor, the arrest count.

<sup>3</sup> Gelman, A., Fagan, J., & Kiss, A. (2007). An Analysis of the New York City Police Department's “Stop-and-Frisk” Policy in the Context of Claims of Racial Bias. *Journal of the American Statistical Association*, 102(479), 813-823; p. 815.

Q: What are the implications of these concerns for how I think about the results?

A: Certainly, pay attention to the results. They are robust in some ways. For example the Black-white difference noted at the district level appears for three out of the four different policy periods examined. But they may be fragile in other ways. So interpret with caution.

### Focus is geographies, not individuals

Q: Is this report about individuals?

A: No. This report is about the community ecology of stops. **Monthly sets of stops** are organized either by the **entire city**, or by **police district** within the city of Chicago. If you will, it is potential ethnoracial disparities in the geography of police stops.

Q: Why do you consider two different geographies, the entire city and police districts?

A: The two different geographies are important, albeit for different reasons. Examining the overall city provides a birds' eye view. Examining differences by districts reveals how the situation can vary across the city. Both views may be important to the Parties for different reasons.

Q: What does it mean if the ethnoracial patterning suggested by the city level picture is different from the ethnoracial patterning suggested by the district level picture?

A: These discrepancies do emerge when using the violent arrest benchmark. We are trying to learn more about why. We will know more about these discrepancies in the near future as we explore further. Nonetheless, at this point, we can say two things about the discrepancies by geographic scale when using the violent arrest benchmark.

First, it is not necessarily true that one answer, the city answer or the district answer, is necessarily better than the other answer. They are just different.

Second, the differences could arise from any number of sources. For example, discrepancies could arise from the fact that the geographies themselves, and thus the associated geographic processes, are quite different from one another. What is happening *theoretically* at the city level and the district level can be quite different. Or it could arise from some features about how the chosen benchmarking variable operates differently at the city vs. the district level.

Q: Does the ethnoracial patterning revealed in the geographies of police stops apply to individual members of each ethnoracial category?

A: **Not necessarily.** Social scientists are trained to be extremely cautious when making inferences about the behaviors of individuals based on analyses of groups of individuals.<sup>4</sup> To blindly assume that a community level connection or difference applies to individuals represents a mistake in scientific reasoning.

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<sup>4</sup> For example, consider this. Suppose one were to find in a particular city that males aged 10 to 15 were more likely to become delinquent if they lived in lower income communities. This does **not** mean that Johnny, an 11-year-old boy, who lives in a low-income household, is more likely to become delinquent than an 11-year-old male neighbor living in a higher income household.

### How to interpret differences across geographies

Q: Suppose you do find significant ecological differences in stop rates by ethnoracial category after controlling for violent arrests and for community characteristics. Is that relationship necessarily telling you something about the **people in their respective communities**?

A: **Not necessarily.** Communities are affected by nearby communities. For example, police districts right outside the Loop are affected by things going on in the Loop. Further, decades of social science scholarship documents how individual communities can be adversely affected by forces originating outside of those communities.

Q: Did your analysis take into account these potential impacts of nearby communities?

A: No we did not. Time constraints did not allow controlling for these impacts of adjacency. Future analyses will take these into account.

### Different benchmark variables and differences between ethnoracial groups

Q: You said the different benchmark variables create rates that mean different things. Does the benchmark used alter the picture shown of differences across the different ethnoracial groups?

A: It does.

Examine, for example, Figure 2. Here, monthly counts for each ethnoracial category are divided by the young population, in thousands, of *the total population*. So for each month the figure is showing the number of stops per 1,000 young persons for the whole city. Note how the contrast between black non-Hispanic stops and Hispanic stops has shifted for calendar year 2014. Whereas in figure 1 the counts for the first group were about four times the second set of counts for the second group. In figure 2 stop rates for the first group are now about 2 to 2 ½ times the stop rates for the second group. So the ethnoracial disparity has shifted as an additional factor was taken into account; here that additional factor was a relative size of the young population across all ethnoracial categories.

Continue your examination by looking at Figure 3 which uses ethnoracial-specific total crime arrests as the benchmark variable. Now in most months of 2014 the White non-Hispanic stop rate is slightly higher than both the Hispanic and Black non-Hispanic stop rate.

Look further at Figure 4, which uses the ethnoracial-specific violent crime arrest benchmark. Now the White non-Hispanic stop rate is markedly higher than either the Hispanic or Black non-Hispanic stop rate for many months of 2014.

### Other variables beyond the benchmark variable

Q: Besides the benchmarking variable, do analyses take additional factors into account?

A: They do, for the district level analyses.

More specifically, fundamental demographic features of community residents are considered: their socioeconomic status, their length of time living in the community, and their racial composition.

Further, additional variables control for when the stop took place.

Q: Suppose your model had **expanded** the set of **other factors** that you took into account? Could that have changed the results shown here?

A: Yes it could. Statistically significant (see below) results shown here are **specific to the predictors used in these models**. Different models with different predictors could have resulted in a statistically significant race effect shown here in some models (Table 3 for example) becoming non-significant.

### Net Impacts

Q: What's the idea behind taking these other factors into account?

A: When all these additional factors are controlled, the remaining ethnoracial differences in stop rates, the **net ethnoracial impacts** on stop rates, capture ethnoracial effects **unrelated** to these control factors.

Q: So you are trying to isolate the portion of the outcome that is **just** due to the ethnoracial categories?

A: Yes.

Q: Did you succeed?

A: Partially. As noted above, nearby influences have not yet been removed. Further, analyses in many studies like these are able to control for differences in police deployment. We do not have police deployment variables here. And finally, we have not yet done extensive diagnostics on these models that would assure us that we have succeeded in isolating what we want.

Q: Are there any implications of the fact that you cannot be sure you have isolated just the link between ethnoracial categories and the counts?

A: Yes, most importantly, it means that the significant (see below) links between counts and ethnoracial categories should be interpreted as a **correlational rather than causal**. There is a link, but we are **not sure** the ethnoracial difference **itself** is **causing** the differences in the stop counts.

### Statistical significance when controlling for several factors

Q: After you start controlling for different factors, how do you decide whether the remaining net ethnoracial differences on the outcome are important?

A: On the one hand, importance is in the eye of the beholder. Different readers, with different backgrounds or different policy concerns may conclude that some or all or none of the descriptive differences we have just been noting are important. On the other hand, from a social science perspective, statistical tests are used to decide whether a difference is important. The logic is that if an observed net difference between two ethnoracial categories of civilians has a **statistically significant** impact on the stop rate it is important in the following way: it is unlikely to be a chance finding, that is, it is unlikely to be due just to noise in the data.

Q: Is statistical significance the same as practical significance?

A: Not necessarily. A difference might be statistically significant, that is not due to noise in the data, but be quite small in practical terms. Whether a statistically significant difference also

has practical significance depends on the outcome in question, the size of the difference in question, and other factors.

### Statistical significance and cause

Q: If race has a statistically significant impact on stop counts at the district level, like it does here, does this mean that the race of this group of citizens is *causing* the higher stop count?

A: In a social science framework, not necessarily. In social science, correlation does not always mean causation. Figuring out whether the impact might be causal, wholly or in part, requires additional social science steps not undertaken here.

### Legal standards

Q: I do not see anything in your report about legal standards like disparate impact and disparate treatment. Why not?

A: For two reasons. First, the authors are social scientists, not legal scholars. From a social science perspective, the purpose of the analysis is to gauge gross impacts of race or ethnicity, or net impacts of race or ethnicity, on stop activity, where net impacts are defined in progressively stricter ways. Second, for the outcomes in question here, the authors are not aware of a widely accepted mapping of gross or net statistical impacts onto disparate impact or disparate treatment standards. It is up to legal scholars to decide how any of these *particular* findings might cross reference with legal standards of disparate impact or disparate treatment, given the particular context under examination.

### Changes during the period examined

Q: Your analysis examines stops over an extremely long timeframe, longer than two years. During this entire timeframe, did the Chicago Police Department have the same rules about which types of stops recorded were entered into the database you analyzed? Did they use the same type of database?

A: No they did not, on both counts. In fact, there were four different data collection regimes during the period examined. A regime change might involve a change in which stops got recorded in the stop database, or the form used to record the stop. The approximate dates for data collection regime changes were:

- April 3, 2014 (approximate start of second regime)
- January 7, 2015 (approximate start of third regime)
- January 1, 2016 (approximate start of fourth regime)

### Bottom line

Q: What are the most important take away lessons from the work you have done here?

A: There are four.

First, the clearest discrepancy in stop rates is between stops of non-Hispanic White vs. non-Hispanic Black civilians.

Second, the size and direction of that discrepancy depends on both the benchmarking variable used and the geography used. For example, using the violent arrest benchmark variable at the

city level the rate appears higher for White than Black non-Hispanics, while at the district level using the same benchmark variable it is higher for Black as compared to White non-Hispanics.

Third, the district level discrepancy with significantly higher stop rates for Black as compared to White non-Hispanics using the violent arrest variable is robust in some ways but may be fragile in other ways. It is robust because it replicates across three of the four different sub-periods within the overall period examined. But it may be fragile because of low counts for the benchmarking variable. These models need further diagnoses as well as additional variables like controls for nearby stop activity, and for police stops.

Fourth, the problems associated with interpreting the ecological analyses in this study are not worse here than they are in other studies with ecological models examining potential racial and ethnic disparities in stops. The interpretative challenges seen here arise from **the nature of the inquiry** and the availability of only **crude proxy measures** to capture key dynamics and attributes. These challenges are endemic to this field of inquiry.

## PURPOSE

This report analyzes investigatory stops<sup>5</sup> conducted by the Chicago Police Department. Descriptive analyses of stop counts and stop rates focus on 30 months from January 2014 through June 2016.

It focuses on stop counts and races for three ethnoracial categories of individuals: Non-Hispanic Whites, Non-Hispanic Blacks, and Hispanic Whites. These aims are addressed using a two-step process.

First, the report provides descriptive statistics of ethnoracial-specific stop counts for the city of Chicago, and each police district for the 30-month time series. These counts are supplemented with district-level maps displaying the spatial arrangement of stop rates for select months at the beginning and the end of the overall period.

Second, the report examines the relationship between ethnoracial-specific arrest counts, in a police district, in the previous month, and ethnoracial-specific stop counts in that same district in the month following. Stated differently, for each of the three racial/ethnic groups the ratio of later stops to earlier arrests are considered. In essence this arrangement permits examining “whether stop rates ... exceed what we would predict from knowledge of the crime rates of different racial [and ethnic] groups” (Gelman, Fagan & Kiss, 2007: 815). The arrest variables are in essence benchmarking variables that also allow turning stop counts into stop rates.

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<sup>5</sup> Authors use the terms “stop” and “investigatory stop” as a shorthand referencing: records in the Chicago Police Department’s Contact Cards database during 2014-2015, and records from its Investigatory Stop Reports database for 2016. Authors recognize this term is not entirely accurate because not all these records reflect investigatory stops. Different inclusion rules obtained at different times. See below on analysis by sub-periods.



Earlier arrests are **also** ethnoracial-specific, and are considered in two different forms: total arrests, and violent (serious Part I) crime arrests. Using different arrest variables as the benchmarking variable alters the meaning of the resulting stop rate.

Of interest are whether those ratios of (later stops/earlier arrests) are different for the three groups. Stated differently and more specifically:

At the district level, are arrests earlier producing more stops later for Black non-Hispanics as compared to White non-Hispanics, and for Hispanics as compared to White non-Hispanics?

The ethnoracial links between earlier arrests and later investigatory stops are sometimes considered while controlling for changes over time and for differences in demographic community social structure across different police districts.

Models will use **only** ethnoracial-specific counts while examining ecological connections between earlier arrests and later stops. The same race and ethnicity combination appears in **both** the stop count and the arrest count. This in effect creates **ethnoracial-specific rates** when the arrest variables are used as the benchmarking variable.

Analyses with non-ethnoracial-specific population controls appear as well. Some models use just the number of young people, aged 15-29, as denominators. The latter approach assumes that, in light of criminological knowledge on the age-crime curve (Gottfredson & Hirschi, 1990), that a larger youthful population will result in more stops because this population has higher rates of criminal participation.

## Questions Addressed

Models conducted and results displayed address two questions.

1. **History.** How have things changed over time? Have the rates at which Chicago police officers have stopped members of Chicago's three most numerically predominant ethnoracial groups shifted over time? How have the total number of stops, and the relevant numbers for each of these three groups, varied across the period considered?

Only descriptive answers for the above question are sought at this time. That is, no statistical tests of specific temporal trends, either overall or for specific locations of citizen groups, are pursued. Further there are no statistical tests of the city-wide differences between these three different groups. The approach is a broad brush one for this question. That does not mean the differences across groups revealed in the city-wide picture are not important. They are.

2. **Potential ethnoracial disparities at the district level.** During the period, have stopped citizens in Chicago who belong to these three ethnoracial groups experienced different levels of police scrutiny? More specifically, is the ratio of stops for each of these three groups, relative to local criminal involvement as reflected by the number of those of the same race/ethnic group previously arrested in the same locale, higher for some groups than others? These questions are addressed at the level of the police districts. Past research (see below) suggests that ratios of stops relative to earlier arrests will be higher



for Non-Hispanic Black as compared to Non-Hispanic White citizens, and higher for Hispanics as compared to Non-Hispanic Whites.

Examinations of the above question seek to gauge not only gross or overall disparities, but net ones as well. With net impacts, the question becomes the following. Once we have set aside district averages on the outcome, removed sources of temporal variation, and removed variation arising from the fundamental demographic fabric of the community, are previous gross racial/ethnic disparities, if observed, statistically significant? <sup>6</sup> If so, how sizable are those differences?

Answers to the second question have significant limitations. Until residual analyses and extensive diagnostics are conducted, the answers obtained could be arising from any of the following: models improperly specified, model assumptions not met, selection on observed covariates, or selection on unobserved covariates. **Those additional steps have not yet been completed.** Consequently, if significant racial or ethnic disparities arise they should be seen as **provisional**, and **only correlational**, not causal, in nature.

## Relevant Background

Police differentials in the rates at which they stop members of different groups can arise from three main sources: differentials across those groups in their rates of criminal involvement; differentials across groups in their rates of exposure to patrolling officers; and differentials across groups in how police view them and act toward them.

Challenges figuring out how to control for the first two differentials create the widely recognized external benchmarking problem (Fagan, 2002; Ridgeway & MacDonald, 2010; Walker, 2001).<sup>7</sup> How do we estimate, across racial or ethnic groups, the ethnoracial-specific numbers of persons exposed to patrolling police who are engaging in the same behaviors that have the potential to draw an officer's attention (e.g., running a stop sign, drinking liquor from an open container)? And unless those first two differentials can be isolated, how can the net contribution of the third differential be estimated?

This problem has been known for some time, first pointed out by one of the leading scholars of policing in the US, Sam Walker (2001, p. 63). One immediate implication of this problem is a caution against using either census or crime data. "Resident population data and/or official crime data are not adequate as baselines" against which to compare "the racial and ethnic

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<sup>6</sup> District variation has *not* been removed from the race and ethnicity predictors. Doing that would have required district-mean centering the Black and Hispanic dummy variables. We did not do that. So the impacts seen with the Black and Hispanic dummy variables combine **both** within-district and between-district impacts of these variables. All that the mixed effects models do controlled for clustered errors within districts across months, and allow each district to have its own Empirically Bayes adjusted mean score on the **outcome**. Our language in an earlier version of the report may have misled some reviewers.

<sup>7</sup> This problem has different names: external benchmarking, the denominator problem, the base rate problem, or the baseline problem, among others.

distribution of people stopped.” Such concerns led to using baseline indicators which, albeit flawed, are arguably less flawed than either resident population data or official crime data.

More specifically, the preferred baseline indicator used here will be race and ethnic specific counts of violent crime arrests in a district in the month preceding the stop count examined.<sup>8</sup> The assumption – and it is an untested one – is that these counts serve as rough proxies for the ethnoracial-specific volume of serious criminal activity – activity which would be likely to draw police attention – in that locale in that period. A further assumption is that such activities direct police investigatory stop practices.

The approach here roughly<sup>9</sup> follows that of Gelman and colleagues (2007). In effect, ethnoracial-specific CC (contact card) and ISR (investigatory stop report) counts are standardized by the number of ethnoracial-specific violent arrests in that district in the previous month. This approach asks: are there ethnoracial differences in the extent to which earlier ethnoracial-specific serious law violating behaviors, reflected in violent arrests, generate police investigatory stops in the month following?

Each ethnoracial -specific ISR count for a month for a district is matched with the arrest count in the selected codes for the same district for the same ethnoracial group. The arrest count variable, in hundreds of arrests, in natural log form, becomes a special type of predictor, an exposure variable in a count model.

There are dummy variables indicating whether the ISR and arrest count in question reflects Black non-Hispanic stopped citizens, or Hispanics. Non-Hispanic White stopped citizens are the reference category. The b weight attached to each race/ethnic dummy predictor reflects how many more stops per 100 arrests from the month earlier that *that* group generates, compared to Non-Hispanic Whites. The b weight, when converted to an incident rate ratio (IRR) indicates by what factor the expected stop count for the Hispanic group differs from the White non-Hispanic stop count, or the factor by which the expected stop count for Black non-Hispanics differs from the expected stop count for White non-Hispanics.

This approach using ethnoracial-specific arrestees as the external benchmark has its critics (Ridgeway & MacDonald, 2010). But the criticisms of this approach may be overstated, and should not at this point, in these authors’ opinion, cause a rejection of this benchmarking approach, flawed though it may be.<sup>10</sup>

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<sup>8</sup> Another possible denominator would be the ethnoracial-specific population, or the ethnoracial-specific population age adjusted so that population age segments are weighted by the fraction of stops involving persons in the same age range. Yet another one is controlling for the number of arrestees or crime victims in a locale (Fagan, Braga, Brunson, & Pattavina, 2015).

<sup>9</sup> This approach only corresponds roughly with what Gelman et al. (2007) did for the following reason. In their research since stops were coded according to different crime types, they could match stops with arrests by crime type. Here, a crime type correspondence is not feasible.

<sup>10</sup> Ridgeway and MacDonald’s first criticism is that the arrestee benchmark “is too narrow.” “For example, the police make stops for trespassing, vandalism, suspected drug sales, and a variety of other causes. Many stop decisions might be made for minor infractions, not serious crime incidents involving

## Implications for Proposed Ecological Analyses

Given all these concerns about external benchmarking problems, *any* ecological analyses attempting to gauge ethnoracial disproportionality in stop rates should be viewed with **extreme** caution.

Second, it is likely that markedly different patterns of ethnoracial disparities could surface depending on the external benchmarking indicator used.

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violence. The group of individuals stopped by the police in most large cities, therefore, far exceeds the group comprising the arrestee population.” Although that point is true, there still might be a rough *ecological* correspondence between the arrestee benchmark and the kinds of citizen behaviors that lead to police stopping them. This seems plausible given strong ecological connections between serious crimes and disorder crimes, and between crimes and assessed incivilities generally (Taylor, 2001, Chapter 5).

Their second critique is about a potential spatial mismatch. But it is not clear at this point a) the extent to which these mismatches are spatially non-random across an entire city and thus biasing; or b) whether the mismatches are of such distances that they result in events being attributed to the wrong spatial unit when that unit is sizable, like a police district in Chicago. The spatial mismatch problem seems potentially more problematic the smaller the geographic unit used to assign location-based arrest counts to location-based stop counts. To learn more about the severity of this problem, researchers could investigate how connections between previous race-specific, crime-specific arrest rates link to later stop rates *across* a range of spatial units. The degree of mismatch suggested by Ridgeway and MacDonald (2010) should more adversely affect the connection at smaller geographic scales. In short, this idea could be empirically examined to learn how problematic it is.

Ridgeway and MacDonald’s (2010) third critique and the one they label “most problematic” is that both stops and arrests are driven by racial biases, biases whose degree may differ by district (Klinger, 1997). So “Such a benchmark could actually hide bias.” This third critique is correct as stated, but is not problematic for investigating race or ethnic differentials in ecological stop rates *within a district* unless additional assumptions are made. These additional assumptions may or may not be plausible.

Basically, this point says that at an organizational level like a district or a precinct, localized norms drive both earlier race-specific arrest rates and later race-specific stop rates. This is an implication on work about the ecology of policing (Klinger, 1997; Taniguchi, 2010). Absent an independent assessment of relevant district-level norms to control for this third factor causing such a potentially spurious correlation, there is no way to address this potential problem.

But this potentially spurious correlation *in a specific district might affect earlier ethnoracial-specific arrest rates and later ethnoracial-specific stop rates **to the same degree***. If so, *within each district* the earlier arrest rates are *not* problematic as proxy variables for race differentials in criminal activity. The degree of biased policing that may be present in a district could affect both of these variables similarly. If so, the spuriousness does not invalidate the exposure variable but rather introduces additional variation, district-to-district variation in the strength of the spurious correlation. That additional variation just adds to the variance in district-to-district variation if districts are treated as random effects. Examining race differences can be confined to within-district sources of variation by district-centering the arrest counts for each month. **That step has not yet been taken.**

In this study three types of external benchmarks are used: ethnoracial-specific counts of violent arrests, ethnoracial-specific counts of total arrests, and the non-ethnoracial-specific population between the ages of 15 and 29.

Of these different external benchmarks, the authors favor the violent arrest count for two reasons. Violent arrests, as compared to total arrests, allow for less officer discretion. Less officer discretion means a lower likelihood that police bias, **if** it were present, could simultaneously influence both arrest counts and later stop counts. In addition, **we are assuming** that investigatory stops themselves have as their highest priority disrupting potential serious crimes, and learning more about the causes of previous serious crimes. That assumption has not been directly confirmed by CPD personnel.

At the same time, the authors recognize the violent arrest ethnoracial-specific benchmarking variable is problematic analytically. This is because there are times when these numbers are quite low. In general, it is not wise to build a rate when the denominator used, which is *roughly* what the benchmarking variable is, often has very low numbers. In a future iteration of these analyses we will address this issue by building stop rates based on calendar quarters rather than months and contrasting the results.

## Methodology

Stop data were derived from the Contact Card (CC) and Investigatory Stop Report forms (ISRs) of the Chicago Police Department. Contact Cards were used to record stop data throughout 2014 and 2015 before the city switched to the current ISR form in 2016. Both sets of data were compiled to analyze the entire period January 2014 to June 2016. Stop counts were aggregated by months, within districts, by ethnoracial combination. Next, race and ethnicity-specific total arrest and violent arrest counts were matched with each month of stop data, time-lagged by one month.

Demographic data were compiled to account for the major demographic structural ways in which districts may vary. Composite variables were extracted from the 2010-2014 American Community Survey at the block group level and aggregated to districts. The process of aggregating census block group count data to spatially incongruent units such as police beats and districts is known as areal interpolation. This process entails using a geographic information system (GIS) to, for every block group, extract a value for a variable relative to each block group's contribution to a police beat and district. Area was used as the contribution. GIS is then used to cut portions of block groups that form the area of beats and districts. The proportion of area is measured within each beat and district that truncated block groups compose, and weighted values are computed. Values are then summed across truncated block groups within beats and districts to create new measures (Ratcliffe & McCullagh, 1999; Zhang & Qiu, 2011).

Following the interpolation of demographic data to districts, index measures of socioeconomic status and residential stability were computed. Socioeconomic status represents the standardized average of the following variables: percent of households with incomes less than \$20,000 (reverse factored), percent of households with incomes greater than \$50,000, natural log median home value, and natural log median household income. Residential stability is the

average of three standardized values: the percent of owner occupied households, the percent of housing units occupied by current residents before 2000, and the percent of housing units occupied by current residents before 1990. Both indices had acceptable levels of internal consistency.

All arrest counts, and violent arrest counts, are explained in other reports dedicated to each data source. These data were provided by the CPD by racial/ethnic group, and district, and month. Violent arrest counts included the arrests related to murders, aggravated assaults, and robberies. Arrest data were provided on a monthly basis for January 2014-May 2016.

APPENDIX A contains descriptive statistics for the outcome variable, the exposure variables, and all other predictors.

### Analysis

Since the dependent variable represents district-level monthly stop counts, we performed model estimation using count models.<sup>11</sup>

The nesting of stop counts over time within districts, however, calls for multilevel negative binomial modeling. The multilevel model variation adjusts estimates and error terms for within- and between-group scores, considering the likelihood that observations within districts are more likely to be similar than between-district observations (Snijders & Bosker, 1999). Failing to do so would undermine the assumption of independent error terms. All models are fitted using Stata's `menbreg` (Mixed Effects Negative Binomial Regression).

`menbreg` was used to model race and ethnicity-specific stop counts. As a type of count modeling, `menbreg` requires the use of an exposure variable to normalize observed events relative to their opportunities for occurrence. For example, one could collect data on the number of individuals diagnosed with Alzheimer's disease across Chicago neighborhoods. But, to examine relative differences across neighborhoods a researcher also needs to select an appropriate denominator to compute prevalence rates. As such, an appropriate denominator might be the number of elderly residents, considering the association of age with the disease. In modeling stop counts we have taken note of ongoing scholarly discussion regarding the use of different variables as potential denominators (see footnote 6).

As mentioned above, three different exposure variables are used for three different model series. Those exposure variables are monthly violent arrest counts for each of the three major racial/ethnic groups of interest, monthly total arrest counts for each of the three major racial/ethnic groups of interest, and young population, **regardless of race or ethnicity**, aged 15-

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<sup>11</sup> Count models such as Poisson regression are appropriate for data with a Poisson distribution (Osgood, 2000). Poisson models assume that the outcome variable has a mean and variance that are roughly equal. The condition of overdispersion occurs in instances where the variance exceeds the mean. Yet, overdispersion can be accommodated by adding an additional error term to the model function. Due to the presence of overdispersion in the data (mean = 680.14, variance = 1,142,970), negative binomial regression is appropriate to model stop counts.

29 years of age. The first two exposure variables can vary from month to month. The last one, young population, is constant within each district for the entire period.

The units of analysis is district-months or more specifically, monthly stop counts nested within police districts. In other words, each of Chicago's 22 police districts has 30 monthly observations (January 2014 – June 2016) each for Non-Hispanic Black, Non-Hispanic White, and Hispanic stops. This computes to a total of 1,980 district-month-race/ethnic-specific observations. Because our arrest denominators are time-lagged by one month, we exclude January 2014 stops from all subsequent analyses. This leaves a final n of 1,914 district months. The following models only consider stops of the three racial and ethnic groups identified in the consent agreement. Limiting analysis to these groups of interest within the specified study period leaves a total stop count of 1,295,790.

## Results

### Monthly Stop Counts and Rates

Table 1 displays monthly stop counts and rates for the city of Chicago from January 2014 to June 2016 for all races and ethnicities, Non-Hispanic Blacks, Non-Hispanic Whites, and White Hispanics. Stop rates are calculated as the ratio of the city stop counts to the ethnoracial-specific city population, multiplied by 1,000. As such stop rates can be interpreted as the number of expected race/ethnic-specific stops, normalized for every 1,000 residents of said racial or ethnic group.

A grand total of 1,371,567 stops occurred from January 2014 through June 2016.<sup>12</sup> Specifically, 716,360 took place in 2014, 600,506 in 2015, and 54,701 in the first six months of 2016. When comparing races and ethnicities across the time series, Non-Hispanic Blacks demonstrated the highest intra-year average monthly stop rate (50.37 in 2014, 41.97 in 2015, and 7.51 in 2016). The monthly intra-year stop rates for Non-Hispanic Whites, however, were the lowest of the three groups (6.61 in 2014, 5.21 in 2015, and 0.80 in 2016). Within-year stop rates of Hispanics fell above those of Non-Hispanic Whites, but below those of Non-Hispanic Blacks (22.58 in 2014, 20.05 in 2015, and 4.37 in 2016).

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<sup>12</sup> This number excludes 4,640 stops with missing district and/or date information.

Table 1: City-Level Race-Specific Stop Counts and Rates, by Population

Month and Year	Counts				Rates			
	All	Black	White	Hispanic	All	Black	White	Hispanic
Jan-14	52,069	35,797	6,119	8,974	19.07	42.03	7.03	20.38
Feb-14	59,175	40,741	6,713	10,342	21.68	47.84	7.71	23.49
Mar-14	71,069	49,425	7,590	12,543	26.03	58.03	8.72	28.49
Apr-14	60,213	43,411	5,480	10,232	22.06	50.97	6.29	23.24
May-14	63,101	46,062	5,559	10,468	23.11	54.08	6.38	23.78
Jun-14	62,424	45,216	5,628	10,601	22.87	53.09	6.46	24.08
Jul-14	63,067	45,831	5,856	10,174	23.10	53.81	6.73	23.11
Aug-14	64,345	46,760	5,961	10,592	23.57	54.90	6.85	24.06
Sep-14	58,924	42,159	5,499	10,239	21.58	49.50	6.32	23.26
Oct-14	60,802	44,730	5,382	9,645	22.27	52.52	6.18	21.91
Nov-14	54,904	40,572	5,015	8,434	20.11	47.64	5.76	19.16
Dec-14	46,267	34,070	4,283	7,076	16.95	40.00	4.92	16.07
Jan-15	60,310	43,287	5,695	10,231	22.09	50.83	6.54	23.24
Feb-15	51,521	36,004	5,186	9,333	18.87	42.27	5.96	21.20
Mar-15	66,624	47,049	6,281	11,955	24.40	55.24	7.21	27.15
Apr-15	49,936	35,900	4,266	8,875	18.29	42.15	4.90	20.16
May-15	50,249	35,529	4,404	9,375	18.41	41.72	5.06	21.29
Jun-15	45,782	31,556	4,260	9,102	16.77	37.05	4.89	20.67
Jul-15	48,609	33,672	4,734	9,304	17.81	39.54	5.44	21.13
Aug-15	49,155	34,763	4,459	9,122	18.01	40.82	5.12	20.72
Sep-15	52,788	38,509	4,496	8,833	19.34	45.22	5.16	20.06
Oct-15	54,051	40,454	4,369	8,310	19.80	47.50	5.02	18.87
Nov-15	44,695	32,923	3,696	7,216	16.37	38.66	4.24	16.39
Dec-15	26,786	19,326	2,614	4,297	9.81	22.69	3.00	9.76
Jan-16	8,726	6,207	729	1,676	3.20	7.29	0.84	3.81
Feb-16	5,969	4,050	482	1,366	2.19	4.76	0.55	3.10
Mar-16	9,117	6,083	675	2,250	3.34	7.14	0.78	5.11
Apr-16	9,641	7,027	668	1,857	3.53	8.25	0.77	4.22

May-16	10,910	7,831	770	2,202	4.00	9.19	0.88	5.00
Jun-16	10,338	7,163	874	2,206	3.79	8.41	1.00	5.01

Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, and Investigatory Stop Reports. Rates are per 1,000 residents (All rates) or per 1,000 residents of the same ethnoracial group as those stopped.

Line graphs of monthly stop counts and rates are displayed in Figures 1 and 2, respectively. Rates are either stops for all races/ethnicities per 1,000 residents of all races/ethnicities; or they are specific, in terms of both stops and population, to one of the three key ethnoracial groups. In January of 2014, the all races/ethnicities stop rate was 19.07 per 1,000 residents. A slight uptick was noted in March as the rate rose to 26.03. By December of the same year, however, the rate had fallen to roughly 17. The stop rate increased to 24.4 by March 2015, followed by decreases through June, and peaks again in October 2015 at 19.80.

The sharpest stop rate decrease of the time series was noted from October 2015 through the New Year. To some extent this paralleled decreases at the same time of year a year earlier in late 2014.

By January of 2016 the all race/ethnicities stop rate had dropped to 3.20, and reached its lowest point in the 30-month series by February at 2.19. In subsequent months, the rate increased somewhat yet hovered around 4 stops per 1,000 residents.

Turning to race and ethnicity-specific stop rates, it appears that the trend for Hispanics closely resembled that of the all stops trend with some divergence noticeable from April – September 2015. Although the pattern of the stop rate for Blacks was similar to that of the all races/ethnicities trend, which is not surprising since numerically they are the largest fraction of the total, the Black stop rate was generally about twice as high as the all races/ethnicities rate through October 2015. While all racial and ethnic groups experience declines in stops from October 2015 to February 2016, this change was most noticeable on the graph for stops of Blacks. During that period the stop rate for that group decreased from 47.5 per 1,000 to 4.76 per 1,000. Stops of Whites peaked at about 9 per 1,000 in March of 2014 before decreasing to less than 1 per 1,000 from January-May 2016.



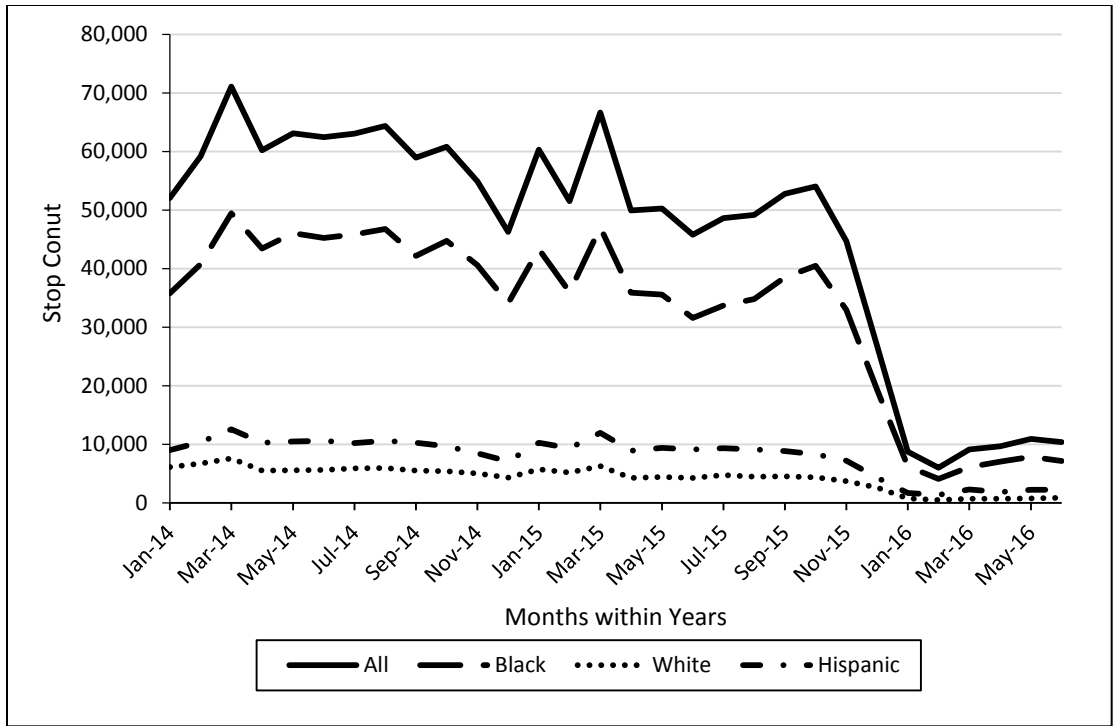


Figure 1: City-Level Stop Counts, Jan 2014 - Jun 2016

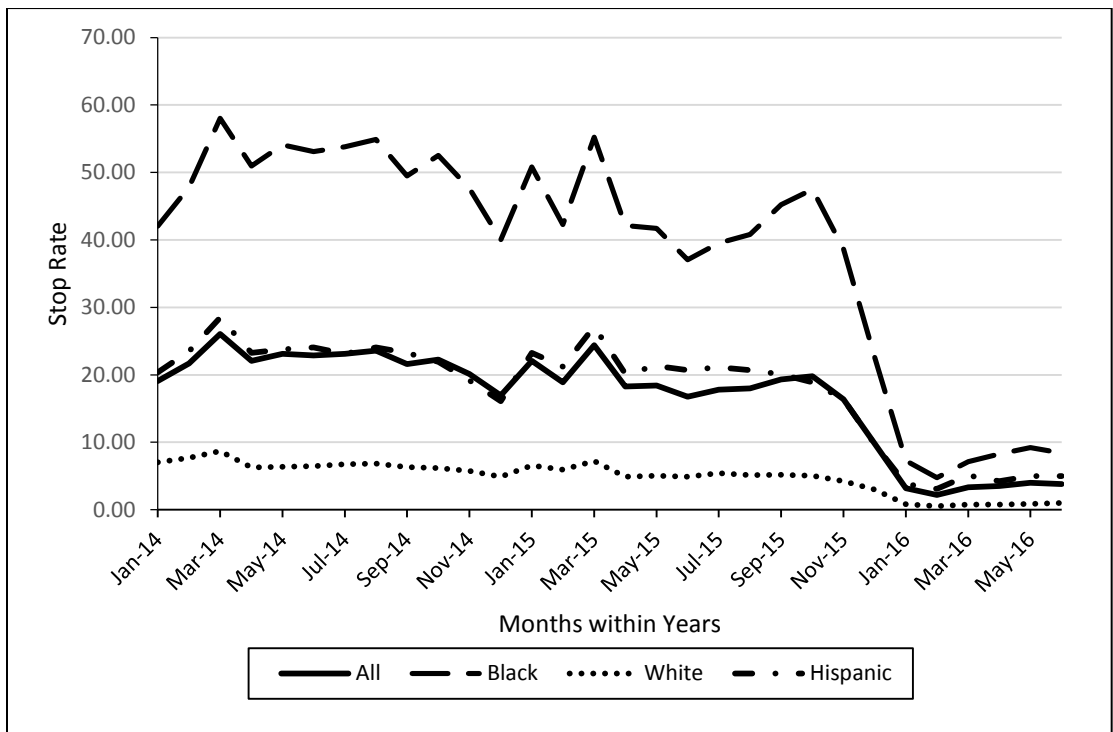


Figure 2: City-Level Stop Rates by 1,000 Population

Figure 3 displays Chicago monthly stop rates per 100 previous month's total arrests. Again, figures are shown for all races/ethnicities, and for each of the three focal racial/ethnic groups using both race/ethnic specific numerators and denominators for the three groups.

The general temporal pattern of stops in Figure 3 was similar to that of monthly stop rates computed per 1,000 residents. Different, however, was that the trend lines for each racial and ethnic group were close to convergence throughout much of the time series. **Stated differently, using different variables for external benchmarks produces strikingly different pictures of the level of ethnoracial disparities in stop rates.**

Stated differently, the factor by which stops exceeded arrests was generally consistent across the three focal racial/ethnic groups. This became increasingly evident over time and noteworthy from October 2015 onward. For example, by June 2016 the ratio of stops to total arrests was 2.5 for all races and ethnicities (10,338/4,092), 2.5 for Non-Hispanic Blacks (7,163/2,902), 2.9 for Hispanics (2,206/754), and 2.4 for Non-Hispanic Whites (874/368).

It bears mentioning that for several months in 2014 and early 2015 the Non-Hispanic Whites' rate of stops/100 total arrests appeared to be slightly above the corresponding rates for Hispanics and Non-Hispanic Black civilians.

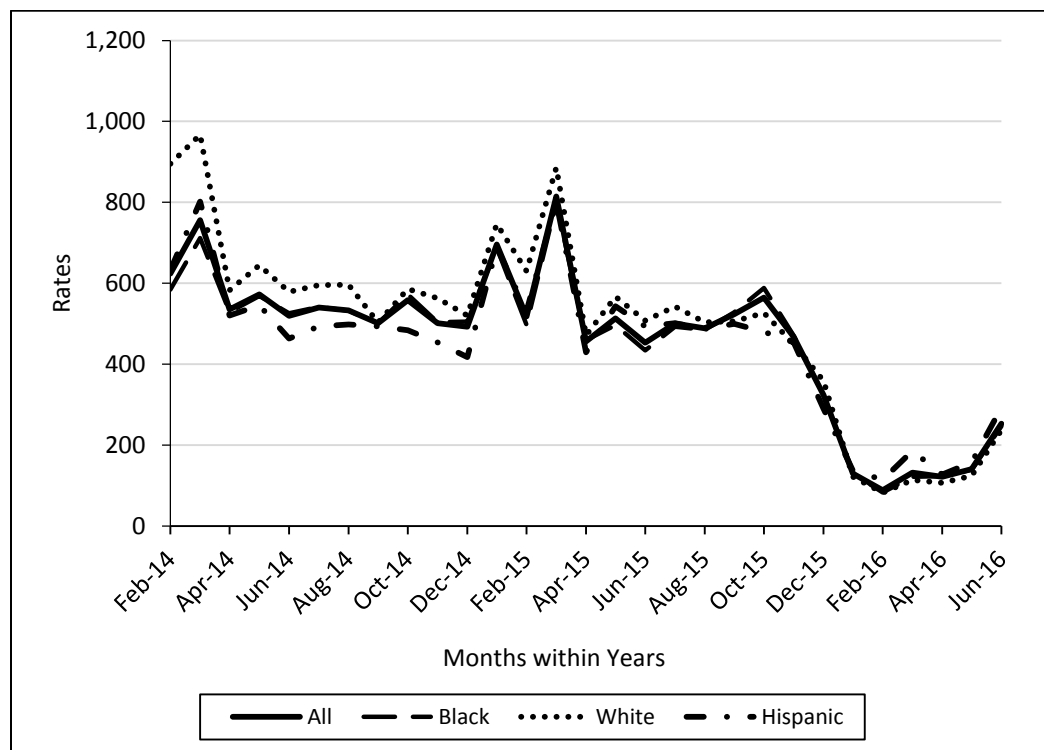


Figure 3: City-Level Stops per 100 Previous Month's Total Arrests

Using the previous month's violent arrests creates yet a third picture of group differences in stop rates. This is displayed graphically in Figure 4.

The trend lines for all races/ethnicities and Non-Hispanic Blacks followed each other closely from February 2014 through June of 2016. Yet, the Hispanic stop rates diverged upward from these two groups in March and May of 2014, from December 2014 through March 2015, and in November 2015.

More obvious are the exaggerated peaks and valleys of stops per violent arrests for Non-Hispanic Whites. City-wide, stops per 100 violent arrests for this group increased from about 27,000 in January 2015 to 74,000 the following month. By June, that rate had fallen again to about 19,000 stops per 100 violent arrests.

Figure 4 shows that the stop/previous violent arrest ratio for Non-Hispanic Whites was higher than the ratios for the other groups in mid-2014 and again in mid-2015 as well as a couple of months in late 2015.

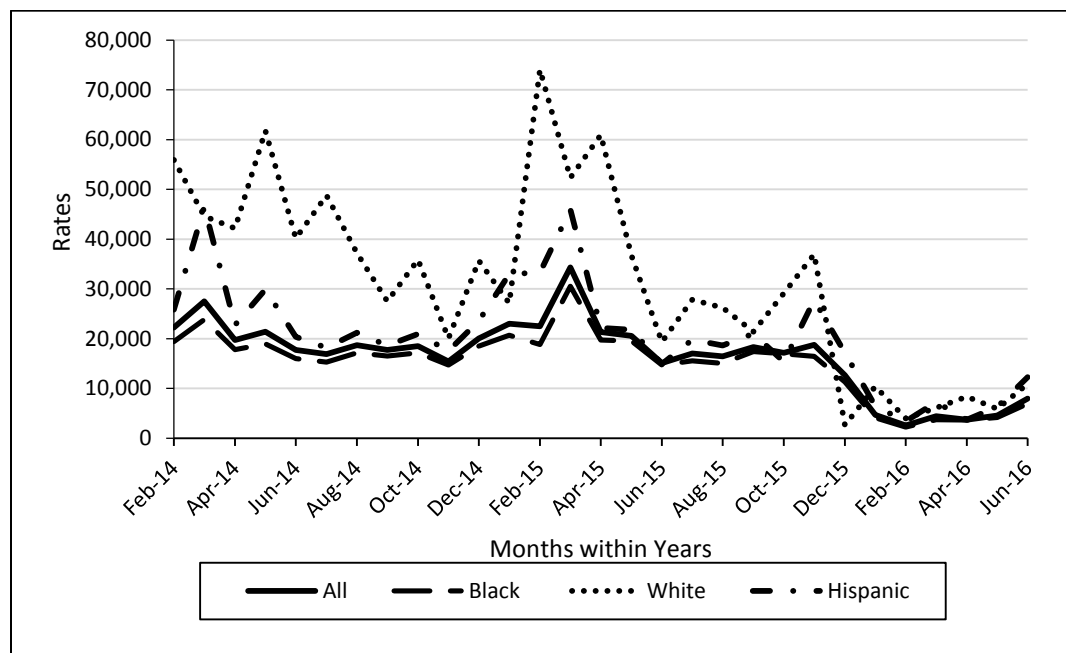


Figure 4: City-Level Stops per 100 Previous Month's Violent Arrests

This figure suggests that for many months in the period White non-Hispanic violent arrests produced more later stops than did Black non-Hispanic violent arrests. This is a descriptive difference, not a statistical conclusion. The suggestion about rate differences across these two groups, however, should be viewed cautiously for two reasons. The white rate is the most volatile of the three group-based rates, due in part – perhaps – to this group having the lowest violent arrest counts and lowest stop counts. Further, the white vs. black difference seen here at the city level will conflict with the district level picture of that same difference using the same denominator.

City-Level monthly stop rates by total and violent arrests are shown in Table 2.

Table 2: City-Level Stop Rates per 100 Previous Month's Arrests

Month and Year	Violent Arrests				Total Arrests			
	All	Black	White	Hispanic	All	Black	White	Hispanic
Feb-14	22,246.2	19,400.5	55,941.7	25,855.0	624.2	584.1	895.1	632.5
Mar-14	27,546.1	23,876.8	44,647.1	46,455.6	755.6	711.6	966.9	802.0
Apr-14	19,742.0	17,791.4	42,153.8	22,737.8	535.6	529.3	583.0	519.9
May-14	21,390.2	18,955.6	61,766.7	29,908.6	572.4	568.2	644.1	545.2
Jun-14	17,734.1	16,034.0	40,200.0	20,386.5	519.6	526.1	578.4	462.9
Jul-14	16,908.0	15,277.0	48,800.0	18,167.9	540.3	541.5	595.1	493.2
Aug-14	18,704.9	17,191.2	37,256.3	21,184.0	533.0	532.0	596.1	497.7
Sep-14	17,748.2	16,532.9	27,495.0	18,616.4	501.7	500.7	502.7	493.2
Oct-14	18,537.2	17,137.9	35,880.0	20,967.4	558.4	572.8	585.6	483.9
Nov-14	15,422.5	14,700.0	20,060.0	17,570.8	501.0	503.7	562.9	453.9
Dec-14	20,029.0	18,516.3	35,691.7	23,586.7	492.5	505.0	520.4	417.7
Jan-15	23,019.1	20,711.5	27,119.0	33,003.2	695.6	687.8	746.4	689.0
Feb-15	22,498.3	18,850.3	74,085.7	33,332.1	518.1	497.1	630.1	530.9
Mar-15	34,342.3	30,551.3	52,341.7	45,980.8	814.4	799.9	885.9	801.3
Apr-15	21,340.2	19,725.3	60,942.9	22,187.5	456.5	459.6	471.4	429.2
May-15	20,593.9	19,521.4	36,700.0	21,802.3	513.4	496.7	566.8	542.8
Jun-15	15,109.6	14,745.8	19,363.6	14,921.3	453.3	434.4	507.7	493.3
Jul-15	17,055.8	15,588.9	27,847.1	19,795.7	500.5	492.5	542.3	501.0
Aug-15	16,439.8	15,048.9	26,229.4	18,616.3	488.2	484.4	503.8	487.0
Sep-15	18,329.2	17,424.9	21,409.5	21,031.0	523.6	528.2	505.7	499.3
Oct-15	17,159.0	16,997.5	29,126.7	15,388.9	564.9	587.9	526.4	479.5
Nov-15	18,779.4	16,461.5	36,960.0	27,753.8	467.6	470.3	450.2	454.1
Dec-15	12,634.9	11,301.8	26,140.0	17,188.0	323.8	324.1	359.1	289.8
Jan-16	4,666.3	4,083.6	10,414.3	6,446.2	129.9	130.2	121.5	136.5
Feb-16	2,550.9	2,262.6	4,016.7	3,415.0	88.4	83.0	82.1	114.5
Mar-16	4,425.7	3,709.1	6,136.4	7,258.1	132.3	121.6	113.6	185.3
Apr-16	3,736.8	3,659.9	8,350.0	3,714.0	121.8	122.9	106.9	126.8
May-16	4,564.9	4,165.4	5,923.1	6,291.4	140.3	139.0	124.4	156.8
Jun-16	7,952.3	6,954.4	10,925.0	12,255.6	252.6	246.2	237.5	292.6

Sources: 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

District-level monthly stop counts and rates per 1,000 population are shown in APPENDIX B. District-level monthly stop rates per 100 previous month's violent and total arrests are shown in APPENDIX C.

### Maps of District-Level Monthly Stop Rates

Thematic maps are used to display data associated with places—in this case, police districts. Each map reveals district-level stop rates for a given month, organized by five quantiles. These

are stop rates per 1,000 population of the same ethnoracial category.<sup>13</sup> Each quantile includes roughly 20 percent of Chicago's 22 police districts, if the data permit such a separation. The lowest quantile, indicated by the lightest gray shading on each map, denotes districts with a stop rate for the specified month falling within the lowest 20 percent.<sup>14</sup> The highest quantile, indicated by the darkest shading on each map, identifies districts with stop rates falling in the highest 20 percent. The 31<sup>st</sup> district is excluded (denoted by the cross-hatched features in each map), since arrests in these areas occurred outside of the Chicago city limits. Stop rate maps are displayed for the first four months of 2014, and the first four months of 2016. For each of these months there are maps for Non-Hispanic Blacks, Non-Hispanic Whites and Hispanic Whites in Appendices E - BB.

#### Non-Hispanic Black stop rates

Generally, the highest stop rates of Non-Hispanic Blacks (indicated by the darkest shading on the maps) appear often in the 16<sup>th</sup> district, and in the districts located around The Loop and Near North (1<sup>st</sup> and 18<sup>th</sup>). Districts throughout the West Side also demonstrate stop rates in the highest quantiles, with some variability throughout the time series. Some of these districts with the highest rates include the 9<sup>th</sup>, 10<sup>th</sup>, and 11<sup>th</sup> districts; and, at times the 12<sup>th</sup> and 15<sup>th</sup> districts. On the other hand, districts with the lowest Black stop rates tend to cluster in the North Side or South Side of the city. For example, by March of 2014 the 17<sup>th</sup>, 25<sup>th</sup>, and 14<sup>th</sup> districts collectively score in the lowest quantile for Black stops relative to their population there. On the South Side these include the 6<sup>th</sup>, 22<sup>nd</sup>, 5<sup>th</sup>, and 8<sup>th</sup> districts from March-April 2016. The 2<sup>nd</sup> district also emerges with low stop rates in January 2014, and January-March 2016.

#### Hispanic Stop Rates

The lowest Hispanic stop rates are revealed in the city's northern districts. From January-April 2014 these include the 24<sup>th</sup>, 17<sup>th</sup>, 19<sup>th</sup>, and 14<sup>th</sup> districts. To a lesser extent, the 16<sup>th</sup> and 25<sup>th</sup> districts also score low on stop rates relative to other districts. Checkered throughout are a few additional districts with the lowest stop rates for this ethnic group such as 8<sup>th</sup>, 4<sup>th</sup>, 22<sup>nd</sup>, 5<sup>th</sup>, 3<sup>rd</sup>, and 2<sup>nd</sup> districts.

Elevated stop rates for this group are often found in Chicago's West Side and Near South sections. Such places almost consistently include the 15<sup>th</sup>, 11<sup>th</sup>, 7<sup>th</sup>, and 6<sup>th</sup> districts. The 9<sup>th</sup> and 10<sup>th</sup> districts also score in mid to high stop rate quantiles throughout much of first four months of 2016.

#### Non-Hispanic White Stop Rates

The ordering of district-level stop rates for Non-Hispanic Whites demonstrate more geographic consistency, at least in comparison to the rates for Non-Hispanic Blacks and Hispanics Whites. Throughout almost all of the 8 months of maps presented, the 22<sup>nd</sup>, 19<sup>th</sup>, 18<sup>th</sup>, and 14<sup>th</sup> districts remained within the lowest two quantiles of the distribution. On the other hand,

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<sup>13</sup> The denominator, ethnoracial-specific population, includes residents of all ages, not just young residents.

<sup>14</sup> These are unweighted percentiles, and population differences across districts are not taken into account. Stated more simply, these are simply telling us about the number of districts scoring above and below a particular district's rate.

districts with the highest stop rates consistently include the 11<sup>th</sup> and 15<sup>th</sup> districts in the West Side, and the 6<sup>th</sup> and 7<sup>th</sup> districts in the South Side. Spatially situated between these two highest rate subregions are the 8<sup>th</sup>, 9<sup>th</sup>, 10<sup>th</sup>, 12<sup>th</sup>, and 1<sup>st</sup> districts with rates clustering above the 20<sup>th</sup> and below the 80<sup>th</sup> percentiles.

### Inferential Models

Attention shifts now to mixed effects or multilevel negative binomial models and statistical inference. These models allow testing for the statistical significance of the Black vs. White non-Hispanic differences in stop rates, and the Hispanic vs. White non-Hispanic differences in stop rates.

Results are described using three different benchmarking or exposure variables: non-ethnoracial specific young population aged 15-29; total ethnoracial-specific arrests, and violent ethnoracial-specific arrests. The latter two were lagged (earlier) by a month relative to the stop count.

### ANOVAs

The analysis of variance (Crapanzano, Frick, Childs, & Terranova, 2011) or unconditional model with no predictors indicated that there was significant ( $p < .001$ ) between-district variation in monthly stop counts (see APPENDIX D). This underscored the need for multilevel modeling. Stated differently, district context needs to be taken into account.

This finding held regardless of the exposure variable included in the model (violent arrests IRR = 123.171,  $p < .001$ ; young population IRR=.021,  $p < .001$ ; total arrest IRR=4.882,  $p < .001$ ). In these ANOVA models the IRR represents the incidence rate ratio, or expected average count per exposure unit, across all three focal racial/ethnic groups, over the entire period, in an average district. More specifically, we could say the following after the data adjustments made by the statistical model:<sup>15</sup>

- In a typical district, in a typical month during the period, across all three focal racial/ethnic groups, on average, there were about 123 stops per violent arrest in that district the previous month;
- In a typical district, in a typical month during the period, across all three focal racial/ethnic groups, on average, there were about .02 stops per person aged 15-29; and
- In a typical district, in a typical month during the period, across all three focal racial/ethnic groups, on average, there were about 5 stops per arrest – of any kind -- in that district the previous month.

### Model Series with Violent Arrests as Exposure Variable

The first model series reported used race/ethnic specific violent arrest counts as the exposure variable. This effectively transformed stop counts into rates of stops/violent arrest the month previous.

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<sup>15</sup> The Empirical Bayes adjustments to the data adjust data properties in specific cells based on overall data properties.

Table 3 displays results that regress stop counts on race (Black vs. White non-Hispanic) and ethnicity (Hispanic vs. White non-Hispanic) indicators, while controlling for relevant measures of community demographic structure. Model A used two dummy predictors to examine the extent to which Non-Hispanic Black and Hispanic White stop counts differed from Non-Hispanic White counts. The latter racial/ethnic group is the reference category of the model against which the two other groups are benchmarked. IRRs (incidence rate ratios) for each predictor indicate the factor by which expected stop counts are predicted to change when that predictor changes by one unit. Since the Hispanic and the Black variables are coded 0/1, the IRRs for these variables tell us by how much the expected count for each of these groups will be different compared to the Whites, after controlling for district and whatever other factors appear in the model.

Model A indicates that Non-Hispanic Black stop counts per violent arrest are expected to exceed Non-Hispanic White counts per arrest across district-months by approximately 28 percent (IRR=1.283). This finding is statistically significant—surpassing the odds of mere chance ( $p < .001$ ). Black Non-Hispanic violent arrests produce a higher number of Black Non-Hispanic stops the next month than is true for Non-Hispanic White violent arrests and later stops.

On the other hand, Hispanic White stop counts per violent arrest are predicted to be lower than those of Non-Hispanic Whites. The Hispanic IRR of 0.898 indicated that that group's *expected* stop counts are generally 10 percent lower than Non-Hispanic Whites' expected stop counts across all districts during the study period. This finding is also statistically significant ( $p < .05$ ). Model A, however, does not control for temporal variation.

Model B incorporated time effects by way of two measures. The first measure—Time (Linear)—is a centered numeric linear sequence variable representing each of the 29 months in the time series. The addition of this measure will determine if 1.) there is a net linear shift in expected counts across the period and 2.) if race and ethnicity effects remain when considering the linear influence of time. The second measure—Time (Curvilinear)—is a squared version of the above measure. It accounts for the possibility that the rate at which monthly district stops are changing could vary at different points in the period.

For every one-unit increase in the linear time trend, every additional month, expected stop counts for all three groups are predicted to decrease by almost 6 percent (IRR=0.944,  $p < .001$ ). This negative effect is consistent with what would be expected when reviewing Figure 4. The curvilinear effect of time also was significant and negative (IRR=.996,  $p < .001$ ). The specific metric of the curvilinear effect defies easy interpretation, but the overall message is clear. The negative curvilinear impact of time, combined with a negative linear impact of time, means that stops/violent arrest rates are declining *faster later* in the period. This confirms the impression from the earlier graphs.

Most important, however, is that the addition of time altered the IRRs predicting Non-Hispanic Black and Hispanic White stop counts, relative to Non-Hispanic White counts. After controlling for time, stops of Hispanics are predicted to be 7 percent lower than those of Non-Hispanic Whites (IRR=0.931), but that difference is no longer statistically significant ( $p > .05$ ). The race effect for Non-Hispanic Blacks, however, remains statistically significant and perhaps slightly

increased in size relative to Model A. In this model, controlling for time and ethnicity, expected stop counts of Non-Hispanic Blacks are 37 percent greater than those of Non-Hispanic Whites (IRR=1.37,  $p<.001$ ).

Model C of Table 3 controls for community demographic structure, adding in residential stability, socioeconomic status, and percent Non-Hispanic Black.<sup>16</sup> Controlling for time, ethnicity, and demographic structure, the race effect remains. Investigatory stops of Non-Hispanic Blacks are predicted to be about 38 percent greater than those of Non-Hispanic Whites (IRR=1.379,  $p<.001$ ). Residential stability (IRR=.822,  $p>.05$ ) and socioeconomic status (IRR=.813,  $p>.05$ ) appear statistically irrelevant to predicting stop counts. The same holds true for racial composition (Percent Black IRR=.34,  $p>.05$ ).

Models D and E of Table 3 consider more robust controls for time effects by substituting two dummy indicators for 2015 and 2016, and eleven monthly dummy indicators. The year 2014 in the month of February is the reference period.<sup>17</sup>

Model D demonstrates significant race effects. Stop counts of Non-Hispanic Blacks are predicted to be 39 percent higher than those of Whites (IRR=1.394,  $p<.001$ ). Ethnicity remains statistically non-significant (IRR=.940,  $p>.05$ ).

That said, monthly and yearly time measurements do add nuance to the understanding of predicted stop counts. The 2015 and 2016 dummies indicate significant decreases in stops performed by the Chicago Police Department, compared to February, 2014, the reference month and year. In fact, stops decrease by 16 percent in 2015 (IRR=.837,  $p<.001$ ) and 84 percent in 2016 (IRR=.159,  $p<.001$ ) relative to the reference period. Moreover, while stops are generally greater in March (IRR=1.236,  $p<.01$ ), they tend to be fewer (on a monthly basis) from July through December, compared to February 2014. Adding the structural correlates of Model E does not appreciably alter that temporal effect pattern.

District racial composition does emerge as a significant predictor of stops, however. For every 1-unit increase in the percentage of Non-Hispanic Black residents, investigatory stops are predicted to decrease by 67 percent (IRR=.329,  $p<.001$ ). We refrain from interpreting this substantively given modeling concerns (see fn. 10).

#### *Sensitivity analysis: Low violent arrest counts*

For the violent arrest counts, and total arrest counts, 1 is added before it was entered as an exposure variable and the menbreg program used it in natural log form. Due to an abundance of zero values on monthly ethnoracial specific violent arrest counts, we conducted a sensitivity analysis to consider if findings are robust when excluding district-months with less than three arrests. Comparisons focus on Model E and are shown in Table 4. Limiting analysis to district-

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<sup>16</sup> Adding in district-level predictors with only 22 districts is potentially problematic from a modeling perspective (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016). The interpretations of significant district-level factors are presented with that limitation in mind. But introducing these factors does at least begin to control, albeit perhaps imperfectly, for district features.

<sup>17</sup> February was chosen for the reference category since it is the earliest month available for which stop counts are available across all three years of the study (2014, 2015, 2016).



months with three or more violent arrests for a racial/ethnic group, as opposed to the full sample, results in a somewhat larger predicted Non-Hispanic Black stop count relative to the White Non-Hispanic stop count (IRR=1.540 vs. 1.398). This restriction also associates the ethnicity difference significantly with stop counts (IRR=1.665). So sensitivity analyses reveal that ethnicity effects only emerged when excluding low count (less than 3) district-months. Excluding such district-months results in a loss of 62 percent of cases from the full model. Stated differently, 62 percent of district months have fewer than 3 arrests of any given racial or ethnic group. Table 4 also shows that size of racial/ethnic impacts were perhaps dependent upon the arrest threshold set for inclusion in the models. As the minimum number of violent arrests increases, the predicted stop counts of Non-Hispanic Blacks and Hispanic Whites increased relative to their Non-Hispanic White counterparts. As will be pointed out in the limitations section, the interpretation of these robustness tests is not completely clear.

Table 3: Predicting Stop Counts using Violent Arrests as Exposure Measure

	Model A				Model B				Model C			
	b	SE	IRR		b	SE	IRR		b	SE	IRR	
Intercept	4.756	0.097	116.224	***	4.889	0.098	132.776	***	4.889	0.075	132.789	***
Black	0.250	0.051	1.283	***	0.317	0.043	1.373	***	0.321	0.043	1.379	***
Hispanic	-0.108	0.049	0.898	* <sup>2</sup>	-0.071	0.042	0.931		-0.074	0.042	0.929	
Time (Linear)					-0.058	0.002	0.944	***	-0.058	0.002	0.944	***
Time (Curvilinear)					-0.004	0.000	0.996	***	-0.004	0.000	0.996	***
Stability									-0.197	0.252	0.822	
SES									-0.206	0.135	0.813	
Percent Black									-1.081	0.293	0.339	
2015												
2016												
January												
March												
April												
May												
June												
July												
August												
September												
October												
November												
December												
Ln(Violent Arrest Count)	1.000				1.000				1.000			
Ln(Alpha)	-0.329	0.030		***	-0.678	0.031		***	-0.678	0.031		***
Level 2 Variance	0.180	0.057			0.186	0.058			0.098	0.031		
Likelihood Ratio $\chi^2$	316.670			***	463.780			***	305.810			***
AIC	25,947.150				25,193.170				25,185.660			
BIC	25,974.880				25,232.010				25,241.140			

Notes: N=1,896 district-months. \*<sup>1</sup>p<0.05, \*<sup>2</sup>p<0.01, \*\*\*p<0.001. IRR=Incidence Rate Ratio. Time measures, Stability, SES, and Percent Black are centered. Exposure measures race/ethnicity specific violent arrest count lagged by 1 month. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and Arrest Data.

Table 3, continued: Predicting Stop Counts using Violent Arrests as Exposure Measure

	Model D				Model E			
	b	SE	IRR		b	SE	IRR	
Intercept	5.066	0.108	158.477	***	5.065	0.087	158.403	***
Black	0.332	0.040	1.394	***	0.335	0.040	1.398	***
Hispanic	-0.062	0.039	0.940		-0.064	0.039	0.938	
Time (Linear)								
Time (Curvilinear)								
Stability					-0.185	0.250	0.831	
SES					-0.217	0.134	0.805	
Percent Black					-1.112	0.291	0.329	***
2015	-0.178	0.035	0.837	***	-0.177	0.035	0.837	***
2016	-1.840	0.048	0.159	***	-1.841	0.048	0.159	***
January	0.080	0.077	1.083		0.080	0.077	1.084	
March	0.212	0.067	1.236	**	0.211	0.067	1.235	**
April	-0.055	0.068	0.946		-0.055	0.068	0.947	
May	0.041	0.068	1.042		0.041	0.068	1.042	
June	0.013	0.068	1.013		0.014	0.068	1.014	
July	-0.153	0.075	0.858	*	-0.153	0.075	0.858	*
August	-0.167	0.076	0.846	*	-0.167	0.076	0.846	*
September	-0.207	0.075	0.813	**	-0.206	0.075	0.814	**
October	-0.229	0.076	0.796	**	-0.228	0.076	0.796	**
November	-0.240	0.076	0.786	***	-0.240	0.076	0.787	**
December	-0.376	0.076	0.687	***	-0.375	0.076	0.687	***
Ln(Violent Arrest Count)	1.000				1.000			
Ln(Alpha)	-0.828			***	-0.828			***
Level of Variance	0.189				0.097			
Likelihood Ratio	539.700			***	352.410			***
AIC	24,898.870				24,890.800			
BIC	24,998.730				25,007.300			

Notes: N=1,896 district-months. \*p<0.05, \*\*p<0.01, \*\*\*p<0.001. IRR=Incidence Rate ratio. Time measures, Stability, SES, and Percent Black are centered. Exposure measure is race/ethnicity specific violent arrest count lagged by 1 month. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and Arrest Data.

Table 4: Sensitivity Analysis using Violent Arrests

	Model E - Violent Arrests		
	Black	Hispanic	N
All available records			
IRR	1.398	0.938	1,896
Significant?	Y	N	
Min: 3 violent arrests/district month			
IRR	1.540	1.665	725
Significant?	Y	Y	
Min: 4 violent arrests/district month			
IRR	1.522	1.695	611
Significant?	Y	Y	
Min: 5 violent arrests/district month			
IRR	1.718	1.918	524
Significant?	Y	Y	

Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

### Young Population

Table 5 models stop counts using the total population aged 15-29 years as an exposure variable. This exposure measure is not ethnoracial-specific. Model A indicated that the expected count for Non-Hispanic Black stops exceeded that for Non-Hispanic White stops by factor of 9.5 or 850 percent (IRR=9.479,  $p<.001$ ). Ethnicity effects were evident as well. Hispanic White stops exceeded Non-Hispanic White stops by approximately 42 percent (IRR=1.415,  $p<.001$ ). Both effects remain, even when controlling for time and social structure. Different from the violent arrest denominator models, however, is the significant socioeconomic status effect. For every 1-unit increase in socioeconomic status, stop counts are predicted to decrease by 41 percent (Model C IRR=.590,  $p<.001$ ).<sup>18</sup>

Model E introduces yearly and monthly dummy measures in lieu of the temporal linear and curvilinear trends, and demographics.<sup>19</sup> Similar to parallel Model E in Table 3, there is evidence of fewer stops conducted in 2015 and 2016 relative to 2014. And, month effects are significant during only portions of the time series. Racial composition remains relevant for variation in stop counts. Yet, a socioeconomic status effect emerges. For every 1-unit increase in the district socioeconomic status measure, predicted stop counts decreased 42 percent.

<sup>18</sup> But see fn. 10.

<sup>19</sup> Variance inflation factor (VIF) value of 4.33 suggests some evidence of multicollinearity in Model E.

Table 5: Predicting Stop Counts using Young Population as Exposure Measure

	Model A				Model B				Model C			
	b	SE	IRR		b	SE	IRR		b	SE	IRR	
Intercept	-5.047	0.099	0.006	***	-4.866	0.101	0.008	***	-4.853	0.069	0.008	***
Black	2.249	0.068	9.479	***	2.280	0.060	9.772	***	2.256	0.060	9.545	***
Hispanic	0.347	0.062	1.415	***	0.391	0.054	1.479	***	0.377	0.054	1.458	***
Time (Linear)					-0.065	0.002	0.937	***	-0.065	0.002	0.937	***
Time (Curvilinear)					-0.005	0.000	0.995	***	-0.005	0.000	0.995	***
Stability									-0.178	0.212	0.837	
SES									-0.527	0.114	0.590	***
Percent Black									-0.478	0.252	0.620	
2015												
2016												
January												
March												
April												
May												
June												
July												
August												
September												
October												
November												
December												
Ln(Population Aged 15-29)	1.000				1.000				1.000			
Ln(Alpha)	0.035	0.029			-0.243	0.030		***	-0.243	0.030		***
Level Variance	0.173	0.056			0.183	0.058			0.065	0.022		
Likelihood Ratio	255.050			***	358.400			***	120.350			***
AIC	27,020.810				26,368.380				26,353.250			
BIC	27,048.600				26,407.280				26,408.820			

Notes: N=1,914 district-months. \*p<0.05, \*\*p<0.01, \*\*\*p<0.001. IRR=Incidence Rate Ratio. Time measures, Stability, SES, and Percent Black are centered. Exposure measure is Population Aged 15-29 years. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and Arrest Data.

Table 5, continued: Predicting Stop Counts using Young Population as Exposure Measure

	Model D				Model E			
	b	SE	IRR		b	SE	IRR	
Intercept	-4.799	0.117	0.008	***	-4.788	0.090	0.008	***
Black	2.287	0.057	9.844	***	2.266	0.057	9.640	***
Hispanic	0.404	0.052	1.498	***	0.391	0.051	1.478	***
Time (Linear)								
Time (Curvilinear)								
Stability					-0.159	0.212	0.853	
SES					-0.542	0.114	0.582	***
Percent Black					-0.517	0.251	0.596	*
2015	-0.258	0.044	0.773	***	-0.259	0.044	0.772	***
2016	-2.045	0.059	0.129	***	-2.046	0.059	0.129	***
January	0.167	0.097	1.182		0.168	0.097	1.183	
March	0.270	0.085	1.309	**	0.270	0.085	1.310	**
April	0.072	0.085	1.075		0.073	0.085	1.075	
May	0.137	0.085	1.146		0.136	0.085	1.146	
June	0.117	0.086	1.124		0.118	0.085	1.125	
July	0.058	0.096	1.060		0.059	0.096	1.061	
August	0.046	0.096	1.047		0.047	0.096	1.048	
September	0.025	0.096	1.026		0.026	0.096	1.026	
October	0.016	0.096	1.017		0.017	0.096	1.017	
November	-0.107	0.096	0.899		-0.107	0.096	0.899	
December	-0.414	0.096	0.661	***	-0.413	0.096	0.662	***
Ln(Population Aged 15-29)	1.000				1.000			
Ln(Alpha)	-0.343	0.030		***	-0.343	0.030		***
Level Variance	0.185	0.058			0.065	0.022		
Likelihood Ratio	396.830			***	134.950			***
AIC	26,160.850				26,145.440			
BIC	26,260.870				26,262.140			

Notes: N=1,914 district-months. \* p<0.05, \*\* p<0.01, \*\*\* p<0.001. IRR=Incidence Rate ratio. Time measures, Stability, SES, and Percent Black are centered. Exposure measures=Population Aged 15-29 years. Model E VIF=4.33. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and Arrest Data.

It is noteworthy that the size of the discrepancy between Black and Non-Hispanic White stop counts shifts markedly depending on whether an ethnoracial specific and crime relevant indicator is used.

#### Total Arrests

Parallel models were run using the race-specific *total* arrest count as the exposure variable. So here again, the exposure variable is ethnoracial specific. Table 6, Model A, which introduces the race and ethnicity main effects yields estimates and significance values which are contrary to both the violent arrest and young population models. For example, while the prior sets of models predict that Non-Hispanic Black stops are greater in number than Non-Hispanic white stops, the current model predicts them to be 18 percent less (IRR=.817,  $p<.001$ ). Moreover, the effect of ethnicity is now negative and statistically significant (Hispanic IRR=.893,  $p<.001$ ). These findings persist while controlling for temporal patterns and district social structure (Models B and C).

Table 6: Predicting Stop Counts using Total Arrests as Exposure Measure

	Model A				Model B				Model C			
	b	SE	IRR		b	SE	IRR		b	SE	IRR	
Intercept	1.688	0.067	5.409	***	1.819	0.067	6.167	***	1.819	0.061	6.167	***
Black	-0.203	0.035	0.817	***	-0.171	0.026	0.842	***	-0.172	0.026	0.842	***
Hispanic	-0.113	0.035	0.893	***	-0.090	0.027	0.914	***	-0.090	0.027	0.914	***
Time (Linear)					-0.048	0.001	0.953	***	-0.048	0.001	0.953	***
Time (Curvilinear)					-0.003	0.000	0.997	***	-0.003	0.000	0.997	***
Stability									-0.288	0.211	0.749	
SES									-0.080	0.113	0.923	
Percent Black									0.055	0.244	1.057	
2015												
2016												
January												
March												
April												
May												
June												
July												
August												
September												
October												
November												
December												
Ln(Total Arrest Count)	1.000				1.000				1.000			
Ln(Alpha)	-0.962	0.032		***	-1.571	0.034		***	-1.571	0.034		***
Level Variance	0.084	0.027			0.088	0.027			0.070	0.022		
Likelihood Ratio $\chi^2$	312.740			***	567.050			***	452.000			***
AIC	24,828.000				23,681.590				23,682.880			
BIC	24,855.780				23,720.490				23,738.450			

Notes: N=1,914 district-months. † p<0.05, \* p<0.01, \*\*\* p<0.001. IRR=Incidence Rate Ratio. Time measures, Stability, SES, and Percent Black are centered. Exposure measure is race/ethnicity specific total arrest count lagged by 1 month. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and Arrest Data.



Table 6, continued: Predicting Stop counts using Total Arrests as Exposure Measure

	Model D				Model E			
	b	SE	IRR		b	SE	IRR	
Intercept	1.909	0.070	6.744	***	1.909	0.065	6.745	***
Black	-0.162	0.021	0.850	***	-0.162	0.021	0.850	***
Hispanic	-0.085	0.021	0.918	***	-0.086	0.021	0.918	***
Time (Linear)								
Time (Curvilinear)								
Stability					-0.271	0.210	0.763	
SES					-0.094	0.112	0.911	
Percent Black					0.023	0.243	1.024	
2015	-0.134	0.019	0.874	***	-0.134	0.019	0.874	***
2016	-1.563	0.027	0.209	***	-1.563	0.027	0.209	***
January	0.159	0.043	1.173	***	0.159	0.043	1.173	***
March	0.295	0.038	1.344	***	0.296	0.038	1.344	***
April	-0.101	0.038	0.904	**	-0.101	0.038	0.904	**
May	0.026	0.038	1.026		0.026	0.038	1.026	
June	0.177	0.039	1.194	***	0.178	0.039	1.194	***
July	-0.058	0.042	0.944		-0.058	0.042	0.944	
August	-0.106	0.042	0.899	*	-0.106	0.042	0.899	*
September	-0.118	0.042	0.888	**	-0.119	0.042	0.888	**
October	-0.019	0.042	0.981		-0.019	0.042	0.981	
November	-0.130	0.043	0.878	**	-0.131	0.043	0.878	**
December	-0.353	0.043	0.703	***	-0.353	0.043	0.703	***
Ln(Total Arrest Count)	1.000				1.000			
Ln(Alpha)	-2.035	0.036		***	-2.035	0.036		***
Level Variance	0.087	0.027			0.070	0.022		
Likelihood Ratio	825.420			***	673.720			***
AIC	22,884.020				22,885.370			
BIC	22,984.040				23,002.060			

Notes: N=1,914 district-months. \* p<0.05, \*\* p<0.01, \*\*\* p<0.001. IRR=Incidence Rate Ratio. Time measures, Stability, SES, and Percent Black are centered. Exposure measures is race/ethnicity specific total arrest count lagged by 1 month. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

Additional Models D and E which substitute annual and monthly dummy variables provide greater detail of temporal effects, but do not alter the race and ethnicity main effects, or structural effects described thus far. Significance and effect sizes of Model E are robust and remain even when excluding district months with less than 5, 10, or 15 total arrests for any given racial or ethnic group (see Table 7).

Table 7: Sensitivity Analysis using Total Arrests

	Model E - Total Arrests		
	Black	Hispanic	N
All available records			
IRR	0.850	0.918	1,914
Significant?	Y	Y	
Min: 5 Total arrests/district month			
IRR	0.876	0.894	1,794
Significant?	Y	Y	
Min: 10 Total arrests/district month			
IRR	0.893	0.914	1,632
Significant?	Y	Y	
Min: 15 Total arrests/district month			
IRR	0.866	0.914	1,550
Significant?	Y	Y	

Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

So, again, it is noteworthy that shifting from an ethnoracial specific denominator, the exposure variable, in essence provides markedly different pictures of the differentials in stop rates across these three groups.

#### Robustness Tests Across Different Data Collection Regimes or Sub-Periods

The above analyses model ethnoracial-specific stop counts using data that are pooled across the entire 29-month study period. Another type of robustness test that can be applied to these models looks at findings in particular time frames *within* this 29 month period. Examination by sub-period seems warranted because there were four different CPD policies about which records to include at different times. The approximate dates for these four distinct data collection regimes or sub-periods for ISRs were:

- A. January 1, 2014 to March 30, 2014 <sup>20</sup>
- B. April 1, 2014 to December 31, 2014<sup>21</sup>
- C. January 1, 2015 to December 31, 2015 <sup>22</sup>
- D. January 1, 2016 to June 30, 2016

A regime change might involve a change in which stops got recorded in the stop database, or the form used to record the stop.

Given the different policy approaches inherent to each period, it is possible not only that the mix of stops varies by period, but so too the racial and ethnic discrepancies observed earlier. Stated differently, the race or ethnicity impacts seen for the entire period may or may not apply to each different sub-period in part because the mix of records varies by sub-period. Therefore, we investigate the robustness of findings shown thus far by running regime-specific models for each denominator. Regime-specific analyses are only described for Models E, which include race and ethnicity, social structure, and time (monthly) effects. Noteworthy examples of model agreement and departure are highlighted. Results are summarized in Table 8.

This testing by sub-period is done for three different types of models: those using violent arrests as the denominator, and those using young population as the denominator, and those using total arrests as the denominator.

#### *Violent Arrests*

Consistent with Table 3, Model E, the effect of race is associated with stop counts for the latter three time regimes in the expected direction. Higher counts of Black non-Hispanic violent arrests a month earlier in a district link significantly with higher numbers of stops a month later of members of those same groups, compared to White non-Hispanics, in regimes B, C and D. The effect also is positive in regime A, but just not statistically significant. Bear in mind that although regime A includes three months, because of the time lagging only two months are analyzed. This makes for a low number of observations relative to the other two regimes. So the race effect is consistent in direction throughout all four regimes, and reaches statistical significance in the last three of four sub-periods.

Ethnicity is found to be statistically irrelevant the pooled data model. But the analysis by regime shows varying effects depending on the time in question. The Hispanic-white difference demonstrates a significant negative effect from April-December 2014 (IRR=.867,  $p<.05$ ), and a positive effect from January to June 2016 (IRR=1.251,  $p<.05$ ). The varying Hispanic-white impact could arise from the different mix of records over the four regimes, or from something else.

A further point of departure is that higher SES districts demonstrate fewer investigatory stops, but only during regime D, from January to June 2016.

There are two main “take away” lessons from this examination by sub-period. First, the race impact seen in the initial models generally replicate. The race impact is positive throughout,

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<sup>20</sup> March 30 substituted for April 3

<sup>21</sup> April 1 substituted for April 4

<sup>22</sup> January 1 substituted for January 7

and statistically significant in three of the four regimes, failing to reach significance only for the shortest duration sub-period. Second, the ethnicity impact seems to depend on the sub-period inspected.

### *Young Population*

When young population is used as the denominator, the race difference between Blacks and Whites proves positive and statistically significant for all sub-periods, just as in the model with the entire period. The race effect holds, regardless of the policies in place about whom to include in ISRs.

Turning to ethnicity, contrary to the pooled data Model E in Table 5, Hispanic Whites are no more likely to be stopped by the Chicago Police Department than Non-Hispanic Whites for the January to March 2014 period. Recall that this is the sub-period including the fewest months. But for all other regimes, the ethnicity impact is consistent with the overall finding. Hispanic Whites are predicted have stop counts that significantly surpass Non-Hispanic Whites for regimes B, C and D.

Turning to district attributes, district racial composition only proves relevant in the January to June 2016 model where stops are significantly higher in less predominantly Black districts. This is what was found in the full model. Impacts of racial composition were in the same direction for all four sub-periods, but significant only for the last one.

### *Total Arrests*

In line with the pooled data Model E in Table 6, race and ethnicity are significantly related to stop counts across all four regimes.

The direction of the effect, however, switches in the January to June 2016 model. For the entire period, and for the first three sub-periods, the link is negative: more arrests the month before, fewer non-Hispanic Blacks, or fewer Hispanic whites were stopped a month later, relative to the number of non-Hispanic Whites stopped.

But from January-June 2016 Non-Hispanic Blacks and Hispanic Whites are expected to have higher stop counts per arrest across districts, compared to Non-Hispanic Whites. In the pooled data model both groups were expected to have lower stop counts relative to Whites across districts.

Also, socioeconomic status and district racial composition emerge as significant predictors of stop counts during the same time regime (D), spanning January-June 2016. Neither of these district features proved significant in the analysis of the entire period.

### *Summary*

Taken together, both the time-regime and pooled models suggest that the race effect prevails independent of policies and procedures guiding the collection of investigatory stop data.

More specifically, for two out of three denominators (violent arrest and young population) stop counts of Non-Hispanic Blacks are predicted to exceed those of Non-Hispanic Whites across all time periods. The effect is statistically significant for the three longest four sub-periods using violent arrest, and for all four sub-periods when using young population.

The effect of ethnicity appears to depend more on both the sub-period in question and the denominator used.

Table 8: Robustness Analysis Results

	Pooled	Jan-Mar 2014	Apr-Dec 2014	Jan-Dec 2015	Jan-Jun 2016
<b>Violent Arrests</b>					
Black	+	+	+	+	+
Hispanic	-	-	-	-	+
Stability	-	-	-	-	+
SES	-	-	-	-	-
Percent Black	-	-	-	-	-
<b>Young Population</b>					
Black	+	+	+	+	+
Hispanic	+	+	+	+	+
Stability	-	-	-	-	-
SES	-	-	-	-	-
Percent Black	-	-	-	-	-
<b>Total Arrests</b>					
Black	-	-	-	-	+
Hispanic	-	-	-	-	+
Stability	+	+	-	-	-
SES	-	-	-	-	-
Percent Black	-	-	+	+	-

Notes: - and + indicate negative and positive effects, respectively. Shaded boxes indicate statistical significance at at least the .05 level.

#### Residual Analysis of Models

Due to our use of count models, we analyzed the Anscombe residual distribution of Model E. These are standardized residuals. While Figure 5 displays a normal distribution of residuals, there are a sizable number of quite extreme values. These outliers may possibly skew the findings. Again, additional diagnostics are necessary.

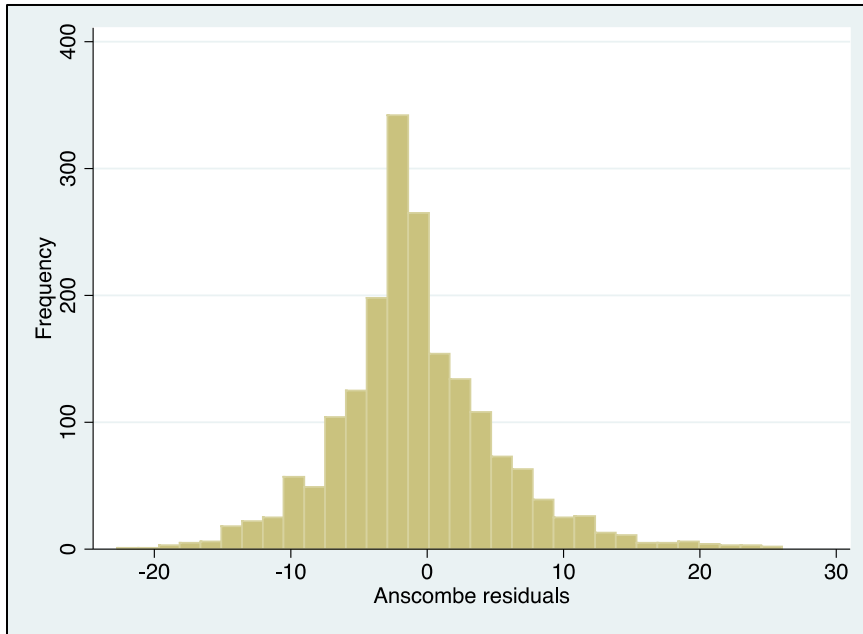


Figure 5: Standardized Residuals: Model E, Violent Arrest Exposure Variable

#### Translating into Predicted Stop Counts

We further examined the results of Model E (violent arrest denominator).

The first examination considered the relationship between predicted counts, and standardized Anscombe residuals. One regression assumption is that errors are relatively evenly distributed above and below zero at different ranges of predicted values, i.e., errors are stochastic in that they are un-associated with predicted values. As can be seen in Figure 6, this assumption is not met.

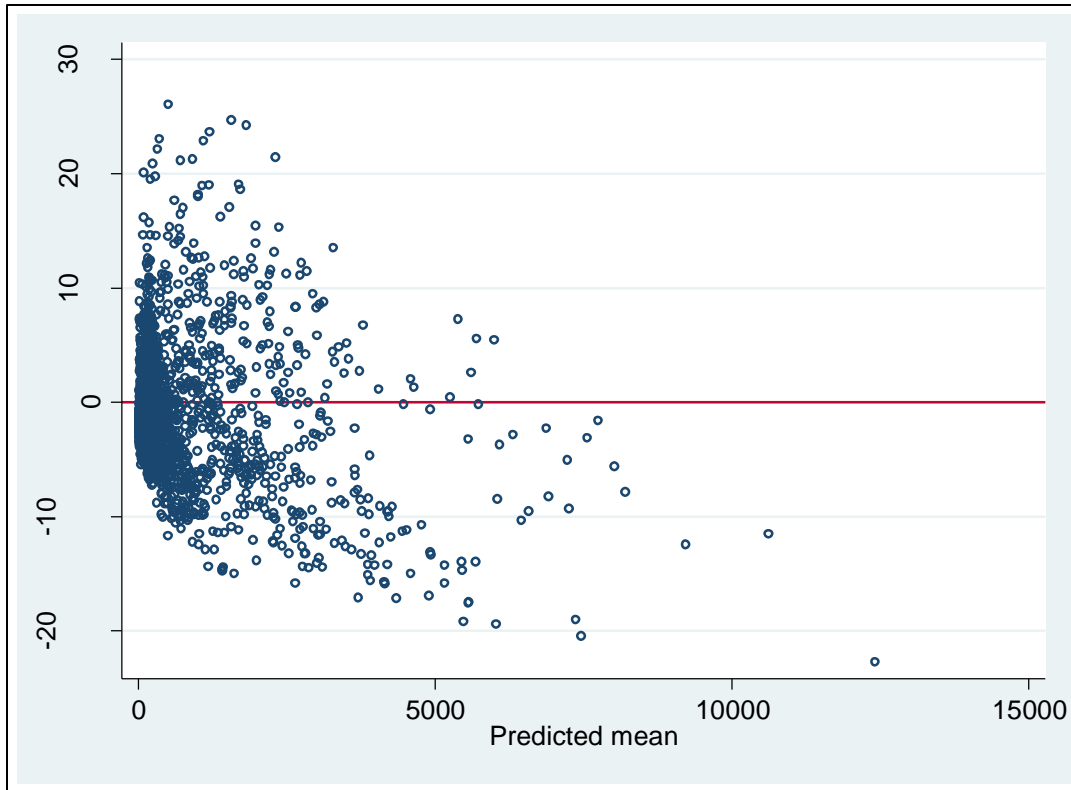


Figure 6: Predicted Stop Counts and Standardized Model E Residuals: Violent Arrest Exposure Variable

More specifically, negative model residuals predominate at higher predicted stop counts. This means that the model is more dramatically *under* predicting at higher expected counts.

There is also the suggestion from the figure that the model is somewhat *over* predicting at extremely low predicted counts. There are a number of positive residuals above a value of 20 at extremely low predicted counts, and no corresponding negative residuals in this range for extremely low predicted counts.

Another way to see this is to examine the observed and predicted counts. Scatterplot points were fitted using Locally Weighted Scatterplot Smoothing (LOWESS). This smoothed function allows us to see how the relationship between observed and predicted counts might shift at different stop count values.

Each dot represents one district-month race/ethnic-specific stop count. The solid, diagonal line indicates the best locally weighted non-parametric fit of the data (Figure 7). The “bend” in the smoothed curve suggests that the under-predicting starts with predicted count values around 4,000, in agreement with the earlier figure. It also shows some markedly discrepant values for district-months with higher stop counts. This suggests that the predicted scores of Model E are less reflective of actual stop counts for district-months with higher stop totals than those with lower stop totals. Additional outlier analyses, examining leverage and influence, are necessary.

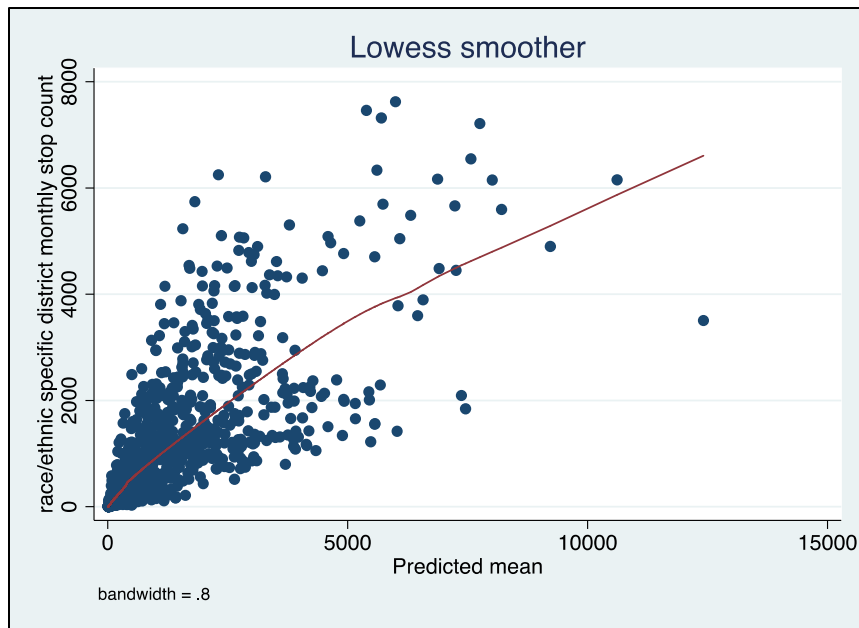


Figure 7: LOWESS Plot of Predicted to Observed Stop Counts

### Model Fit Diagnostics

Model fit can be described as the extent to which a set of chosen correlates account for variation in the outcome measure—in this case, stop counts. When multiple models are employed, researchers need to be able to identify which provides the best statistical explanation of the stop counts. In order to do this we report Akaike Information Criterion (AIC) and Bayesian Information Criterion (BIC) values across all conditional and unconditional models. Lower values represent better model fit while simultaneously controlling for model complexity. The ANOVA model for violent arrests yielded a BIC value of 26,010. This essentially represents a baseline fit measure of the model with just a random effect for districts, prior to entering any predictors.

The addition of independent variables, however, substantially enhanced model prediction. For example, including measures of race and ethnicity in Model A dropped the BIC value by about 36 to 25,974. A change greater than 10 represents “very strong” evidence of improved fit (Raftery, 1995). Controlling for linear and curvilinear time effects in Model B further reduced the BIC value to 25,232, but the addition of social structure variables in Model C did *not* enhance the predictive ability of the model. Recall that Model D substituted linear and curvilinear time effects with a series of annual and monthly dummy predictors. Relative to Model A and the ANOVA, Model D presented the best model fit with a BIC value of 24,998. The inclusion of stability, socioeconomic status, and percent Black in Model E raised this value by about 9, indicating the added complexity outweighed any improvement in fit. Taken together, fit diagnostics which control for model complexity suggest that among the models using the violent crime exposure measure, stop counts over the 29-month study period are best accounted for by race, ethnicity, and monthly and annual temporal effects—Model D. This does not negate the significant district racial composition effect of Model E which has a BIC that is higher, but comparable. But, as noted earlier, given model limitations with only 22 districts, we



strongly recommend *caution* in interpreting this racial composition effect. From a fit-controlling-for-complexity perspective, D and E are equivalent.

When reviewing the fit/complexity statistics of stop count models using total arrests and young population denominators a similar pattern prevailed. For both sets of models, Model D provided the best improvement in fit, controlling for complexity, compared to the respective ANOVA models. Both Models E (controlling for social structure), again, have BIC values that are slightly higher, but are close to that of Models D. So, as before, D and E are essentially equivalent.

## Discussion

The purpose of this study was to describe and explain ethnoracial-specific stop counts, over a 30-month period, from January 2014 to June 2016. Based on our review of descriptive data, we find that stop rates declined over the period. These findings hold whether considering stop rates per 1,000 residents (Figure 2), per 100 previous month's total arrests (Figure 3) or violent arrests (Figure 4). Moreover, we found that stop rates of each racial and ethnic group (Non-Hispanic Blacks, Non-Hispanic Whites, Hispanic Whites) decreased by almost fivefold through the study period. In fact, although absolute disparity remains, *descriptively* race and ethnic specific stop rates *look* closer to one another by February of 2016 because overall the rates are lower. ***But there still may be significant cross group differences specific to 2016 data. That has not been examined.*** Relying on each group's population, Non-Hispanic Blacks have the highest stop rates, followed by Hispanic Whites, and Non-Hispanic Whites.

To form inferences from the above descriptive data, we turned to mixed effects negative binomial regression. This analytical technique allowed us to model Non-Hispanic Black and Hispanic White stop counts relative to Non-Hispanic White stop counts, across districts, over time, while controlling for district context, temporal variation, and some features of districts. Our findings here differed depending on the denominator (exposure or benchmarking variable) of choice.

That said, our preference is towards models employing 1-month lagged ethnoracial-specific violent arrest counts, relative to 1-month lagged ethnoracial-specific total arrests, and the total (not ethnoracial specific) population aged 15-29 years. The preference is based on a process of elimination. The young population variable is not preferred because it is not ethnoracial-specific. It means we are in effect creating a rate where only the numerator is ethnoracial-specific. We want both the numerator and the denominator to have this specificity. The total arrest variable contains a lot more police discretion in it than does the serious violent arrest variable. Less discretion is preferred because we are seeking a benchmark that is more reflective of local conditions. That leaves us with the violent arrest benchmark variable.

This does not mean that the violent arrest exposure variable has no problems. It does. In particular, the low counts represent a serious limitation. Work in the future period will see if moving to calendar quarters reduces the low count problem. A second problem is that the violent arrest exposure variable creates a markedly different picture at the city level versus the district level. The descriptive city level picture suggests higher stops/violent arrest for White as

compared to Black non-Hispanics for many months. The district-level picture suggests the opposite. Whether this discrepancy arises from switching geographic scales, or low benchmarking variable counts for district-months, or something else, is not clear at this time.

Further model tests of residuals, leverage, and influence, and model assumptions must be conducted before we definitively conclude which model outperforms which other model. But the point being made here is that the variable that is arguably the least flawed *conceptually* for addressing the external benchmarking challenge, albeit imperfect, does reveal disparities that align with patterns observed in other jurisdictions (Gelman et al., 2007) and seems *conceptually* preferable here.

***This preference, however, is tempered by a strong level of concern about model adequacy. Initial diagnostics examining the best model in the series using violent arrests as exposure reveal multiple problematic features. We have not yet completed further diagnostics with this series or with other series. At this point all that we can say is that serious violations of key assumptions are apparent and we caution against relying on any finding based solely on these ecological models.***

Although residential stability and socioeconomic status effects are not evident, models suggest that stops are *less* common in districts that composed of more Non-Hispanic Black residents. This may suggest a racial incongruity effect identified in prior literature, whereby individuals face an increased likelihood of being stopped outside of spaces that resemble their own race or ethnicity (Meehan & Ponder, 2002; Rojek, Rosenfeld, & Decker, 2012; Stewart, Baumer, Brunson, & Simons, 2009). It also might be the case that this racial composition impact is part and parcel of the problems associated with such a low number of districts in a multilevel model. Prior researchers have warned about exactly this concern (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016).

Models that included more stringent controls for time by modeling monthly and annual effects were generally consistent with the above findings.

### Limitations

Our current findings are limited in four important ways.

First, as noted above, there are numerous instances of low numbers for the violent arrest benchmarking variable. We cannot know the extent to which this is affecting racial differences seen until we try larger units, district-quarters rather than district-months, for example.

Second, these models do not control for spatial effects. Extensive literature has noted crime and justice outcomes of places are often influenced by their spatial neighbors. Our failure to include such controls at this time means that all these models may be mis-specified to an extent.

Third, the one model carefully considered to see if it meets modeling assumptions, a model from the violent arrest series, revealed **multiple serious concerns. The model violates fundamental assumptions of regression.** We don't yet know if these can be addressed through Winsorizing count outcomes, removing high leverage and/or high influence cases, or not. All of

these problems may be related to low violent arrest counts for some groups for some months for some districts and may prove fundamentally unresolvable. **We may yet learn that all of these ecological models are seriously problematic, and that these problems are not fixable.**

Finally, recent scholarship in political economy has pointed out serious limitations when doing multilevel models with a low number of groups, here districts (Bryan & Jenkins, 2016; Schmidt-Catran & Fairbrother, 2016). We would like to recommend moving to beats within districts as the grouping unit of interest, because there would be so many of them. But doing so means that ethnoracial specific denominator values for things like all arrests or violent arrests become even more problematic. If interest continues in ecological models like these, much more remains to be sorted out.

## Conclusions

We suggest the following conclusions.

First, the clearest discrepancy in stop rates is between stops of non-Hispanic White vs. non-Hispanic Black civilians.

Second, the size and direction of that discrepancy depends on both the benchmarking variable used and the geography used. For example, using the violent arrest benchmark variable at the city level the rate appears (descriptively) higher for White than Black non-Hispanics, while at the district level using the same benchmark variable it is (statistically) higher for Black as compared to White non-Hispanics.

Third, the district level discrepancy with significantly higher stop rates for Black as compared to White non-Hispanics using the violent arrest variable is robust in some ways but may be fragile in other ways. It is robust because it replicates across three of the four different sub-periods within the overall period examined. But it may be fragile because of low counts for the benchmarking variable and potential problems with model assumptions. These models need further diagnoses as well as additional variables like controls for nearby stop activity, and for police stops.

Fourth, the problems associated with interpreting the ecological analyses in this study are not worse here than they are in other studies with ecological models examining potential racial and ethnic disparities in stops. The interpretative challenges seen here arise from **the nature of the inquiry** and the availability of only **crude proxy measures** to capture key dynamics and attributes. These challenges are endemic to this field of inquiry.

APPENDIX A: Descriptive Statistics

	n	Mean	Std. Dev.	Min	Max
Stop Count	1,914	677.006	1072.571	0.000	7624.000
Black	1,914	0.333	0.472	0.000	1.000
Hispanic	1,914	0.333	0.472	0.000	1.000
Time (Linear - uncentered)	1,914	14.000	8.369	0.000	28.000
Time (Linear - centered)	1,914	0.467	8.369	-13.533	14.467
Time (Curvilinear - uncentered)	1,914	266.000	242.522	0.000	784.000
Time (Curvilinear - centered)	1,914	70.218	63.000	0.218	209.284
2015	1,914	0.414	0.493	0.000	1.000
2016	1,914	0.207	0.405	0.000	1.000
January	1,914	0.069	0.253	0.000	1.000
March	1,914	0.103	0.305	0.000	1.000
April	1,914	0.103	0.305	0.000	1.000
May	1,914	0.103	0.305	0.000	1.000
June	1,914	0.103	0.305	0.000	1.000
July	1,914	0.069	0.253	0.000	1.000
August	1,914	0.069	0.253	0.000	1.000
September	1,914	0.069	0.253	0.000	1.000
October	1,914	0.069	0.253	0.000	1.000
November	1,914	0.069	0.253	0.000	1.000
December	1,914	0.069	0.253	0.000	1.000
Stability (uncentered)	1,914	0.000	0.295	-0.470	0.772
Stability (centered)	1,914	0.000	0.295	-0.470	0.772
Socioeconomic Status (uncentered)	1,914	0.022	0.812	-1.358	1.776
Socioeconomic Status (centered)	1,914	0.000	0.812	-1.379	1.754
Percent Black (uncentered)	1,914	0.416	0.358	0.012	0.969
Percent Black (centered)	1,914	0.000	0.358	-0.405	0.552
All Arrest Count	1,914	142.686	199.752	1.000	1351.000
Violent Arrest Count	1,896	5.046	6.071	1.000	48.000
Population Aged 15-29 <sup>1</sup>	1,914	30,304.630	13,923.980	14,180.040	66,363.350

Note: <sup>1</sup>Not race-specific. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

APPENDIX B: District-Level Stop Counts and Rates, January 2014 - June 2016

District	Month and Year	Counts				Rates per 1,000 population			
		All	Black	White	Hispanic	All	Black	White	Hispanic
01	Jan-14	1,457	925	340	107	21.78	65.88	10.13	42.97
01	Feb-14	1,473	936	336	113	22.02	66.66	10.02	45.38
01	Mar-14	1,827	1,124	445	156	27.31	80.05	13.26	62.64
01	Apr-14	1,363	887	317	91	20.38	63.17	9.45	36.54
01	May-14	1,352	868	339	94	20.21	61.82	10.10	37.75
01	Jun-14	1,298	808	331	106	19.40	57.54	9.87	42.57
01	Jul-14	1,235	770	319	100	18.46	54.84	9.51	40.16
01	Aug-14	1,247	765	313	115	18.64	54.48	9.33	46.18
01	Sep-14	1,236	797	294	96	18.48	56.76	8.76	38.55
01	Oct-14	1,514	1,002	334	105	22.63	71.36	9.96	42.16
01	Nov-14	1,456	942	337	114	21.77	67.09	10.04	45.78
01	Dec-14	1,160	769	257	84	17.34	54.77	7.66	33.73
01	Jan-15	1,394	911	324	97	20.84	64.88	9.66	38.95
01	Feb-15	1,283	876	269	89	19.18	62.39	8.02	35.74
01	Mar-15	1,730	1,187	335	142	25.86	84.54	9.99	57.02
01	Apr-15	1,003	660	217	85	14.99	47.00	6.47	34.13
01	May-15	845	516	207	93	12.63	36.75	6.17	37.34
01	Jun-15	843	507	213	91	12.60	36.11	6.35	36.54
01	Jul-15	937	518	274	116	14.01	36.89	8.17	46.58
01	Aug-15	848	578	172	68	12.68	41.16	5.13	27.31
01	Sep-15	1,002	710	187	78	14.98	50.57	5.57	31.32
01	Oct-15	1,248	908	193	88	18.66	64.67	5.75	35.34
01	Nov-15	939	646	158	90	14.04	46.01	4.71	36.14
01	Dec-15	596	392	127	50	8.91	27.92	3.79	20.08
01	Jan-16	161	125	27	9	2.41	8.90	0.80	3.61
01	Feb-16	84	61	11	11	1.26	4.34	0.33	4.42
01	Mar-16	163	115	30	14	2.44	8.19	0.89	5.62

01	Apr-16	141	108	24	9	2.11	7.69	0.72	3.61
01	May-16	146	117	19	10	2.18	8.33	0.57	4.02
01	Jun-16	136	114	15	6	2.03	8.12	0.45	2.41
02	Jan-14	2,074	1,918	84	39	21.69	28.89	4.82	27.88
02	Feb-14	2,931	2,723	104	53	30.65	41.01	5.97	37.89
02	Mar-14	3,199	3,001	95	53	33.45	45.20	5.45	37.89
02	Apr-14	2,651	2,476	101	38	27.72	37.29	5.79	27.17
02	May-14	2,587	2,434	86	41	27.05	36.66	4.93	29.31
02	Jun-14	2,497	2,385	61	27	26.11	35.92	3.50	19.30
02	Jul-14	3,088	2,938	74	28	32.29	44.25	4.24	20.02
02	Aug-14	3,044	2,865	93	51	31.83	43.15	5.33	36.46
02	Sep-14	2,541	2,416	63	37	26.57	36.39	3.61	26.45
02	Oct-14	3,183	3,009	105	39	33.28	45.32	6.02	27.88
02	Nov-14	3,191	2,995	115	42	33.37	45.11	6.60	30.02
02	Dec-14	2,776	2,596	96	48	29.03	39.10	5.51	34.31
02	Jan-15	3,298	3,106	108	43	34.49	46.78	6.19	30.74
02	Feb-15	2,665	2,461	114	54	27.87	37.07	6.54	38.60
02	Mar-15	3,385	3,131	155	42	35.40	47.16	8.89	30.02
02	Apr-15	2,492	2,302	82	40	26.06	34.67	4.70	28.59
02	May-15	2,664	2,478	75	43	27.86	37.32	4.30	30.74
02	Jun-15	2,583	2,426	66	32	27.01	36.54	3.79	22.88
02	Jul-15	2,405	2,238	87	34	25.15	33.71	4.99	24.31
02	Aug-15	2,623	2,443	87	35	27.43	36.80	4.99	25.02
02	Sep-15	3,159	2,951	112	45	33.03	44.45	6.42	32.17
02	Oct-15	3,449	3,220	104	50	36.07	48.50	5.97	35.74
02	Nov-15	2,799	2,615	83	36	29.27	39.39	4.76	25.74
02	Dec-15	1,802	1,656	50	34	18.84	24.94	2.87	24.31
02	Jan-16	304	297	5	1	3.18	4.47	0.29	0.71
02	Feb-16	229	221	3	2	2.39	3.33	0.17	1.43
02	Mar-16	276	263	6	6	2.89	3.96	0.34	4.29

02	Apr-16	422	412	7	1	4.41	6.21	0.40	0.71
02	May-16	491	470	9	7	5.13	7.08	0.52	5.00
02	Jun-16	590	574	7	5	6.17	8.65	0.40	3.57
03	Jan-14	3,685	3,550	70	39	47.26	50.18	22.46	81.75
03	Feb-14	4,320	4,147	84	51	55.40	58.61	26.96	106.90
03	Mar-14	5,235	5,074	68	45	67.14	71.72	21.82	94.32
03	Apr-14	4,624	4,488	55	46	59.30	63.43	17.65	96.42
03	May-14	4,113	3,995	62	41	52.75	56.47	19.90	85.94
03	Jun-14	3,694	3,585	43	35	47.38	50.67	13.80	73.36
03	Jul-14	4,266	4,149	53	34	54.71	58.64	17.01	71.27
03	Aug-14	4,920	4,784	50	62	63.10	67.62	16.04	129.96
03	Sep-14	4,630	4,493	54	51	59.38	63.51	17.33	106.90
03	Oct-14	4,303	4,161	67	55	55.19	58.81	21.50	115.28
03	Nov-14	4,578	4,429	62	68	58.71	62.60	19.90	142.53
03	Dec-14	4,043	3,877	80	59	51.85	54.80	25.67	123.67
03	Jan-15	4,317	4,168	70	53	55.37	58.91	22.46	111.09
03	Feb-15	3,398	3,256	55	57	43.58	46.02	17.65	119.48
03	Mar-15	4,979	4,825	76	49	63.86	68.20	24.39	102.71
03	Apr-15	3,483	3,413	37	17	44.67	48.24	11.87	35.63
03	May-15	3,537	3,451	45	29	45.36	48.78	14.44	60.79
03	Jun-15	3,551	3,445	43	54	45.54	48.69	13.80	113.19
03	Jul-15	3,647	3,544	49	19	46.77	50.09	15.72	39.83
03	Aug-15	3,559	3,462	41	30	45.64	48.93	13.16	62.88
03	Sep-15	3,393	3,297	45	36	43.52	46.60	14.44	75.46
03	Oct-15	3,745	3,635	34	50	48.03	51.38	10.91	104.80
03	Nov-15	3,015	2,938	32	30	38.67	41.53	10.27	62.88
03	Dec-15	1,525	1,446	45	21	19.56	20.44	14.44	44.02
03	Jan-16	494	479	9	3	6.34	6.77	2.89	6.29
03	Feb-16	250	243	2	2	3.21	3.43	0.64	4.19
03	Mar-16	493	486	3	1	6.32	6.87	0.96	2.10

03	Apr-16	473	463	6	0	6.07	6.54	1.93	0.00
03	May-16	700	684	10	4	8.98	9.67	3.21	8.38
03	Jun-16	684	672	4	3	8.77	9.50	1.28	6.29
04	Jan-14	1,756	1,425	56	263	14.67	19.86	5.39	9.64
04	Feb-14	1,990	1,552	86	337	16.63	21.63	8.28	12.35
04	Mar-14	2,967	2,254	161	517	24.79	31.42	15.50	18.95
04	Apr-14	3,549	2,848	127	531	29.66	39.70	12.23	19.46
04	May-14	2,814	2,251	105	426	23.52	31.38	10.11	15.61
04	Jun-14	3,123	2,501	103	480	26.10	34.86	9.91	17.59
04	Jul-14	2,745	2,222	88	401	22.94	30.97	8.47	14.70
04	Aug-14	3,826	3,181	92	523	31.97	44.34	8.86	19.17
04	Sep-14	3,480	2,905	91	447	29.08	40.49	8.76	16.38
04	Oct-14	3,033	2,500	85	417	25.35	34.85	8.18	15.28
04	Nov-14	2,558	2,090	86	357	21.38	29.13	8.28	13.08
04	Dec-14	2,308	1,965	70	254	19.29	27.39	6.74	9.31
04	Jan-15	3,373	2,720	122	495	28.19	37.91	11.74	18.14
04	Feb-15	2,436	1,951	74	382	20.36	27.19	7.12	14.00
04	Mar-15	3,215	2,594	106	479	26.87	36.16	10.20	17.56
04	Apr-15	2,019	1,716	55	236	16.87	23.92	5.29	8.65
04	May-15	1,611	1,305	62	226	13.46	18.19	5.97	8.28
04	Jun-15	1,366	1,058	41	253	11.42	14.75	3.95	9.27
04	Jul-15	1,863	1,492	62	283	15.57	20.80	5.97	10.37
04	Aug-15	1,896	1,533	64	285	15.84	21.37	6.16	10.45
04	Sep-15	2,081	1,647	70	333	17.39	22.96	6.74	12.20
04	Oct-15	1,757	1,364	52	311	14.68	19.01	5.01	11.40
04	Nov-15	1,600	1,246	56	264	13.37	17.37	5.39	9.68
04	Dec-15	939	707	31	190	7.85	9.85	2.98	6.96
04	Jan-16	626	521	14	87	5.23	7.26	1.35	3.19
04	Feb-16	453	385	9	57	3.79	5.37	0.87	2.09
04	Mar-16	624	481	13	127	5.21	6.70	1.25	4.65



04	Apr-16	608	469	23	114	5.08	6.54	2.21	4.18
04	May-16	681	526	16	135	5.69	7.33	1.54	4.95
04	Jun-16	613	518	10	82	5.12	7.22	0.96	3.01
05	Jan-14	3,414	3,277	59	43	47.03	48.07	44.41	31.73
05	Feb-14	3,956	3,808	59	56	54.49	55.86	44.41	41.33
05	Mar-14	4,308	4,162	52	60	59.34	61.05	39.14	44.28
05	Apr-14	3,966	3,809	54	82	54.63	55.88	40.65	60.51
05	May-14	5,450	5,232	79	100	75.07	76.75	59.47	73.80
05	Jun-14	3,881	3,713	61	87	53.46	54.47	45.92	64.20
05	Jul-14	2,878	2,766	45	51	39.64	40.58	33.87	37.64
05	Aug-14	3,467	3,345	44	58	47.76	49.07	33.12	42.80
05	Sep-14	2,758	2,630	41	74	37.99	38.58	30.86	54.61
05	Oct-14	2,606	2,535	24	35	35.90	37.19	18.07	25.83
05	Nov-14	2,611	2,521	33	42	35.97	36.98	24.84	30.99
05	Dec-14	1,778	1,727	20	21	24.49	25.33	15.05	15.50
05	Jan-15	2,428	2,333	48	36	33.45	34.22	36.13	26.57
05	Feb-15	2,085	2,014	32	20	28.72	29.54	24.09	14.76
05	Mar-15	2,349	2,253	31	47	32.36	33.05	23.33	34.68
05	Apr-15	1,448	1,415	20	6	19.95	20.76	15.05	4.43
05	May-15	1,797	1,749	15	24	24.75	25.66	11.29	17.71
05	Jun-15	1,949	1,872	34	35	26.85	27.46	25.59	25.83
05	Jul-15	2,152	2,093	31	12	29.64	30.70	23.33	8.86
05	Aug-15	2,172	2,116	18	26	29.92	31.04	13.55	19.19
05	Sep-15	2,103	2,040	21	35	28.97	29.93	15.81	25.83
05	Oct-15	1,870	1,804	28	24	25.76	26.46	21.08	17.71
05	Nov-15	1,863	1,809	25	19	25.66	26.54	18.82	14.02
05	Dec-15	1,025	984	15	12	14.12	14.43	11.29	8.86
05	Jan-16	200	192	4	2	2.76	2.82	3.01	1.48
05	Feb-16	118	112	2	1	1.63	1.64	1.51	0.74
05	Mar-16	259	252	3	2	3.57	3.70	2.26	1.48

05	Apr-16	414	395	7	5	5.70	5.79	5.27	3.69
05	May-16	503	488	6	7	6.93	7.16	4.52	5.17
05	Jun-16	446	428	3	10	6.14	6.28	2.26	7.38
06	Jan-14	2,952	2,835	64	31	32.40	32.13	114.70	153.36
06	Feb-14	3,268	3,167	43	35	35.87	35.89	77.07	173.15
06	Mar-14	4,694	4,542	89	40	51.52	51.47	159.51	197.89
06	Apr-14	3,653	3,574	39	21	40.10	40.50	69.90	103.89
06	May-14	3,334	3,232	57	26	36.59	36.62	102.16	128.63
06	Jun-14	2,908	2,833	38	21	31.92	32.10	68.10	103.89
06	Jul-14	3,323	3,218	37	28	36.47	36.47	66.31	138.52
06	Aug-14	2,776	2,705	31	22	30.47	30.65	55.56	108.84
06	Sep-14	2,861	2,779	37	27	31.40	31.49	66.31	133.58
06	Oct-14	3,061	2,988	29	24	33.60	33.86	51.97	118.73
06	Nov-14	3,036	2,952	36	33	33.32	33.45	64.52	163.26
06	Dec-14	2,574	2,486	46	15	28.25	28.17	82.44	74.21
06	Jan-15	3,125	3,043	52	19	34.30	34.48	93.20	94.00
06	Feb-15	2,546	2,482	32	18	27.94	28.13	57.35	89.05
06	Mar-15	3,375	3,281	43	26	37.04	37.18	77.07	128.63
06	Apr-15	2,304	2,239	33	21	25.29	25.37	59.14	103.89
06	May-15	2,124	2,077	31	9	23.31	23.54	55.56	44.53
06	Jun-15	1,896	1,858	16	11	20.81	21.05	28.68	54.42
06	Jul-15	2,200	2,131	30	17	24.15	24.15	53.77	84.10
06	Aug-15	2,159	2,094	36	13	23.70	23.73	64.52	64.31
06	Sep-15	2,581	2,523	19	16	28.33	28.59	34.05	79.16
06	Oct-15	2,513	2,441	37	19	27.58	27.66	66.31	94.00
06	Nov-15	2,603	2,538	33	15	28.57	28.76	59.14	74.21
06	Dec-15	1,325	1,281	18	15	14.54	14.52	32.26	74.21
06	Jan-16	537	514	7	8	5.89	5.82	12.55	39.58
06	Feb-16	236	225	5	3	2.59	2.55	8.96	14.84
06	Mar-16	380	364	8	6	4.17	4.12	14.34	29.68

06	Apr-16	447	429	6	9	4.91	4.86	10.75	44.53
06	May-16	495	480	7	2	5.43	5.44	12.55	9.89
06	Jun-16	413	405	4	2	4.53	4.59	7.17	9.89
07	Jan-14	3,787	3,633	66	66	57.72	58.64	96.92	146.40
07	Feb-14	4,491	4,303	67	95	68.45	69.46	98.39	210.72
07	Mar-14	5,708	5,484	83	117	87.00	88.52	121.89	259.52
07	Apr-14	5,257	5,060	58	95	80.13	81.68	85.18	210.72
07	May-14	5,237	5,086	51	70	79.83	82.10	74.90	155.27
07	Jun-14	5,124	4,967	69	62	78.10	80.18	101.33	137.52
07	Jul-14	4,598	4,448	53	54	70.09	71.80	77.83	119.78
07	Aug-14	4,582	4,441	49	64	69.84	71.69	71.96	141.96
07	Sep-14	4,473	4,326	62	70	68.18	69.83	91.05	155.27
07	Oct-14	5,475	5,304	67	75	83.45	85.62	98.39	166.36
07	Nov-14	4,914	4,745	53	96	74.90	76.59	77.83	212.94
07	Dec-14	3,977	3,829	45	81	60.62	61.81	66.08	179.67
07	Jan-15	4,783	4,614	59	94	72.91	74.48	86.64	208.50
07	Feb-15	4,250	4,061	59	97	64.78	65.55	86.64	215.16
07	Mar-15	5,299	5,102	61	102	80.77	82.36	89.58	226.25
07	Apr-15	5,914	5,741	72	71	90.14	92.67	105.73	157.49
07	May-15	6,396	6,248	64	65	97.49	100.86	93.99	144.18
07	Jun-15	4,742	4,618	45	63	72.28	74.54	66.08	139.74
07	Jul-15	4,701	4,530	62	90	71.66	73.12	91.05	199.63
07	Aug-15	4,540	4,366	66	94	69.20	70.48	96.92	208.50
07	Sep-15	5,075	4,898	43	82	77.36	79.06	63.15	181.89
07	Oct-15	6,405	6,210	55	109	97.63	100.24	80.77	241.78
07	Nov-15	4,310	4,155	54	73	65.70	67.07	79.30	161.92
07	Dec-15	2,429	2,311	40	51	37.02	37.30	58.74	113.12
07	Jan-16	871	837	11	14	13.28	13.51	16.15	31.05
07	Feb-16	603	578	9	11	9.19	9.33	13.22	24.40
07	Mar-16	810	790	9	3	12.35	12.75	13.22	6.65

07	Apr-16	1,018	1,000	11	5	15.52	16.14	16.15	11.09
07	May-16	806	779	11	10	12.29	12.57	16.15	22.18
07	Jun-16	730	710	8	11	11.13	11.46	11.75	24.40
08	Jan-14	3,631	2,168	379	1,053	14.42	42.11	7.62	16.06
08	Feb-14	3,496	1,842	450	1,183	13.88	35.78	9.05	18.04
08	Mar-14	3,775	2,093	430	1,222	14.99	40.65	8.65	18.63
08	Apr-14	3,770	2,227	310	1,207	14.97	43.25	6.24	18.40
08	May-14	3,844	2,369	340	1,090	15.27	46.01	6.84	16.62
08	Jun-14	3,366	2,020	374	949	13.37	39.23	7.52	14.47
08	Jul-14	3,602	2,161	395	1,007	14.31	41.97	7.95	15.35
08	Aug-14	3,912	2,291	414	1,166	15.54	44.50	8.33	17.78
08	Sep-14	3,328	1,942	367	992	13.22	37.72	7.38	15.13
08	Oct-14	3,490	2,075	360	1,026	13.86	40.30	7.24	15.64
08	Nov-14	2,962	1,660	330	950	11.76	32.24	6.64	14.49
08	Dec-14	2,724	1,397	361	945	10.82	27.13	7.26	14.41
08	Jan-15	4,006	2,385	388	1,200	15.91	46.32	7.80	18.30
08	Feb-15	3,232	1,671	489	1,039	12.84	32.45	9.84	15.84
08	Mar-15	4,437	2,468	495	1,422	17.62	47.93	9.96	21.68
08	Apr-15	3,131	1,707	384	1,006	12.43	33.15	7.72	15.34
08	May-15	3,257	1,729	387	1,102	12.94	33.58	7.78	16.80
08	Jun-15	3,324	1,872	360	1,062	13.20	36.36	7.24	16.19
08	Jul-15	3,106	1,578	385	1,108	12.34	30.65	7.74	16.89
08	Aug-15	3,317	1,785	390	1,110	13.17	34.67	7.85	16.92
08	Sep-15	3,128	1,786	322	975	12.42	34.69	6.48	14.87
08	Oct-15	3,624	2,169	342	1,072	14.39	42.13	6.88	16.35
08	Nov-15	3,241	1,860	324	1,028	12.87	36.12	6.52	15.67
08	Dec-15	1,723	862	195	655	6.84	16.74	3.92	9.99
08	Jan-16	769	360	76	329	3.05	6.99	1.53	5.02
08	Feb-16	511	199	48	263	2.03	3.86	0.97	4.01
08	Mar-16	769	322	67	374	3.05	6.25	1.35	5.70

08	Apr-16	467	195	46	220	1.85	3.79	0.93	3.35
08	May-16	476	247	45	183	1.89	4.80	0.91	2.79
08	Jun-16	528	229	50	247	2.10	4.45	1.01	3.77
09	Jan-14	2,949	1,187	368	1,280	17.88	65.67	15.21	23.25
09	Feb-14	3,047	1,216	389	1,302	18.48	67.27	16.07	23.65
09	Mar-14	4,086	1,562	433	1,895	24.78	86.42	17.89	34.42
09	Apr-14	2,634	1,083	282	1,196	15.97	59.92	11.65	21.72
09	May-14	2,966	1,221	284	1,395	17.99	67.55	11.74	25.34
09	Jun-14	3,391	1,341	362	1,626	20.56	74.19	14.96	29.53
09	Jul-14	3,501	1,426	333	1,674	21.23	78.89	13.76	30.41
09	Aug-14	3,601	1,469	326	1,744	21.84	81.27	13.47	31.68
09	Sep-14	3,469	1,340	337	1,690	21.04	74.13	13.93	30.70
09	Oct-14	3,313	1,313	293	1,639	20.09	72.64	12.11	29.77
09	Nov-14	2,734	1,174	243	1,268	16.58	64.95	10.04	23.03
09	Dec-14	2,793	1,270	253	1,209	16.94	70.26	10.45	21.96
09	Jan-15	4,273	1,871	358	1,939	25.91	103.51	14.79	35.22
09	Feb-15	3,968	1,890	354	1,650	24.06	104.56	14.63	29.97
09	Mar-15	4,770	2,088	416	2,164	28.93	115.52	17.19	39.31
09	Apr-15	3,832	1,497	290	1,975	23.24	82.82	11.98	35.87
09	May-15	4,184	1,505	401	2,171	25.37	83.26	16.57	39.43
09	Jun-15	3,451	1,149	315	1,919	20.93	63.57	13.02	34.86
09	Jul-15	3,523	1,289	328	1,827	21.36	71.31	13.55	33.18
09	Aug-15	3,684	1,427	334	1,864	22.34	78.95	13.80	33.86
09	Sep-15	3,467	1,531	325	1,561	21.03	84.70	13.43	28.35
09	Oct-15	4,075	2,017	313	1,675	24.71	111.59	12.93	30.42
09	Nov-15	2,700	1,285	230	1,127	16.37	71.09	9.50	20.47
09	Dec-15	1,594	786	151	625	9.67	43.48	6.24	11.35
09	Jan-16	661	288	54	312	4.01	15.93	2.23	5.67
09	Feb-16	494	183	56	253	3.00	10.12	2.31	4.60
09	Mar-16	796	289	69	428	4.83	15.99	2.85	7.77

09	Apr-16	657	254	68	331	3.98	14.05	2.81	6.01
09	May-16	783	282	58	439	4.75	15.60	2.40	7.97
09	Jun-16	934	276	75	580	5.66	15.27	3.10	10.53
10	Jan-14	3,716	1,890	167	1,616	34.36	53.88	46.94	33.87
10	Feb-14	3,959	2,009	139	1,761	36.60	57.27	39.07	36.91
10	Mar-14	3,997	1,865	170	1,929	36.95	53.16	47.79	40.43
10	Apr-14	3,339	1,655	120	1,532	30.87	47.18	33.73	32.11
10	May-14	4,388	2,222	149	1,975	40.57	63.34	41.88	41.39
10	Jun-14	4,533	2,241	162	2,090	41.91	63.88	45.54	43.80
10	Jul-14	3,901	1,985	122	1,751	36.07	56.58	34.29	36.70
10	Aug-14	3,708	1,988	134	1,555	34.28	56.67	37.67	32.59
10	Sep-14	3,988	2,104	105	1,746	36.87	59.98	29.51	36.59
10	Oct-14	4,109	2,409	144	1,516	37.99	68.67	40.48	31.77
10	Nov-14	3,720	2,134	156	1,393	34.39	60.83	43.85	29.19
10	Dec-14	2,988	1,820	106	1,027	27.63	51.88	29.80	21.52
10	Jan-15	3,936	2,144	144	1,601	36.39	61.12	40.48	33.55
10	Feb-15	3,630	1,881	146	1,572	33.56	53.62	41.04	32.95
10	Mar-15	3,901	2,087	144	1,624	36.07	59.49	40.48	34.04
10	Apr-15	2,974	1,614	101	1,222	27.50	46.01	28.39	25.61
10	May-15	3,253	1,622	128	1,470	30.08	46.24	35.98	30.81
10	Jun-15	2,756	1,325	130	1,275	25.48	37.77	36.54	26.72
10	Jul-15	2,813	1,421	99	1,272	26.01	40.51	27.83	26.66
10	Aug-15	3,143	1,619	135	1,354	29.06	46.15	37.95	28.38
10	Sep-15	3,076	1,718	110	1,221	28.44	48.97	30.92	25.59
10	Oct-15	2,197	1,203	84	889	20.31	34.29	23.61	18.63
10	Nov-15	2,246	1,180	98	937	20.77	33.64	27.55	19.64
10	Dec-15	963	515	26	404	8.90	14.68	7.31	8.47
10	Jan-16	437	255	14	167	4.04	7.27	3.94	3.50
10	Feb-16	316	200	11	104	2.92	5.70	3.09	2.18
10	Mar-16	857	499	18	337	7.92	14.22	5.06	7.06

10	Apr-16	845	568	27	247	7.81	16.19	7.59	5.18
10	May-16	1,165	916	29	210	10.77	26.11	8.15	4.40
10	Jun-16	382	245	11	125	3.53	6.98	3.09	2.62
11	Jan-14	4,780	3,968	328	428	66.37	65.55	160.57	149.49
11	Feb-14	5,282	4,347	358	511	73.34	71.81	175.26	178.48
11	Mar-14	6,592	5,595	431	488	91.53	92.43	210.99	170.45
11	Apr-14	5,751	4,899	362	433	79.85	80.93	177.22	151.24
11	May-14	7,001	6,153	379	408	97.21	101.65	185.54	142.51
11	Jun-14	8,335	7,460	400	408	115.73	123.24	195.82	142.51
11	Jul-14	8,346	7,318	557	390	115.88	120.90	272.68	136.22
11	Aug-14	8,732	7,624	540	490	121.24	125.95	264.36	171.15
11	Sep-14	7,522	6,547	529	386	104.44	108.16	258.97	134.82
11	Oct-14	8,185	7,212	486	427	113.65	119.15	237.92	149.14
11	Nov-14	7,143	6,149	476	452	99.18	101.58	233.02	157.88
11	Dec-14	5,828	5,045	361	352	80.92	83.35	176.73	122.95
11	Jan-15	6,607	5,663	469	410	91.74	93.56	229.60	143.21
11	Feb-15	5,493	4,704	388	337	76.27	77.71	189.94	117.71
11	Mar-15	7,049	6,166	422	385	97.88	101.87	206.59	134.47
11	Apr-15	5,577	4,765	377	347	77.44	78.72	184.56	121.20
11	May-15	4,546	3,894	308	295	63.12	64.33	150.78	103.04
11	Jun-15	4,058	3,505	283	228	56.35	57.90	138.54	79.64
11	Jul-15	4,471	3,782	383	268	62.08	62.48	187.50	93.61
11	Aug-15	5,365	4,483	426	406	74.49	74.06	208.55	141.81
11	Sep-15	6,600	5,695	481	378	91.64	94.08	235.47	132.03
11	Oct-15	7,394	6,335	524	474	102.67	104.66	256.52	165.56
11	Nov-15	6,056	5,379	306	324	84.09	88.86	149.80	113.17
11	Dec-15	4,609	4,019	264	294	64.00	66.40	129.24	102.69
11	Jan-16	946	856	40	41	13.14	14.14	19.58	14.32
11	Feb-16	581	507	21	46	8.07	8.38	10.28	16.07
11	Mar-16	749	646	43	52	10.40	10.67	21.05	18.16

11	Apr-16	1,005	896	46	54	13.95	14.80	22.52	18.86
11	May-16	1,099	993	64	38	15.26	16.40	31.33	13.27
11	Jun-16	1,431	1,215	127	82	19.87	20.07	62.17	28.64
12	Jan-14	1,943	917	426	531	14.92	39.23	7.86	23.02
12	Feb-14	1,974	1,017	374	536	15.16	43.51	6.90	23.24
12	Mar-14	2,351	1,122	375	785	18.05	48.00	6.92	34.04
12	Apr-14	2,875	1,419	406	965	22.08	60.71	7.49	41.84
12	May-14	2,865	1,301	391	1,118	22.00	55.66	7.21	48.47
12	Jun-14	2,535	1,262	313	897	19.47	53.99	5.77	38.89
12	Jul-14	2,566	1,208	333	960	19.71	51.68	6.14	41.62
12	Aug-14	2,603	1,310	434	802	19.99	56.05	8.00	34.77
12	Sep-14	2,356	1,074	386	838	18.09	45.95	7.12	36.33
12	Oct-14	2,267	1,002	351	841	17.41	42.87	6.47	36.46
12	Nov-14	1,896	934	310	605	14.56	39.96	5.72	26.23
12	Dec-14	1,496	715	284	460	11.49	30.59	5.24	19.94
12	Jan-15	1,807	938	299	523	13.88	40.13	5.51	22.68
12	Feb-15	1,652	864	312	434	12.69	36.96	5.75	18.82
12	Mar-15	2,461	1,174	389	808	18.90	50.23	7.17	35.03
12	Apr-15	1,789	941	227	574	13.74	40.26	4.19	24.89
12	May-15	2,154	1,169	307	634	16.54	50.01	5.66	27.49
12	Jun-15	2,054	943	279	766	15.77	40.34	5.15	33.21
12	Jul-15	2,042	1,055	283	662	15.68	45.14	5.22	28.70
12	Aug-15	2,330	1,202	341	739	17.89	51.42	6.29	32.04
12	Sep-15	2,622	1,276	426	851	20.14	54.59	7.86	36.90
12	Oct-15	2,363	1,314	357	643	18.15	56.22	6.58	27.88
12	Nov-15	2,185	1,200	340	581	16.78	51.34	6.27	25.19
12	Dec-15	1,127	578	174	345	8.65	24.73	3.21	14.96
12	Jan-16	392	224	52	112	3.01	9.58	0.96	4.86
12	Feb-16	321	152	31	135	2.47	6.50	0.57	5.85
12	Mar-16	420	184	39	193	3.23	7.87	0.72	8.37



12	Apr-16	352	158	40	153	2.70	6.76	0.74	6.63
12	May-16	423	156	46	219	3.25	6.67	0.85	9.50
12	Jun-16	423	167	48	204	3.25	7.14	0.89	8.84
14	Jan-14	1,261	299	382	521	10.55	34.32	7.01	14.95
14	Feb-14	1,391	295	409	619	11.64	33.86	7.51	17.76
14	Mar-14	1,730	342	401	904	14.48	39.25	7.36	25.94
14	Apr-14	1,023	248	187	551	8.56	28.46	3.43	15.81
14	May-14	910	262	149	472	7.62	30.07	2.74	13.54
14	Jun-14	913	222	145	513	7.64	25.48	2.66	14.72
14	Jul-14	785	225	167	361	6.57	25.82	3.07	10.36
14	Aug-14	820	176	179	435	6.86	20.20	3.29	12.48
14	Sep-14	805	187	169	415	6.74	21.46	3.10	11.91
14	Oct-14	774	171	178	399	6.48	19.63	3.27	11.45
14	Nov-14	665	161	143	334	5.57	18.48	2.63	9.58
14	Dec-14	565	129	131	286	4.73	14.81	2.41	8.21
14	Jan-15	806	189	193	394	6.75	21.69	3.54	11.31
14	Feb-15	788	184	203	366	6.60	21.12	3.73	10.50
14	Mar-15	865	198	173	451	7.24	22.72	3.18	12.94
14	Apr-15	762	207	146	382	6.38	23.76	2.68	10.96
14	May-15	682	171	150	330	5.71	19.63	2.75	9.47
14	Jun-15	742	150	188	382	6.21	17.22	3.45	10.96
14	Jul-15	917	251	205	417	7.68	28.81	3.76	11.97
14	Aug-15	753	192	178	354	6.30	22.04	3.27	10.16
14	Sep-15	755	203	163	356	6.32	23.30	2.99	10.21
14	Oct-15	663	146	151	333	5.55	16.76	2.77	9.55
14	Nov-15	546	119	127	278	4.57	13.66	2.33	7.98
14	Dec-15	365	82	106	164	3.06	9.41	1.95	4.71
14	Jan-16	131	24	18	85	1.10	2.75	0.33	2.44
14	Feb-16	97	30	15	50	0.81	3.44	0.28	1.43
14	Mar-16	136	37	21	75	1.14	4.25	0.39	2.15

14	Apr-16	163	32	23	106	1.36	3.67	0.42	3.04
14	May-16	167	38	27	100	1.40	4.36	0.50	2.87
14	Jun-16	222	63	22	135	1.86	7.23	0.40	3.87
15	Jan-14	2,935	2,669	107	136	49.56	48.57	88.84	194.18
15	Feb-14	3,853	3,485	160	168	65.06	63.42	132.85	239.87
15	Mar-14	4,595	4,125	188	238	77.59	75.07	156.10	339.82
15	Apr-14	3,106	2,880	99	95	52.45	52.41	82.20	135.64
15	May-14	3,154	2,943	111	80	53.26	53.56	92.16	114.22
15	Jun-14	3,851	3,596	118	118	65.03	65.44	97.98	168.48
15	Jul-14	3,782	3,525	110	105	63.86	64.15	91.33	149.92
15	Aug-14	3,810	3,501	130	152	64.33	63.71	107.94	217.02
15	Sep-14	3,193	2,888	128	150	53.92	52.55	106.28	214.17
15	Oct-14	3,118	2,839	122	130	52.65	51.66	101.30	185.61
15	Nov-14	2,809	2,548	93	139	47.43	46.37	77.22	198.46
15	Dec-14	2,196	1,999	91	91	37.08	36.38	75.56	129.93
15	Jan-15	3,080	2,755	135	154	52.01	50.13	112.09	219.88
15	Feb-15	2,991	2,708	101	152	50.50	49.28	83.86	217.02
15	Mar-15	3,884	3,580	131	151	65.58	65.15	108.77	215.60
15	Apr-15	2,844	2,646	76	106	48.02	48.15	63.10	151.35
15	May-15	2,488	2,291	76	106	42.01	41.69	63.10	151.35
15	Jun-15	2,199	2,025	73	89	37.13	36.85	60.61	127.07
15	Jul-15	2,444	2,234	87	113	41.27	40.65	72.24	161.34
15	Aug-15	2,285	2,099	73	93	38.58	38.20	60.61	132.78
15	Sep-15	2,944	2,687	118	121	49.71	48.90	97.98	172.76
15	Oct-15	2,453	2,263	75	101	41.42	41.18	62.27	144.21
15	Nov-15	2,064	1,888	69	84	34.85	34.36	57.29	119.93
15	Dec-15	1,321	1,187	54	66	22.31	21.60	44.84	94.23
15	Jan-16	408	385	7	13	6.89	7.01	5.81	18.56
15	Feb-16	361	337	10	9	6.10	6.13	8.30	12.85
15	Mar-16	557	540	7	7	9.41	9.83	5.81	9.99

15	Apr-16	751	712	13	24	12.68	12.96	10.79	34.27
15	May-16	591	565	11	12	9.98	10.28	9.13	17.13
15	Jun-16	535	494	20	20	9.03	8.99	16.61	28.56
16	Jan-14	1,727	285	855	507	8.41	118.15	6.20	18.24
16	Feb-14	1,834	322	918	518	8.93	133.49	6.66	18.64
16	Mar-14	2,019	287	998	652	9.83	118.98	7.24	23.46
16	Apr-14	1,584	180	780	560	7.71	74.62	5.66	20.15
16	May-14	1,554	207	775	505	7.56	85.81	5.62	18.17
16	Jun-14	1,312	226	630	393	6.39	93.69	4.57	14.14
16	Jul-14	1,229	200	593	377	5.98	82.91	4.30	13.56
16	Aug-14	1,142	167	591	334	5.56	69.23	4.29	12.02
16	Sep-14	1,407	220	694	432	6.85	91.20	5.03	15.54
16	Oct-14	1,298	211	664	375	6.32	87.47	4.82	13.49
16	Nov-14	1,071	145	517	364	5.21	60.11	3.75	13.10
16	Dec-14	927	141	458	284	4.51	58.45	3.32	10.22
16	Jan-15	1,229	191	646	350	5.98	79.18	4.69	12.59
16	Feb-15	1,001	146	470	344	4.87	60.53	3.41	12.38
16	Mar-15	1,387	189	609	537	6.75	78.35	4.42	19.32
16	Apr-15	737	115	370	221	3.59	47.67	2.68	7.95
16	May-15	671	114	302	227	3.27	47.26	2.19	8.17
16	Jun-15	595	110	256	206	2.90	45.60	1.86	7.41
16	Jul-15	586	108	293	157	2.85	44.77	2.13	5.65
16	Aug-15	625	114	284	210	3.04	47.26	2.06	7.56
16	Sep-15	690	119	293	245	3.36	49.33	2.13	8.81
16	Oct-15	708	153	289	243	3.45	63.43	2.10	8.74
16	Nov-15	623	107	272	218	3.03	44.36	1.97	7.84
16	Dec-15	472	94	222	142	2.30	38.97	1.61	5.11
16	Jan-16	234	55	118	57	1.14	22.80	0.86	2.05
16	Feb-16	174	54	80	37	0.85	22.39	0.58	1.33
16	Mar-16	234	59	97	73	1.14	24.46	0.70	2.63

16	Apr-16	201	60	85	51	0.98	24.87	0.62	1.83
16	May-16	243	63	115	59	1.18	26.12	0.83	2.12
16	Jun-16	237	39	117	75	1.15	16.17	0.85	2.70
17	Jan-14	832	119	251	406	5.59	26.04	4.42	10.47
17	Feb-14	1,056	145	307	533	7.10	31.73	5.40	13.74
17	Mar-14	1,344	150	381	736	9.03	32.82	6.70	18.97
17	Apr-14	1,080	156	242	636	7.26	34.14	4.26	16.40
17	May-14	770	111	170	458	5.18	24.29	2.99	11.81
17	Jun-14	814	127	198	444	5.47	27.79	3.48	11.45
17	Jul-14	966	165	237	503	6.49	36.11	4.17	12.97
17	Aug-14	963	149	286	485	6.47	32.61	5.03	12.50
17	Sep-14	854	113	228	459	5.74	24.73	4.01	11.83
17	Oct-14	824	133	216	438	5.54	29.10	3.80	11.29
17	Nov-14	781	129	222	392	5.25	28.23	3.91	10.11
17	Dec-14	613	82	187	307	4.12	17.94	3.29	7.91
17	Jan-15	784	123	236	390	5.27	26.92	4.15	10.05
17	Feb-15	599	85	145	343	4.03	18.60	2.55	8.84
17	Mar-15	977	141	241	536	6.57	30.86	4.24	13.82
17	Apr-15	646	111	182	318	4.34	24.29	3.20	8.20
17	May-15	678	87	180	370	4.56	19.04	3.17	9.54
17	Jun-15	709	93	178	392	4.77	20.35	3.13	10.11
17	Jul-15	962	170	276	476	6.47	37.20	4.86	12.27
17	Aug-15	915	150	256	478	6.15	32.82	4.51	12.32
17	Sep-15	926	163	302	407	6.22	35.67	5.31	10.49
17	Oct-15	885	147	284	416	5.95	32.17	5.00	10.72
17	Nov-15	806	119	248	400	5.42	26.04	4.36	10.31
17	Dec-15	525	79	179	245	3.53	17.29	3.15	6.32
17	Jan-16	157	22	44	85	1.06	4.81	0.77	2.19
17	Feb-16	143	16	27	94	0.96	3.50	0.48	2.42
17	Mar-16	218	36	62	114	1.47	7.88	1.09	2.94

17	Apr-16	188	32	51	99	1.26	7.00	0.90	2.55
17	May-16	249	35	69	136	1.67	7.66	1.21	3.51
17	Jun-16	207	40	50	109	1.39	8.75	0.88	2.81
18	Jan-14	1,691	877	560	152	13.98	90.75	6.23	31.12
18	Feb-14	1,760	981	531	157	14.56	101.51	5.91	32.14
18	Mar-14	2,091	1,144	674	172	17.29	118.38	7.50	35.21
18	Apr-14	1,744	990	514	153	14.42	102.45	5.72	31.32
18	May-14	1,987	1,217	481	215	16.43	125.94	5.35	44.01
18	Jun-14	2,024	1,116	634	200	16.74	115.48	7.05	40.94
18	Jul-14	2,042	1,203	572	175	16.89	124.49	6.36	35.83
18	Aug-14	1,993	1,195	547	192	16.48	123.66	6.09	39.31
18	Sep-14	1,965	1,293	421	180	16.25	133.80	4.68	36.85
18	Oct-14	1,803	1,207	375	165	14.91	124.90	4.17	33.78
18	Nov-14	1,383	896	346	99	11.44	92.72	3.85	20.27
18	Dec-14	1,249	821	285	103	10.33	84.96	3.17	21.09
18	Jan-15	1,962	1,291	411	159	16.23	133.59	4.57	32.55
18	Feb-15	1,510	861	426	140	12.49	89.10	4.74	28.66
18	Mar-15	2,052	1,231	553	179	16.97	127.38	6.15	36.64
18	Apr-15	1,278	833	262	131	10.57	86.20	2.91	26.82
18	May-15	1,506	897	414	135	12.45	92.82	4.61	27.64
18	Jun-15	1,671	927	498	177	13.82	95.93	5.54	36.24
18	Jul-15	1,722	947	528	190	14.24	98.00	5.87	38.90
18	Aug-15	1,623	1,012	391	164	13.42	104.72	4.35	33.57
18	Sep-15	1,355	864	314	128	11.21	89.41	3.49	26.20
18	Oct-15	1,221	755	295	116	10.10	78.13	3.28	23.75
18	Nov-15	952	596	239	81	7.87	61.67	2.66	16.58
18	Dec-15	663	414	171	51	5.48	42.84	1.90	10.44
18	Jan-16	191	126	34	24	1.58	13.04	0.38	4.91
18	Feb-16	102	75	17	7	0.84	7.76	0.19	1.43
18	Mar-16	138	100	17	17	1.14	10.35	0.19	3.48

18	Apr-16	199	164	17	14	1.65	16.97	0.19	2.87
18	May-16	175	122	22	31	1.45	12.62	0.24	6.35
18	Jun-16	153	118	18	16	1.27	12.21	0.20	3.28
19	Jan-14	1,267	534	492	179	6.11	43.07	3.16	11.24
19	Feb-14	1,538	601	584	271	7.42	48.48	3.75	17.02
19	Mar-14	1,921	797	683	346	9.27	64.29	4.39	21.73
19	Apr-14	1,889	926	564	315	9.12	74.69	3.62	19.78
19	May-14	1,798	919	553	253	8.68	74.13	3.55	15.89
19	Jun-14	1,688	917	459	265	8.15	73.97	2.95	16.64
19	Jul-14	1,958	998	562	314	9.45	80.50	3.61	19.72
19	Aug-14	1,884	939	547	311	9.09	75.74	3.52	19.53
19	Sep-14	1,398	687	440	209	6.75	55.42	2.83	13.12
19	Oct-14	1,370	656	448	199	6.61	52.92	2.88	12.50
19	Nov-14	1,273	562	452	182	6.14	45.33	2.90	11.43
19	Dec-14	1,198	535	429	181	5.78	43.16	2.76	11.37
19	Jan-15	1,480	646	513	237	7.14	52.11	3.30	14.88
19	Feb-15	1,252	522	438	213	6.04	42.11	2.81	13.37
19	Mar-15	1,769	736	594	314	8.54	59.37	3.82	19.72
19	Apr-15	1,363	581	446	264	6.58	46.87	2.87	16.58
19	May-15	1,141	530	319	213	5.51	42.75	2.05	13.37
19	Jun-15	1,126	502	347	218	5.43	40.49	2.23	13.69
19	Jul-15	1,090	480	349	213	5.26	38.72	2.24	13.37
19	Aug-15	1,158	538	381	195	5.59	43.40	2.45	12.24
19	Sep-15	1,263	635	337	224	6.10	51.22	2.17	14.06
19	Oct-15	1,004	452	330	181	4.85	36.46	2.12	11.37
19	Nov-15	852	376	277	150	4.11	30.33	1.78	9.42
19	Dec-15	588	213	216	123	2.84	17.18	1.39	7.72
19	Jan-16	185	101	47	28	0.89	8.15	0.30	1.76
19	Feb-16	98	40	28	25	0.47	3.23	0.18	1.57
19	Mar-16	149	83	35	27	0.72	6.70	0.22	1.70

19	Apr-16	197	100	39	51	0.95	8.07	0.25	3.20
19	May-16	321	192	59	63	1.55	15.49	0.38	3.96
19	Jun-16	372	201	71	88	1.80	16.21	0.46	5.53
20	Jan-14	1,067	307	429	227	12.25	31.58	8.84	24.07
20	Feb-14	1,166	338	440	287	13.39	34.77	9.06	30.43
20	Mar-14	1,224	398	451	298	14.05	40.94	9.29	31.60
20	Apr-14	925	292	284	271	10.62	30.04	5.85	28.73
20	May-14	876	255	315	235	10.06	26.23	6.49	24.92
20	Jun-14	973	328	333	263	11.17	33.74	6.86	27.88
20	Jul-14	1,116	467	340	245	12.81	48.04	7.00	25.98
20	Aug-14	1,004	393	319	223	11.53	40.43	6.57	23.64
20	Sep-14	767	262	267	182	8.81	26.95	5.50	19.30
20	Oct-14	971	377	311	209	11.15	38.78	6.40	22.16
20	Nov-14	782	265	288	174	8.98	27.26	5.93	18.45
20	Dec-14	580	236	179	104	6.66	24.28	3.69	11.03
20	Jan-15	641	224	215	146	7.36	23.04	4.43	15.48
20	Feb-15	586	206	197	122	6.73	21.19	4.06	12.94
20	Mar-15	882	305	258	261	10.13	31.37	5.31	27.67
20	Apr-15	563	210	174	143	6.46	21.60	3.58	15.16
20	May-15	600	261	175	120	6.89	26.85	3.60	12.72
20	Jun-15	512	185	158	114	5.88	19.03	3.25	12.09
20	Jul-15	500	199	129	130	5.74	20.47	2.66	13.78
20	Aug-15	449	199	125	91	5.16	20.47	2.57	9.65
20	Sep-15	484	231	137	82	5.56	23.76	2.82	8.69
20	Oct-15	421	166	128	101	4.83	17.08	2.64	10.71
20	Nov-15	392	168	106	72	4.50	17.28	2.18	7.63
20	Dec-15	283	94	107	61	3.25	9.67	2.20	6.47
20	Jan-16	116	45	30	30	1.33	4.63	0.62	3.18
20	Feb-16	98	33	24	37	1.13	3.39	0.49	3.92
20	Mar-16	151	55	28	64	1.73	5.66	0.58	6.79

20	Apr-16	123	40	24	54	1.41	4.11	0.49	5.73
20	May-16	195	55	32	98	2.24	5.66	0.66	10.39
20	Jun-16	165	60	41	60	1.89	6.17	0.84	6.36
22	Jan-14	1,573	1,410	119	33	15.31	22.72	3.40	15.84
22	Feb-14	1,663	1,462	149	33	16.18	23.56	4.25	15.84
22	Mar-14	2,265	2,043	174	37	22.04	32.92	4.97	17.75
22	Apr-14	1,800	1,619	125	39	17.52	26.09	3.57	18.71
22	May-14	1,999	1,840	108	37	19.45	29.65	3.08	17.75
22	Jun-14	1,640	1,463	132	26	15.96	23.57	3.77	12.48
22	Jul-14	2,231	2,017	153	36	21.71	32.50	4.37	17.27
22	Aug-14	1,506	1,328	143	23	14.66	21.40	4.08	11.04
22	Sep-14	1,305	1,125	137	31	12.70	18.13	3.91	14.88
22	Oct-14	1,504	1,372	89	36	14.64	22.11	2.54	17.27
22	Nov-14	1,432	1,278	121	29	13.94	20.59	3.45	13.92
22	Dec-14	1,246	1,107	96	35	12.13	17.84	2.74	16.79
22	Jan-15	1,918	1,750	115	33	18.67	28.20	3.28	15.84
22	Feb-15	1,433	1,300	95	31	13.95	20.95	2.71	14.88
22	Mar-15	1,769	1,621	104	32	17.22	26.12	2.97	15.36
22	Apr-15	1,145	1,012	98	26	11.14	16.31	2.80	12.48
22	May-15	1,284	1,164	86	26	12.50	18.76	2.45	12.48
22	Jun-15	1,147	1,014	96	26	11.16	16.34	2.74	12.48
22	Jul-15	1,390	1,263	90	25	13.53	20.35	2.57	12.00
22	Aug-15	1,285	1,143	98	31	12.51	18.42	2.80	14.88
22	Sep-15	1,517	1,390	88	23	14.76	22.40	2.51	11.04
22	Oct-15	1,694	1,544	102	38	16.49	24.88	2.91	18.23
22	Nov-15	1,190	1,038	109	28	11.58	16.73	3.11	13.44
22	Dec-15	787	677	72	22	7.66	10.91	2.06	10.56
22	Jan-16	203	187	10	4	1.98	3.01	0.29	1.92
22	Feb-16	157	144	7	5	1.53	2.32	0.20	2.40
22	Mar-16	177	157	11	8	1.72	2.53	0.31	3.84



22	Apr-16	245	232	10	1	2.38	3.74	0.29	0.48
22	May-16	193	179	6	5	1.88	2.88	0.17	2.40
22	Jun-16	247	221	21	2	2.40	3.56	0.60	0.96
24	Jan-14	1,121	578	269	179	7.93	23.52	4.32	8.92
24	Feb-14	1,897	864	482	383	13.42	35.16	7.74	19.08
24	Mar-14	2,130	1,037	541	383	15.06	42.20	8.69	19.08
24	Apr-14	1,529	981	272	213	10.81	39.92	4.37	10.61
24	May-14	1,947	1,114	366	364	13.77	45.34	5.88	18.13
24	Jun-14	2,205	1,212	464	404	15.59	49.33	7.45	20.12
24	Jul-14	2,408	1,348	469	456	17.03	54.86	7.53	22.71
24	Aug-14	2,071	1,157	402	396	14.65	47.09	6.45	19.73
24	Sep-14	1,896	1,034	379	362	13.41	42.08	6.09	18.03
24	Oct-14	1,972	1,088	402	319	13.95	44.28	6.45	15.89
24	Nov-14	1,420	744	339	235	10.04	30.28	5.44	11.71
24	Dec-14	1,435	782	272	263	10.15	31.83	4.37	13.10
24	Jan-15	1,770	874	429	337	12.52	35.57	6.89	16.79
24	Feb-15	1,637	790	420	307	11.58	32.15	6.74	15.29
24	Mar-15	2,441	1,232	598	433	17.26	50.14	9.60	21.57
24	Apr-15	1,729	973	385	277	12.23	39.60	6.18	13.80
24	May-15	2,061	1,160	415	345	14.58	47.21	6.66	17.19
24	Jun-15	1,983	1,133	377	348	14.02	46.11	6.05	17.33
24	Jul-15	2,074	1,144	380	408	14.67	46.56	6.10	20.32
24	Aug-15	1,910	1,134	299	357	13.51	46.15	4.80	17.78
24	Sep-15	2,047	1,184	331	384	14.48	48.19	5.31	19.13
24	Oct-15	1,944	1,170	352	299	13.75	47.62	5.65	14.89
24	Nov-15	1,441	821	262	242	10.19	33.41	4.21	12.05
24	Dec-15	958	515	193	170	6.78	20.96	3.10	8.47
24	Jan-16	365	187	71	95	2.58	7.61	1.14	4.73
24	Feb-16	252	124	45	75	1.78	5.05	0.72	3.74
24	Mar-16	319	168	49	86	2.26	6.84	0.79	4.28

24	Apr-16	274	148	47	67	1.94	6.02	0.75	3.34
24	May-16	437	253	63	105	3.09	10.30	1.01	5.23
24	Jun-16	457	201	106	134	3.23	8.18	1.70	6.67
25	Jan-14	2,451	1,026	248	1,138	12.24	31.31	8.78	19.68
25	Feb-14	2,830	1,181	244	1,340	14.13	36.04	8.64	23.18
25	Mar-14	3,011	1,224	267	1,470	15.04	37.36	9.46	25.43
25	Apr-14	2,101	714	182	1,162	10.49	21.79	6.45	20.10
25	May-14	2,155	830	209	1,065	10.76	25.33	7.40	18.42
25	Jun-14	2,319	893	198	1,187	11.58	27.25	7.01	20.53
25	Jul-14	2,501	1,074	244	1,124	12.49	32.78	8.64	19.44
25	Aug-14	2,734	987	297	1,389	13.65	30.12	10.52	24.03
25	Sep-14	2,692	997	270	1,365	13.44	30.43	9.56	23.61
25	Oct-14	2,629	1,166	232	1,176	13.13	35.59	8.22	20.34
25	Nov-14	2,489	1,119	257	1,066	12.43	34.15	9.10	18.44
25	Dec-14	1,813	742	176	867	9.05	22.65	6.23	15.00
25	Jan-15	3,293	1,348	361	1,521	16.45	41.14	12.79	26.31
25	Feb-15	3,086	1,091	367	1,566	15.41	33.30	13.00	27.09
25	Mar-15	3,648	1,460	347	1,771	18.22	44.56	12.29	30.63
25	Apr-15	2,903	1,202	232	1,407	14.50	36.69	8.22	24.34
25	May-15	2,770	1,111	257	1,342	13.83	33.91	9.10	23.21
25	Jun-15	2,525	839	264	1,361	12.61	25.61	9.35	23.54
25	Jul-15	3,064	1,205	324	1,467	15.30	36.78	11.47	25.38
25	Aug-15	2,516	1,074	264	1,125	12.57	32.78	9.35	19.46
25	Sep-15	2,520	961	252	1,252	12.59	29.33	8.92	21.66
25	Oct-15	2,418	1,038	240	1,078	12.08	31.68	8.50	18.65
25	Nov-15	2,272	840	248	1,139	11.35	25.64	8.78	19.70
25	Dec-15	1,167	434	158	557	5.83	13.25	5.60	9.63
25	Jan-16	338	127	37	170	1.69	3.88	1.31	2.94
25	Feb-16	291	131	21	139	1.45	4.00	0.74	2.40
25	Mar-16	442	157	40	236	2.21	4.79	1.42	4.08

25	Apr-16	451	160	48	242	2.25	4.88	1.70	4.19
25	May-16	571	191	46	329	2.85	5.83	1.63	5.69
25	Jun-16	433	173	46	210	2.16	5.28	1.63	3.63

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Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, and Investigatory Stop Reports.

APPENDIX C: District-Level Stops Per 100 Previous Month's Arrests, February 2014 – June 2016

District	Month and Year	Rates per 100 Previous Month's Violent Arrests				Rates Per 100 Previous Month's Total Arrests			
		All	Black	White	Hispanic	All	Black	White	Hispanic
01	Feb-14	36,825.0	46,800.0	0.0	5,650.0	314.7	258.6	541.9	342.4
01	Mar-14	45,675.0	28,100.0	0.0	0.0	379.0	300.5	794.6	371.4
01	Apr-14	27,260.0	17,740.0	0.0	0.0	244.3	207.7	396.3	222.0
01	May-14	10,400.0	6,676.9	0.0	0.0	306.6	259.1	513.6	391.7
01	Jun-14	11,800.0	7,345.5	0.0	0.0	291.7	236.3	525.4	353.3
01	Jul-14	12,350.0	9,625.0	31,900.0	0.0	295.5	255.8	469.1	322.6
01	Aug-14	12,470.0	7,650.0	0.0	0.0	288.7	243.6	406.5	383.3
01	Sep-14	15,450.0	15,940.0	29,400.0	4,800.0	285.5	255.4	363.0	331.0
01	Oct-14	13,763.6	11,133.3	33,400.0	10,500.0	354.6	309.3	506.1	350.0
01	Nov-14	20,800.0	15,700.0	33,700.0	0.0	336.3	289.0	481.4	422.2
01	Dec-14	19,333.3	12,816.7	0.0	0.0	316.9	298.1	407.9	280.0
01	Jan-15	34,850.0	30,366.7	0.0	9,700.0	417.4	396.1	462.9	421.7
01	Feb-15	42,766.7	29,200.0	0.0	0.0	325.6	288.2	527.5	306.9
01	Mar-15	28,833.3	23,740.0	33,500.0	0.0	569.1	525.2	632.1	645.5
01	Apr-15	11,144.4	9,428.6	0.0	8,500.0	246.4	217.1	374.1	229.7
01	May-15	42,250.0	25,800.0	0.0	0.0	243.5	200.8	328.6	442.9
01	Jun-15	14,050.0	10,140.0	21,300.0	0.0	236.8	203.6	355.0	227.5
01	Jul-15	11,712.5	7,400.0	0.0	0.0	277.2	220.4	397.1	414.3
01	Aug-15	12,114.3	8,257.1	0.0	0.0	239.5	254.6	232.4	161.9
01	Sep-15	11,133.3	11,833.3	18,700.0	7,800.0	270.1	275.2	292.2	236.4
01	Oct-15	6,568.4	5,675.0	19,300.0	4,400.0	317.6	312.0	271.8	325.9
01	Nov-15	8,536.4	6,460.0	15,800.0	0.0	226.3	222.0	219.4	250.0
01	Dec-15	5,960.0	4,355.6	12,700.0	0.0	154.0	147.9	146.0	192.3
01	Jan-16	5,366.7	4,166.7	0.0	0.0	44.6	49.2	42.2	28.1
01	Feb-16	1,200.0	871.4	0.0	0.0	24.6	24.1	22.9	31.4
01	Mar-16	958.8	821.4	3,000.0	700.0	53.6	61.8	50.0	31.1
01	Apr-16	1,281.8	1,080.0	2,400.0	0.0	43.9	46.0	58.5	26.5
01	May-16	2,433.3	1,950.0	0.0	0.0	46.8	52.9	41.3	27.8
01	Jun-16	2,720.0	2,850.0	1,500.0	0.0	61.8	77.6	30.0	37.5
02	Feb-14	18,318.8	19,450.0	0.0	5,300.0	745.8	726.1	1,485.7	588.9
02	Mar-14	26,658.3	25,008.3	0.0	0.0	758.1	741.0	1,357.1	588.9
02	Apr-14	16,568.8	15,475.0	0.0	0.0	558.1	546.6	918.2	475.0
02	May-14	17,246.7	20,283.3	0.0	1,366.7	601.6	592.2	860.0	455.6
02	Jun-14	7,134.3	7,693.5	0.0	675.0	488.6	491.8	610.0	207.7
02	Jul-14	16,252.6	16,322.2	0.0	2,800.0	655.6	641.5	1,480.0	400.0
02	Aug-14	12,683.3	13,022.7	0.0	2,550.0	575.4	561.8	1,328.6	463.6
02	Sep-14	13,373.7	12,715.8	0.0	0.0	573.6	569.8	572.7	528.6
02	Oct-14	18,723.5	21,492.9	0.0	1,300.0	955.9	955.2	1,500.0	487.5

02	Nov-14	17,727.8	16,638.9	0.0	0.0	991.0	988.4	1,916.7	381.8
02	Dec-14	27,760.0	43,266.7	4,800.0	2,400.0	1,201.7	1,201.9	1,920.0	480.0
02	Jan-15	29,981.8	28,236.4	0.0	0.0	1,329.8	1,288.8	3,600.0	1,075.0
02	Feb-15	14,026.3	12,952.6	0.0	0.0	912.7	908.1	876.9	1,080.0
02	Mar-15	67,700.0	62,620.0	0.0	0.0	1,589.2	1,613.9	2,214.3	350.0
02	Apr-15	35,600.0	32,885.7	0.0	0.0	771.5	745.0	1,171.4	800.0
02	May-15	12,109.1	11,263.6	0.0	0.0	1,020.7	1,003.2	1,071.4	860.0
02	Jun-15	23,481.8	24,260.0	0.0	3,200.0	852.5	872.7	733.3	246.2
02	Jul-15	21,863.6	24,866.7	8,700.0	3,400.0	907.5	913.5	1,740.0	283.3
02	Aug-15	17,486.7	17,450.0	8,700.0	0.0	862.8	860.2	870.0	583.3
02	Sep-15	35,100.0	32,788.9	0.0	0.0	1,219.7	1,224.5	1,600.0	409.1
02	Oct-15	34,490.0	32,200.0	0.0	0.0	1,185.2	1,145.9	2,080.0	1,250.0
02	Nov-15	18,660.0	17,433.3	0.0	0.0	746.4	751.4	518.8	360.0
02	Dec-15	16,381.8	15,054.5	0.0	0.0	606.7	593.5	714.3	340.0
02	Jan-16	10,133.3	9,900.0	0.0	0.0	132.8	134.4	166.7	20.0
02	Feb-16	2,290.0	2,210.0	0.0	0.0	110.6	113.9	42.9	40.0
02	Mar-16	3,450.0	3,287.5	0.0	0.0	122.1	122.3	150.0	120.0
02	Apr-16	3,836.4	3,745.5	0.0	0.0	150.7	160.9	70.0	9.1
02	May-16	3,273.3	3,357.1	900.0	0.0	184.6	185.8	112.5	140.0
02	Jun-16	7,375.0	7,175.0	0.0	0.0	500.0	499.1	233.3	0.0
03	Feb-14	24,000.0	23,038.9	0.0	0.0	874.5	858.6	1,200.0	1,275.0
03	Mar-14	34,900.0	33,826.7	0.0	0.0	1,095.2	1,095.9	1,700.0	500.0
03	Apr-14	35,569.2	37,400.0	0.0	0.0	777.1	769.8	916.7	920.0
03	May-14	16,452.0	17,369.6	6,200.0	4,100.0	707.9	700.9	1,240.0	1,366.7
03	Jun-14	19,442.1	18,868.4	0.0	0.0	636.9	626.7	1,075.0	1,166.7
03	Jul-14	47,400.0	46,100.0	0.0	0.0	707.5	693.8	1,766.7	1,700.0
03	Aug-14	19,680.0	19,933.3	5,000.0	0.0	785.9	772.9	1,250.0	3,100.0
03	Sep-14	22,047.6	21,395.2	0.0	0.0	790.1	774.7	1,350.0	5,100.0
03	Oct-14	18,708.7	18,091.3	0.0	0.0	761.6	751.1	1,340.0	1,833.3
03	Nov-14	26,929.4	26,052.9	0.0	0.0	823.4	806.7	1,550.0	6,800.0
03	Dec-14	26,953.3	25,846.7	0.0	0.0	854.8	835.6	2,666.7	1,180.0
03	Jan-15	17,268.0	16,672.0	0.0	0.0	868.6	859.4	1,166.7	1,060.0
03	Feb-15	17,884.2	18,088.9	5,500.0	0.0	733.9	710.9	1,833.3	5,700.0
03	Mar-15	27,661.1	26,805.6	0.0	0.0	1,279.9	1,250.0	3,800.0	4,900.0
03	Apr-15	21,768.8	22,753.3	0.0	0.0	660.9	655.1	1,233.3	0.0
03	May-15	22,106.3	21,568.8	0.0	0.0	760.6	753.5	1,500.0	1,450.0
03	Jun-15	39,455.6	38,277.8	0.0	0.0	855.7	844.4	1,075.0	5,400.0
03	Jul-15	14,026.9	13,630.8	0.0	0.0	840.3	833.9	980.0	633.3
03	Aug-15	27,376.9	26,630.8	0.0	0.0	775.4	781.5	1,025.0	300.0
03	Sep-15	19,958.8	20,606.3	0.0	3,600.0	666.6	659.4	900.0	1,800.0
03	Oct-15	17,833.3	17,309.5	0.0	0.0	936.3	922.6	1,700.0	1,666.7
03	Nov-15	30,150.0	29,380.0	0.0	0.0	810.5	807.1	3,200.0	1,000.0
03	Dec-15	13,863.6	14,460.0	0.0	2,100.0	406.7	391.9	1,500.0	1,050.0

03	Jan-16	4,490.9	4,354.5	0.0	0.0	156.8	157.0	450.0	50.0
03	Feb-16	1,250.0	1,350.0	100.0	0.0	89.3	89.0	100.0	50.0
03	Mar-16	7,042.9	6,942.9	0.0	0.0	163.2	164.2	150.0	33.3
03	Apr-16	2,627.8	2,572.2	0.0	0.0	126.5	126.5	150.0	0.0
03	May-16	3,500.0	3,420.0	0.0	0.0	192.8	190.0	500.0	400.0
03	Jun-16	8,550.0	8,400.0	0.0	0.0	345.5	342.9	0.0	300.0
04	Feb-14	6,218.8	5,173.3	4,300.0	0.0	453.3	425.2	409.5	648.1
04	Mar-14	13,486.4	12,522.2	16,100.0	25,850.0	622.0	572.1	1,238.5	795.4
04	Apr-14	16,900.0	16,752.9	0.0	13,275.0	708.4	717.4	604.8	680.8
04	May-14	18,760.0	16,078.6	0.0	42,600.0	431.6	427.9	420.0	463.0
04	Jun-14	14,195.5	13,163.2	0.0	16,000.0	473.9	481.0	686.7	413.8
04	Jul-14	10,166.7	9,660.9	0.0	10,025.0	394.4	381.8	352.0	466.3
04	Aug-14	14,715.4	13,830.4	0.0	17,433.3	561.0	575.2	340.7	544.8
04	Sep-14	16,571.4	14,525.0	0.0	44,700.0	438.3	423.5	395.7	545.1
04	Oct-14	10,458.6	12,500.0	0.0	4,633.3	391.4	382.8	472.2	417.0
04	Nov-14	15,047.1	16,076.9	0.0	8,925.0	380.1	373.2	537.5	420.0
04	Dec-14	15,386.7	15,115.4	7,000.0	25,400.0	374.7	381.6	388.9	317.5
04	Jan-15	19,841.2	19,428.6	6,100.0	49,500.0	634.0	625.3	762.5	626.6
04	Feb-15	9,744.0	8,129.2	0.0	38,200.0	373.6	359.3	336.4	465.9
04	Mar-15	21,433.3	23,581.8	10,600.0	15,966.7	594.3	589.5	460.9	638.7
04	Apr-15	12,618.8	10,725.0	0.0	0.0	298.7	301.6	275.0	298.7
04	May-15	9,476.5	10,875.0	6,200.0	5,650.0	291.8	277.7	413.3	347.7
04	Jun-15	12,418.2	9,618.2	0.0	0.0	247.9	232.0	205.0	377.6
04	Jul-15	9,805.3	10,657.1	6,200.0	7,075.0	333.9	339.9	442.9	285.9
04	Aug-15	8,618.2	8,068.4	6,400.0	14,250.0	333.8	331.8	376.5	343.4
04	Sep-15	12,241.2	12,669.2	0.0	8,325.0	339.5	327.4	350.0	396.4
04	Oct-15	9,761.1	9,093.3	0.0	10,366.7	318.3	316.5	305.9	304.9
04	Nov-15	5,714.3	4,614.8	0.0	26,400.0	315.6	297.4	400.0	400.0
04	Dec-15	4,471.4	3,366.7	0.0	0.0	192.4	177.6	134.8	296.9
04	Jan-16	4,815.4	5,210.0	0.0	4,350.0	166.0	175.4	107.7	145.0
04	Feb-16	2,157.1	1,925.0	0.0	5,700.0	113.8	117.4	112.5	96.6
04	Mar-16	2,496.0	2,290.5	1,300.0	4,233.3	156.0	145.3	118.2	235.2
04	Apr-16	3,377.8	2,758.8	0.0	11,400.0	141.4	129.9	287.5	196.6
04	May-16	4,005.9	4,383.3	0.0	2,700.0	199.7	189.2	106.7	293.5
04	Jun-16	5,108.3	4,316.7	0.0	0.0	256.5	252.7	142.9	356.5
05	Feb-14	39,560.0	38,080.0	0.0	0.0	915.7	931.1	655.6	430.8
05	Mar-14	23,933.3	24,482.4	0.0	6,000.0	1,008.9	1,007.7	1,040.0	857.1
05	Apr-14	17,243.5	20,047.4	5,400.0	4,100.0	751.1	745.4	1,080.0	820.0
05	May-14	34,062.5	37,371.4	7,900.0	0.0	1,032.2	1,029.9	790.0	1,250.0
05	Jun-14	20,426.3	20,627.8	0.0	8,700.0	544.3	535.0	1,016.7	669.2
05	Jul-14	12,513.0	12,026.1	0.0	0.0	442.8	445.4	300.0	392.3
05	Aug-14	17,335.0	16,725.0	0.0	0.0	582.7	591.0	366.7	446.2
05	Sep-14	14,515.8	14,611.1	0.0	7,400.0	515.5	506.7	512.5	925.0

05	Oct-14	10,424.0	11,522.7	2,400.0	1,750.0	514.0	515.2	400.0	500.0
05	Nov-14	11,868.2	13,268.4	1,650.0	4,200.0	478.2	479.3	412.5	525.0
05	Dec-14	8,081.8	7,850.0	0.0	0.0	349.3	353.2	250.0	210.0
05	Jan-15	15,175.0	15,553.3	0.0	3,600.0	506.9	512.7	400.0	450.0
05	Feb-15	14,892.9	14,385.7	0.0	0.0	367.1	372.3	200.0	222.2
05	Mar-15	16,778.6	17,330.8	0.0	4,700.0	545.0	540.3	775.0	587.5
05	Apr-15	9,050.0	8,843.8	0.0	0.0	300.4	305.6	333.3	50.0
05	May-15	16,336.4	15,900.0	0.0	0.0	406.6	414.5	214.3	218.2
05	Jun-15	10,827.8	12,480.0	3,400.0	1,750.0	410.3	416.9	226.7	388.9
05	Jul-15	14,346.7	13,953.3	0.0	0.0	500.5	506.8	238.5	400.0
05	Aug-15	24,133.3	23,511.1	0.0	0.0	468.1	470.2	300.0	325.0
05	Sep-15	14,020.0	14,571.4	2,100.0	0.0	528.4	528.5	210.0	1,750.0
05	Oct-15	18,700.0	20,044.4	0.0	2,400.0	502.7	503.9	311.1	800.0
05	Nov-15	15,525.0	15,075.0	0.0	0.0	554.5	556.6	312.5	950.0
05	Dec-15	12,812.5	12,300.0	0.0	0.0	306.9	309.4	250.0	133.3
05	Jan-16	2,000.0	1,920.0	0.0	0.0	69.2	69.8	66.7	33.3
05	Feb-16	786.7	800.0	0.0	0.0	37.9	37.1	66.7	25.0
05	Mar-16	2,354.5	2,290.9	0.0	0.0	88.1	88.7	50.0	50.0
05	Apr-16	10,350.0	9,875.0	0.0	0.0	111.3	109.1	350.0	83.3
05	May-16	3,869.2	3,753.8	0.0	0.0	124.8	124.8	85.7	175.0
05	Jun-16	4,054.5	4,280.0	0.0	1,000.0	203.7	205.8	75.0	250.0
06	Feb-14	12,569.2	12,668.0	0.0	3,500.0	603.0	597.5	716.7	875.0
06	Mar-14	33,528.6	32,442.9	0.0	0.0	807.9	806.7	684.6	1,333.3
06	Apr-14	12,176.7	11,913.3	0.0	0.0	491.0	492.3	390.0	262.5
06	May-14	12,348.1	11,970.4	0.0	0.0	519.3	512.2	1,900.0	650.0
06	Jun-14	12,116.7	11,804.2	0.0	0.0	447.4	444.0	633.3	525.0
06	Jul-14	8,520.5	8,251.3	0.0	0.0	504.2	492.0	740.0	0.0
06	Aug-14	12,069.6	12,295.5	0.0	0.0	401.7	399.6	442.9	550.0
06	Sep-14	14,305.0	13,895.0	0.0	0.0	455.6	453.3	462.5	450.0
06	Oct-14	16,110.5	15,726.3	0.0	0.0	485.9	481.2	483.3	1,200.0
06	Nov-14	8,433.3	8,682.4	1,800.0	0.0	484.2	480.8	720.0	660.0
06	Dec-14	36,771.4	35,514.3	0.0	0.0	507.7	511.5	353.8	300.0
06	Jan-15	14,204.5	14,490.5	5,200.0	0.0	639.1	635.3	577.8	1,900.0
06	Feb-15	11,572.7	11,281.8	0.0	0.0	414.0	413.7	320.0	360.0
06	Mar-15	15,340.9	14,913.6	0.0	0.0	669.6	668.2	477.8	2,600.0
06	Apr-15	16,457.1	15,992.9	0.0	0.0	362.3	358.2	660.0	420.0
06	May-15	14,160.0	13,846.7	0.0	0.0	342.0	337.7	3,100.0	225.0
06	Jun-15	7,584.0	7,432.0	0.0	0.0	295.8	294.5	320.0	275.0
06	Jul-15	16,923.1	16,392.3	0.0	0.0	359.5	360.6	230.8	242.9
06	Aug-15	13,493.8	13,087.5	0.0	0.0	358.6	356.7	400.0	260.0
06	Sep-15	15,182.4	14,841.2	0.0	0.0	438.9	439.5	211.1	800.0
06	Oct-15	10,926.1	10,613.0	0.0	0.0	385.4	380.2	411.1	1,900.0
06	Nov-15	13,700.0	13,357.9	0.0	0.0	439.0	430.9	1,650.0	750.0

06	Dec-15	8,281.3	8,006.3	0.0	0.0	247.2	243.5	300.0	375.0
06	Jan-16	6,712.5	6,425.0	0.0	0.0	120.4	117.6	175.0	266.7
06	Feb-16	2,360.0	2,250.0	0.0	0.0	57.7	57.1	62.5	60.0
06	Mar-16	2,923.1	2,800.0	0.0	0.0	89.8	90.1	57.1	150.0
06	Apr-16	2,352.6	2,257.9	0.0	0.0	78.0	77.2	50.0	180.0
06	May-16	2,357.1	2,285.7	0.0	0.0	94.3	93.9	70.0	0.0
06	Jun-16	4,130.0	4,050.0	0.0	0.0	154.7	154.6	133.3	200.0
07	Feb-14	20,413.6	19,559.1	0.0	0.0	751.0	736.8	3,350.0	1,357.1
07	Mar-14	19,682.8	19,585.7	0.0	0.0	970.7	963.8	1,037.5	1,300.0
07	Apr-14	32,856.3	31,625.0	0.0	0.0	777.7	770.2	1,933.3	633.3
07	May-14	21,820.8	21,191.7	0.0	0.0	722.3	722.4	510.0	700.0
07	Jun-14	20,496.0	19,868.0	0.0	0.0	740.5	732.6	985.7	1,240.0
07	Jul-14	9,783.0	9,463.8	0.0	0.0	628.1	623.0	757.1	600.0
07	Aug-14	15,800.0	15,313.8	0.0	0.0	640.8	630.8	980.0	1,066.7
07	Sep-14	17,892.0	17,304.0	0.0	0.0	675.7	673.8	688.9	875.0
07	Oct-14	20,277.8	20,400.0	6,700.0	0.0	855.5	852.7	957.1	937.5
07	Nov-14	23,400.0	22,595.2	0.0	0.0	690.2	675.0	2,650.0	1,920.0
07	Dec-14	23,394.1	22,523.5	0.0	0.0	669.5	669.4	642.9	675.0
07	Jan-15	22,776.2	21,971.4	0.0	0.0	785.4	774.2	1,475.0	1,044.4
07	Feb-15	30,357.1	29,007.1	0.0	0.0	638.1	627.7	1,475.0	692.9
07	Mar-15	44,158.3	42,516.7	0.0	0.0	1,079.2	1,074.1	1,016.7	1,133.3
07	Apr-15	49,283.3	47,841.7	0.0	0.0	859.6	849.3	1,440.0	1,183.3
07	May-15	45,685.7	44,628.6	0.0	0.0	751.6	750.1	1,280.0	650.0
07	Jun-15	24,957.9	24,305.3	0.0	0.0	468.6	465.5	900.0	525.0
07	Jul-15	27,652.9	26,647.1	0.0	0.0	553.7	546.4	1,240.0	818.2
07	Aug-15	16,814.8	16,792.3	6,600.0	0.0	550.3	538.3	825.0	1,880.0
07	Sep-15	20,300.0	19,592.0	0.0	0.0	615.2	606.9	614.3	1,171.4
07	Oct-15	23,722.2	23,000.0	0.0	0.0	860.9	857.7	1,100.0	838.5
07	Nov-15	26,937.5	25,968.8	0.0	0.0	536.1	526.6	675.0	1,042.9
07	Dec-15	24,290.0	25,677.8	0.0	5,100.0	387.4	380.1	500.0	510.0
07	Jan-16	5,806.7	5,580.0	0.0	0.0	194.4	191.1	220.0	466.7
07	Feb-16	3,547.1	4,128.6	0.0	366.7	115.7	113.8	300.0	110.0
07	Mar-16	16,200.0	15,800.0	0.0	0.0	171.2	172.9	128.6	37.5
07	Apr-16	10,180.0	10,000.0	0.0	0.0	184.1	185.2	183.3	100.0
07	May-16	2,600.0	2,596.7	0.0	0.0	139.4	137.9	366.7	142.9
07	Jun-16	5,214.3	5,071.4	0.0	0.0	264.5	261.0	266.7	1,100.0
08	Feb-14	12,485.7	9,694.7	22,500.0	16,900.0	651.0	662.6	1,323.5	550.2
08	Mar-14	20,972.2	13,953.3	0.0	40,733.3	704.3	747.5	826.9	605.0
08	Apr-14	26,928.6	22,270.0	7,750.0	0.0	600.3	670.8	508.2	522.5
08	May-14	25,626.7	23,690.0	0.0	21,800.0	519.5	584.9	478.9	417.6
08	Jun-14	16,830.0	16,833.3	18,700.0	15,816.7	448.8	570.6	534.3	297.5
08	Jul-14	18,010.0	13,506.3	0.0	33,566.7	517.5	568.7	627.0	407.7
08	Aug-14	16,300.0	13,476.5	20,700.0	29,150.0	560.5	612.6	524.1	492.0



08	Sep-14	14,469.6	12,137.5	18,350.0	19,840.0	442.6	507.0	421.8	360.7
08	Oct-14	21,812.5	14,821.4	0.0	51,300.0	515.5	617.6	600.0	375.8
08	Nov-14	19,746.7	13,833.3	0.0	31,666.7	470.2	494.0	458.3	443.9
08	Dec-14	22,700.0	17,462.5	36,100.0	31,500.0	449.5	485.1	591.8	381.0
08	Jan-15	30,815.4	34,071.4	7,760.0	120,000.0	898.2	1,114.5	606.3	731.7
08	Feb-15	26,933.3	13,925.0	0.0	0.0	567.0	574.2	698.6	509.3
08	Mar-15	40,336.4	49,360.0	24,750.0	35,550.0	924.4	1,028.3	883.9	790.0
08	Apr-15	26,091.7	21,337.5	0.0	25,150.0	461.1	511.1	650.8	361.9
08	May-15	25,053.8	19,211.1	12,900.0	110,200.0	585.8	570.6	841.3	537.6
08	Jun-15	15,828.6	18,720.0	12,000.0	15,171.4	579.1	714.5	610.2	423.1
08	Jul-15	17,255.6	19,725.0	38,500.0	12,311.1	518.5	485.5	621.0	530.1
08	Aug-15	15,795.2	17,850.0	7,800.0	18,500.0	615.4	714.0	661.0	489.0
08	Sep-15	24,061.5	25,514.3	10,733.3	32,500.0	498.9	551.2	460.0	433.3
08	Oct-15	13,422.2	13,556.3	34,200.0	10,720.0	669.9	806.3	684.0	491.7
08	Nov-15	54,016.7	62,000.0	32,400.0	51,400.0	541.1	570.6	600.0	487.2
08	Dec-15	11,486.7	6,630.8	0.0	32,750.0	377.0	362.2	453.5	380.8
08	Jan-16	5,126.7	4,500.0	7,600.0	5,483.3	215.4	251.7	172.7	198.2
08	Feb-16	3,650.0	4,975.0	1,200.0	4,383.3	154.4	125.9	145.5	193.4
08	Mar-16	5,492.9	2,927.3	6,700.0	18,700.0	233.0	190.5	268.0	275.0
08	Apr-16	2,457.9	1,392.9	4,600.0	5,500.0	112.3	96.1	95.8	137.5
08	May-16	6,800.0	0.0	4,500.0	3,050.0	119.6	135.7	107.1	108.9
08	Jun-16	6,600.0	5,725.0	5,000.0	8,233.3	224.7	216.0	200.0	244.6
09	Feb-14	27,700.0	24,320.0	0.0	21,700.0	653.9	560.4	1,389.3	614.2
09	Mar-14	19,457.1	14,200.0	8,660.0	47,375.0	926.5	872.6	883.7	924.4
09	Apr-14	18,814.3	10,830.0	0.0	29,900.0	543.1	555.4	640.9	508.9
09	May-14	15,610.5	9,392.3	28,400.0	27,900.0	535.4	475.1	747.4	564.8
09	Jun-14	19,947.1	16,762.5	36,200.0	20,325.0	593.9	604.1	624.1	570.5
09	Jul-14	19,450.0	11,883.3	33,300.0	41,850.0	655.6	645.2	537.1	694.6
09	Aug-14	27,700.0	24,483.3	16,300.0	34,880.0	602.2	644.3	582.1	591.2
09	Sep-14	14,454.2	8,933.3	8,425.0	42,250.0	617.3	560.7	732.6	630.6
09	Oct-14	22,086.7	11,936.4	29,300.0	81,950.0	635.9	637.4	542.6	642.7
09	Nov-14	9,427.6	9,030.8	8,100.0	9,753.8	510.1	514.9	419.0	528.3
09	Dec-14	15,516.7	9,071.4	0.0	30,225.0	663.4	668.4	665.8	650.0
09	Jan-15	35,608.3	20,788.9	35,800.0	96,950.0	1,136.4	1,075.3	1,790.0	1,114.4
09	Feb-15	28,342.9	27,000.0	17,700.0	33,000.0	804.9	891.5	786.7	726.9
09	Mar-15	43,363.6	34,800.0	41,600.0	54,100.0	1,081.6	966.7	1,540.7	1,109.7
09	Apr-15	42,577.8	24,950.0	0.0	98,750.0	703.1	650.9	568.6	771.5
09	May-15	24,611.8	11,576.9	0.0	54,275.0	793.9	741.4	771.2	825.5
09	Jun-15	13,273.1	9,575.0	10,500.0	21,322.2	683.4	527.1	562.5	888.4
09	Jul-15	19,572.2	18,414.3	10,933.3	22,837.5	759.3	708.2	762.8	797.8
09	Aug-15	13,644.4	14,270.0	5,566.7	16,945.5	687.3	673.1	654.9	708.7
09	Sep-15	24,764.3	21,871.4	16,250.0	31,220.0	711.9	712.1	691.5	706.3
09	Oct-15	21,447.4	16,808.3	10,433.3	41,875.0	843.7	979.1	613.7	771.9

09	Nov-15	27,000.0	18,357.1	0.0	56,350.0	546.6	563.6	676.5	507.7
09	Dec-15	10,626.7	11,228.6	15,100.0	8,928.6	410.8	497.5	397.4	343.4
09	Jan-16	8,262.5	7,200.0	5,400.0	10,400.0	231.9	234.1	180.0	247.6
09	Feb-16	3,293.3	3,050.0	5,600.0	3,162.5	159.4	122.8	266.7	190.2
09	Mar-16	15,920.0	28,900.0	6,900.0	14,266.7	261.0	210.9	215.6	326.7
09	Apr-16	4,380.0	2,822.2	0.0	5,516.7	173.4	191.0	170.0	168.0
09	May-16	7,830.0	7,050.0	1,933.3	14,633.3	229.6	233.1	156.8	256.7
09	Jun-16	11,675.0	6,900.0	2,500.0	58,000.0	469.3	383.3	375.0	557.7
10	Feb-14	18,852.4	11,161.1	0.0	58,700.0	671.0	558.1	695.0	859.0
10	Mar-14	30,746.2	16,954.5	0.0	96,450.0	748.5	545.3	708.3	1,155.1
10	Apr-14	14,517.4	9,194.4	0.0	30,640.0	554.7	497.0	800.0	610.4
10	May-14	27,425.0	17,092.3	0.0	65,833.3	705.5	577.1	931.3	906.0
10	Jun-14	23,857.9	17,238.5	0.0	34,833.3	575.3	504.7	490.9	685.2
10	Jul-14	11,473.5	10,447.4	0.0	11,673.3	518.1	442.1	610.0	627.6
10	Aug-14	18,540.0	13,253.3	13,400.0	38,875.0	519.3	464.5	536.0	607.4
10	Sep-14	15,952.0	14,026.7	10,500.0	19,400.0	550.8	498.6	308.8	661.4
10	Oct-14	17,120.8	16,060.0	14,400.0	21,657.1	606.0	630.6	654.5	576.4
10	Nov-14	13,777.8	11,231.6	15,600.0	19,900.0	506.8	443.7	520.0	636.1
10	Dec-14	37,350.0	30,333.3	0.0	51,350.0	479.6	446.1	504.8	534.9
10	Jan-15	26,240.0	16,492.3	0.0	80,050.0	808.2	717.1	1,107.7	925.4
10	Feb-15	45,375.0	26,871.4	0.0	157,200.0	730.4	608.7	1,042.9	930.2
10	Mar-15	55,728.6	104,350.0	0.0	32,480.0	953.8	756.2	1,309.1	1,376.3
10	Apr-15	16,522.2	14,672.7	5,050.0	24,440.0	447.9	411.7	388.5	511.3
10	May-15	29,572.7	23,171.4	12,800.0	49,000.0	528.1	433.7	474.1	713.6
10	Jun-15	13,123.8	11,041.7	0.0	14,166.7	450.3	346.9	764.7	607.1
10	Jul-15	13,395.2	7,894.4	4,950.0	127,200.0	506.8	408.3	1,100.0	655.7
10	Aug-15	19,643.8	13,491.7	0.0	33,850.0	519.5	449.7	900.0	599.1
10	Sep-15	18,094.1	13,215.4	0.0	30,525.0	482.1	468.1	366.7	508.8
10	Oct-15	8,137.0	8,020.0	8,400.0	8,081.8	348.7	302.3	365.2	433.7
10	Nov-15	10,209.1	7,866.7	0.0	13,385.7	400.4	326.9	576.5	551.2
10	Dec-15	5,068.4	3,433.3	0.0	13,466.7	189.6	176.4	162.5	206.1
10	Jan-16	1,680.8	1,416.7	0.0	2,087.5	99.3	93.4	140.0	109.9
10	Feb-16	2,633.3	2,857.1	0.0	2,600.0	88.5	89.3	122.2	85.2
10	Mar-16	6,592.3	7,128.6	0.0	5,616.7	224.9	218.9	100.0	255.3
10	Apr-16	4,694.4	4,733.3	0.0	4,116.7	139.9	139.9	150.0	139.5
10	May-16	6,852.9	7,046.2	0.0	5,250.0	211.1	281.0	207.1	101.9
10	Jun-16	6,366.7	8,166.7	0.0	4,166.7	130.8	130.3	122.2	131.6
11	Feb-14	66,025.0	62,100.0	0.0	51,100.0	391.8	366.8	534.3	601.2
11	Mar-14	47,085.7	39,964.3	0.0	0.0	527.4	498.7	917.0	659.5
11	Apr-14	25,004.3	23,328.6	0.0	21,650.0	379.6	362.9	476.3	541.3
11	May-14	30,439.1	27,968.2	37,900.0	0.0	495.5	489.9	485.9	591.3
11	Jun-14	64,115.4	67,818.2	40,000.0	40,800.0	599.2	603.6	571.4	544.0
11	Jul-14	52,162.5	52,271.4	55,700.0	0.0	608.3	596.9	723.4	661.0

11	Aug-14	54,575.0	50,826.7	0.0	49,000.0	608.9	594.2	750.0	720.6
11	Sep-14	32,704.3	32,735.0	0.0	12,866.7	518.8	518.8	601.1	433.7
11	Oct-14	37,204.5	34,342.9	48,600.0	0.0	628.6	623.9	648.0	688.7
11	Nov-14	29,762.5	27,950.0	0.0	45,200.0	522.1	504.8	626.3	664.7
11	Dec-14	29,140.0	26,552.6	0.0	0.0	478.9	463.7	722.0	495.8
11	Jan-15	36,705.6	33,311.8	0.0	41,000.0	569.1	549.8	1,172.5	539.5
11	Feb-15	39,235.7	33,600.0	0.0	0.0	422.2	410.8	732.1	343.9
11	Mar-15	44,056.3	44,042.9	42,200.0	0.0	690.4	684.4	827.5	641.7
11	Apr-15	39,835.7	36,653.8	0.0	34,700.0	376.3	355.6	685.5	444.9
11	May-15	25,255.6	24,337.5	0.0	29,500.0	360.8	342.5	628.6	427.5
11	Jun-15	11,935.3	10,953.1	0.0	22,800.0	357.8	344.0	628.9	386.4
11	Jul-15	23,531.6	21,011.1	38,300.0	0.0	407.2	391.9	517.6	536.0
11	Aug-15	23,326.1	21,347.6	0.0	20,300.0	429.2	411.3	539.2	588.4
11	Sep-15	33,000.0	31,638.9	0.0	18,900.0	536.1	525.9	650.0	564.2
11	Oct-15	33,609.1	35,194.4	52,400.0	15,800.0	649.2	627.8	859.0	777.0
11	Nov-15	31,873.7	31,641.2	0.0	16,200.0	464.1	468.6	425.0	443.8
11	Dec-15	38,408.3	33,491.7	0.0	0.0	404.3	398.3	419.0	482.0
11	Jan-16	4,300.0	4,505.3	0.0	1,366.7	103.1	103.9	102.6	82.0
11	Feb-16	2,905.0	2,535.0	0.0	0.0	61.4	58.6	70.0	100.0
11	Mar-16	5,761.5	4,969.2	0.0	0.0	75.7	71.6	130.3	106.1
11	Apr-16	7,730.8	8,145.5	0.0	5,400.0	104.1	105.2	121.1	75.0
11	May-16	6,105.6	5,516.7	0.0	0.0	102.2	104.2	139.1	57.6
11	Jun-16	23,850.0	20,250.0	0.0	0.0	313.1	303.0	577.3	248.5
12	Feb-14	28,200.0	25,425.0	12,466.7	0.0	596.4	503.5	959.0	645.8
12	Mar-14	47,020.0	37,400.0	0.0	39,250.0	697.6	561.0	1,250.0	740.6
12	Apr-14	13,068.2	7,883.3	20,300.0	48,250.0	746.8	645.0	863.8	869.4
12	May-14	23,875.0	14,455.6	0.0	111,800.0	798.1	634.6	1,056.8	1,016.4
12	Jun-14	23,045.5	18,028.6	31,300.0	29,900.0	722.2	721.1	601.9	760.2
12	Jul-14	25,660.0	15,100.0	33,300.0	96,000.0	766.0	642.6	723.9	1,000.0
12	Aug-14	17,353.3	10,076.9	43,400.0	80,200.0	910.1	766.1	1,240.0	1,028.2
12	Sep-14	21,418.2	11,933.3	38,600.0	83,800.0	692.9	617.2	632.8	829.7
12	Oct-14	22,670.0	16,700.0	35,100.0	28,033.3	803.9	710.6	948.6	894.7
12	Nov-14	18,960.0	10,377.8	31,000.0	0.0	603.8	569.5	815.8	565.4
12	Dec-14	8,800.0	6,500.0	0.0	9,200.0	613.1	507.1	1,051.9	638.9
12	Jan-15	13,900.0	10,422.2	14,950.0	26,150.0	775.5	700.0	808.1	933.9
12	Feb-15	16,520.0	9,600.0	0.0	43,400.0	706.0	630.7	800.0	834.6
12	Mar-15	41,016.7	39,133.3	19,450.0	80,800.0	1,103.6	1,162.4	1,051.4	985.4
12	Apr-15	13,761.5	13,442.9	0.0	11,480.0	590.4	607.1	597.4	568.3
12	May-15	35,900.0	38,966.7	0.0	21,133.3	730.2	683.6	714.0	812.8
12	Jun-15	18,672.7	13,471.4	0.0	25,533.3	637.9	542.0	634.1	773.7
12	Jul-15	9,723.8	6,205.9	28,300.0	22,066.7	515.7	500.0	577.6	513.2
12	Aug-15	21,181.8	15,025.0	0.0	24,633.3	761.4	690.8	852.5	830.3
12	Sep-15	15,423.5	15,950.0	8,520.0	21,275.0	824.5	712.8	1,039.0	935.2

12	Oct-15	23,630.0	26,280.0	17,850.0	21,433.3	790.3	768.4	850.0	803.8
12	Nov-15	19,863.6	12,000.0	34,000.0	0.0	733.2	621.8	1,062.5	854.4
12	Dec-15	18,783.3	11,560.0	0.0	34,500.0	414.3	336.0	756.5	479.2
12	Jan-16	2,613.3	1,866.7	5,200.0	5,600.0	172.7	177.8	123.8	211.3
12	Feb-16	1,689.5	844.4	0.0	13,500.0	136.0	102.0	100.0	254.7
12	Mar-16	2,800.0	2,044.4	1,950.0	4,825.0	185.8	128.7	118.2	402.1
12	Apr-16	2,514.3	1,755.6	0.0	3,060.0	142.5	114.5	125.0	206.8
12	May-16	5,287.5	2,600.0	0.0	10,950.0	185.5	113.9	135.3	413.2
12	Jun-16	14,100.0	8,350.0	0.0	20,400.0	330.5	269.4	266.7	434.0
14	Feb-14	34,775.0	0.0	0.0	15,475.0	915.1	921.9	1,704.2	680.2
14	Mar-14	34,600.0	17,100.0	0.0	45,200.0	1,281.5	855.0	1,822.7	1,255.6
14	Apr-14	11,366.7	8,266.7	0.0	9,183.3	462.9	467.9	534.3	430.5
14	May-14	9,100.0	6,550.0	0.0	9,440.0	395.7	374.3	438.2	393.3
14	Jun-14	6,521.4	3,700.0	7,250.0	10,260.0	340.7	255.2	345.2	407.1
14	Jul-14	7,136.4	0.0	8,350.0	4,011.1	269.8	340.9	417.5	212.4
14	Aug-14	5,857.1	8,800.0	4,475.0	6,214.3	279.9	204.7	372.9	297.9
14	Sep-14	10,062.5	4,675.0	0.0	10,375.0	301.5	316.9	393.0	269.5
14	Oct-14	8,600.0	4,275.0	17,800.0	9,975.0	325.2	294.8	414.0	324.4
14	Nov-14	5,115.4	1,788.9	14,300.0	16,700.0	302.3	233.3	446.9	309.3
14	Dec-14	14,125.0	12,900.0	0.0	14,300.0	379.2	477.8	354.1	371.4
14	Jan-15	16,120.0	9,450.0	19,300.0	19,700.0	540.9	429.5	772.0	532.4
14	Feb-15	26,266.7	18,400.0	0.0	18,300.0	501.9	460.0	654.8	451.9
14	Mar-15	28,833.3	0.0	0.0	15,033.3	569.1	565.7	540.6	556.8
14	Apr-15	19,050.0	0.0	0.0	9,550.0	377.2	575.0	339.5	318.3
14	May-15	3,788.9	2,137.5	7,500.0	4,125.0	372.7	310.9	428.6	388.2
14	Jun-15	9,275.0	7,500.0	6,266.7	12,733.3	360.2	312.5	783.3	321.0
14	Jul-15	13,100.0	0.0	0.0	5,957.1	443.0	557.8	554.1	369.0
14	Aug-15	7,530.0	3,840.0	17,800.0	8,850.0	369.1	391.8	574.2	305.2
14	Sep-15	6,863.6	6,766.7	3,260.0	35,600.0	319.9	338.3	332.7	306.9
14	Oct-15	13,260.0	7,300.0	15,100.0	16,650.0	336.5	280.8	457.6	308.3
14	Nov-15	9,100.0	2,975.0	12,700.0	27,800.0	287.4	371.9	396.9	235.6
14	Dec-15	9,125.0	8,200.0	10,600.0	16,400.0	243.3	200.0	424.0	207.6
14	Jan-16	13,100.0	2,400.0	0.0	0.0	94.2	58.5	81.8	128.8
14	Feb-16	3,233.3	0.0	0.0	1,666.7	71.3	125.0	51.7	64.1
14	Mar-16	2,266.7	925.0	0.0	3,750.0	96.5	82.2	123.5	98.7
14	Apr-16	1,811.1	800.0	2,300.0	2,650.0	94.2	97.0	62.2	114.0
14	May-16	5,566.7	1,900.0	2,700.0	0.0	89.8	95.0	81.8	96.2
14	Jun-16	5,550.0	0.0	2,200.0	4,500.0	226.5	190.9	146.7	293.5
15	Feb-14	24,081.3	21,781.3	0.0	0.0	654.2	627.9	761.9	1,680.0
15	Mar-14	38,291.7	34,375.0	0.0	0.0	832.4	818.5	671.4	1,400.0
15	Apr-14	16,347.4	16,941.2	0.0	9,500.0	429.0	422.9	430.4	527.8
15	May-14	16,600.0	15,489.5	0.0	0.0	418.3	418.6	382.8	421.1
15	Jun-14	11,669.7	10,897.0	0.0	0.0	456.8	462.8	337.1	472.0

15	Jul-14	31,516.7	29,375.0	0.0	0.0	495.7	489.6	478.3	750.0
15	Aug-14	22,411.8	26,930.8	13,000.0	5,066.7	429.5	419.3	433.3	723.8
15	Sep-14	18,782.4	16,988.2	0.0	0.0	345.2	335.4	426.7	441.2
15	Oct-14	20,786.7	21,838.5	0.0	13,000.0	378.9	366.8	488.0	650.0
15	Nov-14	13,376.2	12,740.0	0.0	13,900.0	373.0	364.5	404.3	479.3
15	Dec-14	19,963.6	18,172.7	0.0	0.0	332.2	322.9	395.7	606.7
15	Jan-15	15,400.0	15,305.6	13,500.0	15,400.0	522.9	504.6	482.1	1,100.0
15	Feb-15	33,233.3	30,088.9	0.0	0.0	441.8	434.0	325.8	760.0
15	Mar-15	32,366.7	29,833.3	0.0	0.0	687.4	687.1	569.6	794.7
15	Apr-15	16,729.4	15,564.7	0.0	0.0	414.6	406.5	506.7	530.0
15	May-15	14,635.3	13,476.5	0.0	0.0	393.0	379.9	506.7	883.3
15	Jun-15	21,990.0	20,250.0	0.0	0.0	363.5	354.0	663.6	445.0
15	Jul-15	24,440.0	24,822.2	0.0	11,300.0	416.4	400.4	669.2	706.3
15	Aug-15	17,576.9	17,491.7	0.0	9,300.0	348.9	339.1	429.4	516.7
15	Sep-15	15,494.7	14,142.1	0.0	0.0	448.8	435.5	536.4	756.3
15	Oct-15	35,042.9	45,260.0	7,500.0	10,100.0	362.3	356.4	357.1	531.6
15	Nov-15	20,640.0	18,880.0	0.0	0.0	380.1	367.3	530.8	600.0
15	Dec-15	14,677.8	16,957.1	5,400.0	6,600.0	263.1	258.0	360.0	275.0
15	Jan-16	2,720.0	2,566.7	0.0	0.0	112.7	112.2	77.8	130.0
15	Feb-16	4,011.1	3,744.4	0.0	0.0	105.2	103.7	200.0	81.8
15	Mar-16	5,570.0	5,400.0	0.0	0.0	125.2	126.5	58.3	116.7
15	Apr-16	3,755.0	3,747.4	0.0	2,400.0	146.4	142.4	185.7	400.0
15	May-16	6,566.7	6,277.8	0.0	0.0	119.9	122.8	68.8	75.0
15	Jun-16	17,833.3	24,700.0	0.0	2,000.0	253.6	243.3	2,000.0	333.3
16	Feb-14	30,566.7	10,733.3	91,800.0	51,800.0	767.4	575.0	891.3	719.4
16	Mar-14	25,237.5	14,350.0	24,950.0	32,600.0	917.7	1,304.5	924.1	785.5
16	Apr-14	79,200.0	0.0	0.0	28,000.0	536.9	486.5	639.3	448.0
16	May-14	0.0	0.0	0.0	0.0	664.1	667.7	745.2	554.9
16	Jun-14	14,577.8	22,600.0	12,600.0	19,650.0	437.3	538.1	456.5	357.3
16	Jul-14	17,557.1	0.0	19,766.7	9,425.0	431.2	425.5	478.2	362.5
16	Aug-14	22,840.0	0.0	0.0	6,680.0	430.9	407.3	480.5	347.9
16	Sep-14	28,140.0	22,000.0	17,350.0	0.0	535.0	536.6	517.9	583.8
16	Oct-14	43,266.7	0.0	66,400.0	37,500.0	489.8	639.4	457.9	506.8
16	Nov-14	6,693.8	0.0	5,170.0	7,280.0	395.2	278.8	427.3	423.3
16	Dec-14	92,700.0	0.0	45,800.0	0.0	423.3	335.7	482.1	394.4
16	Jan-15	17,557.1	9,550.0	32,300.0	11,666.7	543.8	545.7	552.1	546.9
16	Feb-15	50,050.0	0.0	47,000.0	34,400.0	348.8	243.3	358.8	382.2
16	Mar-15	69,350.0	0.0	30,450.0	0.0	582.8	510.8	529.6	647.0
16	Apr-15	36,850.0	0.0	37,000.0	22,100.0	251.5	205.4	291.3	227.8
16	May-15	8,387.5	11,400.0	15,100.0	5,675.0	268.4	285.0	272.1	255.1
16	Jun-15	11,900.0	11,000.0	8,533.3	20,600.0	194.4	196.4	179.0	226.4
16	Jul-15	19,533.3	10,800.0	0.0	15,700.0	227.1	372.4	218.7	189.2
16	Aug-15	8,928.6	5,700.0	0.0	4,200.0	255.1	345.5	218.5	280.0

16	Sep-15	13,800.0	11,900.0	14,650.0	24,500.0	297.4	290.2	281.7	314.1
16	Oct-15	7,866.7	0.0	9,633.3	12,150.0	298.7	413.5	244.9	357.4
16	Nov-15	8,900.0	3,566.7	13,600.0	10,900.0	267.4	254.8	236.5	311.4
16	Dec-15	4,290.9	3,133.3	5,550.0	7,100.0	228.0	229.3	224.2	249.1
16	Jan-16	0.0	0.0	0.0	0.0	134.5	177.4	157.3	98.3
16	Feb-16	3,480.0	5,400.0	4,000.0	1,850.0	98.3	131.7	96.4	72.5
16	Mar-16	5,850.0	2,950.0	4,850.0	0.0	131.5	151.3	115.5	158.7
16	Apr-16	2,871.4	3,000.0	0.0	1,275.0	105.8	181.8	118.1	67.1
16	May-16	6,075.0	0.0	0.0	1,966.7	144.6	165.8	176.9	100.0
16	Jun-16	11,850.0	3,900.0	11,700.0	0.0	244.3	185.7	278.6	267.9
17	Feb-14	17,600.0	0.0	10,233.3	17,766.7	733.3	690.5	930.3	666.3
17	Mar-14	11,200.0	15,000.0	12,700.0	10,514.3	776.9	454.5	668.4	1,036.6
17	Apr-14	18,000.0	15,600.0	12,100.0	21,200.0	577.5	678.3	432.1	636.0
17	May-14	12,833.3	2,775.0	0.0	45,800.0	425.4	382.8	395.3	458.0
17	Jun-14	16,280.0	4,233.3	0.0	22,200.0	342.0	409.7	341.4	321.7
17	Jul-14	13,800.0	5,500.0	0.0	16,766.7	503.1	569.0	430.9	513.3
17	Aug-14	48,150.0	14,900.0	0.0	48,500.0	469.8	573.1	520.0	433.0
17	Sep-14	17,080.0	11,300.0	22,800.0	15,300.0	437.9	322.9	393.1	504.4
17	Oct-14	41,200.0	0.0	0.0	21,900.0	445.4	403.0	440.8	476.1
17	Nov-14	9,762.5	4,300.0	0.0	9,800.0	473.3	330.8	822.2	440.4
17	Dec-14	15,325.0	0.0	18,700.0	30,700.0	369.3	356.5	397.9	365.5
17	Jan-15	19,600.0	6,150.0	23,600.0	39,000.0	478.0	286.0	786.7	481.5
17	Feb-15	11,980.0	4,250.0	14,500.0	17,150.0	363.0	236.1	284.3	490.0
17	Mar-15	32,566.7	7,050.0	0.0	0.0	533.9	414.7	491.8	609.1
17	Apr-15	16,150.0	0.0	18,200.0	10,600.0	306.2	336.4	319.3	294.4
17	May-15	9,685.7	8,700.0	9,000.0	12,333.3	353.1	280.6	375.0	366.3
17	Jun-15	14,180.0	0.0	17,800.0	9,800.0	424.6	273.5	539.4	421.5
17	Jul-15	6,871.4	2,833.3	6,900.0	15,866.7	514.4	548.4	575.0	506.4
17	Aug-15	22,875.0	7,500.0	0.0	47,800.0	547.9	576.9	544.7	562.4
17	Sep-15	30,866.7	0.0	30,200.0	20,350.0	613.2	815.0	736.6	502.5
17	Oct-15	17,700.0	14,700.0	28,400.0	20,800.0	560.1	918.8	728.2	478.2
17	Nov-15	20,150.0	11,900.0	24,800.0	40,000.0	465.9	396.7	670.3	421.1
17	Dec-15	52,500.0	0.0	0.0	24,500.0	364.6	316.0	526.5	291.7
17	Jan-16	5,233.3	2,200.0	0.0	8,500.0	109.8	71.0	122.2	126.9
17	Feb-16	2,042.9	0.0	1,350.0	2,350.0	96.0	57.1	58.7	138.2
17	Mar-16	3,114.3	900.0	0.0	3,800.0	165.2	133.3	159.0	178.1
17	Apr-16	1,566.7	3,200.0	1,700.0	1,650.0	140.3	145.5	124.4	152.3
17	May-16	24,900.0	0.0	6,900.0	0.0	169.4	100.0	186.5	197.1
17	Jun-16	10,350.0	4,000.0	0.0	10,900.0	240.7	210.5	263.2	253.5
18	Feb-14	44,000.0	49,050.0	53,100.0	0.0	659.2	587.4	727.4	713.6
18	Mar-14	34,850.0	22,880.0	67,400.0	0.0	697.0	537.1	1,248.1	819.0
18	Apr-14	58,133.3	49,500.0	0.0	15,300.0	489.9	382.2	790.8	956.3
18	May-14	28,385.7	24,340.0	24,050.0	0.0	638.9	531.4	874.5	860.0

18	Jun-14	67,466.7	37,200.0	0.0	0.0	559.1	451.8	868.5	540.5
18	Jul-14	22,688.9	13,366.7	0.0	0.0	581.8	469.9	1,003.5	530.3
18	Aug-14	28,471.4	19,916.7	0.0	19,200.0	540.1	429.9	1,215.6	457.1
18	Sep-14	28,071.4	43,100.0	0.0	4,500.0	563.0	604.2	501.2	409.1
18	Oct-14	18,030.0	12,070.0	0.0	0.0	593.1	580.3	852.3	383.7
18	Nov-14	11,525.0	9,955.6	34,600.0	4,950.0	429.5	409.1	549.2	300.0
18	Dec-14	12,490.0	10,262.5	28,500.0	10,300.0	497.6	494.6	438.5	686.7
18	Jan-15	49,050.0	32,275.0	0.0	0.0	700.7	626.7	893.5	662.5
18	Feb-15	151,000.0	86,100.0	0.0	0.0	515.4	434.8	835.3	388.9
18	Mar-15	51,300.0	41,033.3	55,300.0	0.0	1,010.8	849.0	1,580.0	1,193.3
18	Apr-15	25,560.0	16,660.0	0.0	0.0	445.3	452.7	363.9	545.8
18	May-15	75,300.0	44,850.0	0.0	0.0	607.3	543.6	900.0	465.5
18	Jun-15	27,850.0	18,540.0	0.0	17,700.0	603.2	517.9	922.2	491.7
18	Jul-15	24,600.0	18,940.0	52,800.0	19,000.0	623.9	511.9	926.3	633.3
18	Aug-15	27,050.0	20,240.0	0.0	0.0	550.2	481.9	724.1	713.0
18	Sep-15	9,678.6	6,171.4	0.0	0.0	442.8	427.7	514.8	365.7
18	Oct-15	9,392.3	6,863.6	0.0	11,600.0	500.4	444.1	737.5	483.3
18	Nov-15	31,733.3	29,800.0	0.0	8,100.0	425.0	379.6	519.6	426.3
18	Dec-15	16,575.0	10,350.0	0.0	0.0	287.0	247.9	397.7	300.0
18	Jan-16	9,550.0	12,600.0	3,400.0	0.0	89.3	87.5	85.0	141.2
18	Feb-16	3,400.0	2,500.0	0.0	0.0	54.5	70.8	33.3	29.2
18	Mar-16	1,533.3	1,111.1	0.0	0.0	67.3	73.0	41.5	77.3
18	Apr-16	4,975.0	5,466.7	1,700.0	0.0	102.1	150.5	30.9	56.0
18	May-16	2,916.7	2,440.0	2,200.0	0.0	91.1	88.4	71.0	155.0
18	Jun-16	0.0	0.0	0.0	0.0	136.6	155.3	64.3	266.7
19	Feb-14	51,266.7	20,033.3	0.0	0.0	541.5	373.3	758.4	713.2
19	Mar-14	14,776.9	7,970.0	34,150.0	34,600.0	669.3	510.9	975.7	720.8
19	Apr-14	37,780.0	18,520.0	0.0	0.0	546.0	589.8	508.1	463.2
19	May-14	59,933.3	91,900.0	55,300.0	0.0	507.9	531.2	582.1	361.4
19	Jun-14	12,057.1	10,188.9	45,900.0	8,833.3	472.8	470.3	581.0	358.1
19	Jul-14	15,061.5	12,475.0	56,200.0	7,850.0	417.5	430.2	453.2	337.6
19	Aug-14	15,700.0	10,433.3	54,700.0	15,550.0	419.6	406.5	463.6	345.6
19	Sep-14	9,320.0	9,814.3	14,666.7	5,225.0	329.7	298.7	440.0	261.3
19	Oct-14	27,400.0	21,866.7	44,800.0	19,900.0	391.4	370.6	466.7	306.2
19	Nov-14	11,572.7	7,025.0	45,200.0	0.0	346.9	294.2	480.9	293.5
19	Dec-14	14,975.0	8,916.7	21,450.0	0.0	477.3	477.7	595.8	287.3
19	Jan-15	24,666.7	21,533.3	25,650.0	23,700.0	567.0	592.7	657.7	382.3
19	Feb-15	41,733.3	26,100.0	0.0	21,300.0	546.7	453.9	663.6	519.5
19	Mar-15	22,112.5	9,200.0	0.0	0.0	800.5	681.5	913.8	747.6
19	Apr-15	17,037.5	29,050.0	44,600.0	6,600.0	418.1	395.2	479.6	394.0
19	May-15	22,820.0	53,000.0	0.0	21,300.0	417.9	417.3	425.3	367.2
19	Jun-15	18,766.7	12,550.0	34,700.0	21,800.0	418.6	392.2	450.6	389.3
19	Jul-15	12,111.1	6,857.1	34,900.0	21,300.0	306.2	292.7	338.8	276.6

19	Aug-15	23,160.0	26,900.0	38,100.0	9,750.0	403.5	373.6	470.4	348.2
19	Sep-15	12,630.0	7,937.5	0.0	11,200.0	411.4	382.5	443.4	379.7
19	Oct-15	20,080.0	11,300.0	0.0	18,100.0	314.7	286.1	351.1	282.8
19	Nov-15	12,171.4	9,400.0	13,850.0	15,000.0	283.1	270.5	271.6	306.1
19	Dec-15	9,800.0	10,650.0	21,600.0	6,150.0	272.2	239.3	288.0	273.3
19	Jan-16	9,250.0	5,050.0	0.0	0.0	116.4	132.9	104.4	87.5
19	Feb-16	1,088.9	500.0	2,800.0	0.0	53.3	47.1	45.9	75.8
19	Mar-16	7,450.0	8,300.0	0.0	2,700.0	86.6	98.8	68.6	81.8
19	Apr-16	2,188.9	2,500.0	3,900.0	1,700.0	94.3	104.2	72.2	96.2
19	May-16	5,350.0	6,400.0	0.0	2,100.0	141.4	188.2	98.3	112.5
19	Jun-16	7,440.0	5,025.0	0.0	0.0	271.5	346.6	165.1	303.4
20	Feb-14	116,600.0	33,800.0	0.0	0.0	1,267.4	734.8	2,750.0	956.7
20	Mar-14	61,200.0	19,900.0	0.0	0.0	1,028.6	723.6	1,555.2	931.3
20	Apr-14	30,833.3	14,600.0	0.0	27,100.0	833.3	521.4	1,234.8	1,084.0
20	May-14	29,200.0	0.0	15,750.0	23,500.0	775.2	671.1	875.0	671.4
20	Jun-14	32,433.3	16,400.0	0.0	26,300.0	685.2	520.6	1,189.3	641.5
20	Jul-14	55,800.0	23,350.0	0.0	0.0	759.2	881.1	755.6	628.2
20	Aug-14	50,200.0	39,300.0	0.0	22,300.0	580.3	497.5	911.4	428.8
20	Sep-14	38,350.0	13,100.0	0.0	0.0	491.7	409.4	620.9	443.9
20	Oct-14	48,550.0	37,700.0	31,100.0	0.0	708.8	598.4	758.5	836.0
20	Nov-14	78,200.0	0.0	0.0	17,400.0	494.9	389.7	872.7	348.0
20	Dec-14	29,000.0	23,600.0	0.0	10,400.0	379.1	400.0	416.3	236.4
20	Jan-15	21,366.7	7,466.7	0.0	0.0	567.3	487.0	826.9	442.4
20	Feb-15	29,300.0	0.0	0.0	6,100.0	568.9	515.0	895.5	381.3
20	Mar-15	44,100.0	30,500.0	25,800.0	0.0	1,002.3	802.6	1,075.0	1,186.4
20	Apr-15	18,766.7	21,000.0	8,700.0	0.0	443.3	428.6	511.8	386.5
20	May-15	0.0	0.0	0.0	0.0	588.2	501.9	875.0	521.7
20	Jun-15	8,533.3	3,083.3	0.0	0.0	390.8	264.3	718.2	393.1
20	Jul-15	25,000.0	9,950.0	0.0	0.0	390.6	331.7	645.0	351.4
20	Aug-15	8,980.0	3,980.0	0.0	0.0	316.2	284.3	403.2	252.8
20	Sep-15	9,680.0	11,550.0	0.0	2,733.3	363.9	350.0	360.5	303.7
20	Oct-15	10,525.0	5,533.3	0.0	10,100.0	345.1	240.6	492.3	531.6
20	Nov-15	39,200.0	16,800.0	0.0	0.0	329.4	311.1	365.5	225.0
20	Dec-15	28,300.0	9,400.0	0.0	0.0	304.3	223.8	563.2	217.9
20	Jan-16	5,800.0	4,500.0	3,000.0	0.0	107.4	155.2	88.2	81.1
20	Feb-16	1,400.0	825.0	0.0	1,233.3	88.3	62.3	92.3	127.6
20	Mar-16	5,033.3	2,750.0	0.0	6,400.0	152.5	141.0	133.3	182.9
20	Apr-16	0.0	0.0	0.0	0.0	120.6	102.6	160.0	138.5
20	May-16	9,750.0	5,500.0	3,200.0	0.0	193.1	131.0	139.1	363.0
20	Jun-16	8,250.0	6,000.0	4,100.0	0.0	351.1	300.0	273.3	600.0
22	Feb-14	27,716.7	24,366.7	0.0	0.0	670.6	649.8	876.5	550.0
22	Mar-14	45,300.0	40,860.0	0.0	0.0	871.2	837.3	1,242.9	1,850.0
22	Apr-14	13,846.2	12,453.8	0.0	0.0	622.8	613.3	657.9	650.0



22	May-14	18,172.7	16,727.3	0.0	0.0	696.5	666.7	1,200.0	3,700.0
22	Jun-14	10,250.0	9,753.3	13,200.0	0.0	437.3	431.6	455.2	433.3
22	Jul-14	14,873.3	14,407.1	15,300.0	0.0	701.6	688.4	765.0	720.0
22	Aug-14	13,690.9	12,072.7	0.0	0.0	437.8	408.6	893.8	766.7
22	Sep-14	14,500.0	14,062.5	13,700.0	0.0	517.9	500.0	526.9	3,100.0
22	Oct-14	9,400.0	8,575.0	0.0	0.0	569.7	551.0	635.7	3,600.0
22	Nov-14	11,933.3	11,618.2	12,100.0	0.0	463.4	440.7	756.3	2,900.0
22	Dec-14	24,920.0	27,675.0	9,600.0	0.0	576.9	556.3	738.5	1,166.7
22	Jan-15	95,900.0	87,500.0	0.0	0.0	1,025.7	1,017.4	958.3	1,650.0
22	Feb-15	15,922.2	16,250.0	0.0	0.0	519.2	511.8	791.7	387.5
22	Mar-15	19,655.6	18,011.1	0.0	0.0	815.2	775.6	2,080.0	3,200.0
22	Apr-15	8,807.7	7,784.6	0.0	0.0	350.2	325.4	753.8	1,300.0
22	May-15	21,400.0	19,400.0	0.0	0.0	522.0	515.0	573.3	866.7
22	Jun-15	9,558.3	11,266.7	4,800.0	2,600.0	444.6	435.2	436.4	1,300.0
22	Jul-15	17,375.0	15,787.5	0.0	0.0	586.5	563.8	900.0	833.3
22	Aug-15	11,681.8	10,390.9	0.0	0.0	586.8	583.2	612.5	620.0
22	Sep-15	21,671.4	19,857.1	0.0	0.0	559.8	569.7	488.9	328.6
22	Oct-15	14,116.7	12,866.7	0.0	0.0	688.6	680.2	680.0	1,266.7
22	Nov-15	19,833.3	17,300.0	0.0	0.0	457.7	443.6	519.0	560.0
22	Dec-15	9,837.5	8,462.5	0.0	0.0	389.6	378.2	342.9	1,100.0
22	Jan-16	5,075.0	4,675.0	0.0	0.0	133.6	136.5	71.4	400.0
22	Feb-16	5,233.3	4,800.0	0.0	0.0	91.8	90.6	70.0	500.0
22	Mar-16	3,540.0	5,233.3	550.0	0.0	88.1	86.7	61.1	400.0
22	Apr-16	2,041.7	2,109.1	0.0	100.0	102.5	105.9	62.5	25.0
22	May-16	2,757.1	2,983.3	600.0	0.0	87.7	86.9	54.5	166.7
22	Jun-16	8,233.3	7,366.7	0.0	0.0	220.5	214.6	420.0	66.7
24	Feb-14	63,233.3	0.0	0.0	19,150.0	1,185.6	1,041.0	2,190.9	766.0
24	Mar-14	42,600.0	34,566.7	54,100.0	0.0	1,151.4	934.2	2,003.7	911.9
24	Apr-14	15,290.0	49,050.0	13,600.0	3,550.0	667.7	662.8	877.4	453.2
24	May-14	27,814.3	27,850.0	0.0	18,200.0	954.4	813.1	1,407.7	1,011.1
24	Jun-14	27,562.5	17,314.3	0.0	0.0	938.3	897.8	1,221.1	762.3
24	Jul-14	18,523.1	12,254.5	46,900.0	45,600.0	1,089.6	1,087.1	1,340.0	950.0
24	Aug-14	25,887.5	23,140.0	0.0	39,600.0	859.3	876.5	934.9	733.3
24	Sep-14	63,200.0	34,466.7	0.0	0.0	786.7	820.6	971.8	593.4
24	Oct-14	17,927.3	13,600.0	20,100.0	0.0	872.6	788.4	1,116.7	741.9
24	Nov-14	15,777.8	8,266.7	0.0	0.0	893.1	759.2	1,540.9	691.2
24	Dec-14	28,700.0	19,550.0	27,200.0	0.0	755.3	806.2	663.4	584.4
24	Jan-15	16,090.9	17,480.0	21,450.0	8,425.0	1,127.4	1,181.1	1,191.7	864.1
24	Feb-15	23,385.7	39,500.0	21,000.0	30,700.0	826.8	759.6	1,615.4	538.6
24	Mar-15	0.0	0.0	0.0	0.0	1,516.1	1,232.0	3,147.4	1,493.1
24	Apr-15	34,580.0	19,460.0	0.0	0.0	778.8	810.8	875.0	565.3
24	May-15	51,525.0	38,666.7	0.0	0.0	1,241.6	1,432.1	1,185.7	784.1
24	Jun-15	18,027.3	14,162.5	18,850.0	34,800.0	822.8	708.1	837.8	1,122.6

24	Jul-15	20,740.0	22,880.0	38,000.0	20,400.0	1,037.0	1,100.0	1,187.5	769.8
24	Aug-15	11,937.5	8,100.0	0.0	17,850.0	799.2	865.6	729.3	743.8
24	Sep-15	25,587.5	23,680.0	0.0	12,800.0	882.3	925.0	871.1	698.2
24	Oct-15	24,300.0	19,500.0	0.0	29,900.0	917.0	879.7	1,066.7	808.1
24	Nov-15	36,025.0	41,050.0	26,200.0	24,200.0	692.8	636.4	727.8	756.3
24	Dec-15	15,966.7	12,875.0	0.0	17,000.0	512.3	520.2	536.1	386.4
24	Jan-16	6,083.3	3,740.0	7,100.0	0.0	266.4	228.0	322.7	351.9
24	Feb-16	25,200.0	12,400.0	0.0	0.0	153.7	137.8	173.1	208.3
24	Mar-16	4,557.1	3,360.0	0.0	4,300.0	184.4	160.0	222.7	226.3
24	Apr-16	3,914.3	4,933.3	0.0	6,700.0	168.1	155.8	204.3	209.4
24	May-16	4,855.6	6,325.0	6,300.0	3,500.0	260.1	250.5	225.0	338.7
24	Jun-16	15,233.3	10,050.0	0.0	13,400.0	601.3	410.2	1,766.7	705.3
25	Feb-14	20,214.3	23,620.0	0.0	14,888.9	424.3	423.3	393.5	426.8
25	Mar-14	60,220.0	40,800.0	0.0	147,000.0	484.1	463.6	392.6	528.8
25	Apr-14	14,006.7	10,200.0	9,100.0	19,366.7	265.3	210.0	239.5	321.0
25	May-14	23,944.4	83,000.0	0.0	15,214.3	322.1	365.6	331.7	289.4
25	Jun-14	19,325.0	17,860.0	0.0	16,957.1	292.8	280.8	335.6	301.3
25	Jul-14	20,841.7	21,480.0	0.0	16,057.1	348.8	426.2	375.4	288.9
25	Aug-14	13,019.0	12,337.5	9,900.0	15,433.3	322.8	300.0	366.7	333.9
25	Sep-14	12,236.4	16,616.7	13,500.0	9,750.0	351.9	322.7	350.6	381.3
25	Oct-14	15,464.7	19,433.3	11,600.0	14,700.0	346.4	425.5	368.3	292.5
25	Nov-14	24,890.0	27,975.0	25,700.0	26,650.0	317.5	347.5	342.7	283.5
25	Dec-14	12,950.0	14,840.0	17,600.0	10,837.5	248.0	258.5	241.1	244.2
25	Jan-15	25,330.8	44,933.3	36,100.0	19,012.5	504.3	545.7	508.5	475.3
25	Feb-15	22,042.9	36,366.7	0.0	14,236.4	378.7	347.5	591.9	365.9
25	Mar-15	45,600.0	48,666.7	0.0	35,420.0	516.7	500.0	619.6	507.4
25	Apr-15	17,076.5	17,171.4	0.0	14,070.0	343.1	392.8	313.5	314.1
25	May-15	18,466.7	37,033.3	25,700.0	12,200.0	394.6	392.6	435.6	390.1
25	Jun-15	11,477.3	41,950.0	13,200.0	8,005.9	341.7	315.4	382.6	351.7
25	Jul-15	34,044.4	30,125.0	0.0	29,340.0	451.3	526.2	558.6	389.1
25	Aug-15	16,773.3	13,425.0	26,400.0	18,750.0	313.3	308.6	406.2	297.6
25	Sep-15	15,750.0	10,677.8	25,200.0	20,866.7	363.6	311.0	434.5	403.9
25	Oct-15	17,271.4	14,828.6	0.0	17,966.7	366.4	494.3	363.6	291.4
25	Nov-15	20,654.5	14,000.0	0.0	22,780.0	350.6	290.7	413.3	399.6
25	Dec-15	14,587.5	8,680.0	15,800.0	27,850.0	219.4	230.9	415.8	188.8
25	Jan-16	11,266.7	12,700.0	3,700.0	17,000.0	76.8	93.4	90.2	67.7
25	Feb-16	4,157.1	6,550.0	0.0	2,780.0	60.6	77.5	44.7	55.6
25	Mar-16	6,314.3	3,925.0	4,000.0	11,800.0	89.8	95.2	90.9	86.4
25	Apr-16	5,637.5	16,000.0	0.0	3,457.1	92.8	98.8	104.3	89.6
25	May-16	6,344.4	19,100.0	2,300.0	5,483.3	115.8	109.1	90.2	128.5
25	Jun-16	6,185.7	4,325.0	0.0	7,000.0	161.6	186.0	153.3	152.2

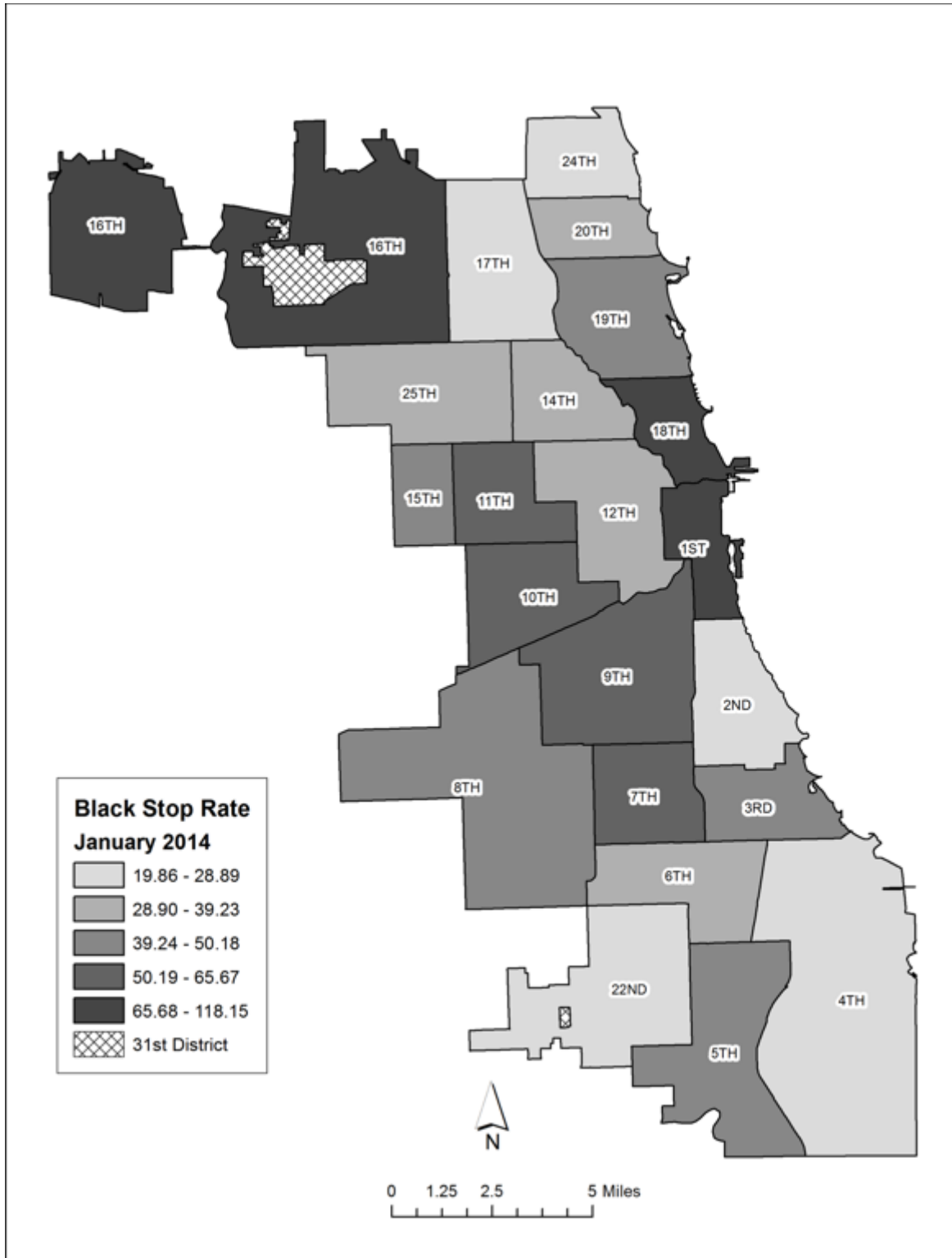
Sources: 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

APPENDIX D: ANOVAS

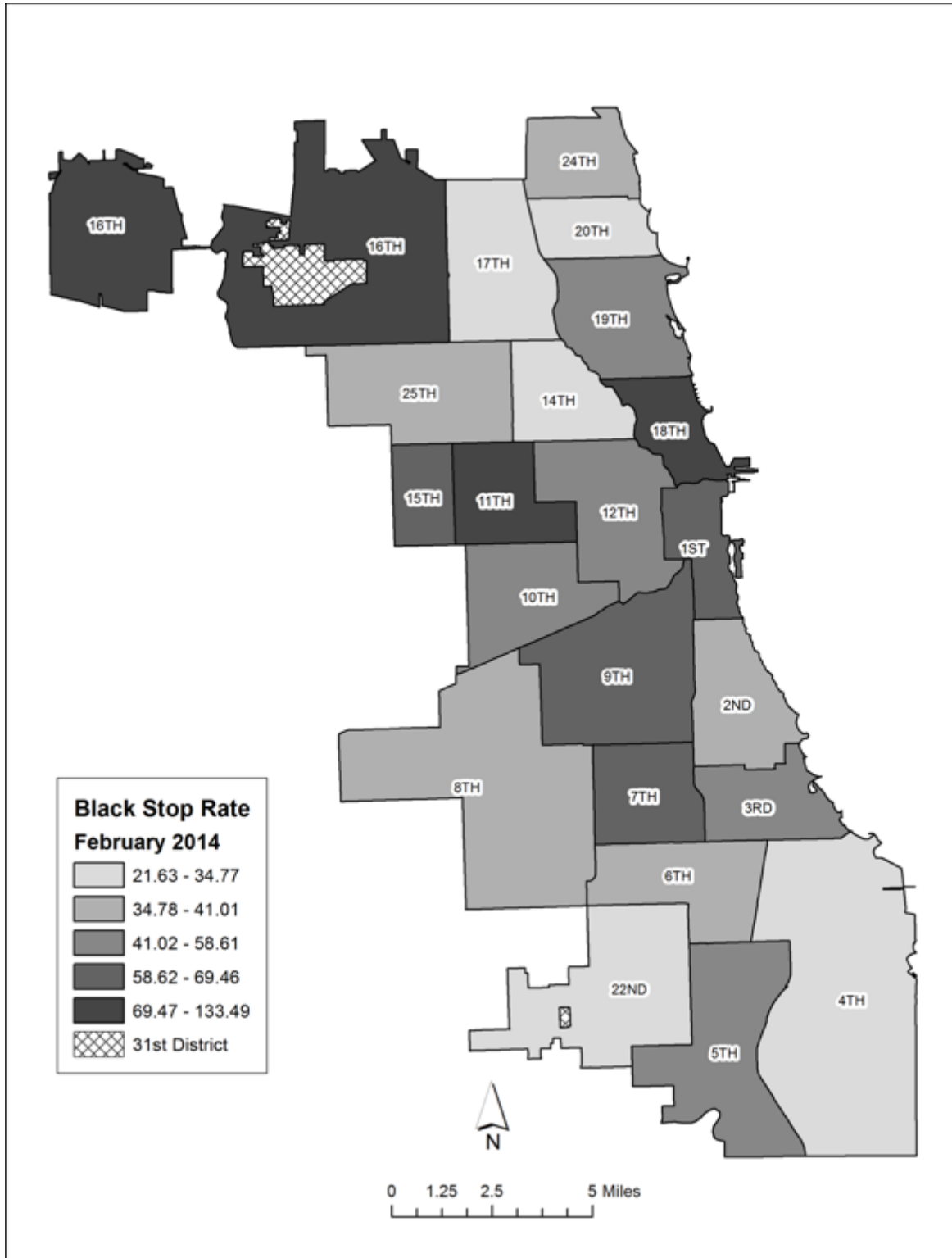
	Violent Arrests				Young Population				Total Arrests			
	b	SE	IRR		b	SE	IRR		b	SE	IRR	
Intercept	4.814	0.084	123.171	***	-3.858	0.175	0.021	***	1.586	0.064	4.882	***
Ln(Exposure)	1.000				1.000				1.000			
Ln(alpha)	-0.305	0.030		***	0.419	0.028		***	-0.945	0.032		***
Level 2 Variance	0.146	0.047			0.657	0.203			0.085	0.027		
Likelihood Ratio $\chi^2$	271.850			***	637.640			***	331.030			***
AIC	25,993.940				28,011.310				24,857.460			
BIC	26,010.580				28,027.980				24,874.140			

Notes: Violent arrests N=1,896 district-months. Young population N=1,914. Total arrests N=1,914. \* p<0.05, \*\* p<0.01, \*\*\* p<0.001. IRR = Incidence rate ratio. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

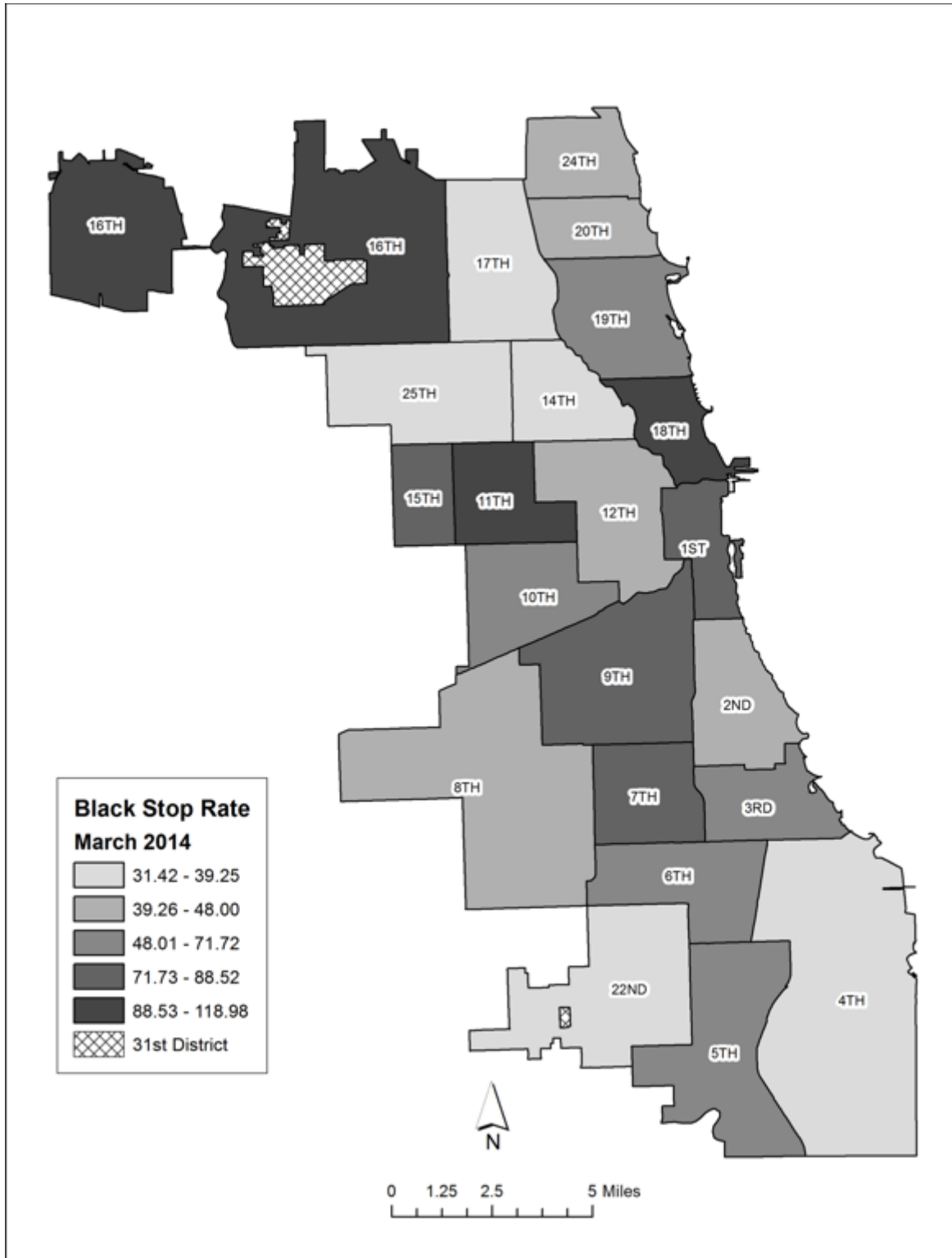
APPENDIX E: Non-Hispanic Black Stop Rate (per 1,000 race/ethnic specific population), January 2014



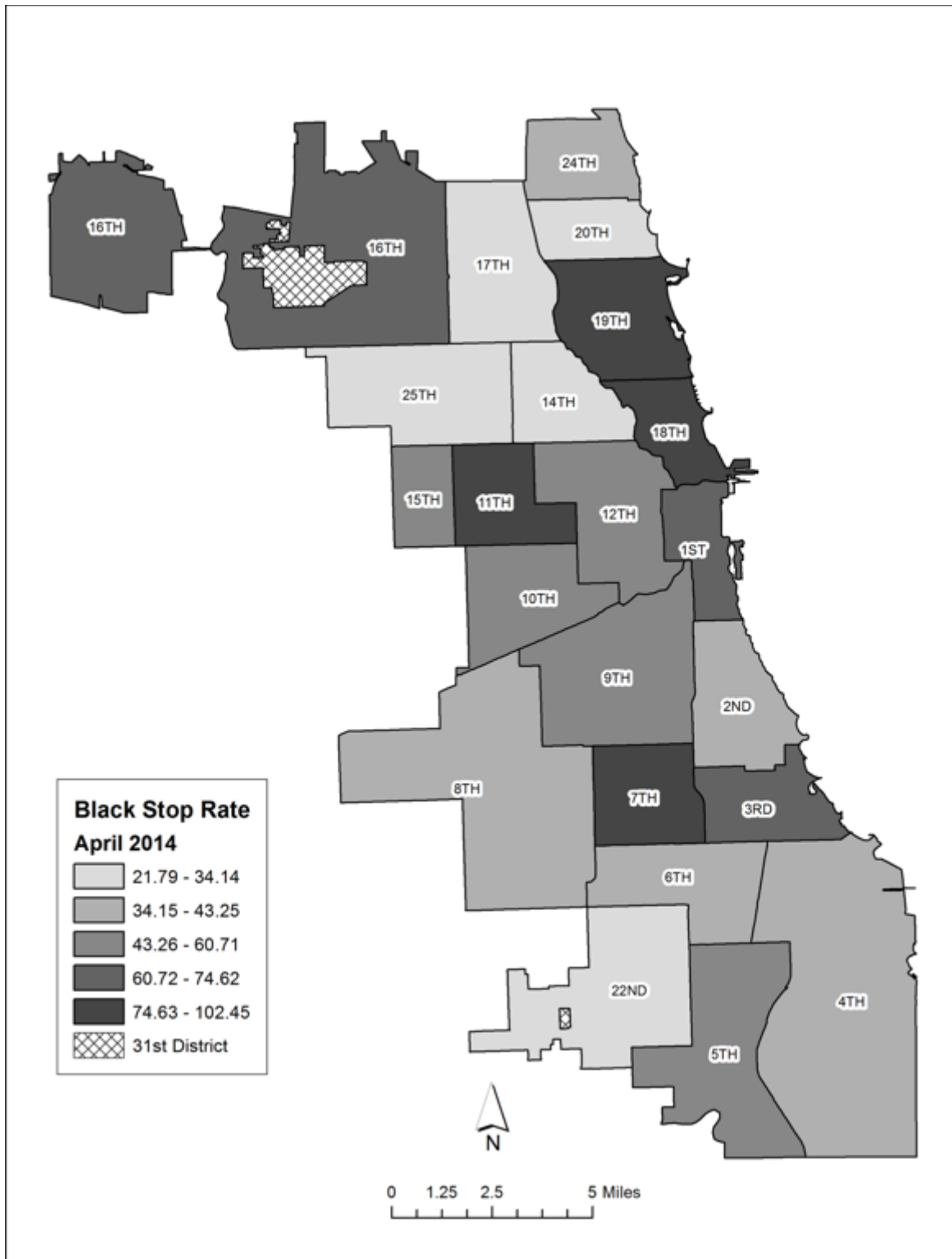
APPENDIX F: Non-Hispanic Black Stop Rate (per 1,000), February 2014

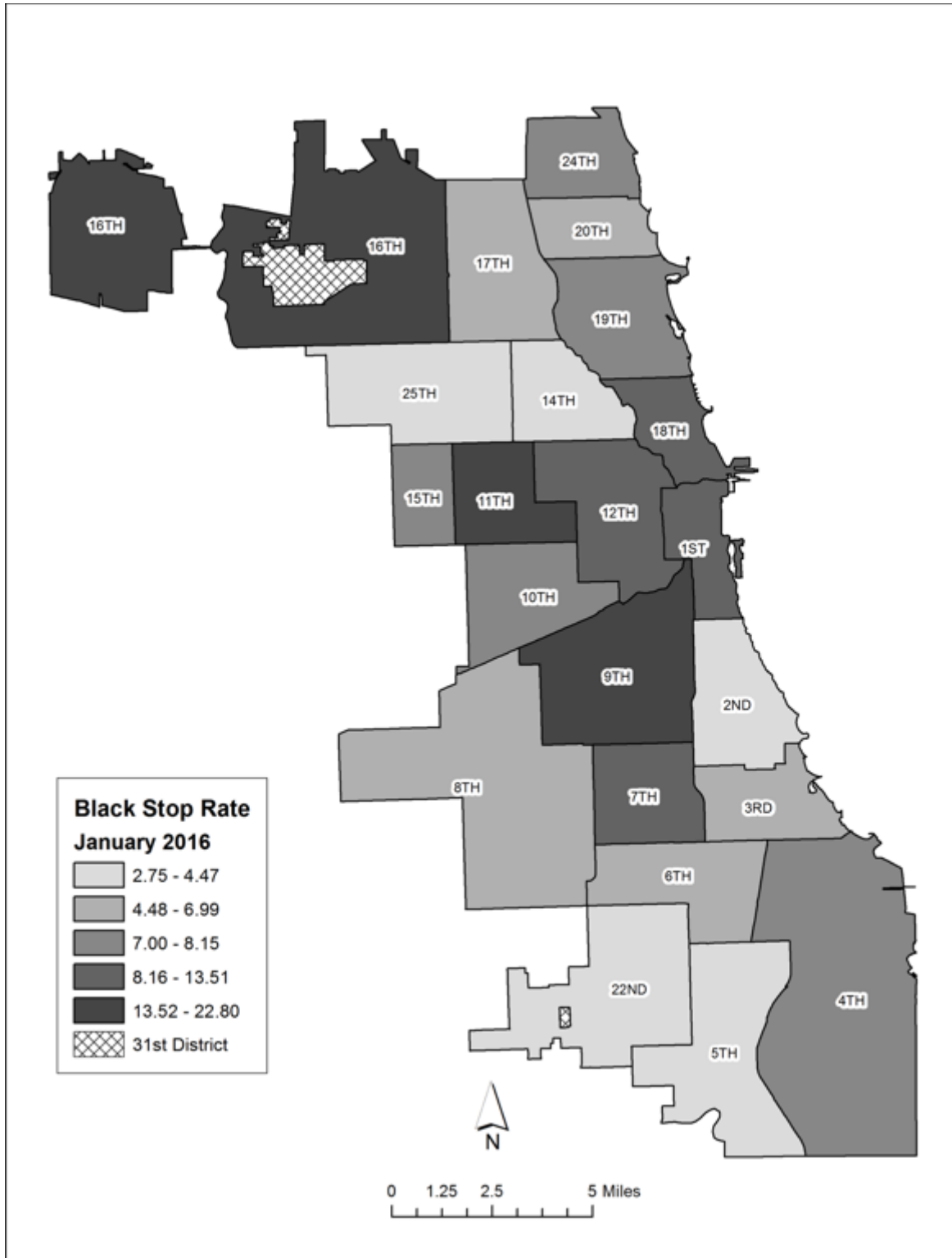


APPENDIX G: Non-Hispanic Black Stop Rate (per 1,000), March 2014

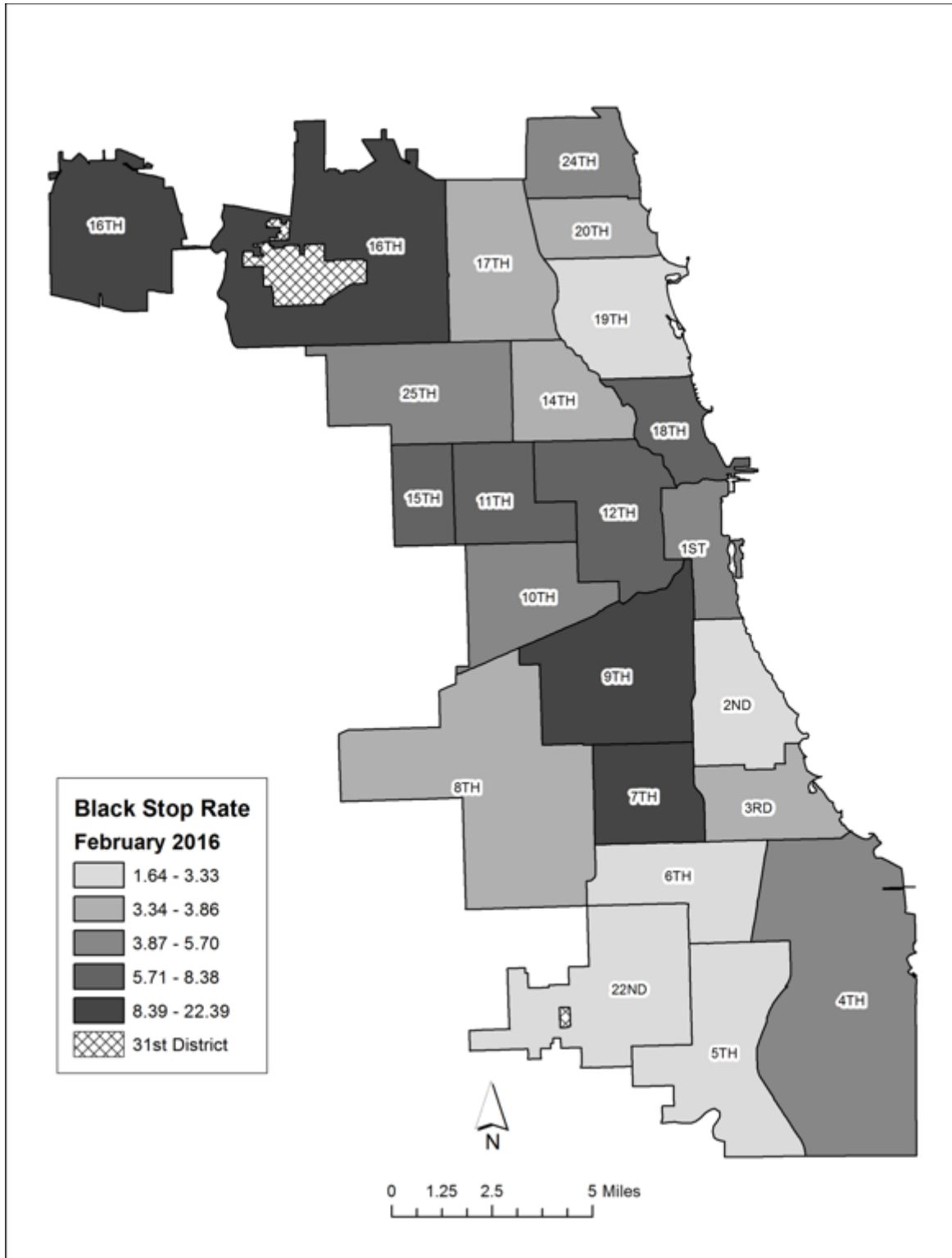


APPENDIX H: Non-Hispanic Black Stop Rate (per 1,000), April 2014

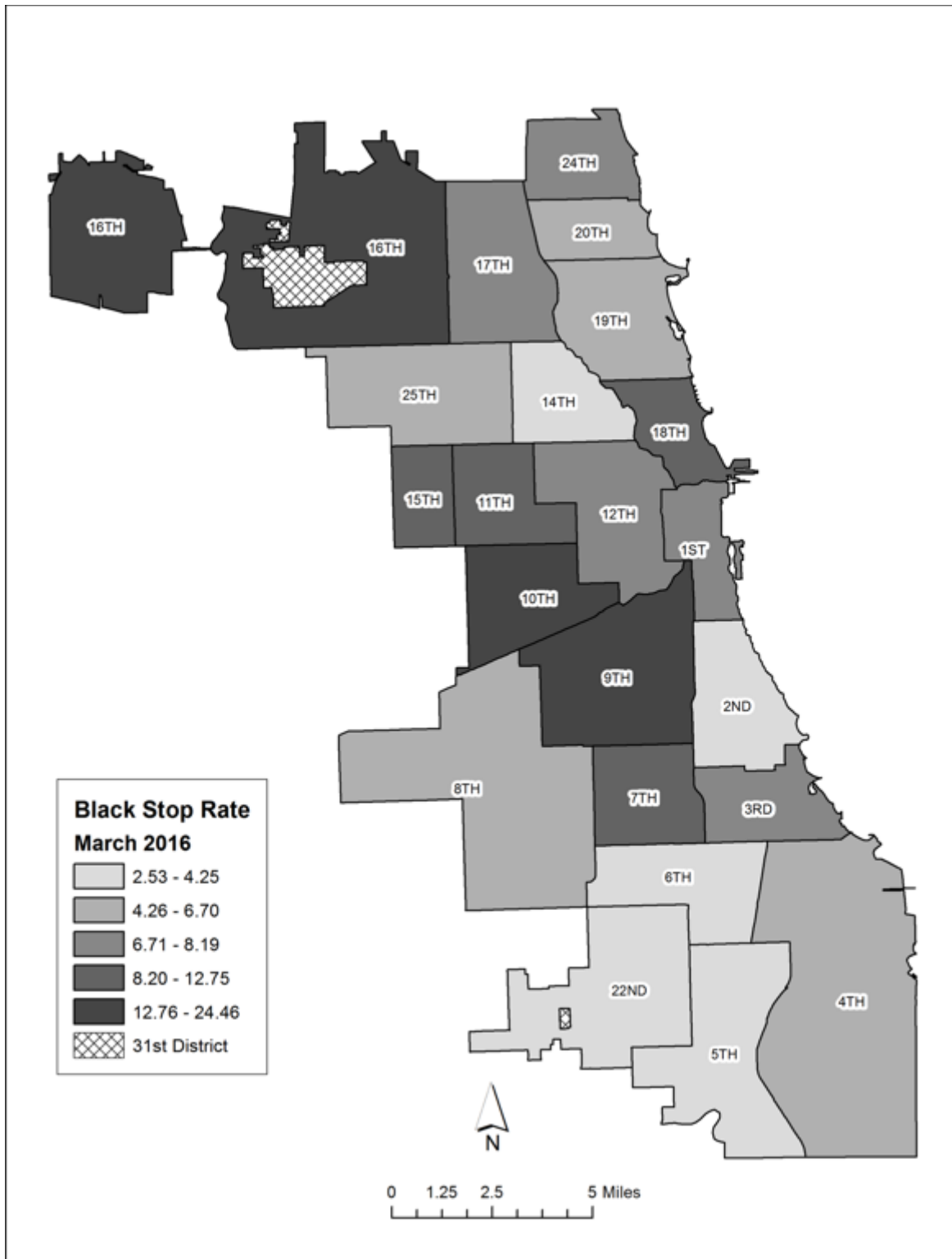




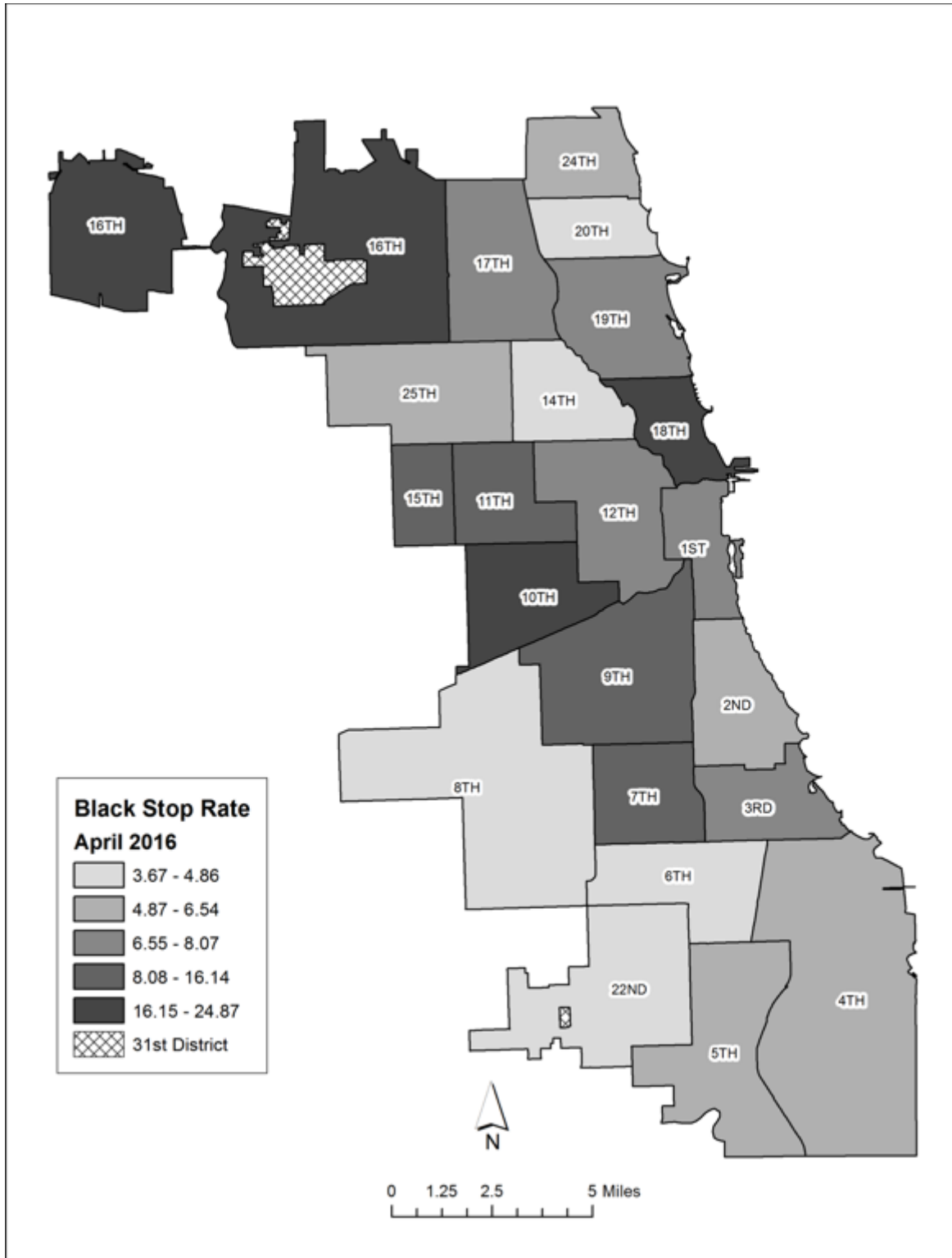


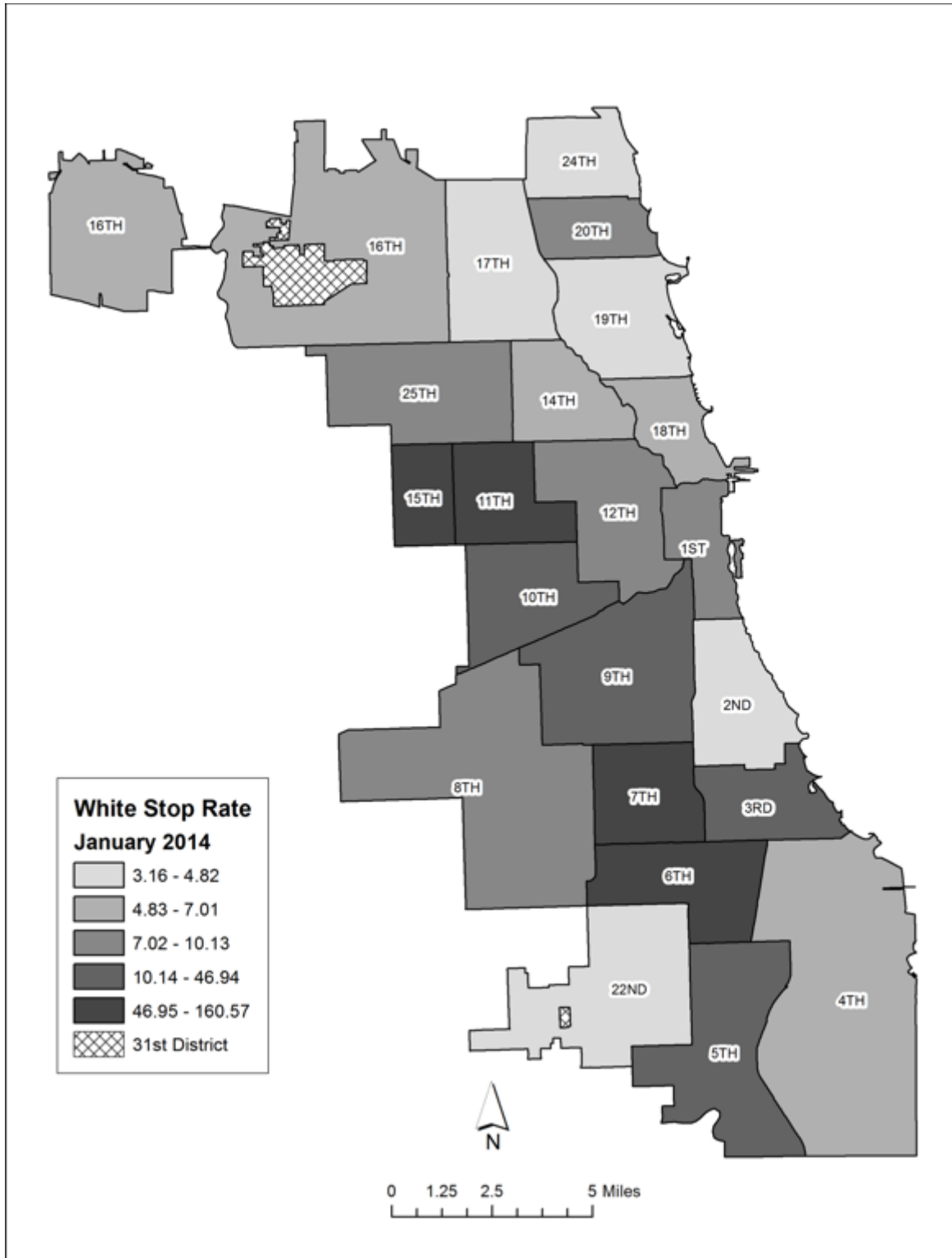


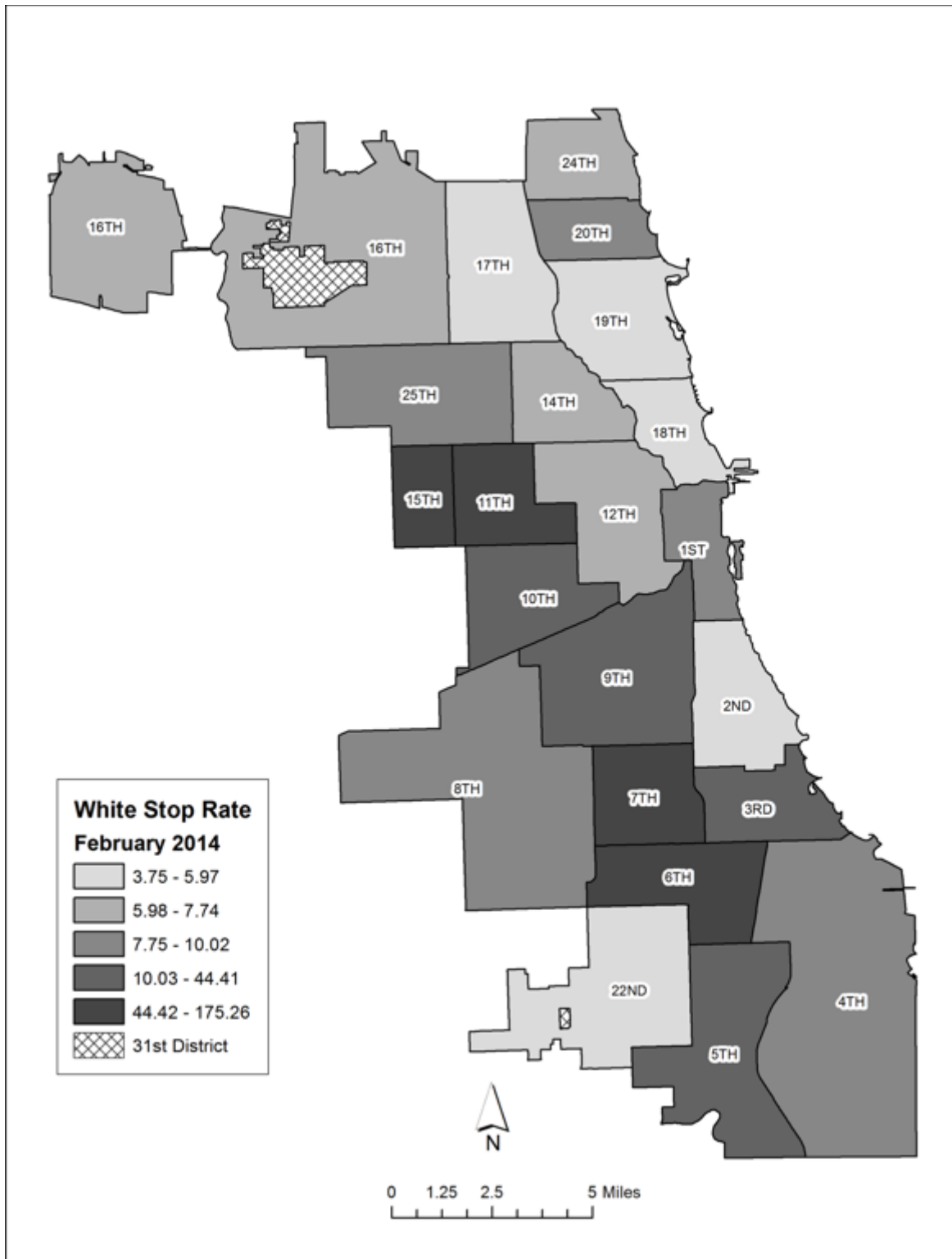
APPENDIX K: Non-Hispanic Black Stop Rate (per 1,000), March 2014



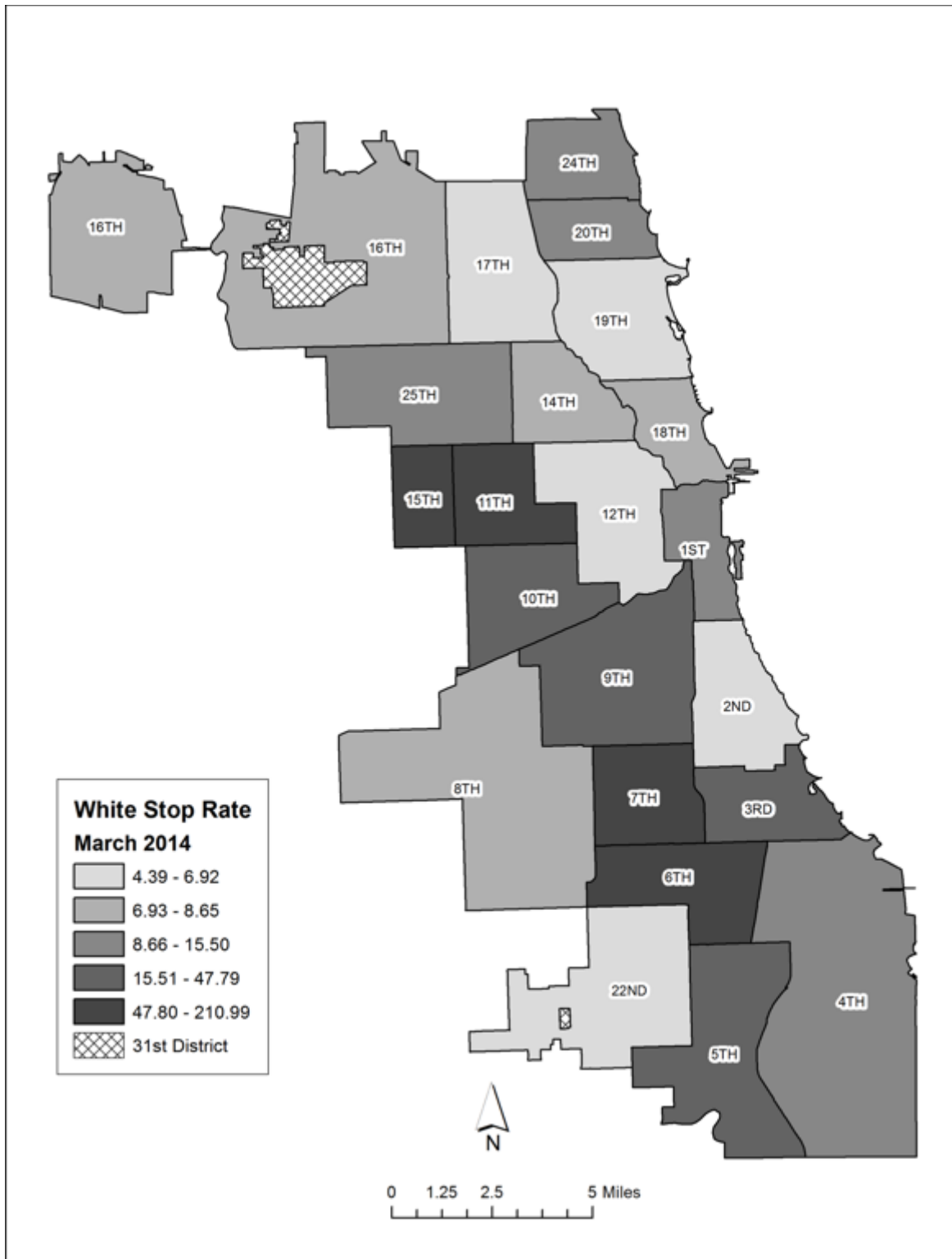
APPENDIX L: Non-Hispanic Black Stop Rate (per 1,000), April 2016

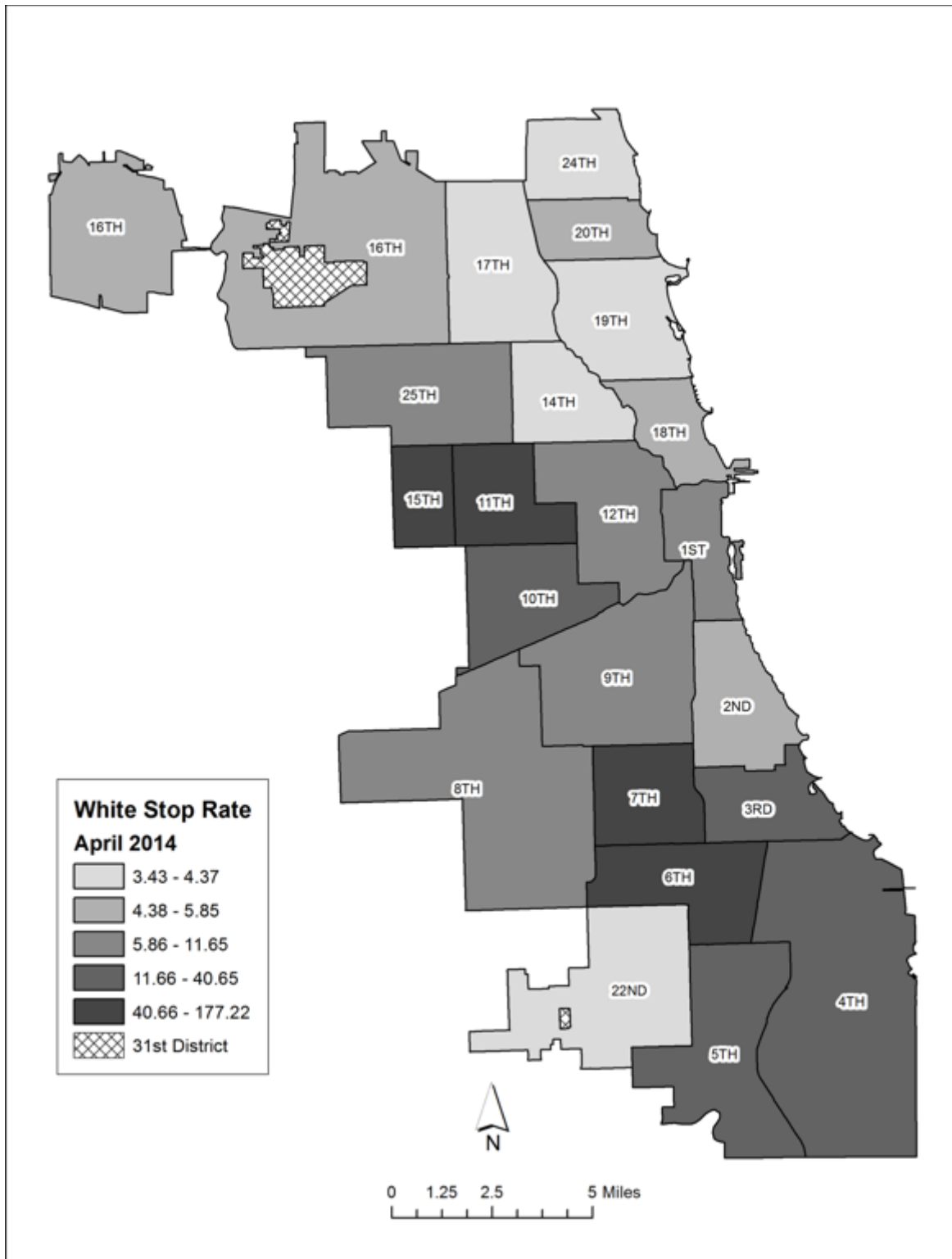




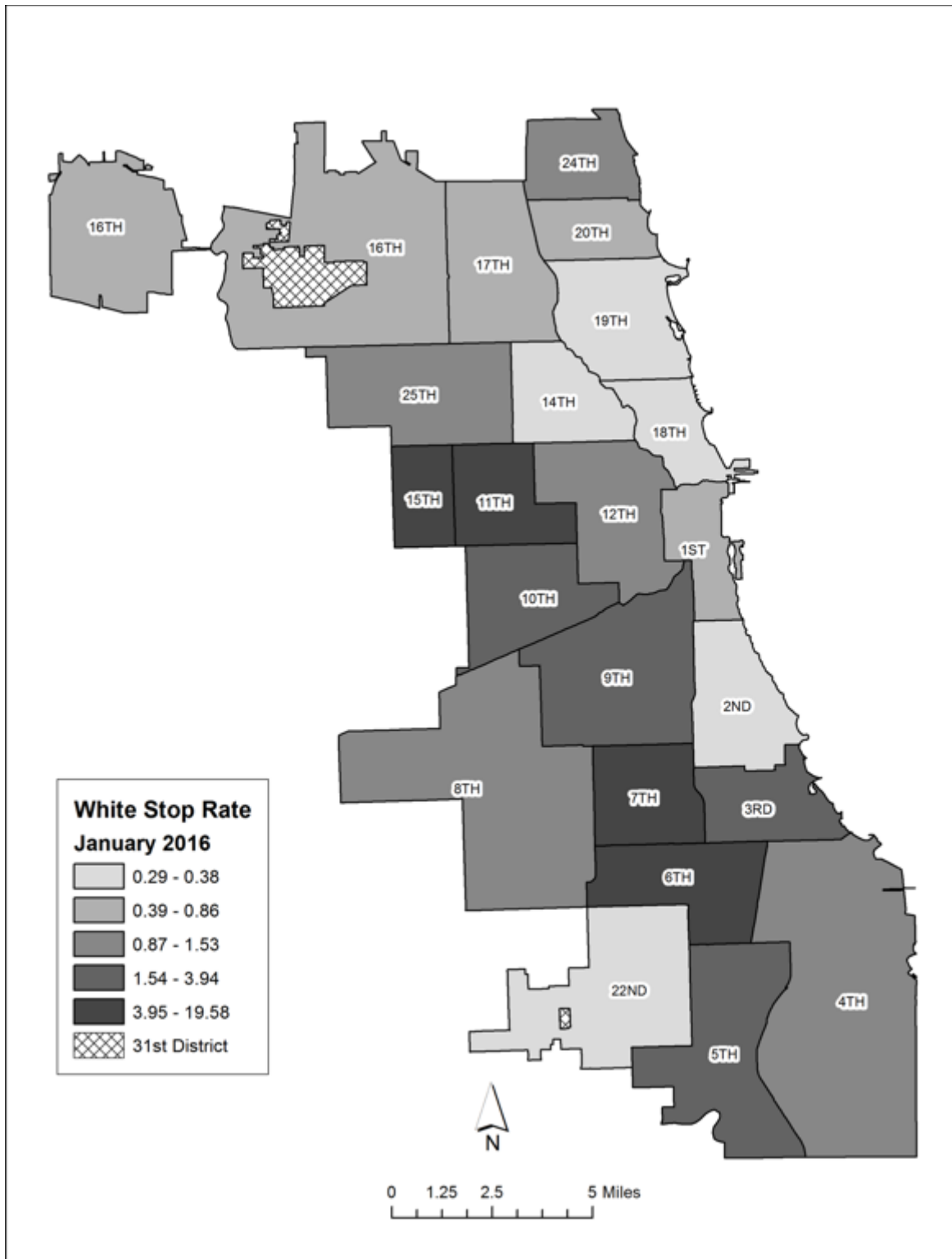


APPENDIX O: Non-Hispanic White Stop Rate (per 1,000), March 2014

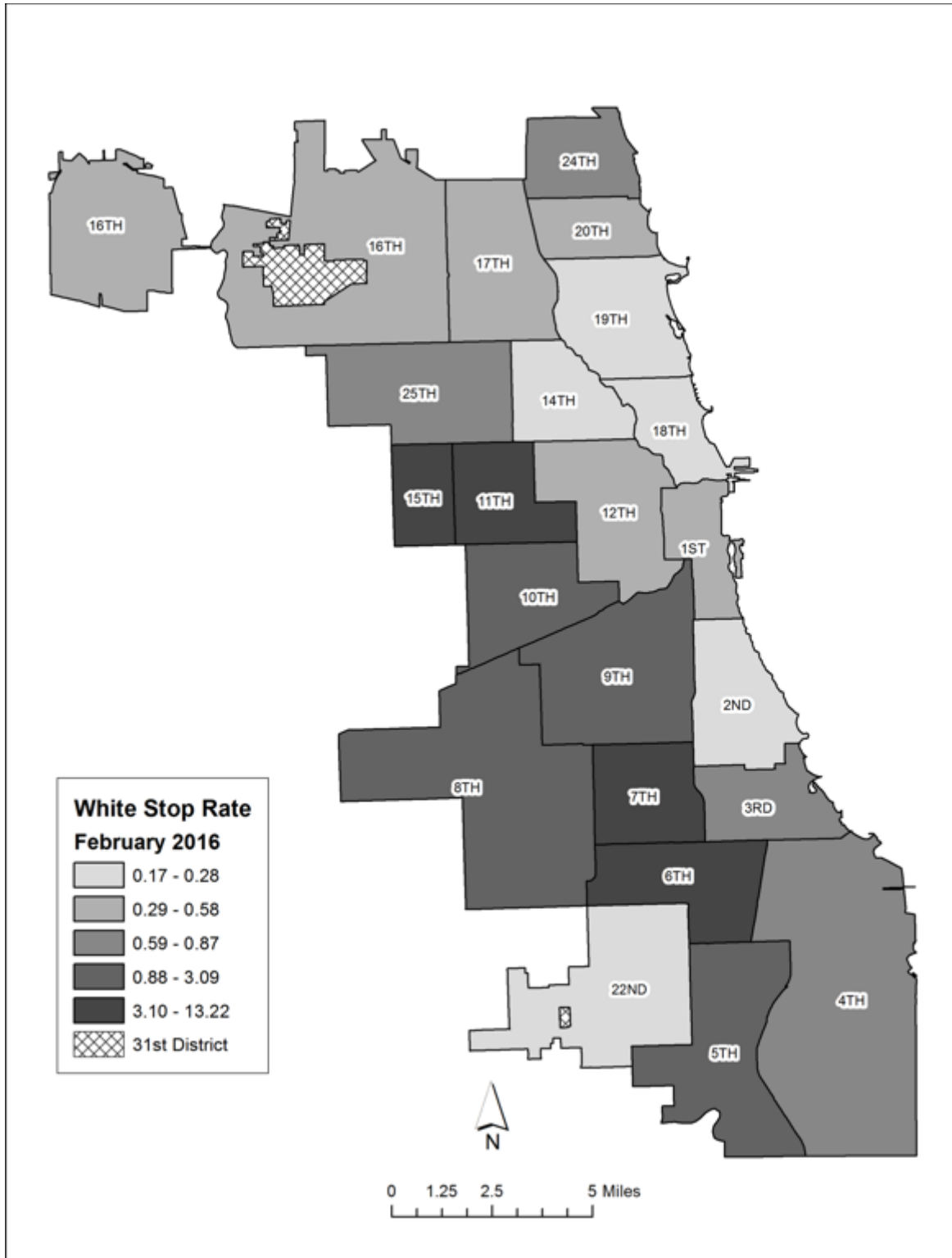


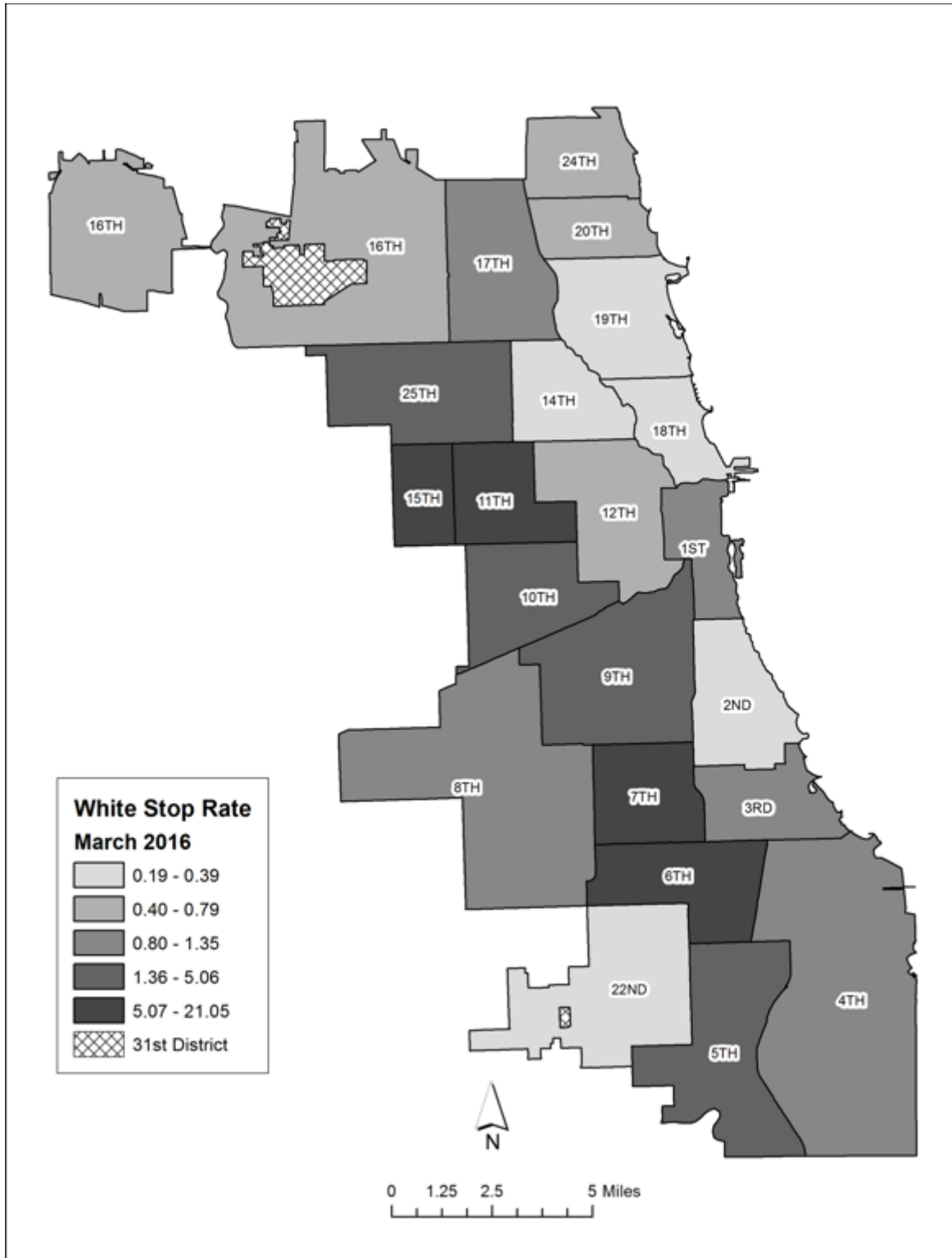


APPENDIX Q: Non-Hispanic White Stop Rate (per 1,000), January 2016

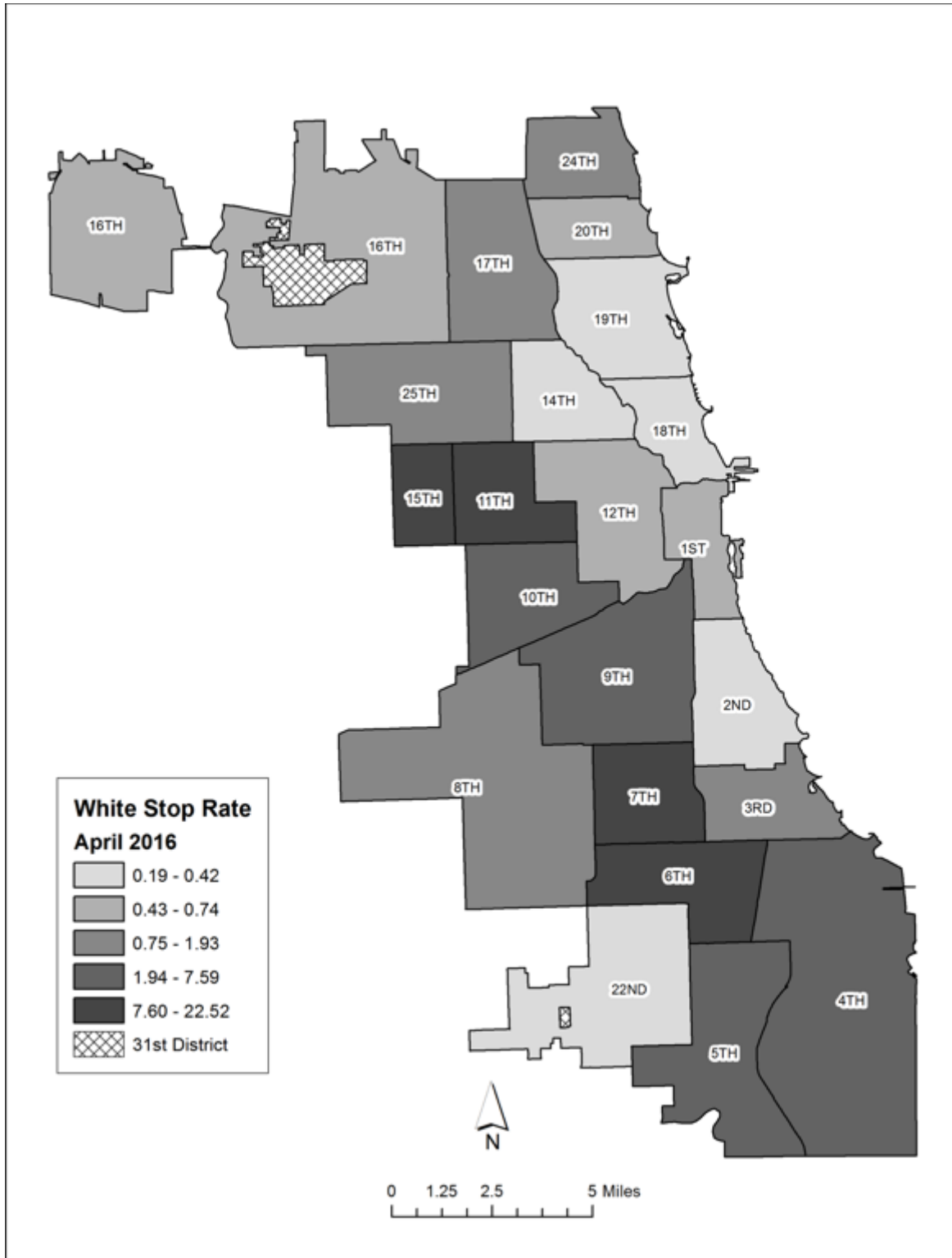


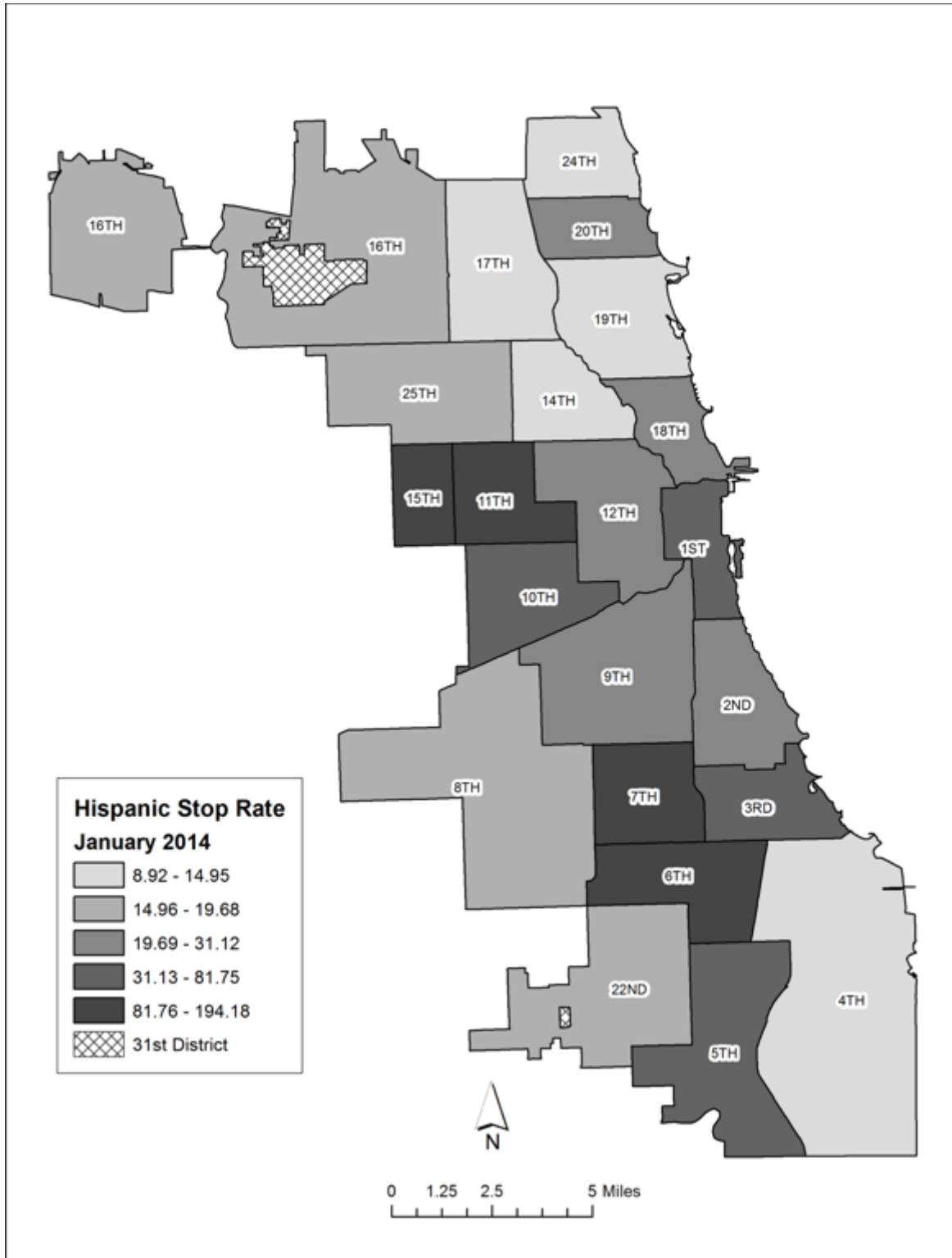


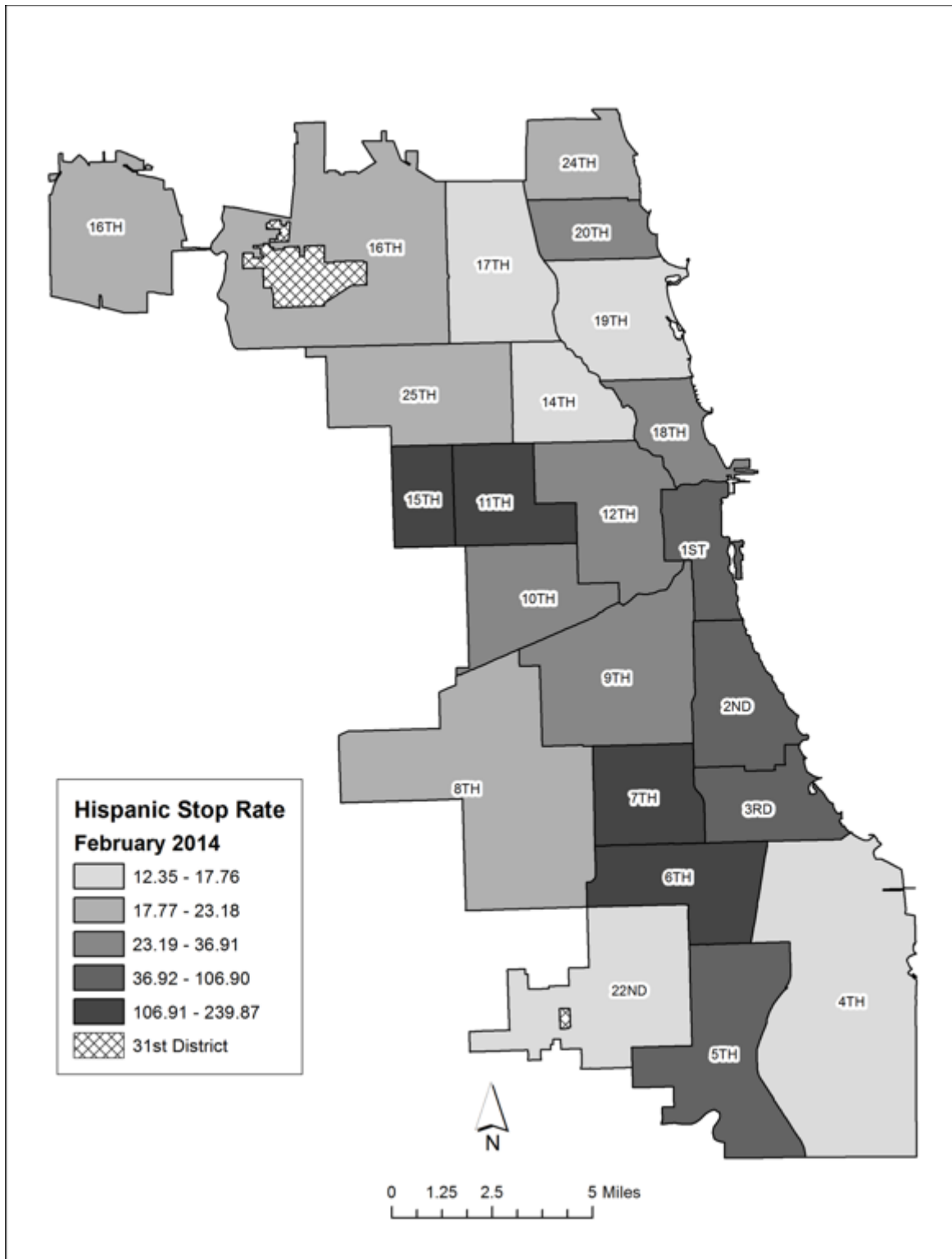




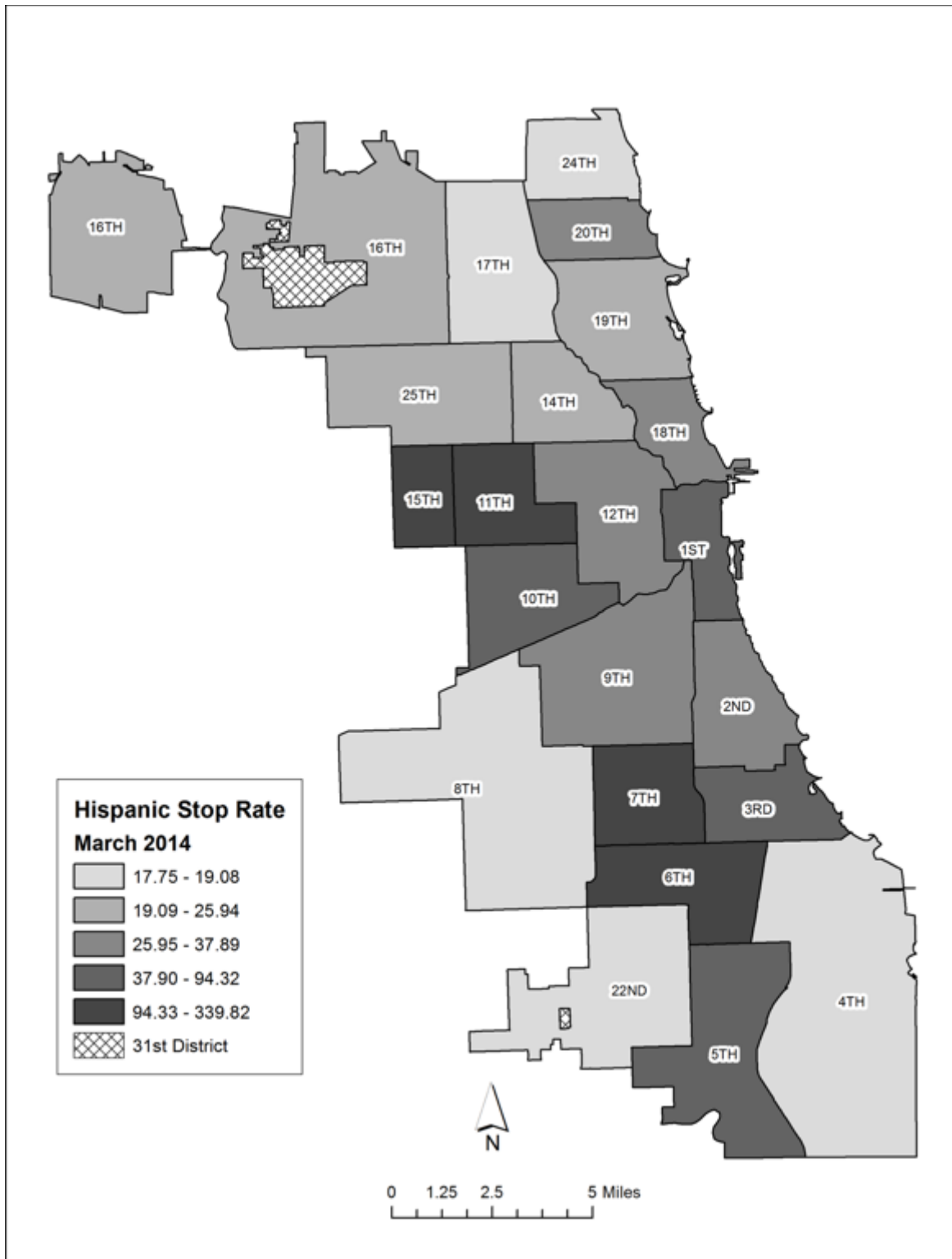
APPENDIX T: Non-Hispanic White Stop Rate (per 1,000), April 2016

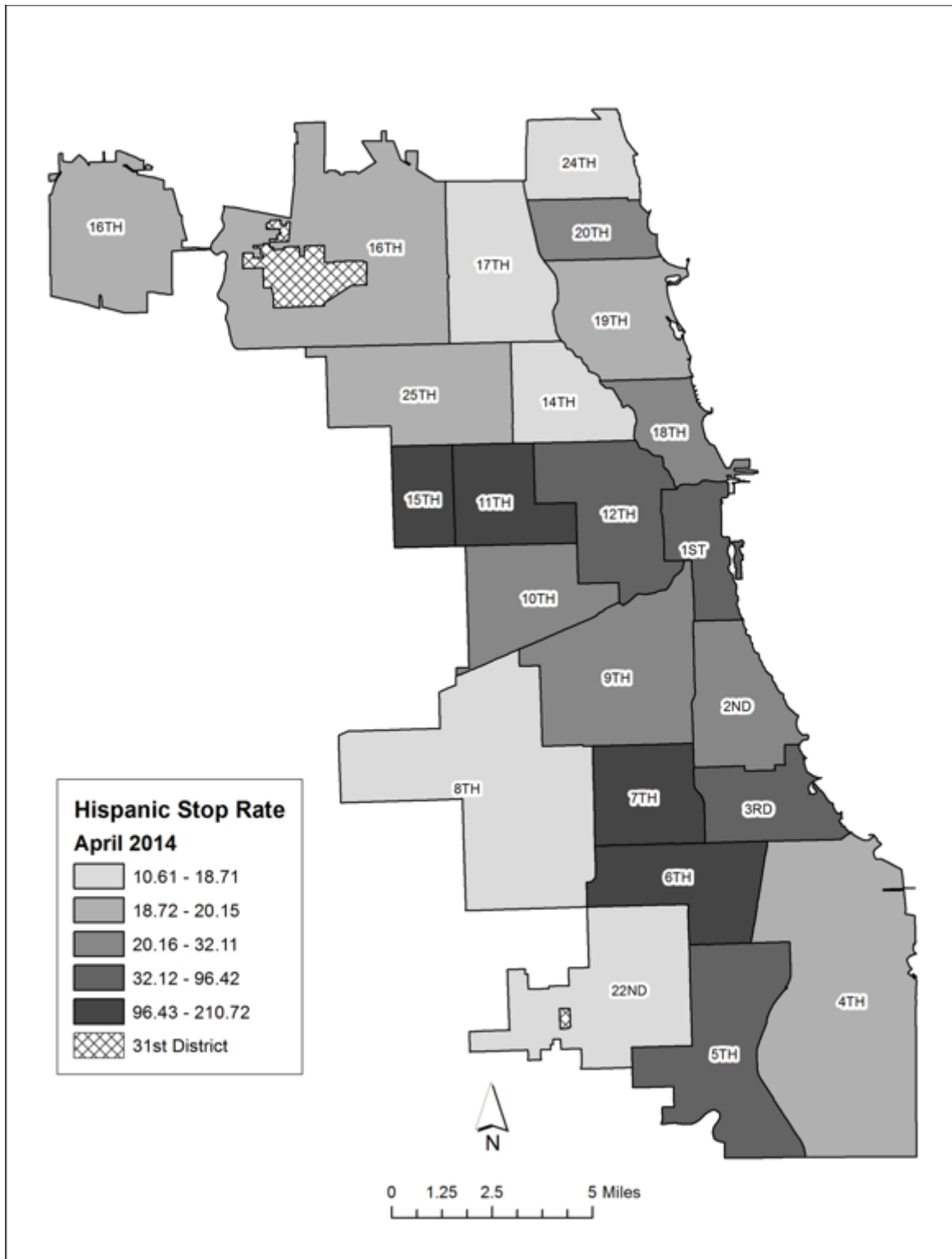


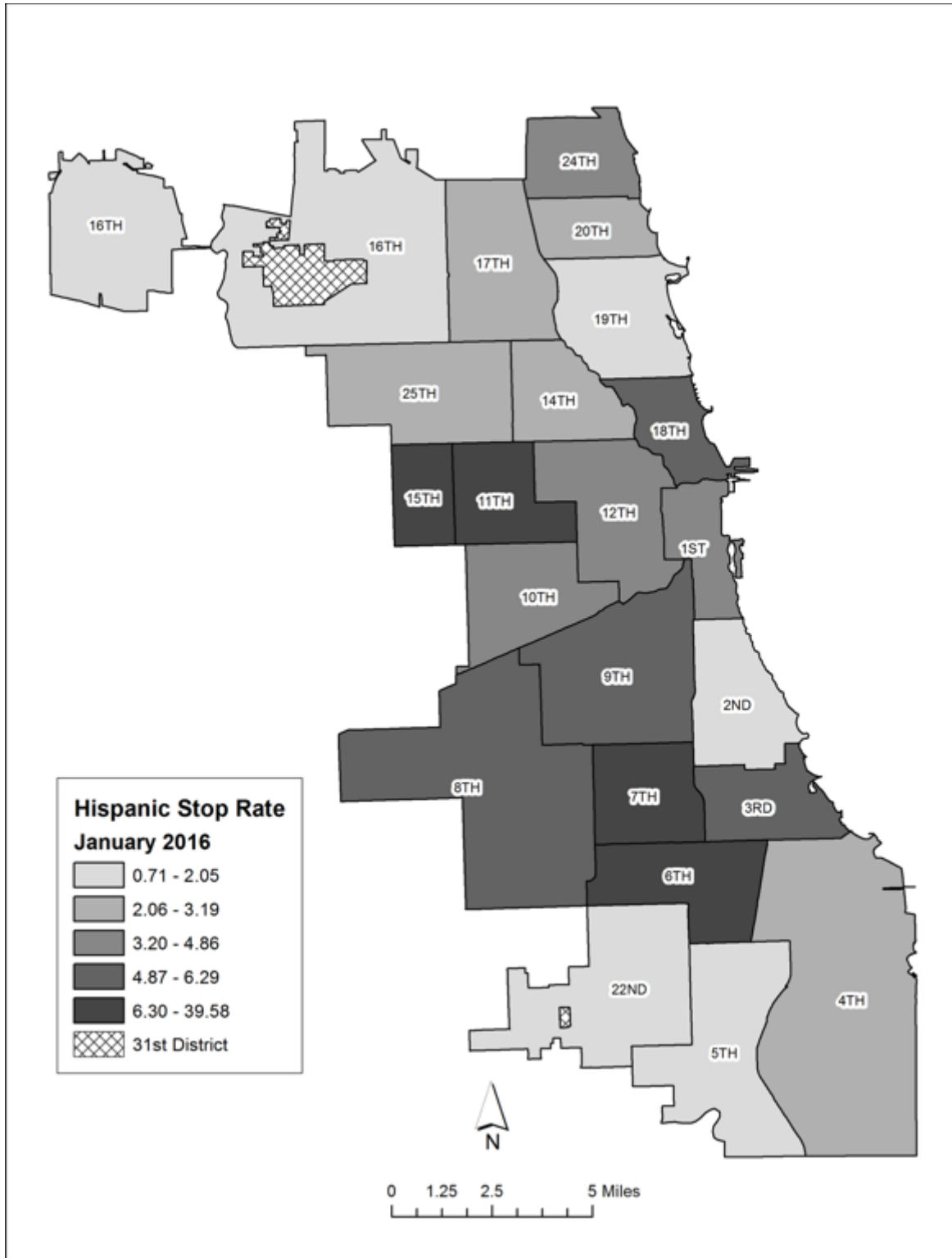




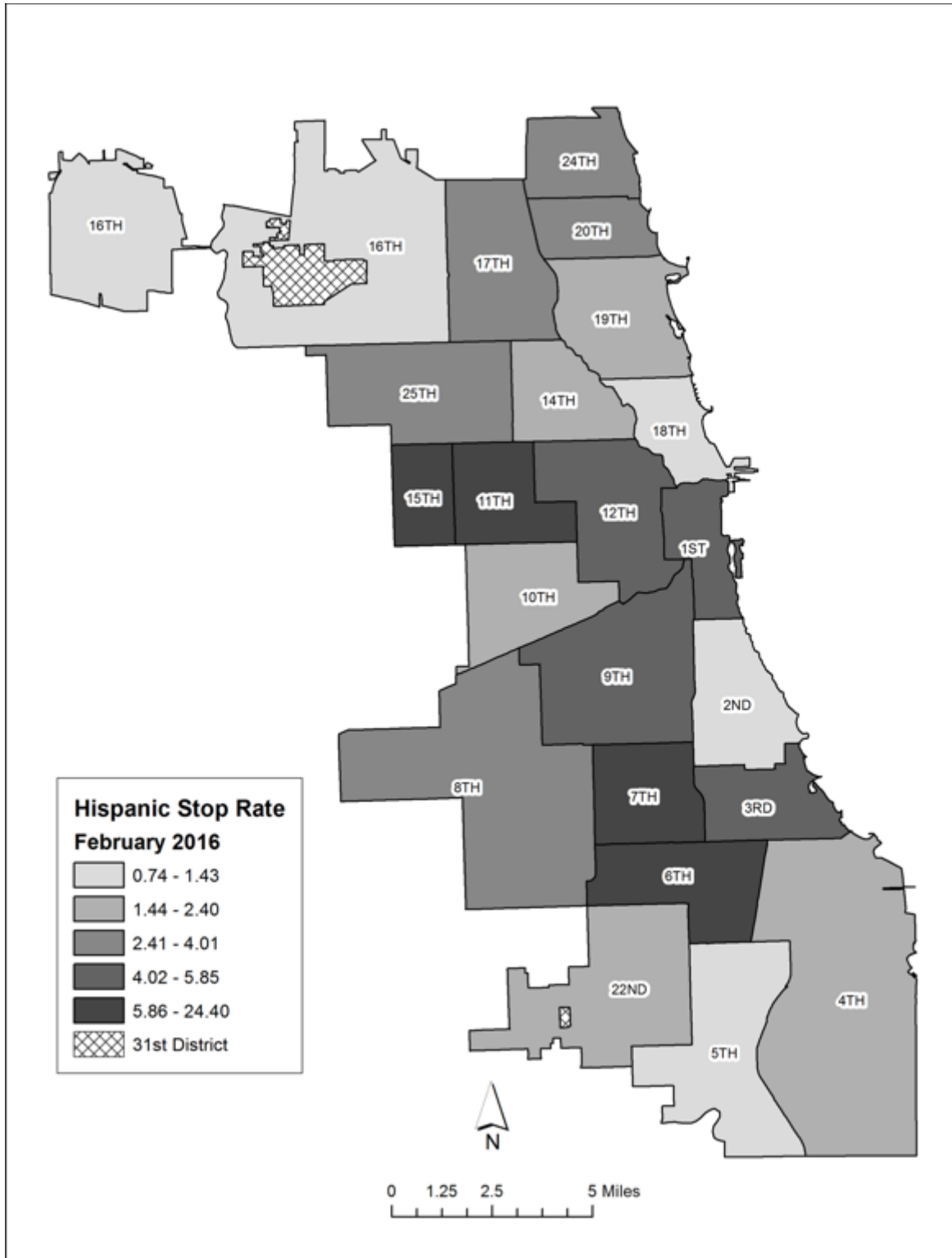
APPENDIX W: Hispanic White Stop Rate (per 1,000), March 2014

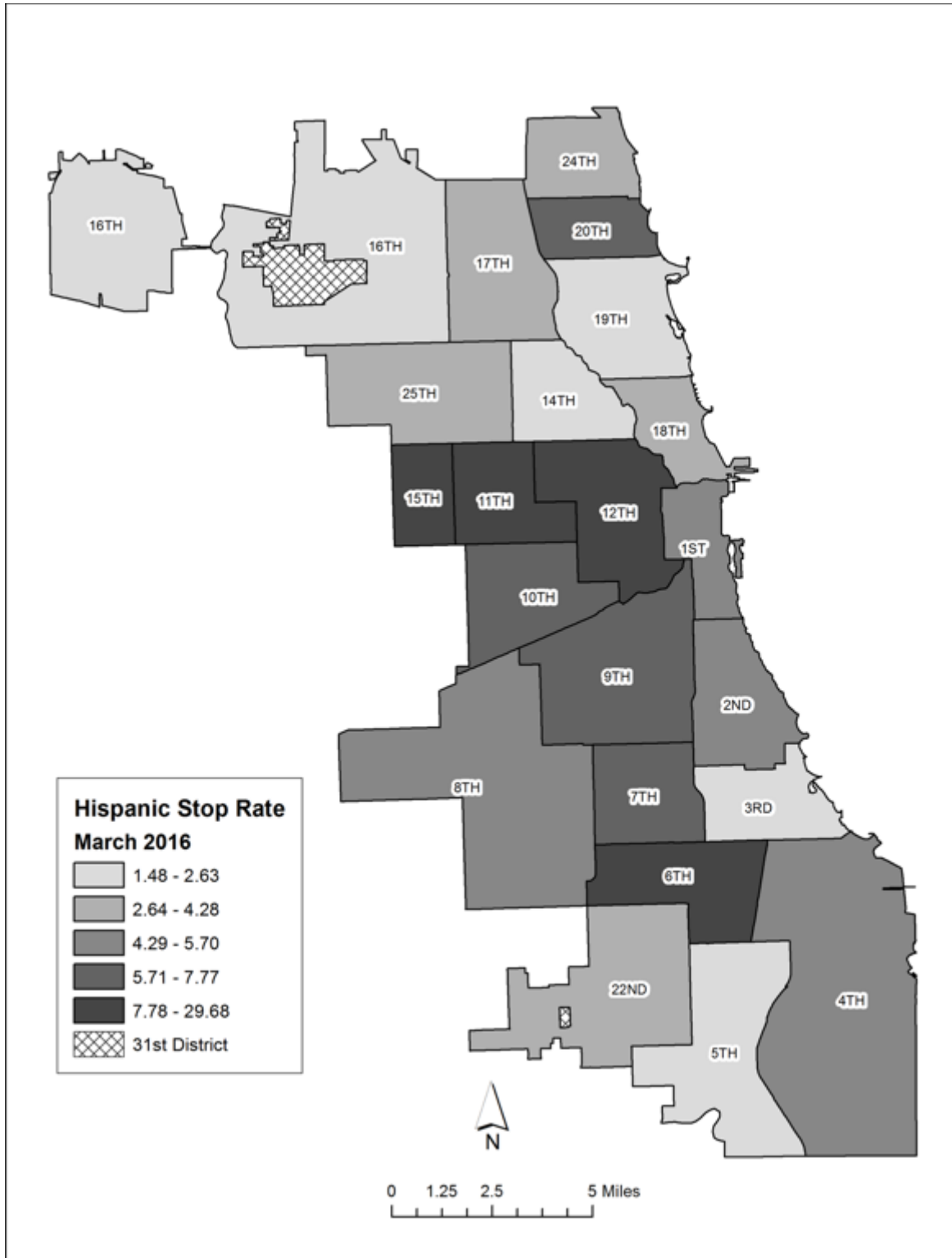


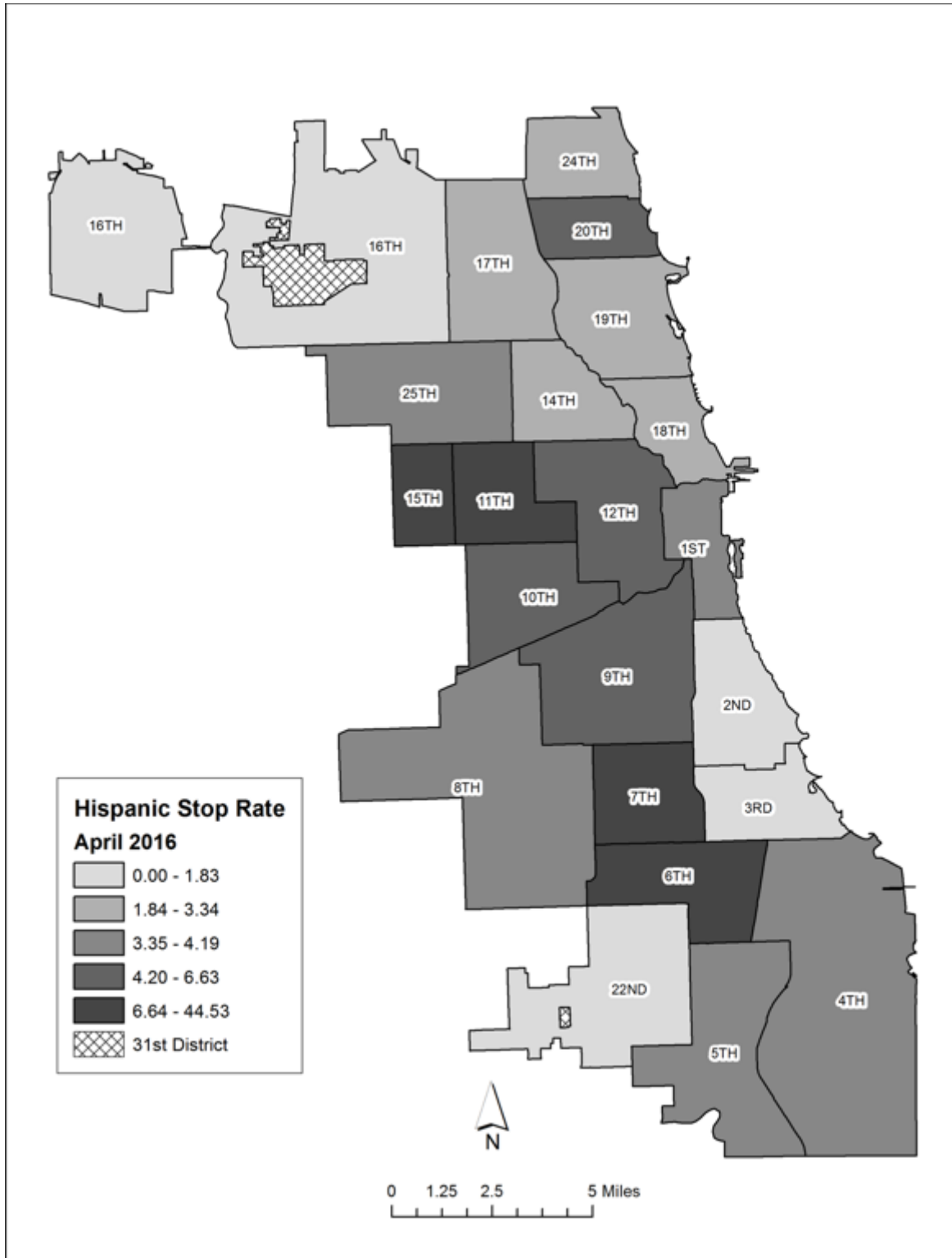












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# Summary Report of Violent Arrest Data, January 2014 – April 2016

Lallen T. Johnson and Ralph B. Taylor

VERSION: 20161213

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## Introduction

This report describes monthly arrest patterns, city wide and at the police district levels, for serious violent incidents (homicides, robberies, and aggravated assaults) conducted by the Chicago Police Department from January 2014 to April 2016.<sup>1</sup> Its format parallels another report for all arrests (Summary Report of Arrest Data, January 2014 – April 2016). The data compiled and presented provide a sketch of changes in arrests for violent incidents over time.

This report's purpose is *purely* descriptive. It will not form inferences about any changes in violence rates. In other words, interpretations of data patterns offered here merely tell what *is* happening where and when, according to these indicators, as opposed *why*.

The data presented here will play a role in another report presenting ecological analyses of investigative stop reports (ISRs). Per discussion of the ecological analysis plans with the parties' experts, the data presented here serve as one of three denominators for those ecological analyses of investigative stop reports (ISRs).<sup>2</sup>

The report presents three major items: 1) city-level counts and rates of arrests for violent incidents for all races/ethnicities, by month, for 28 months (Jan 2014 – April 2016); 2) district-level counts and rates of arrests for violent incidents by race/ethnicity, by month, for the same period, for the three largest race/ethnicity combinations in the city (Non-Hispanic Blacks, Non-Hispanic Whites, and Hispanic Whites); and 3) quantile thematic maps of arrest rates for Non-Hispanic Blacks, Non-Hispanic Whites, and Hispanic Whites for the first four months of 2014, and the first four months of 2016. All analyses exclude arrests associated with the 31<sup>st</sup> district, as these events occurred outside of the Chicago city limits.<sup>3</sup>

## Monthly Violent Arrest Counts and Rates

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<sup>1</sup> In line with existing research, we exclude arrests for rape considering that such a measure is rife with reporting and recording biases (Boggess and Hipp, 2010; Peterson and Krivo, 2010; Gottfredson and Gottfredson, 1988). Statute descriptions of arrests included in this analysis are listed in Appendix A.

<sup>2</sup> Those experts were Sharad Goel, Aziz Huq, Jens Ludwig and Justin McCrary. Technically, the variables noted here will serve as exposure variables in count models.

<sup>3</sup> A total of 7,843 arrests for the classes of violent incidents indicated above (homicides, robberies, aggravated assaults) occurred from January 2014 to April 2016. Of those, 176 arrest records, or 2.2% were missing data on the district variable. Our communication with Officer Joseph Candella of the Chicago Police Department, indicated that cases with missing district information were likely arrests that occurred outside of the city and should be associated with the 31<sup>st</sup> district. He also indicated that a small proportion of the cases could not be geocoded due to address entry errors. We decided to exclude all cases with missing district information considering that they represent such a small fraction of the dataset, and because they are generally outside of the study area. This leaves a total violent arrest count of 7,667.



Table 1 displays monthly violent arrest counts and rates for the city of Chicago from January 2014 to April of 2016 for all races/ethnicities (column labeled “All”), Non-Hispanic Blacks (column labeled “Black”), Non-Hispanic Whites (column labeled “White”) and Hispanic Whites (column labeled “Hispanic”). Violent arrest rates are calculated as the ratio of race/ethnicity-specific arrests to the race/ethnicity-specific population. After, that ratio is multiplied by 10,000. As such, arrest rates can be interpreted as the number of expected arrests, normalized for every 10,000 residents of said racial/ethnic group. The respective population denominators are derived from the American Community Survey.<sup>4</sup>

To be clear, the columns labeled “All” are for the entire city-wide or district-wide population, regardless of race or ethnicity. The columns labeled “Black,” “White,” and “Hispanic” have both numerators (violent arrest counts) and, as relevant, denominators (population counts), that are each specific to one of three race/ethnic combinations: Non-Hispanic Blacks, Non-Hispanic Whites, and Hispanic Whites.

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<sup>4</sup> More specifically, the following 2010-2014 ACS variables were used as denominators.

For Non-Hispanic Blacks: B03002004 (ACS total = 880,066)

For Non-Hispanics Whites: B03002003 (ACS total = 980,789)

For Hispanic Whites: B03002013 (ACS total = 469,978)

Table 1: City-Level Violent Arrest Counts and Rates

Month and Year	Counts				Rates per 10,000 Population			
	All	Black	White	Hispanic	All	Black	White	Hispanic
Jan-14	266	210	12	40	0.98	2.47	0.14	0.91
Feb-14	258	207	17	27	0.95	2.43	0.20	0.61
Mar-14	305	244	13	45	1.13	2.86	0.15	1.02
Apr-14	295	243	9	35	1.09	2.85	0.10	0.79
May-14	352	282	14	52	1.30	3.31	0.16	1.18
Jun-14	373	300	12	56	1.38	3.52	0.14	1.27
Jul-14	344	272	16	50	1.27	3.19	0.18	1.14
Aug-14	332	255	20	55	1.23	2.99	0.23	1.25
Sep-14	328	261	15	46	1.21	3.06	0.17	1.04
Oct-14	356	276	25	48	1.31	3.24	0.29	1.09
Nov-14	231	184	12	30	0.85	2.16	0.14	0.68
Dec-14	262	209	21	31	0.97	2.45	0.24	0.70
Jan-15	229	191	7	28	0.85	2.24	0.08	0.64
Feb-15	194	154	12	26	0.72	1.81	0.14	0.59
Mar-15	234	182	7	40	0.86	2.14	0.08	0.91
Apr-15	244	182	12	43	0.90	2.14	0.14	0.98
May-15	303	214	22	61	1.12	2.51	0.25	1.39
Jun-15	285	216	17	47	1.05	2.54	0.20	1.07
Jul-15	299	231	17	49	1.10	2.71	0.20	1.11
Aug-15	288	221	21	42	1.06	2.59	0.24	0.95
Sep-15	315	238	15	54	1.16	2.79	0.17	1.23
Oct-15	238	200	10	26	0.88	2.35	0.11	0.59

Nov-15	212	171	10	25	0.78	2.01	0.11	0.57
Dec-15	187	152	7	26	0.69	1.78	0.08	0.59
Jan-16	234	179	12	40	0.86	2.10	0.14	0.91
Feb-16	206	164	11	31	0.76	1.93	0.13	0.70
Mar-16	258	192	8	50	0.95	2.25	0.09	1.14
Apr-16	239	188	13	35	0.88	2.21	0.15	0.79

Across all races and ethnicities, a total of 7,667 arrests for violent incidents occurred in the city from January 2014 – April 2016. Annually, 3,702 occurred in 2014, 3,028 in 2015, and 937 from January through April 2016. The violent arrest count dropped 18.2 percent from 2014 to 2015. Looking just at the first four months of 2016, and comparing violent arrests to the violent arrest counts in the same months in 2015, this class of arrests went up 4 percent from 901 to 937.

Among specific races and ethnicities for each year and the four-month period January-April of 2016, Non-Hispanic Blacks demonstrated the highest within-year average monthly arrest rates, per 10,000 resident population (2.88 in 2014, 2.30 in 2015, and 2.12 in 2016). Considering unweighted monthly arrest rates for this group, the average monthly violent arrest rate dropped 20.1 percent from 2014 to 2015. Looking at just the first four months of 2015 and the same period in 2016, the unweighted average monthly arrest rate for this group increased 1.9 percent (2.08 up to 2.12).

The within-year average monthly arrest rates of Non-Hispanic Whites were the lowest of the three groups (0.18 in 2014, 0.15 in 2015, and 0.13 in 2016). For this group the unweighted average monthly arrest rate fell 16.7 percent from 2014 to 2015. Looking at just the first four months of 2015 and the same period in 2016, the unweighted average monthly arrest rate for this group increased 15.9 percent (.11 up to .128).

The Hispanic within-year average monthly arrest rates fell in the middle of the rates of the other two racial/ethnic groups (0.97 in 2014, 0.88 in 2015, and 0.89 in 2016). For Hispanics the unweighted average monthly arrest rate declined 9.3 percent from 2014 to 2015. Looking at just the first four months of 2015 and the same period in 2016, the unweighted average monthly arrest rate for this group increased 13.5 percent (.78 up to .885).

Figures 1 and 2 display line graphs of monthly violent arrest counts and rates, respectively. Visual inspection of Figure 2 indicates a slight decrease in the violent arrests rate trend for all groups between January 2014 to April of 2016 (solid black line). More specifically, the arrest rate in January 2014 was at 0.98 per 10,000 residents. It then peaked at about 1.38 in June 2014 before beginning a slight downward trend through September of the same year. By October, the trend increased again to 1.31. A steep decline followed with rates bottoming out at 0.72 in February 2015. This was followed by a notable increase through May. The period of May through September 2015 witnessed a slight increase in arrest activity that peaked around 1.16. A downward trend followed with rates bottoming out at 0.69 in December 2015, increasing to 0.86 in January 2016 and remaining relatively steady thereafter.

Throughout the entire series, the Non-Hispanic Black arrest rate surpassed the general (All) rate by about twofold. The Non-Hispanic Black rate increased from 2.47 per 10,000 in January 2014 to 3.52 by June of the same year. By February of 2015, the Non-Hispanic Black violent arrest rate fell to 1.81. The trend then reversed, increasing again to 2.79 in September, and then remaining around 2 per 10,000 in 2016, perhaps increasing somewhat.

The arrest rate of Hispanics closely followed the general arrest rate trend, with many monthly rates slightly below the “All” rate and a few months above that rate.

Notwithstanding small, sporadic increases, the Non-Hispanic White rate remained below 0.3 violent arrests per 10,000 per month for the entire series.

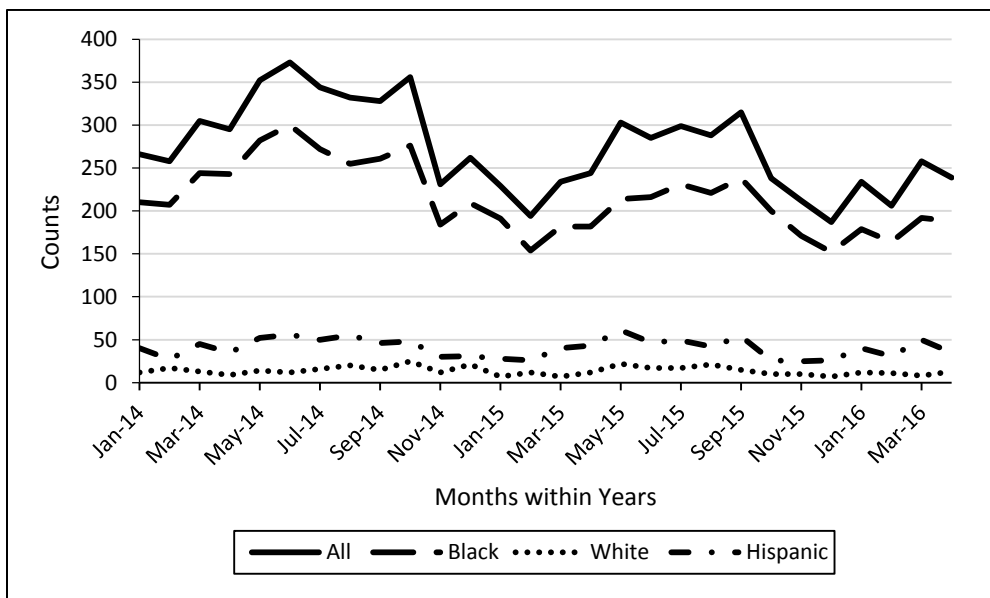


Figure 1: Chicago Monthly Violent Arrest Counts, Jan 2014 - Apr 2016

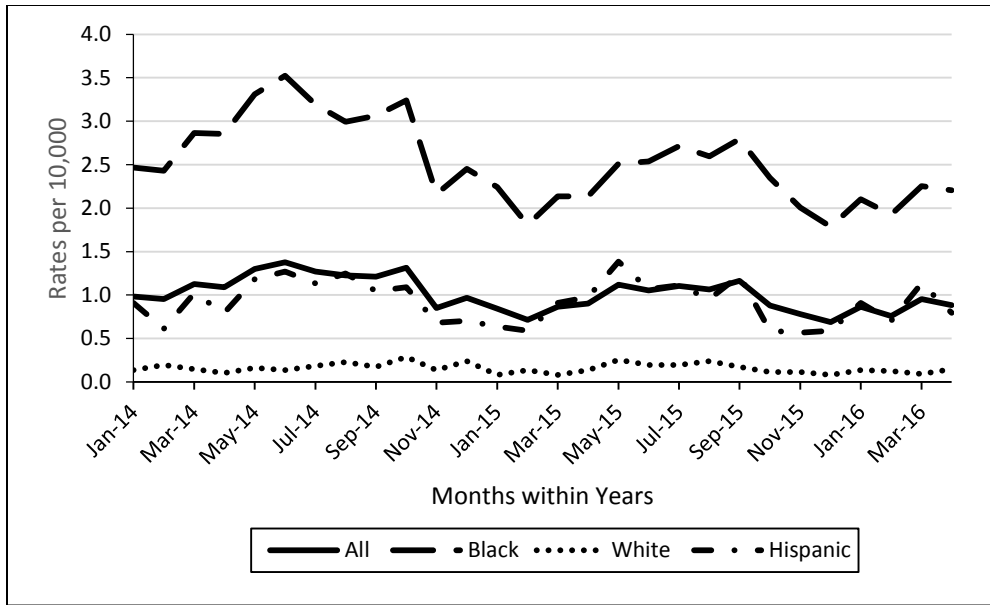


Figure 2: Chicago Monthly Violent Arrest Rates, Jan 2014 - Apr 2016

District-level monthly violent arrest counts and rates by race and ethnicity are shown in APPENDIX B.

## Maps of District-level Monthly Violent Arrest Rates

Thematic maps display data associated with places—in this case, police districts. Each map reveals district-level violent arrest rates for a given month, organized by five quantiles. Each quantile is designed to include 20 percent of Chicago’s 22 police districts. The lowest quantile, indicated by the lightest gray shading on each map, denotes districts where the violent arrest rate for the specified month fell within the lowest 20 percent of districts. The highest quantile, indicated by the darkest shading on each map, identifies districts where violent arrest rates fell within the highest 20 percent of districts.<sup>5</sup> The 31<sup>st</sup> district, denoted by the cross-hatched features in each map, is excluded; arrests in these areas occurred outside of the Chicago city limits. Arrest rate maps appear for the first four months of 2014 and 2016. One map appears for each of the three racial/ethnic groups of key interest: Non-Hispanic Blacks, Non-Hispanic Whites and Hispanic Whites (APPENDICES C – Z).

Visual inspection of Appendices C - J indicate that the highest violent arrest rates for Non-Hispanic Blacks were spatially concentrated primarily in the western and northern districts of Chicago, with the exception of the 4<sup>th</sup> district in the southeastern corner of the city. In the first four months of 2014 the following districts appeared at least once in the top 20 percent of districts on Black violent arrest rates: the 16<sup>th</sup>, 9<sup>th</sup>, 19<sup>th</sup>, 10<sup>th</sup>, 17<sup>th</sup>, 18<sup>th</sup>, and 1<sup>st</sup> districts.

<sup>5</sup> In some maps each quantile, i.e., each shaded group, will not be able to include 20 percent of districts. This happens when many districts have the same score, like a rate of zero.

Districts falling within the lowest quantile tended to be scattered across the northern section of the city as well during various months, but a few were also located in the southern portion (22<sup>nd</sup>, 6<sup>th</sup>, and 7<sup>th</sup> districts).

Violent arrest rates for Non-Hispanic Whites appear in APPENDICES K - R. Violent arrest rates for this group are generally low, often less than 1 per 10,000. Further, the vast majority of districts on any given month have rates of zero for this racial/ethnic group. So this rate represented the lowest quantile. In fact, 16 districts fell within the lowest quantile (rate = 0.00) in January 2014 and 2016. The map series shows that over time, higher arrest rates appeared in various districts scattered across the city.

Violent arrest rates for Hispanics are mapped in APPENDICES S-Z. Oftentimes, some districts in the highest quantile of districts based on arrests of Hispanics for violent offenses clustered west and southwest of The Loop (e.g., Appendices W, X, Y). And, some of these districts experienced increases in violent arrest rates over time from January 2014 to February 2016 (1st, 10th, and 12th). There are some spatial outliers to this pattern, however. For example, the 17<sup>th</sup>, 19<sup>th</sup>, 20<sup>th</sup>, and 24th districts in the far north, and the 4<sup>th</sup>, 5<sup>th</sup>, and 22<sup>nd</sup> districts to the south sometimes appeared in the highest violent arrest rate grouping of districts.

## APPENDIX A: Statute Descriptions of Violent Arrests

### Aggravated Assault

AGG ASSAULT CORR/PROBATION OFF  
AGG ASSAULT HANDICAPPED/60+  
AGG ASSAULT PC OFFICER/VOLUNTEER  
AGG ASSAULT SPORTS OFFICIAL/COACH  
AGG ASSAULT TO A GOV'T EMPLOYEE  
AGG ASSAULT- VOLUNTEER  
AGG ASSAULT/DISCH FIR/MTR VEHICLE  
AGG ASSAULT/DISCHARGE FIREARM  
AGG ASSAULT/OFFICER/FIREARM  
AGG ASSAULT/OP MOTOR VEH/PC OFF  
AGG ASSAULT/OP MOTOR VEH/STRUCK  
AGG ASSAULT/PARK DISTRICT EMP  
AGG ASSAULT/PD/SHERIFF EMP W/FIR  
AGG ASSAULT/PEACE OFFICER/WEAPON  
AGG ASSAULT/POLICE/SHERIFF EMP  
AGG ASSAULT/PRIVATE SEC OFF  
AGG ASSAULT/PROCESS SERVER  
AGG ASSAULT/STATE OF IL EMP  
AGG ASSAULT/TRANSIT EMPLOYEE  
AGG ASSAULT/USE DEADLY WEAPON  
AGG ASSAULT/USE FIR/PEACE OFF  
AGG ASSAULT/WEAR HOOD/ROBE

### Robbery

ROBBERY  
ROBBERY - AGGRAVATED  
ROBBERY - AGG ROBBERY/INDICATE ARM W/FIR  
ROBBERY - ARMED - DISCHARGE FIREARM  
ROBBERY - ARMED - DISCHARGE FIREARM/BODILY HARM  
ROBBERY - ARMED - OTHER DANGEROUS WEAPON  
ROBBERY - ARMED W/ FIREARM  
ROBBERY/SCH/DAY CARE/WORSHIP  
ROBBERY/VIC HANDICAP OR 60+ YR

### Homicide

MURDER - FIRST DEGREE  
MURDER - OTHER FORCIBLE FELONY  
MURDER - SECOND DEGREE MURDER  
MURDER - SOLICITATION FOR HIRE  
MURDER - STRONG PROBABILITY DEATH/INJURE

APPENDIX B: District-Level Monthly Violent Arrest Counts and Rates

District	Month and Year	Counts				Rates per 10,000 population			
		All	Black	White	Hispanic	All	Black	White	Hispanic
01	Jan-14	4	2	0	2	0.60	1.42	0.00	8.03
01	Feb-14	4	4	0	0	0.60	2.85	0.00	0.00
01	Mar-14	5	5	0	0	0.75	3.56	0.00	0.00
01	Apr-14	13	13	0	0	1.94	9.26	0.00	0.00
01	May-14	11	11	0	0	1.64	7.83	0.00	0.00
01	Jun-14	10	8	1	0	1.49	5.70	0.30	0.00
01	Jul-14	10	10	0	0	1.49	7.12	0.00	0.00
01	Aug-14	8	5	1	2	1.20	3.56	0.30	8.03
01	Sep-14	11	9	1	1	1.64	6.41	0.30	4.02
01	Oct-14	7	6	1	0	1.05	4.27	0.30	0.00
01	Nov-14	6	6	0	0	0.90	4.27	0.00	0.00
01	Dec-14	4	3	0	1	0.60	2.14	0.00	4.02
01	Jan-15	3	3	0	0	0.45	2.14	0.00	0.00
01	Feb-15	6	5	1	0	0.90	3.56	0.30	0.00
01	Mar-15	9	7	0	1	1.35	4.99	0.00	4.02
01	Apr-15	2	2	0	0	0.30	1.42	0.00	0.00
01	May-15	6	5	1	0	0.90	3.56	0.30	0.00
01	Jun-15	8	7	0	0	1.20	4.99	0.00	0.00
01	Jul-15	7	7	0	0	1.05	4.99	0.00	0.00
01	Aug-15	9	6	1	1	1.35	4.27	0.30	4.02
01	Sep-15	19	16	1	2	2.84	11.39	0.30	8.03
01	Oct-15	11	10	1	0	1.64	7.12	0.30	0.00
01	Nov-15	10	9	1	0	1.49	6.41	0.30	0.00
01	Dec-15	3	3	0	0	0.45	2.14	0.00	0.00
01	Jan-16	7	7	0	0	1.05	4.99	0.00	0.00
01	Feb-16	17	14	1	2	2.54	9.97	0.30	8.03
01	Mar-16	11	10	1	0	1.64	7.12	0.30	0.00
01	Apr-16	6	6	0	0	0.90	4.27	0.00	0.00
02	Jan-14	16	14	0	1	1.67	2.11	0.00	7.15
02	Feb-14	12	12	0	0	1.25	1.81	0.00	0.00
02	Mar-14	16	16	0	0	1.67	2.41	0.00	0.00
02	Apr-14	15	12	0	3	1.57	1.81	0.00	21.45
02	May-14	35	31	0	4	3.66	4.67	0.00	28.59
02	Jun-14	19	18	0	1	1.99	2.71	0.00	7.15
02	Jul-14	24	22	0	2	2.51	3.31	0.00	14.30
02	Aug-14	19	19	0	0	1.99	2.86	0.00	0.00
02	Sep-14	17	14	0	3	1.78	2.11	0.00	21.45
02	Oct-14	18	18	0	0	1.88	2.71	0.00	0.00



02	Nov-14	10	6	2	2	1.05	0.90	1.15	14.30
02	Dec-14	11	11	0	0	1.15	1.66	0.00	0.00
02	Jan-15	19	19	0	0	1.99	2.86	0.00	0.00
02	Feb-15	5	5	0	0	0.52	0.75	0.00	0.00
02	Mar-15	7	7	0	0	0.73	1.05	0.00	0.00
02	Apr-15	22	22	0	0	2.30	3.31	0.00	0.00
02	May-15	11	10	0	1	1.15	1.51	0.00	7.15
02	Jun-15	11	9	1	1	1.15	1.36	0.57	7.15
02	Jul-15	15	14	1	0	1.57	2.11	0.57	0.00
02	Aug-15	9	9	0	0	0.94	1.36	0.00	0.00
02	Sep-15	10	10	0	0	1.05	1.51	0.00	0.00
02	Oct-15	15	15	0	0	1.57	2.26	0.00	0.00
02	Nov-15	11	11	0	0	1.15	1.66	0.00	0.00
02	Dec-15	3	3	0	0	0.31	0.45	0.00	0.00
02	Jan-16	10	10	0	0	1.05	1.51	0.00	0.00
02	Feb-16	8	8	0	0	0.84	1.20	0.00	0.00
02	Mar-16	11	11	0	0	1.15	1.66	0.00	0.00
02	Apr-16	15	14	1	0	1.57	2.11	0.57	0.00
03	Jan-14	18	18	0	0	2.31	2.54	0.00	0.00
03	Feb-14	15	15	0	0	1.92	2.12	0.00	0.00
03	Mar-14	13	12	0	0	1.67	1.70	0.00	0.00
03	Apr-14	25	23	1	1	3.21	3.25	3.21	20.96
03	May-14	19	19	0	0	2.44	2.69	0.00	0.00
03	Jun-14	9	9	0	0	1.15	1.27	0.00	0.00
03	Jul-14	25	24	1	0	3.21	3.39	3.21	0.00
03	Aug-14	21	21	0	0	2.69	2.97	0.00	0.00
03	Sep-14	23	23	0	0	2.95	3.25	0.00	0.00
03	Oct-14	17	17	0	0	2.18	2.40	0.00	0.00
03	Nov-14	15	15	0	0	1.92	2.12	0.00	0.00
03	Dec-14	25	25	0	0	3.21	3.53	0.00	0.00
03	Jan-15	19	18	1	0	2.44	2.54	3.21	0.00
03	Feb-15	18	18	0	0	2.31	2.54	0.00	0.00
03	Mar-15	16	15	0	0	2.05	2.12	0.00	0.00
03	Apr-15	16	16	0	0	2.05	2.26	0.00	0.00
03	May-15	9	9	0	0	1.15	1.27	0.00	0.00
03	Jun-15	26	26	0	0	3.33	3.67	0.00	0.00
03	Jul-15	13	13	0	0	1.67	1.84	0.00	0.00
03	Aug-15	17	16	0	1	2.18	2.26	0.00	20.96
03	Sep-15	21	21	0	0	2.69	2.97	0.00	0.00
03	Oct-15	10	10	0	0	1.28	1.41	0.00	0.00
03	Nov-15	11	10	0	1	1.41	1.41	0.00	20.96
03	Dec-15	11	11	0	0	1.41	1.55	0.00	0.00
03	Jan-16	20	18	2	0	2.57	2.54	6.42	0.00

03	Feb-16	7	7	0	0	0.90	0.99	0.00	0.00
03	Mar-16	18	18	0	0	2.31	2.54	0.00	0.00
03	Apr-16	20	20	0	0	2.57	2.83	0.00	0.00
04	Jan-14	32	30	2	0	2.67	4.18	1.93	0.00
04	Feb-14	22	18	1	2	1.84	2.51	0.96	0.73
04	Mar-14	21	17	0	4	1.75	2.37	0.00	1.47
04	Apr-14	15	14	0	1	1.25	1.95	0.00	0.37
04	May-14	22	19	0	3	1.84	2.65	0.00	1.10
04	Jun-14	27	23	0	4	2.26	3.21	0.00	1.47
04	Jul-14	26	23	0	3	2.17	3.21	0.00	1.10
04	Aug-14	21	20	0	1	1.75	2.79	0.00	0.37
04	Sep-14	29	20	0	9	2.42	2.79	0.00	3.30
04	Oct-14	17	13	0	4	1.42	1.81	0.00	1.47
04	Nov-14	15	13	1	1	1.25	1.81	0.96	0.37
04	Dec-14	17	14	2	1	1.42	1.95	1.93	0.37
04	Jan-15	25	24	0	1	2.09	3.35	0.00	0.37
04	Feb-15	15	11	1	3	1.25	1.53	0.96	1.10
04	Mar-15	16	16	0	0	1.34	2.23	0.00	0.00
04	Apr-15	17	12	1	4	1.42	1.67	0.96	1.47
04	May-15	11	11	0	0	0.92	1.53	0.00	0.00
04	Jun-15	19	14	1	4	1.59	1.95	0.96	1.47
04	Jul-15	22	19	1	2	1.84	2.65	0.96	0.73
04	Aug-15	17	13	0	4	1.42	1.81	0.00	1.47
04	Sep-15	18	15	0	3	1.50	2.09	0.00	1.10
04	Oct-15	28	27	0	1	2.34	3.76	0.00	0.37
04	Nov-15	21	21	0	0	1.75	2.93	0.00	0.00
04	Dec-15	13	10	0	2	1.09	1.39	0.00	0.73
04	Jan-16	21	20	0	1	1.75	2.79	0.00	0.37
04	Feb-16	25	21	1	3	2.09	2.93	0.96	1.10
04	Mar-16	18	17	0	1	1.50	2.37	0.00	0.37
04	Apr-16	17	12	0	5	1.42	1.67	0.00	1.83
05	Jan-14	10	10	0	0	1.38	1.47	0.00	0.00
05	Feb-14	18	17	0	1	2.48	2.49	0.00	7.38
05	Mar-14	23	19	1	2	3.17	2.79	7.53	14.76
05	Apr-14	16	14	1	0	2.20	2.05	7.53	0.00
05	May-14	19	18	0	1	2.62	2.64	0.00	7.38
05	Jun-14	23	23	0	0	3.17	3.37	0.00	0.00
05	Jul-14	20	20	0	0	2.76	2.93	0.00	0.00
05	Aug-14	19	18	0	1	2.62	2.64	0.00	7.38
05	Sep-14	25	22	1	2	3.44	3.23	7.53	14.76
05	Oct-14	22	19	2	1	3.03	2.79	15.05	7.38
05	Nov-14	22	22	0	0	3.03	3.23	0.00	0.00
05	Dec-14	16	15	0	1	2.20	2.20	0.00	7.38

05	Jan-15	14	14	0	0	1.93	2.05	0.00	0.00
05	Feb-15	14	13	0	1	1.93	1.91	0.00	7.38
05	Mar-15	16	16	0	0	2.20	2.35	0.00	0.00
05	Apr-15	11	11	0	0	1.52	1.61	0.00	0.00
05	May-15	18	15	1	2	2.48	2.20	7.53	14.76
05	Jun-15	15	15	0	0	2.07	2.20	0.00	0.00
05	Jul-15	9	9	0	0	1.24	1.32	0.00	0.00
05	Aug-15	15	14	1	0	2.07	2.05	7.53	0.00
05	Sep-15	10	9	0	1	1.38	1.32	0.00	7.38
05	Oct-15	12	12	0	0	1.65	1.76	0.00	0.00
05	Nov-15	8	8	0	0	1.10	1.17	0.00	0.00
05	Dec-15	10	10	0	0	1.38	1.47	0.00	0.00
05	Jan-16	15	14	0	0	2.07	2.05	0.00	0.00
05	Feb-16	11	11	0	0	1.52	1.61	0.00	0.00
05	Mar-16	4	4	0	0	0.55	0.59	0.00	0.00
05	Apr-16	13	13	0	0	1.79	1.91	0.00	0.00
06	Jan-14	26	25	0	1	2.85	2.83	0.00	49.47
06	Feb-14	14	14	0	0	1.54	1.59	0.00	0.00
06	Mar-14	30	30	0	0	3.29	3.40	0.00	0.00
06	Apr-14	27	27	0	0	2.96	3.06	0.00	0.00
06	May-14	24	24	0	0	2.63	2.72	0.00	0.00
06	Jun-14	39	39	0	0	4.28	4.42	0.00	0.00
06	Jul-14	23	22	0	0	2.52	2.49	0.00	0.00
06	Aug-14	20	20	0	0	2.20	2.27	0.00	0.00
06	Sep-14	19	19	0	0	2.09	2.15	0.00	0.00
06	Oct-14	36	34	2	0	3.95	3.85	35.84	0.00
06	Nov-14	7	7	0	0	0.77	0.79	0.00	0.00
06	Dec-14	22	21	1	0	2.41	2.38	17.92	0.00
06	Jan-15	22	22	0	0	2.41	2.49	0.00	0.00
06	Feb-15	22	22	0	0	2.41	2.49	0.00	0.00
06	Mar-15	14	14	0	0	1.54	1.59	0.00	0.00
06	Apr-15	15	15	0	0	1.65	1.70	0.00	0.00
06	May-15	25	25	0	0	2.74	2.83	0.00	0.00
06	Jun-15	13	13	0	0	1.43	1.47	0.00	0.00
06	Jul-15	16	16	0	0	1.76	1.81	0.00	0.00
06	Aug-15	17	17	0	0	1.87	1.93	0.00	0.00
06	Sep-15	23	23	0	0	2.52	2.61	0.00	0.00
06	Oct-15	19	19	0	0	2.09	2.15	0.00	0.00
06	Nov-15	16	16	0	0	1.76	1.81	0.00	0.00
06	Dec-15	8	8	0	0	0.88	0.91	0.00	0.00
06	Jan-16	10	10	0	0	1.10	1.13	0.00	0.00
06	Feb-16	13	13	0	0	1.43	1.47	0.00	0.00
06	Mar-16	19	19	0	0	2.09	2.15	0.00	0.00

06	Apr-16	21	21	0	0	2.30	2.38	0.00	0.00
07	Jan-14	22	22	0	0	3.35	3.55	0.00	0.00
07	Feb-14	29	28	0	0	4.42	4.52	0.00	0.00
07	Mar-14	16	16	0	0	2.44	2.58	0.00	0.00
07	Apr-14	24	24	0	0	3.66	3.87	0.00	0.00
07	May-14	25	25	0	0	3.81	4.04	0.00	0.00
07	Jun-14	47	47	0	0	7.16	7.59	0.00	0.00
07	Jul-14	29	29	0	0	4.42	4.68	0.00	0.00
07	Aug-14	25	25	0	0	3.81	4.04	0.00	0.00
07	Sep-14	27	26	1	0	4.12	4.20	14.69	0.00
07	Oct-14	21	21	0	0	3.20	3.39	0.00	0.00
07	Nov-14	17	17	0	0	2.59	2.74	0.00	0.00
07	Dec-14	21	21	0	0	3.20	3.39	0.00	0.00
07	Jan-15	14	14	0	0	2.13	2.26	0.00	0.00
07	Feb-15	12	12	0	0	1.83	1.94	0.00	0.00
07	Mar-15	12	12	0	0	1.83	1.94	0.00	0.00
07	Apr-15	14	14	0	0	2.13	2.26	0.00	0.00
07	May-15	19	19	0	0	2.90	3.07	0.00	0.00
07	Jun-15	17	17	0	0	2.59	2.74	0.00	0.00
07	Jul-15	27	26	1	0	4.12	4.20	14.69	0.00
07	Aug-15	25	25	0	0	3.81	4.04	0.00	0.00
07	Sep-15	27	27	0	0	4.12	4.36	0.00	0.00
07	Oct-15	16	16	0	0	2.44	2.58	0.00	0.00
07	Nov-15	10	9	0	1	1.52	1.45	0.00	22.18
07	Dec-15	15	15	0	0	2.29	2.42	0.00	0.00
07	Jan-16	17	14	0	3	2.59	2.26	0.00	66.54
07	Feb-16	5	5	0	0	0.76	0.81	0.00	0.00
07	Mar-16	10	10	0	0	1.52	1.61	0.00	0.00
07	Apr-16	31	30	0	0	4.73	4.84	0.00	0.00
08	Jan-14	28	19	2	7	1.11	3.69	0.40	1.07
08	Feb-14	18	15	0	3	0.71	2.91	0.00	0.46
08	Mar-14	14	10	4	0	0.56	1.94	0.80	0.00
08	Apr-14	15	10	0	5	0.60	1.94	0.00	0.76
08	May-14	20	12	2	6	0.79	2.33	0.40	0.91
08	Jun-14	20	16	0	3	0.79	3.11	0.00	0.46
08	Jul-14	24	17	2	4	0.95	3.30	0.40	0.61
08	Aug-14	23	16	2	5	0.91	3.11	0.40	0.76
08	Sep-14	16	14	0	2	0.64	2.72	0.00	0.30
08	Oct-14	15	12	0	3	0.60	2.33	0.00	0.46
08	Nov-14	12	8	1	3	0.48	1.55	0.20	0.46
08	Dec-14	13	7	5	1	0.52	1.36	1.01	0.15
08	Jan-15	12	12	0	0	0.48	2.33	0.00	0.00
08	Feb-15	11	5	2	4	0.44	0.97	0.40	0.61

08	Mar-15	12	8	0	4	0.48	1.55	0.00	0.61
08	Apr-15	13	9	3	1	0.52	1.75	0.60	0.15
08	May-15	21	10	3	7	0.83	1.94	0.60	1.07
08	Jun-15	18	8	1	9	0.71	1.55	0.20	1.37
08	Jul-15	21	10	5	6	0.83	1.94	1.01	0.91
08	Aug-15	13	7	3	3	0.52	1.36	0.60	0.46
08	Sep-15	27	16	1	10	1.07	3.11	0.20	1.52
08	Oct-15	6	3	1	2	0.24	0.58	0.20	0.30
08	Nov-15	15	13	0	2	0.60	2.52	0.00	0.30
08	Dec-15	15	8	1	6	0.60	1.55	0.20	0.91
08	Jan-16	14	4	4	6	0.56	0.78	0.80	0.91
08	Feb-16	14	11	1	2	0.56	2.14	0.20	0.30
08	Mar-16	19	14	1	4	0.75	2.72	0.20	0.61
08	Apr-16	7	0	1	6	0.28	0.00	0.20	0.91
09	Jan-14	11	5	0	6	0.67	2.77	0.00	1.09
09	Feb-14	21	11	5	4	1.27	6.09	2.07	0.73
09	Mar-14	14	10	0	4	0.85	5.53	0.00	0.73
09	Apr-14	19	13	1	5	1.15	7.19	0.41	0.91
09	May-14	17	8	1	8	1.03	4.43	0.41	1.45
09	Jun-14	18	12	1	4	1.09	6.64	0.41	0.73
09	Jul-14	13	6	2	5	0.79	3.32	0.83	0.91
09	Aug-14	24	15	4	4	1.46	8.30	1.65	0.73
09	Sep-14	15	11	1	2	0.91	6.09	0.41	0.36
09	Oct-14	29	13	3	13	1.76	7.19	1.24	2.36
09	Nov-14	18	14	0	4	1.09	7.75	0.00	0.73
09	Dec-14	12	9	1	2	0.73	4.98	0.41	0.36
09	Jan-15	14	7	2	5	0.85	3.87	0.83	0.91
09	Feb-15	11	6	1	4	0.67	3.32	0.41	0.73
09	Mar-15	9	6	0	2	0.55	3.32	0.00	0.36
09	Apr-15	17	13	0	4	1.03	7.19	0.00	0.73
09	May-15	26	12	3	9	1.58	6.64	1.24	1.63
09	Jun-15	18	7	3	8	1.09	3.87	1.24	1.45
09	Jul-15	27	10	6	11	1.64	5.53	2.48	2.00
09	Aug-15	14	7	2	5	0.85	3.87	0.83	0.91
09	Sep-15	19	12	3	4	1.15	6.64	1.24	0.73
09	Oct-15	10	7	0	2	0.61	3.87	0.00	0.36
09	Nov-15	15	7	1	7	0.91	3.87	0.41	1.27
09	Dec-15	8	4	1	3	0.49	2.21	0.41	0.54
09	Jan-16	15	6	1	8	0.91	3.32	0.41	1.45
09	Feb-16	5	1	1	3	0.30	0.55	0.41	0.54
09	Mar-16	15	9	0	6	0.91	4.98	0.00	1.09
09	Apr-16	10	4	3	3	0.61	2.21	1.24	0.54
10	Jan-14	21	18	0	3	1.94	5.13	0.00	0.63

10	Feb-14	13	11	0	2	1.20	3.14	0.00	0.42
10	Mar-14	23	18	0	5	2.13	5.13	0.00	1.05
10	Apr-14	16	13	0	3	1.48	3.71	0.00	0.63
10	May-14	19	13	0	6	1.76	3.71	0.00	1.26
10	Jun-14	34	19	0	15	3.14	5.42	0.00	3.14
10	Jul-14	20	15	1	4	1.85	4.28	2.81	0.84
10	Aug-14	25	15	1	9	2.31	4.28	2.81	1.89
10	Sep-14	24	15	1	7	2.22	4.28	2.81	1.47
10	Oct-14	27	19	1	7	2.50	5.42	2.81	1.47
10	Nov-14	8	6	0	2	0.74	1.71	0.00	0.42
10	Dec-14	15	13	0	2	1.39	3.71	0.00	0.42
10	Jan-15	8	7	0	1	0.74	2.00	0.00	0.21
10	Feb-15	7	2	0	5	0.65	0.57	0.00	1.05
10	Mar-15	18	11	2	5	1.66	3.14	5.62	1.05
10	Apr-15	11	7	1	3	1.02	2.00	2.81	0.63
10	May-15	21	12	0	9	1.94	3.42	0.00	1.89
10	Jun-15	21	18	2	1	1.94	5.13	5.62	0.21
10	Jul-15	16	12	0	4	1.48	3.42	0.00	0.84
10	Aug-15	17	13	0	4	1.57	3.71	0.00	0.84
10	Sep-15	27	15	1	11	2.50	4.28	2.81	2.31
10	Oct-15	22	15	0	7	2.03	4.28	0.00	1.47
10	Nov-15	19	15	0	3	1.76	4.28	0.00	0.63
10	Dec-15	26	18	0	8	2.40	5.13	0.00	1.68
10	Jan-16	12	7	0	4	1.11	2.00	0.00	0.84
10	Feb-16	13	7	0	6	1.20	2.00	0.00	1.26
10	Mar-16	18	12	0	6	1.66	3.42	0.00	1.26
10	Apr-16	17	13	0	4	1.57	3.71	0.00	0.84
11	Jan-14	8	7	0	1	1.11	1.16	0.00	3.49
11	Feb-14	14	14	0	0	1.94	2.31	0.00	0.00
11	Mar-14	23	21	0	2	3.19	3.47	0.00	6.99
11	Apr-14	23	22	1	0	3.19	3.63	4.90	0.00
11	May-14	13	11	1	1	1.81	1.82	4.90	3.49
11	Jun-14	16	14	1	0	2.22	2.31	4.90	0.00
11	Jul-14	16	15	0	1	2.22	2.48	0.00	3.49
11	Aug-14	23	20	0	3	3.19	3.30	0.00	10.48
11	Sep-14	22	21	1	0	3.05	3.47	4.90	0.00
11	Oct-14	24	22	0	1	3.33	3.63	0.00	3.49
11	Nov-14	20	19	0	0	2.78	3.14	0.00	0.00
11	Dec-14	18	17	0	1	2.50	2.81	0.00	3.49
11	Jan-15	14	14	0	0	1.94	2.31	0.00	0.00
11	Feb-15	16	14	1	0	2.22	2.31	4.90	0.00
11	Mar-15	14	13	0	1	1.94	2.15	0.00	3.49
11	Apr-15	18	16	0	1	2.50	2.64	0.00	3.49

11	May-15	34	32	0	1	4.72	5.29	0.00	3.49
11	Jun-15	19	18	1	0	2.64	2.97	4.90	0.00
11	Jul-15	23	21	0	2	3.19	3.47	0.00	6.99
11	Aug-15	20	18	0	2	2.78	2.97	0.00	6.99
11	Sep-15	22	18	1	3	3.05	2.97	4.90	10.48
11	Oct-15	19	17	0	2	2.64	2.81	0.00	6.99
11	Nov-15	12	12	0	0	1.67	1.98	0.00	0.00
11	Dec-15	22	19	0	3	3.05	3.14	0.00	10.48
11	Jan-16	20	20	0	0	2.78	3.30	0.00	0.00
11	Feb-16	13	13	0	0	1.81	2.15	0.00	0.00
11	Mar-16	13	11	0	1	1.81	1.82	0.00	3.49
11	Apr-16	18	18	0	0	2.50	2.97	0.00	0.00
12	Jan-14	7	4	3	0	0.54	1.71	0.55	0.00
12	Feb-14	5	3	0	2	0.38	1.28	0.00	0.87
12	Mar-14	22	18	2	2	1.69	7.70	0.37	0.87
12	Apr-14	12	9	0	1	0.92	3.85	0.00	0.43
12	May-14	11	7	1	3	0.84	2.99	0.18	1.30
12	Jun-14	10	8	1	1	0.77	3.42	0.18	0.43
12	Jul-14	15	13	1	1	1.15	5.56	0.18	0.43
12	Aug-14	11	9	1	1	0.84	3.85	0.18	0.43
12	Sep-14	10	6	1	3	0.77	2.57	0.18	1.30
12	Oct-14	10	9	1	0	0.77	3.85	0.18	0.00
12	Nov-14	17	11	0	5	1.31	4.71	0.00	2.17
12	Dec-14	13	9	2	2	1.00	3.85	0.37	0.87
12	Jan-15	10	9	0	1	0.77	3.85	0.00	0.43
12	Feb-15	6	3	2	1	0.46	1.28	0.37	0.43
12	Mar-15	13	7	0	5	1.00	2.99	0.00	2.17
12	Apr-15	6	3	0	3	0.46	1.28	0.00	1.30
12	May-15	11	7	0	3	0.84	2.99	0.00	1.30
12	Jun-15	21	17	1	3	1.61	7.27	0.18	1.30
12	Jul-15	11	8	0	3	0.84	3.42	0.00	1.30
12	Aug-15	17	8	5	4	1.31	3.42	0.92	1.73
12	Sep-15	10	5	2	3	0.77	2.14	0.37	1.30
12	Oct-15	11	10	1	0	0.84	4.28	0.18	0.00
12	Nov-15	6	5	0	1	0.46	2.14	0.00	0.43
12	Dec-15	15	12	1	2	1.15	5.13	0.18	0.87
12	Jan-16	19	18	0	1	1.46	7.70	0.00	0.43
12	Feb-16	15	9	2	4	1.15	3.85	0.37	1.73
12	Mar-16	14	9	0	5	1.08	3.85	0.00	2.17
12	Apr-16	8	6	0	2	0.61	2.57	0.00	0.87
14	Jan-14	4	0	0	4	0.33	0.00	0.00	1.15
14	Feb-14	5	2	0	2	0.42	2.30	0.00	0.57
14	Mar-14	9	3	0	6	0.75	3.44	0.00	1.72

14	Apr-14	10	4	0	5	0.84	4.59	0.00	1.43
14	May-14	14	6	2	5	1.17	6.89	0.37	1.43
14	Jun-14	11	0	2	9	0.92	0.00	0.37	2.58
14	Jul-14	14	2	4	7	1.17	2.30	0.73	2.01
14	Aug-14	8	4	0	4	0.67	4.59	0.00	1.15
14	Sep-14	9	4	1	4	0.75	4.59	0.18	1.15
14	Oct-14	13	9	1	2	1.09	10.33	0.18	0.57
14	Nov-14	4	1	0	2	0.33	1.15	0.00	0.57
14	Dec-14	5	2	1	2	0.42	2.30	0.18	0.57
14	Jan-15	3	1	0	2	0.25	1.15	0.00	0.57
14	Feb-15	3	0	0	3	0.25	0.00	0.00	0.86
14	Mar-15	4	0	0	4	0.33	0.00	0.00	1.15
14	Apr-15	18	8	2	8	1.51	9.18	0.37	2.30
14	May-15	8	2	3	3	0.67	2.30	0.55	0.86
14	Jun-15	7	0	0	7	0.59	0.00	0.00	2.01
14	Jul-15	10	5	1	4	0.84	5.74	0.18	1.15
14	Aug-15	11	3	5	1	0.92	3.44	0.92	0.29
14	Sep-15	5	2	1	2	0.42	2.30	0.18	0.57
14	Oct-15	6	4	1	1	0.50	4.59	0.18	0.29
14	Nov-15	4	1	1	1	0.33	1.15	0.18	0.29
14	Dec-15	1	1	0	0	0.08	1.15	0.00	0.00
14	Jan-16	3	0	0	3	0.25	0.00	0.00	0.86
14	Feb-16	6	4	0	2	0.50	4.59	0.00	0.57
14	Mar-16	9	4	1	4	0.75	4.59	0.18	1.15
14	Apr-16	3	2	1	0	0.25	2.30	0.18	0.00
15	Jan-14	16	16	0	0	2.70	2.91	0.00	0.00
15	Feb-14	12	12	0	0	2.03	2.18	0.00	0.00
15	Mar-14	19	17	0	1	3.21	3.09	0.00	14.28
15	Apr-14	19	19	0	0	3.21	3.46	0.00	0.00
15	May-14	33	33	0	0	5.57	6.01	0.00	0.00
15	Jun-14	12	12	0	0	2.03	2.18	0.00	0.00
15	Jul-14	17	13	1	3	2.87	2.37	8.30	42.83
15	Aug-14	17	17	0	0	2.87	3.09	0.00	0.00
15	Sep-14	15	13	0	1	2.53	2.37	0.00	14.28
15	Oct-14	21	20	0	1	3.55	3.64	0.00	14.28
15	Nov-14	11	11	0	0	1.86	2.00	0.00	0.00
15	Dec-14	20	18	1	1	3.38	3.28	8.30	14.28
15	Jan-15	9	9	0	0	1.52	1.64	0.00	0.00
15	Feb-15	12	12	0	0	2.03	2.18	0.00	0.00
15	Mar-15	17	17	0	0	2.87	3.09	0.00	0.00
15	Apr-15	17	17	0	0	2.87	3.09	0.00	0.00
15	May-15	10	10	0	0	1.69	1.82	0.00	0.00
15	Jun-15	10	9	0	1	1.69	1.64	0.00	14.28



15	Jul-15	13	12	0	1	2.20	2.18	0.00	14.28
15	Aug-15	19	19	0	0	3.21	3.46	0.00	0.00
15	Sep-15	7	5	1	1	1.18	0.91	8.30	14.28
15	Oct-15	10	10	0	0	1.69	1.82	0.00	0.00
15	Nov-15	9	7	1	1	1.52	1.27	8.30	14.28
15	Dec-15	15	15	0	0	2.53	2.73	0.00	0.00
15	Jan-16	9	9	0	0	1.52	1.64	0.00	0.00
15	Feb-16	10	10	0	0	1.69	1.82	0.00	0.00
15	Mar-16	20	19	0	1	3.38	3.46	0.00	14.28
15	Apr-16	9	9	0	0	1.52	1.64	0.00	0.00
16	Jan-14	6	3	1	1	0.29	12.44	0.07	0.36
16	Feb-14	8	2	4	2	0.39	8.29	0.29	0.72
16	Mar-14	2	0	0	2	0.10	0.00	0.00	0.72
16	Apr-14	0	0	0	0	0.00	0.00	0.00	0.00
16	May-14	9	1	5	2	0.44	4.15	0.36	0.72
16	Jun-14	7	0	3	4	0.34	0.00	0.22	1.44
16	Jul-14	5	0	0	5	0.24	0.00	0.00	1.80
16	Aug-14	5	1	4	0	0.24	4.15	0.29	0.00
16	Sep-14	3	0	1	1	0.15	0.00	0.07	0.36
16	Oct-14	16	0	10	5	0.78	0.00	0.73	1.80
16	Nov-14	1	0	1	0	0.05	0.00	0.07	0.00
16	Dec-14	7	2	2	3	0.34	8.29	0.15	1.08
16	Jan-15	2	0	1	1	0.10	0.00	0.07	0.36
16	Feb-15	2	0	2	0	0.10	0.00	0.15	0.00
16	Mar-15	2	0	1	1	0.10	0.00	0.07	0.36
16	Apr-15	8	1	2	4	0.39	4.15	0.15	1.44
16	May-15	5	1	3	1	0.24	4.15	0.22	0.36
16	Jun-15	3	1	0	1	0.15	4.15	0.00	0.36
16	Jul-15	7	2	0	5	0.34	8.29	0.00	1.80
16	Aug-15	5	1	2	1	0.24	4.15	0.15	0.36
16	Sep-15	9	0	3	2	0.44	0.00	0.22	0.72
16	Oct-15	7	3	2	2	0.34	12.44	0.15	0.72
16	Nov-15	11	3	4	2	0.54	12.44	0.29	0.72
16	Dec-15	0	0	0	0	0.00	0.00	0.00	0.00
16	Jan-16	5	1	2	2	0.24	4.15	0.15	0.72
16	Feb-16	4	2	2	0	0.19	8.29	0.15	0.00
16	Mar-16	7	2	0	4	0.34	8.29	0.00	1.44
16	Apr-16	4	0	0	3	0.19	0.00	0.00	1.08
17	Jan-14	6	0	3	3	0.40	0.00	0.53	0.77
17	Feb-14	12	1	3	7	0.81	2.19	0.53	1.80
17	Mar-14	6	1	2	3	0.40	2.19	0.35	0.77
17	Apr-14	6	4	0	1	0.40	8.75	0.00	0.26
17	May-14	5	3	0	2	0.34	6.56	0.00	0.52

17	Jun-14	7	3	0	3	0.47	6.56	0.00	0.77
17	Jul-14	2	1	0	1	0.13	2.19	0.00	0.26
17	Aug-14	5	1	1	3	0.34	2.19	0.18	0.77
17	Sep-14	2	0	0	2	0.13	0.00	0.00	0.52
17	Oct-14	8	3	0	4	0.54	6.56	0.00	1.03
17	Nov-14	4	0	1	1	0.27	0.00	0.18	0.26
17	Dec-14	4	2	1	1	0.27	4.38	0.18	0.26
17	Jan-15	5	2	1	2	0.34	4.38	0.18	0.52
17	Feb-15	3	2	0	0	0.20	4.38	0.00	0.00
17	Mar-15	4	0	1	3	0.27	0.00	0.18	0.77
17	Apr-15	7	1	2	3	0.47	2.19	0.35	0.77
17	May-15	5	0	1	4	0.34	0.00	0.18	1.03
17	Jun-15	14	6	4	3	0.94	13.13	0.70	0.77
17	Jul-15	4	2	0	1	0.27	4.38	0.00	0.26
17	Aug-15	3	0	1	2	0.20	0.00	0.18	0.52
17	Sep-15	5	1	1	2	0.34	2.19	0.18	0.52
17	Oct-15	4	1	1	1	0.27	2.19	0.18	0.26
17	Nov-15	1	0	0	1	0.07	0.00	0.00	0.26
17	Dec-15	3	1	0	1	0.20	2.19	0.00	0.26
17	Jan-16	7	0	2	4	0.47	0.00	0.35	1.03
17	Feb-16	7	4	0	3	0.47	8.75	0.00	0.77
17	Mar-16	12	1	3	6	0.81	2.19	0.53	1.55
17	Apr-16	1	0	1	0	0.07	0.00	0.18	0.00
18	Jan-14	4	2	1	0	0.33	2.07	0.11	0.00
18	Feb-14	6	5	1	0	0.50	5.17	0.11	0.00
18	Mar-14	3	2	0	1	0.25	2.07	0.00	2.05
18	Apr-14	7	5	2	0	0.58	5.17	0.22	0.00
18	May-14	3	3	0	0	0.25	3.10	0.00	0.00
18	Jun-14	9	9	0	0	0.74	9.31	0.00	0.00
18	Jul-14	7	6	0	1	0.58	6.21	0.00	2.05
18	Aug-14	7	3	0	4	0.58	3.10	0.00	8.19
18	Sep-14	10	10	0	0	0.83	10.35	0.00	0.00
18	Oct-14	12	9	1	2	0.99	9.31	0.11	4.09
18	Nov-14	10	8	1	1	0.83	8.28	0.11	2.05
18	Dec-14	4	4	0	0	0.33	4.14	0.00	0.00
18	Jan-15	1	1	0	0	0.08	1.03	0.00	0.00
18	Feb-15	4	3	1	0	0.33	3.10	0.11	0.00
18	Mar-15	5	5	0	0	0.41	5.17	0.00	0.00
18	Apr-15	2	2	0	0	0.17	2.07	0.00	0.00
18	May-15	6	5	0	1	0.50	5.17	0.00	2.05
18	Jun-15	7	5	1	1	0.58	5.17	0.11	2.05
18	Jul-15	6	5	0	0	0.50	5.17	0.00	0.00
18	Aug-15	14	14	0	0	1.16	14.49	0.00	0.00

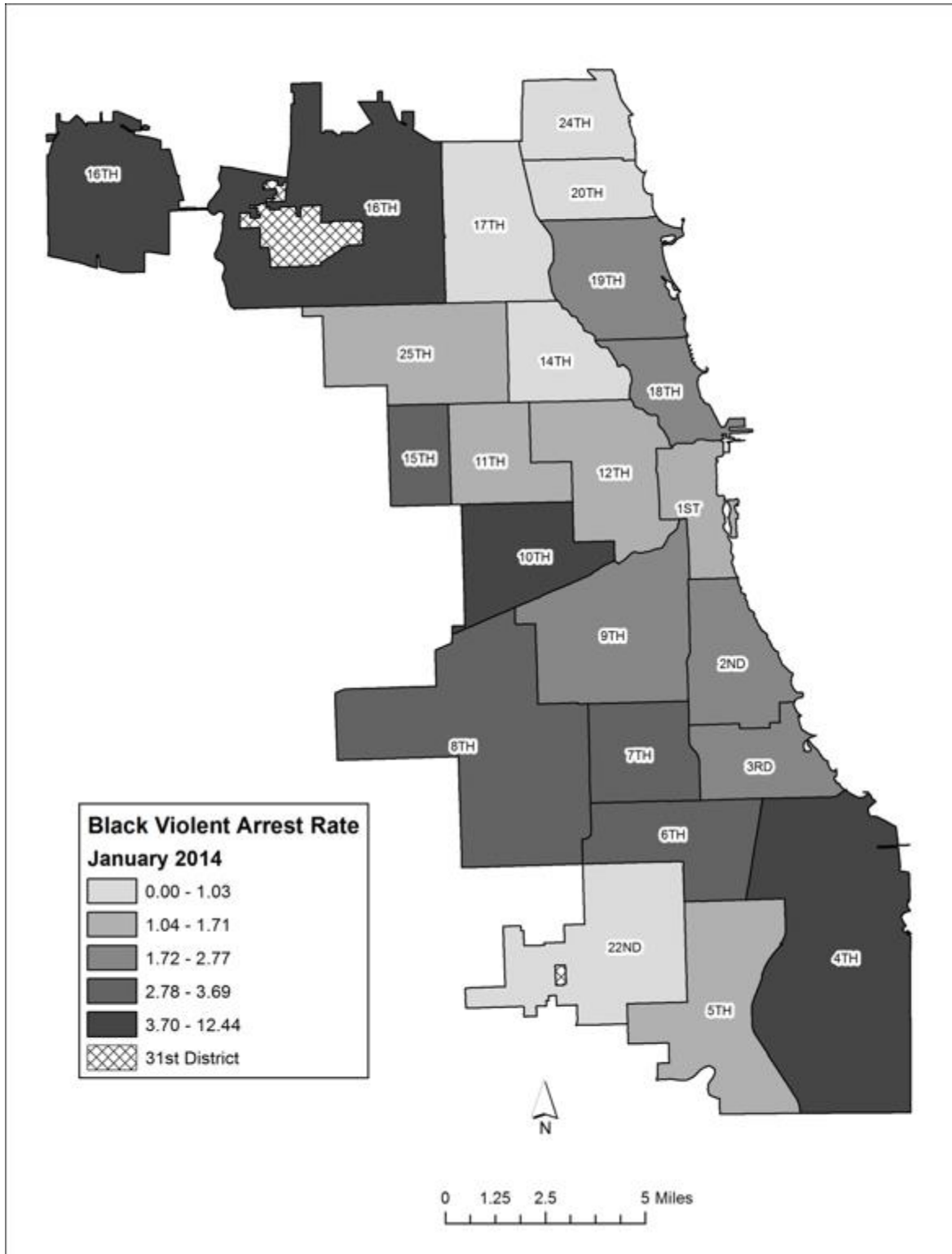
18	Sep-15	13	11	0	1	1.08	11.38	0.00	2.05
18	Oct-15	3	2	0	1	0.25	2.07	0.00	2.05
18	Nov-15	4	4	0	0	0.33	4.14	0.00	0.00
18	Dec-15	2	1	1	0	0.17	1.03	0.11	0.00
18	Jan-16	3	3	0	0	0.25	3.10	0.00	0.00
18	Feb-16	9	9	0	0	0.74	9.31	0.00	0.00
18	Mar-16	4	3	1	0	0.33	3.10	0.11	0.00
18	Apr-16	6	5	1	0	0.50	5.17	0.11	0.00
19	Jan-14	3	3	0	0	0.14	2.42	0.00	0.00
19	Feb-14	13	10	2	1	0.63	8.07	0.13	0.63
19	Mar-14	5	5	0	0	0.24	4.03	0.00	0.00
19	Apr-14	3	1	1	0	0.14	0.81	0.06	0.00
19	May-14	14	9	1	3	0.68	7.26	0.06	1.88
19	Jun-14	13	8	1	4	0.63	6.45	0.06	2.51
19	Jul-14	12	9	1	2	0.58	7.26	0.06	1.26
19	Aug-14	15	7	3	4	0.72	5.65	0.19	2.51
19	Sep-14	5	3	1	1	0.24	2.42	0.06	0.63
19	Oct-14	11	8	1	0	0.53	6.45	0.06	0.00
19	Nov-14	8	6	2	0	0.39	4.84	0.13	0.00
19	Dec-14	6	3	2	1	0.29	2.42	0.13	0.63
19	Jan-15	3	2	0	1	0.14	1.61	0.00	0.63
19	Feb-15	8	8	0	0	0.39	6.45	0.00	0.00
19	Mar-15	8	2	1	4	0.39	1.61	0.06	2.51
19	Apr-15	5	1	0	1	0.24	0.81	0.00	0.63
19	May-15	6	4	1	1	0.29	3.23	0.06	0.63
19	Jun-15	9	7	1	1	0.43	5.65	0.06	0.63
19	Jul-15	5	2	1	2	0.24	1.61	0.06	1.26
19	Aug-15	10	8	0	2	0.48	6.45	0.00	1.26
19	Sep-15	5	4	0	1	0.24	3.23	0.00	0.63
19	Oct-15	7	4	2	1	0.34	3.23	0.13	0.63
19	Nov-15	6	2	1	2	0.29	1.61	0.06	1.26
19	Dec-15	2	2	0	0	0.10	1.61	0.00	0.00
19	Jan-16	9	8	1	0	0.43	6.45	0.06	0.00
19	Feb-16	2	1	0	1	0.10	0.81	0.00	0.63
19	Mar-16	9	4	1	3	0.43	3.23	0.06	1.88
19	Apr-16	6	3	0	3	0.29	2.42	0.00	1.88
20	Jan-14	1	1	0	0	0.11	1.03	0.00	0.00
20	Feb-14	2	2	0	0	0.23	2.06	0.00	0.00
20	Mar-14	3	2	0	1	0.34	2.06	0.00	1.06
20	Apr-14	3	0	2	1	0.34	0.00	0.41	1.06
20	May-14	3	2	0	1	0.34	2.06	0.00	1.06
20	Jun-14	2	2	0	0	0.23	2.06	0.00	0.00
20	Jul-14	2	1	0	1	0.23	1.03	0.00	1.06

20	Aug-14	2	2	0	0	0.23	2.06	0.00	0.00
20	Sep-14	2	1	1	0	0.23	1.03	0.21	0.00
20	Oct-14	1	0	0	1	0.11	0.00	0.00	1.06
20	Nov-14	2	1	0	1	0.23	1.03	0.00	1.06
20	Dec-14	3	3	0	0	0.34	3.09	0.00	0.00
20	Jan-15	2	0	0	2	0.23	0.00	0.00	2.12
20	Feb-15	2	1	1	0	0.23	1.03	0.21	0.00
20	Mar-15	3	1	2	0	0.34	1.03	0.41	0.00
20	Apr-15	0	0	0	0	0.00	0.00	0.00	0.00
20	May-15	6	6	0	0	0.69	6.17	0.00	0.00
20	Jun-15	2	2	0	0	0.23	2.06	0.00	0.00
20	Jul-15	5	5	0	0	0.57	5.14	0.00	0.00
20	Aug-15	5	2	0	3	0.57	2.06	0.00	3.18
20	Sep-15	4	3	0	1	0.46	3.09	0.00	1.06
20	Oct-15	1	1	0	0	0.11	1.03	0.00	0.00
20	Nov-15	1	1	0	0	0.11	1.03	0.00	0.00
20	Dec-15	2	1	1	0	0.23	1.03	0.21	0.00
20	Jan-16	7	4	0	3	0.80	4.11	0.00	3.18
20	Feb-16	3	2	0	1	0.34	2.06	0.00	1.06
20	Mar-16	0	0	0	0	0.00	0.00	0.00	0.00
20	Apr-16	2	1	1	0	0.23	1.03	0.21	0.00
22	Jan-14	6	6	0	0	0.58	0.97	0.00	0.00
22	Feb-14	5	5	0	0	0.49	0.81	0.00	0.00
22	Mar-14	13	13	0	0	1.27	2.09	0.00	0.00
22	Apr-14	11	11	0	0	1.07	1.77	0.00	0.00
22	May-14	16	15	1	0	1.56	2.42	0.29	0.00
22	Jun-14	15	14	1	0	1.46	2.26	0.29	0.00
22	Jul-14	11	11	0	0	1.07	1.77	0.00	0.00
22	Aug-14	9	8	1	0	0.88	1.29	0.29	0.00
22	Sep-14	16	16	0	0	1.56	2.58	0.00	0.00
22	Oct-14	12	11	1	0	1.17	1.77	0.29	0.00
22	Nov-14	5	4	1	0	0.49	0.64	0.29	0.00
22	Dec-14	2	2	0	0	0.19	0.32	0.00	0.00
22	Jan-15	9	8	0	0	0.88	1.29	0.00	0.00
22	Feb-15	9	9	0	0	0.88	1.45	0.00	0.00
22	Mar-15	13	13	0	0	1.27	2.09	0.00	0.00
22	Apr-15	6	6	0	0	0.58	0.97	0.00	0.00
22	May-15	12	9	2	1	1.17	1.45	0.57	4.80
22	Jun-15	8	8	0	0	0.78	1.29	0.00	0.00
22	Jul-15	11	11	0	0	1.07	1.77	0.00	0.00
22	Aug-15	7	7	0	0	0.68	1.13	0.00	0.00
22	Sep-15	12	12	0	0	1.17	1.93	0.00	0.00
22	Oct-15	6	6	0	0	0.58	0.97	0.00	0.00

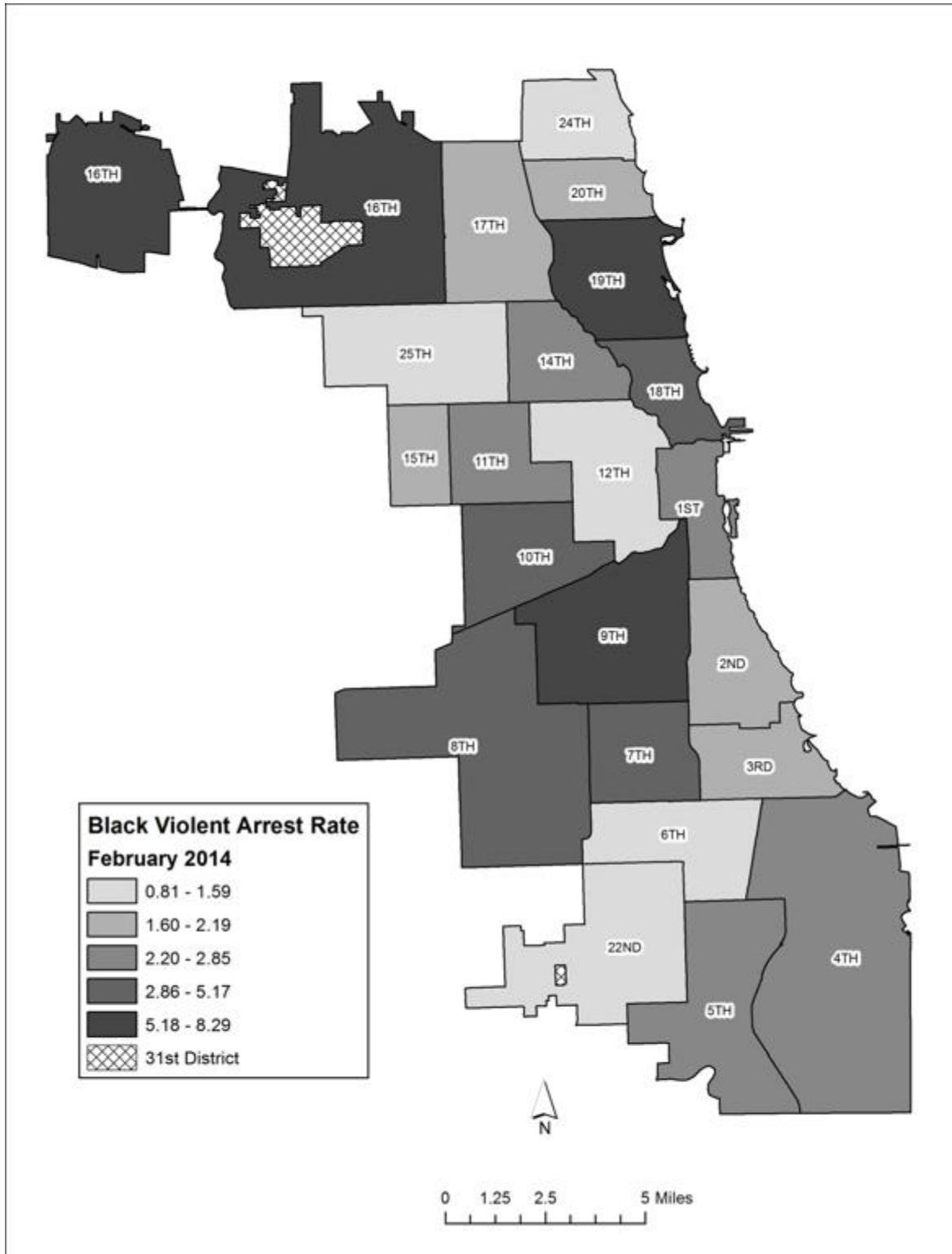
22	Nov-15	8	8	0	0	0.78	1.29	0.00	0.00
22	Dec-15	4	4	0	0	0.39	0.64	0.00	0.00
22	Jan-16	3	3	0	0	0.29	0.48	0.00	0.00
22	Feb-16	5	3	2	0	0.49	0.48	0.57	0.00
22	Mar-16	12	11	0	1	1.17	1.77	0.00	4.80
22	Apr-16	7	6	1	0	0.68	0.97	0.29	0.00
24	Jan-14	3	0	0	2	0.21	0.00	0.00	1.00
24	Feb-14	5	3	1	0	0.35	1.22	0.16	0.00
24	Mar-14	10	2	2	6	0.71	0.81	0.32	2.99
24	Apr-14	7	4	0	2	0.50	1.63	0.00	1.00
24	May-14	8	7	0	0	0.57	2.85	0.00	0.00
24	Jun-14	13	11	1	1	0.92	4.48	0.16	0.50
24	Jul-14	8	5	0	1	0.57	2.03	0.00	0.50
24	Aug-14	3	3	0	0	0.21	1.22	0.00	0.00
24	Sep-14	11	8	2	0	0.78	3.26	0.32	0.00
24	Oct-14	9	9	0	0	0.64	3.66	0.00	0.00
24	Nov-14	5	4	1	0	0.35	1.63	0.16	0.00
24	Dec-14	11	5	2	4	0.78	2.03	0.32	1.99
24	Jan-15	7	2	2	1	0.50	0.81	0.32	0.50
24	Mar-15	5	5	0	0	0.35	2.03	0.00	0.00
24	Apr-15	4	3	0	0	0.28	1.22	0.00	0.00
24	May-15	11	8	2	1	0.78	3.26	0.32	0.50
24	Jun-15	10	5	1	2	0.71	2.03	0.16	1.00
24	Jul-15	16	14	0	2	1.13	5.70	0.00	1.00
24	Aug-15	8	5	0	3	0.57	2.03	0.00	1.49
24	Sep-15	8	6	0	1	0.57	2.44	0.00	0.50
24	Oct-15	4	2	1	1	0.28	0.81	0.16	0.50
24	Nov-15	6	4	0	1	0.42	1.63	0.00	0.50
24	Dec-15	6	5	1	0	0.42	2.03	0.16	0.00
24	Jan-16	1	1	0	0	0.07	0.41	0.00	0.00
24	Feb-16	7	5	0	2	0.50	2.03	0.00	1.00
24	Mar-16	7	3	0	1	0.50	1.22	0.00	0.50
24	Apr-16	9	4	1	3	0.64	1.63	0.16	1.49
25	Jan-14	14	5	0	9	0.70	1.53	0.00	1.56
25	Feb-14	5	3	0	1	0.25	0.92	0.00	0.17
25	Mar-14	15	7	2	6	0.75	2.14	0.71	1.04
25	Apr-14	9	1	0	7	0.45	0.31	0.00	1.21
25	May-14	12	5	0	7	0.60	1.53	0.00	1.21
25	Jun-14	12	5	0	7	0.60	1.53	0.00	1.21
25	Jul-14	21	8	3	9	1.05	2.44	1.06	1.56
25	Aug-14	22	6	2	14	1.10	1.83	0.71	2.42
25	Sep-14	17	6	2	8	0.85	1.83	0.71	1.38
25	Oct-14	10	4	1	4	0.50	1.22	0.35	0.69

25	Nov-14	14	5	1	8	0.70	1.53	0.35	1.38
25	Dec-14	13	3	1	8	0.65	0.92	0.35	1.38
25	Jan-15	14	3	0	11	0.70	0.92	0.00	1.90
25	Feb-15	8	3	0	5	0.40	0.92	0.00	0.86
25	Mar-15	17	7	0	10	0.85	2.14	0.00	1.73
25	Apr-15	15	3	1	11	0.75	0.92	0.35	1.90
25	May-15	22	2	2	17	1.10	0.61	0.71	2.94
25	Jun-15	9	4	0	5	0.45	1.22	0.00	0.86
25	Jul-15	15	8	1	6	0.75	2.44	0.35	1.04
25	Aug-15	16	9	1	6	0.80	2.75	0.35	1.04
25	Sep-15	14	7	0	6	0.70	2.14	0.00	1.04
25	Oct-15	11	6	0	5	0.55	1.83	0.00	0.86
25	Nov-15	8	5	1	2	0.40	1.53	0.35	0.35
25	Dec-15	3	1	1	1	0.15	0.31	0.35	0.17
25	Jan-16	7	2	0	5	0.35	0.61	0.00	0.86
25	Feb-16	7	4	1	2	0.35	1.22	0.35	0.35
25	Mar-16	8	1	0	7	0.40	0.31	0.00	1.21
25	Apr-16	9	1	2	6	0.45	0.31	0.71	1.04

APPENDIX C: Black Violent Arrest Rate, January 2014

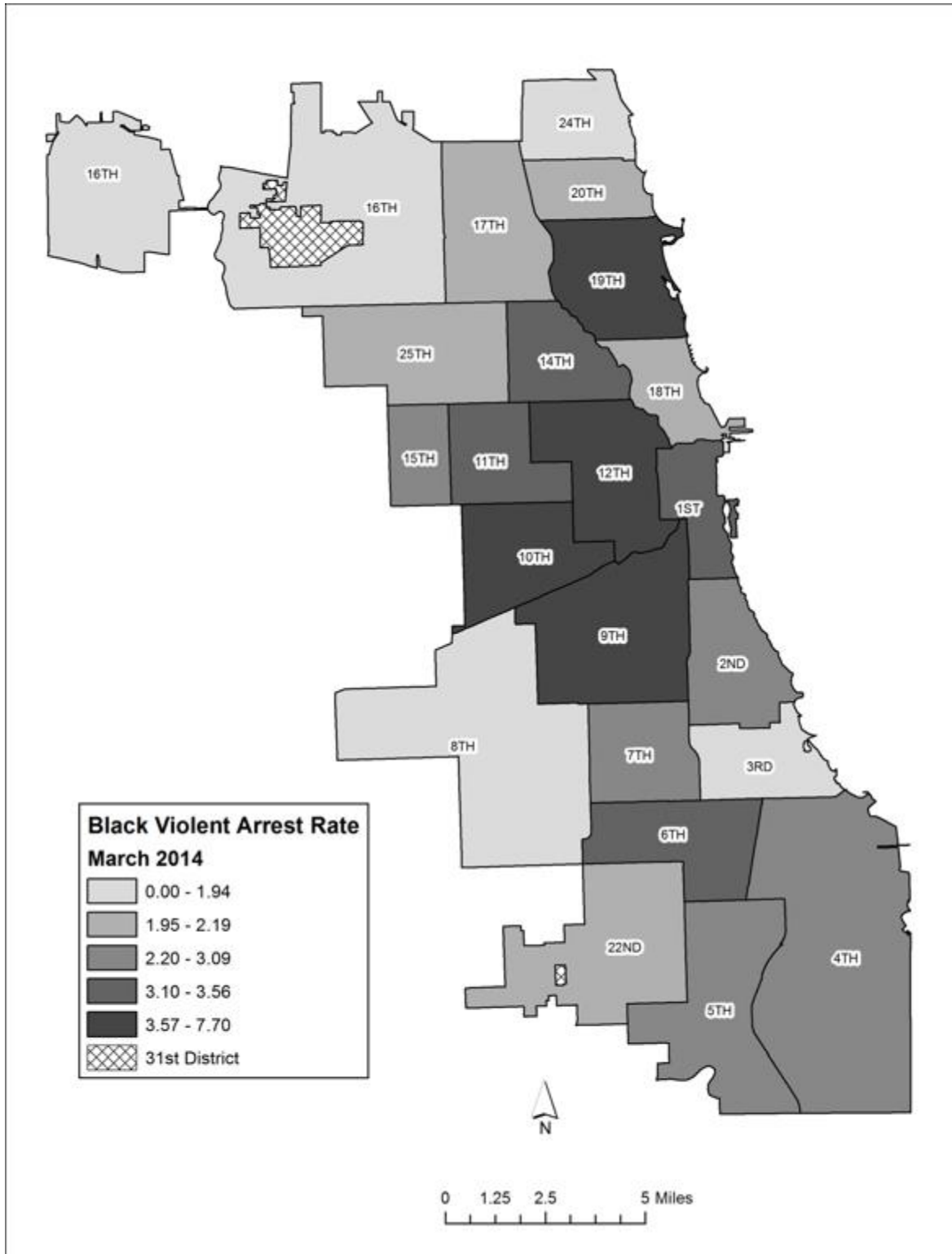


APPENDIX D: Black Violent Arrest Rate, February 2014

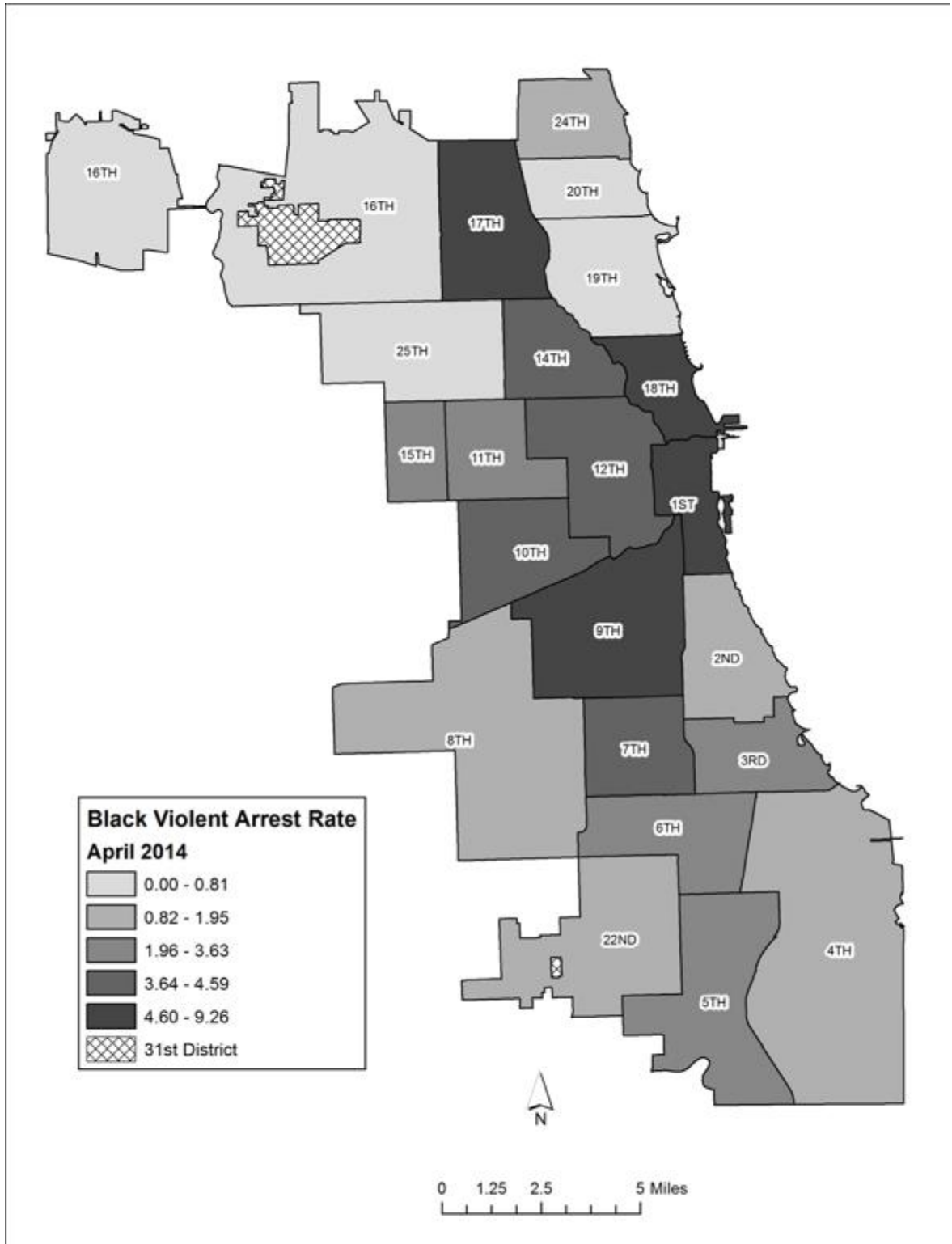




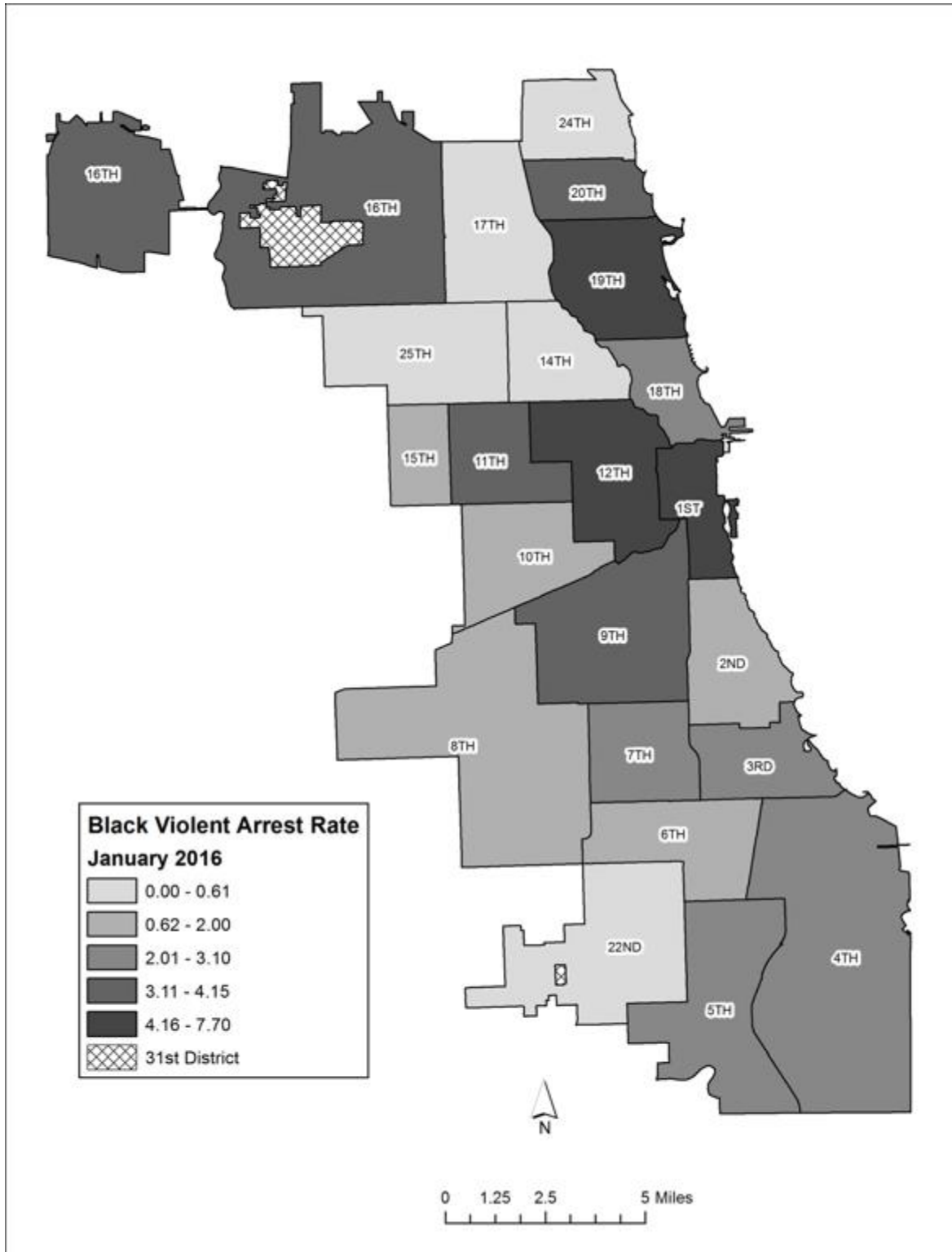
APPENDIX E: Black Violent Arrest Rate, March 2014



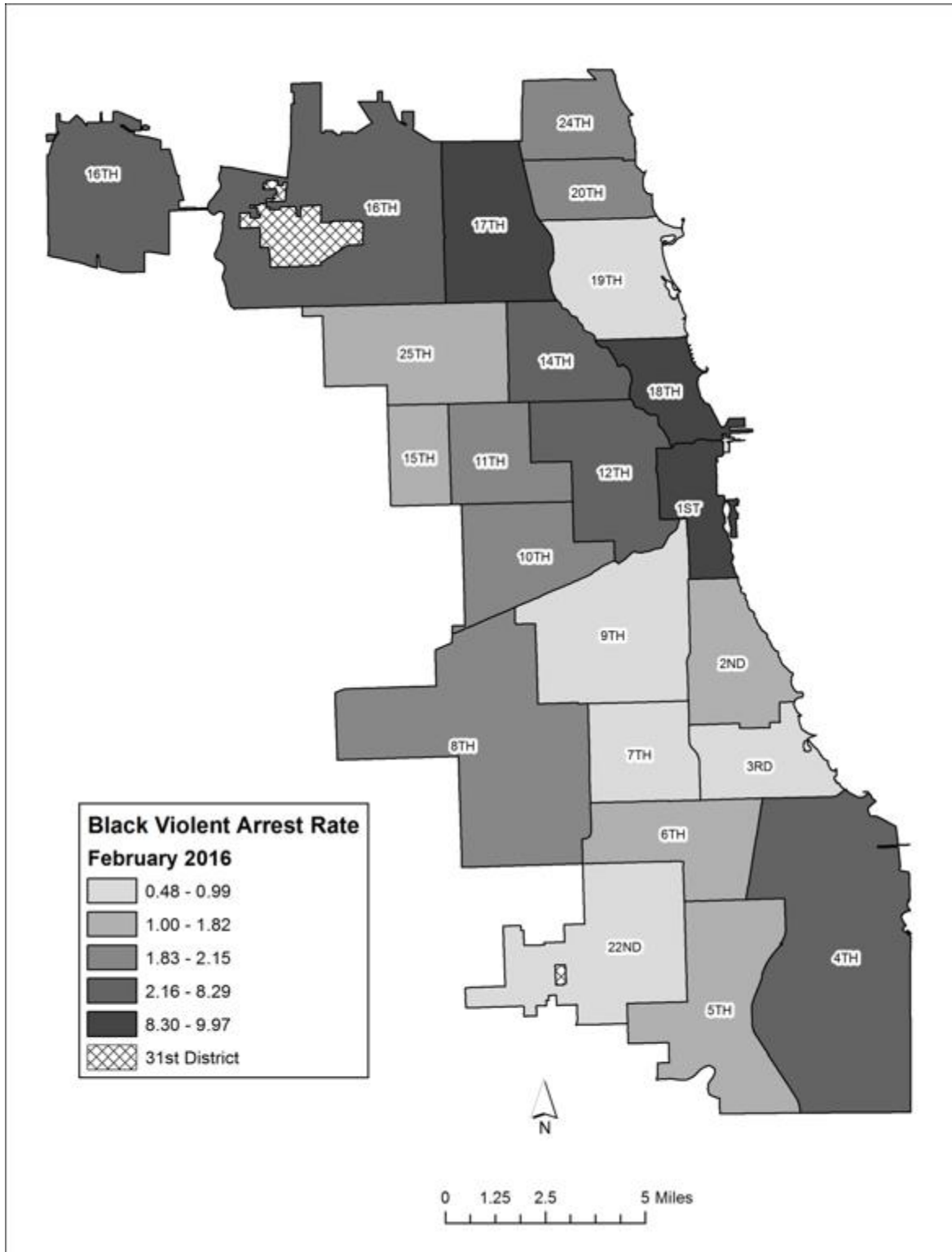
APPENDIX F: Black Violent Arrest Rate, April 2014



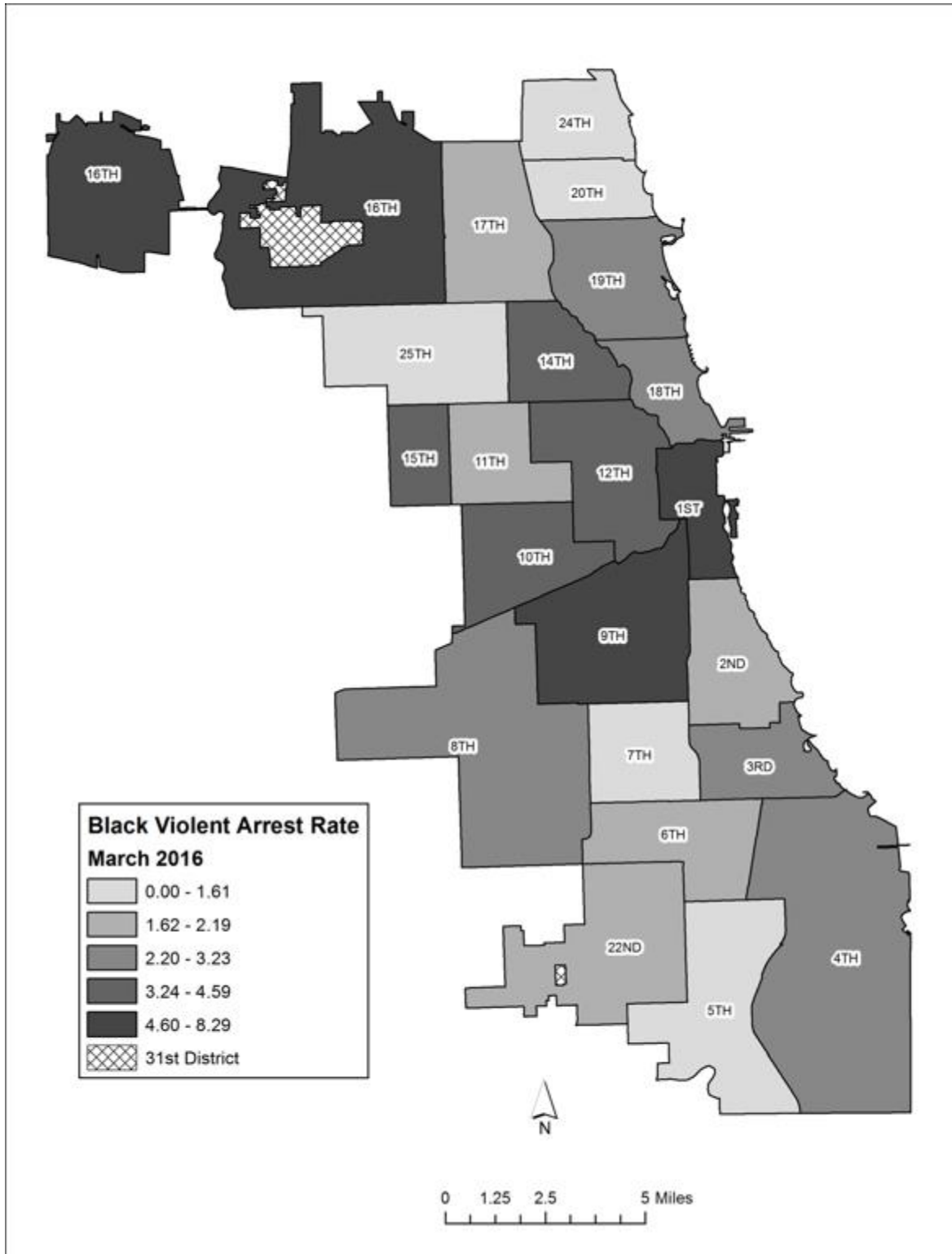
APPENDIX G: Black Violent Arrest Rate, January 2016



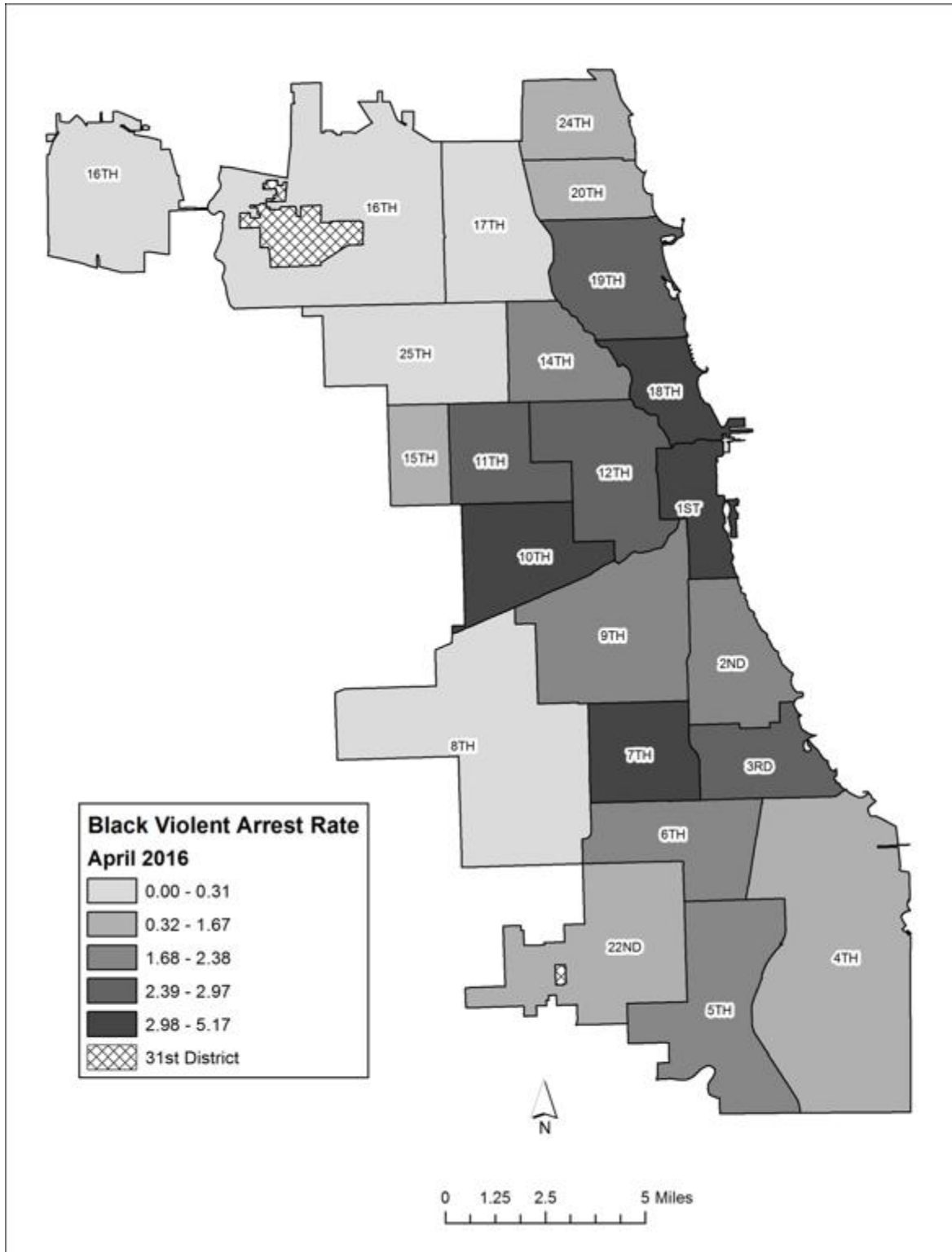
APPENDIX H: Black Violent Arrest Rate, February 2016



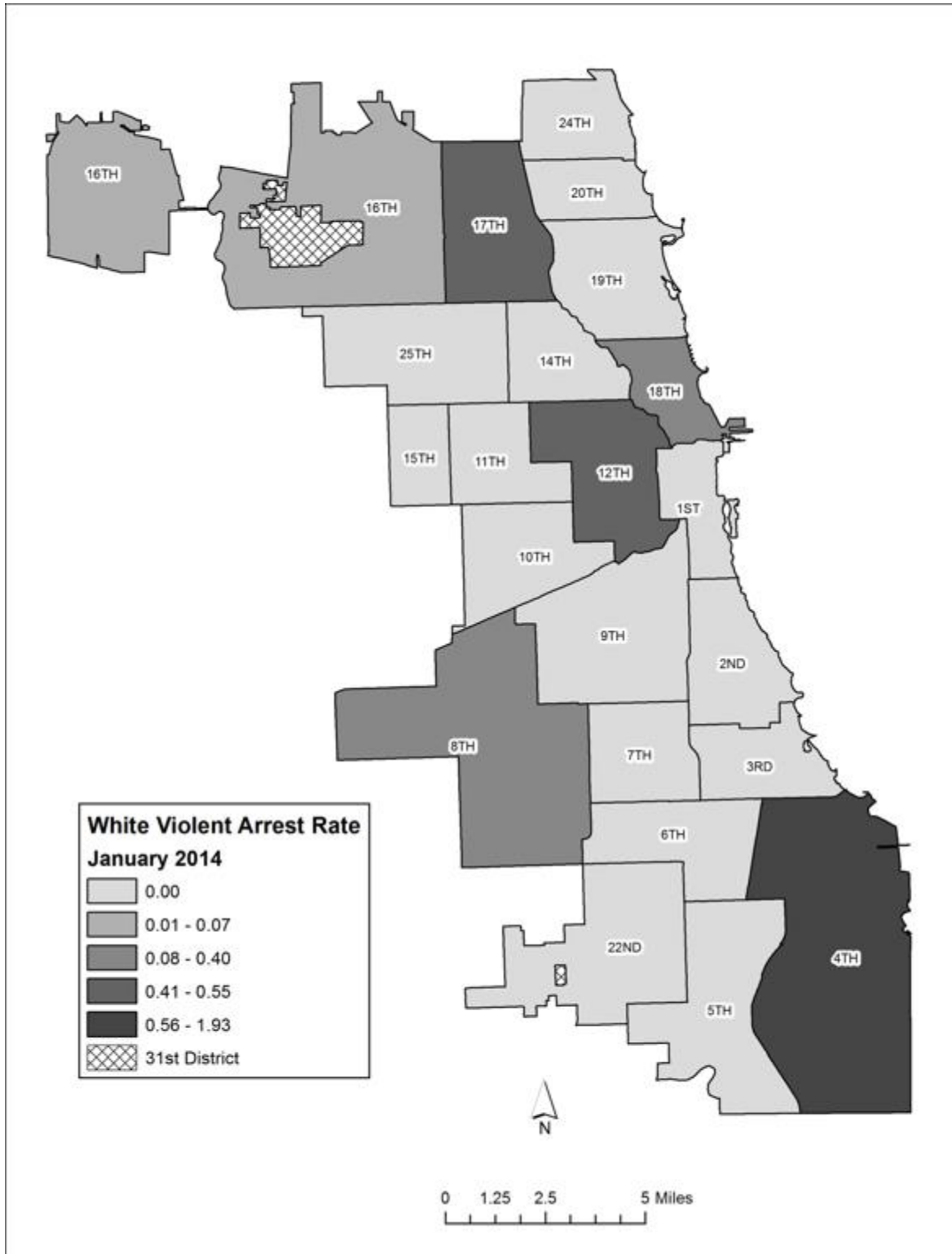
APPENDIX I: Black Violent Arrest Rate, March 2016



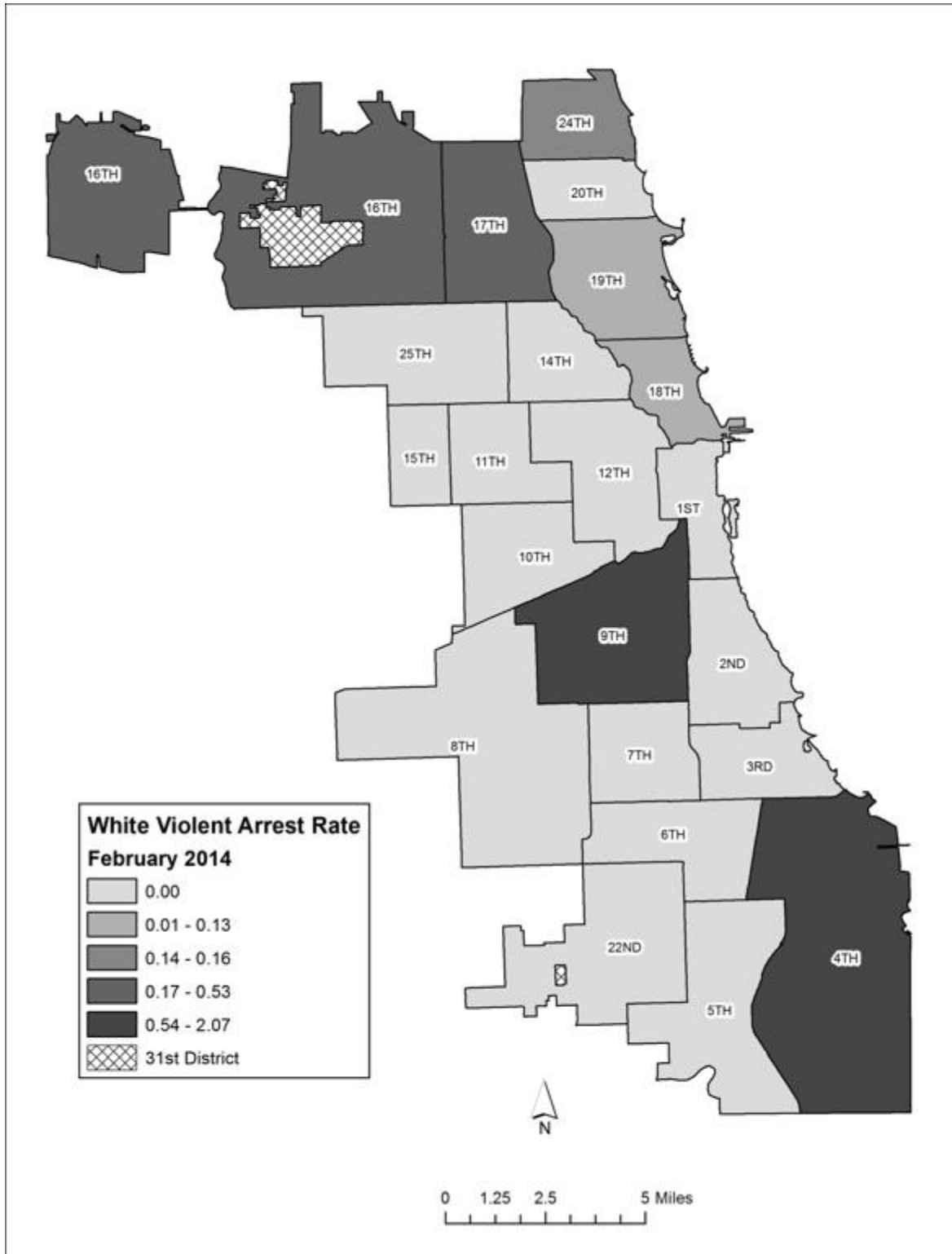
APPENDIX J: Black Violent Arrest Rate, April 2016



APPENDIX K: White Violent Arrest Rate, January 2014

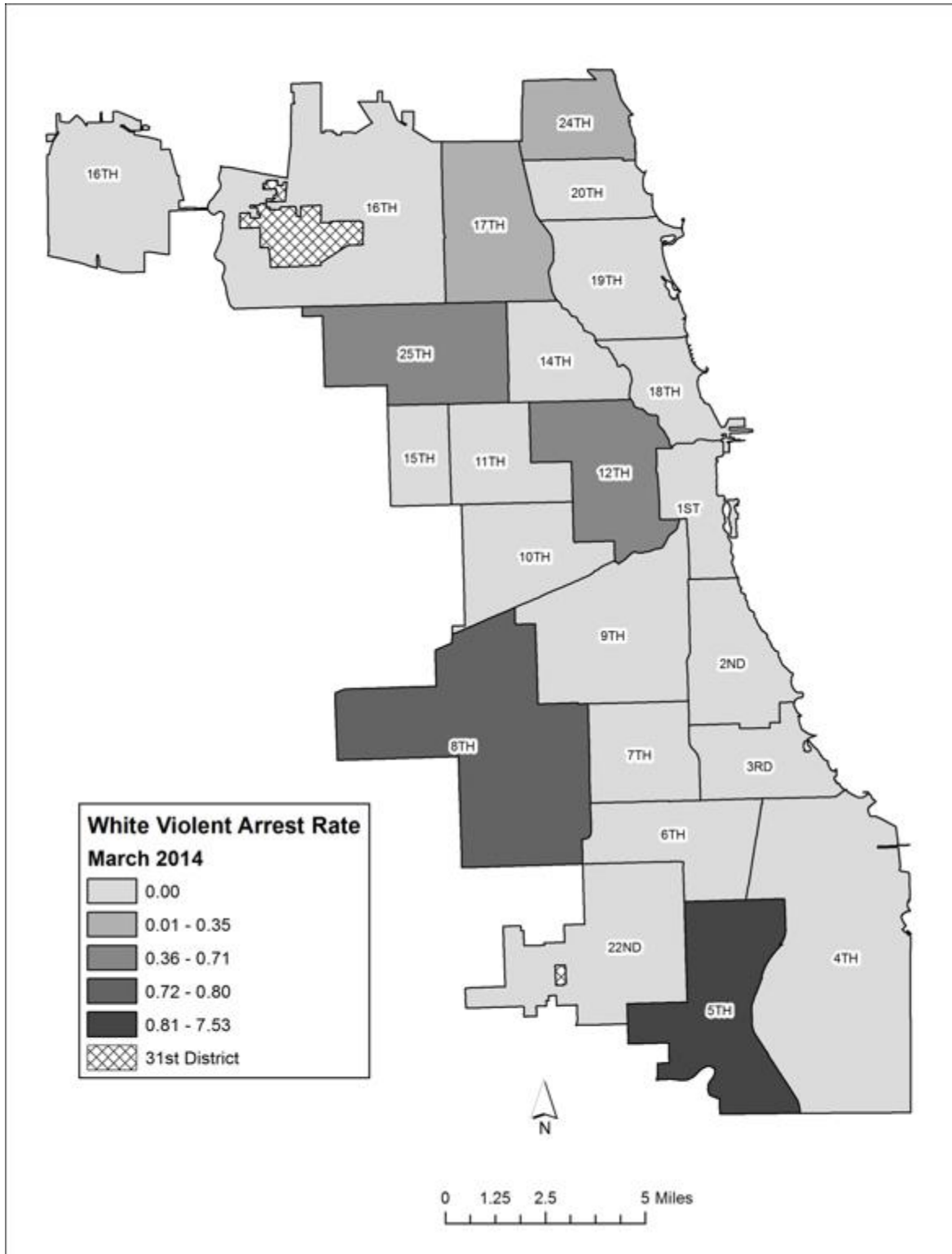


APPENDIX L: White Violent Arrest Rate, February 2014

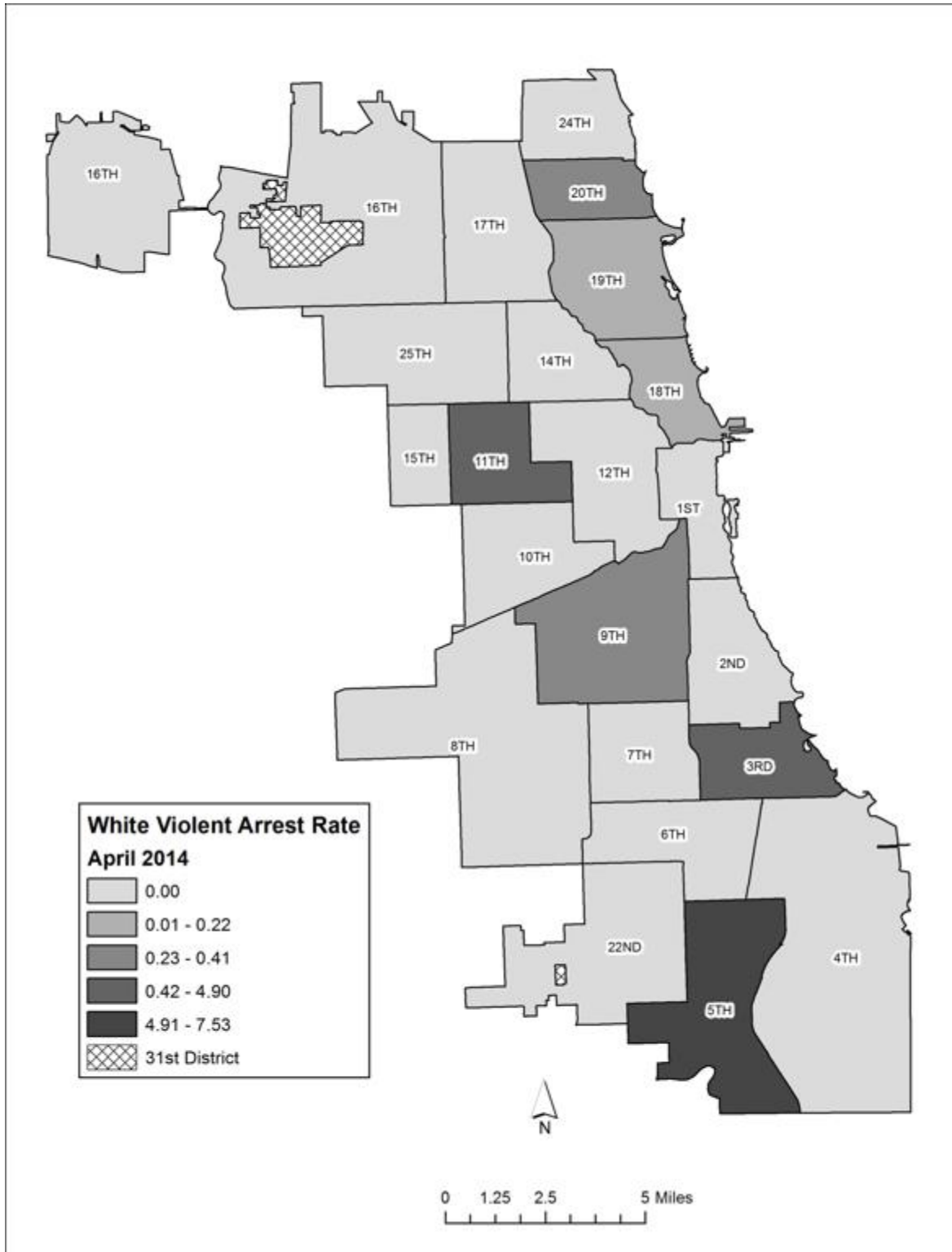




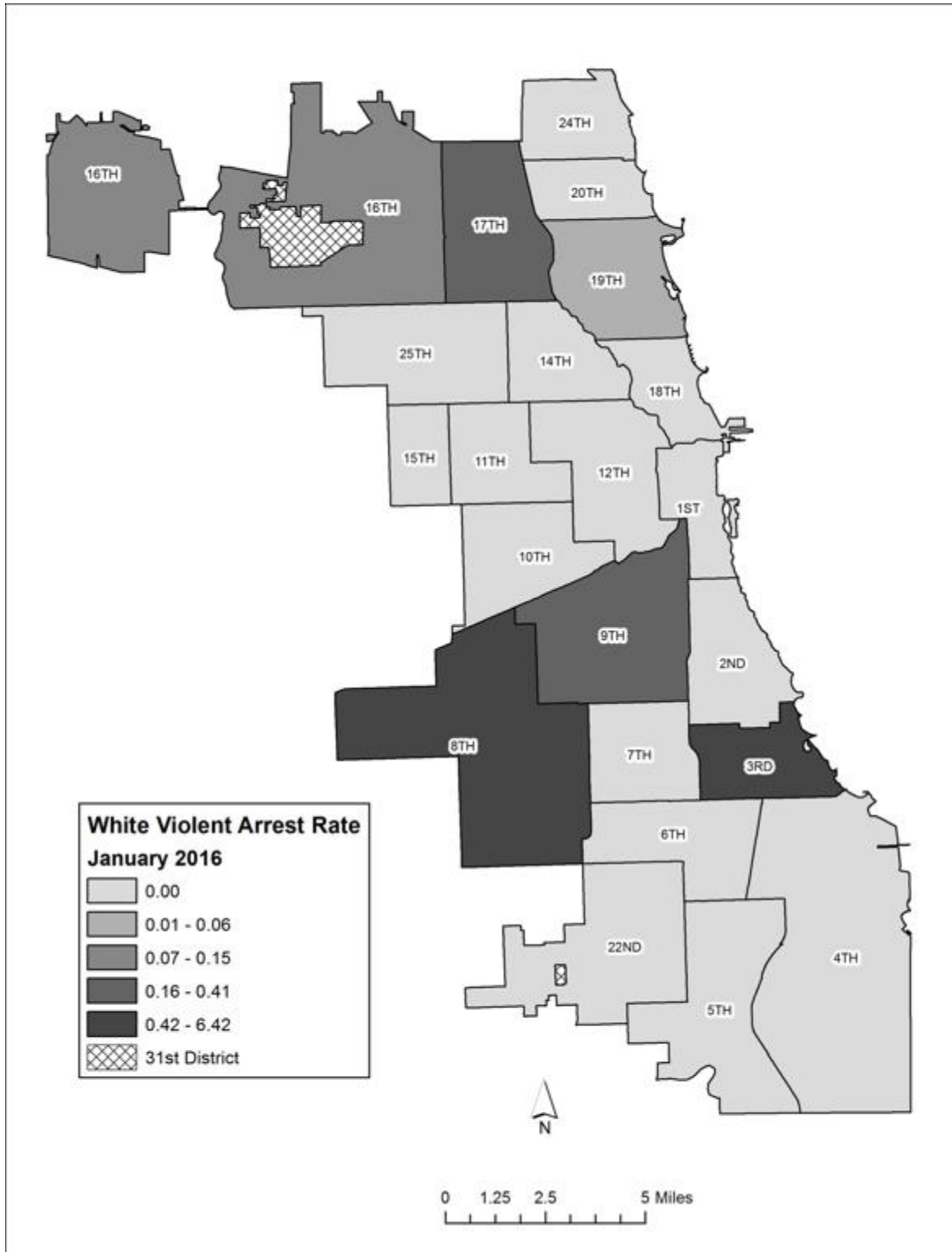
APPENDIX M: White Violent Arrest Rate, March 2014



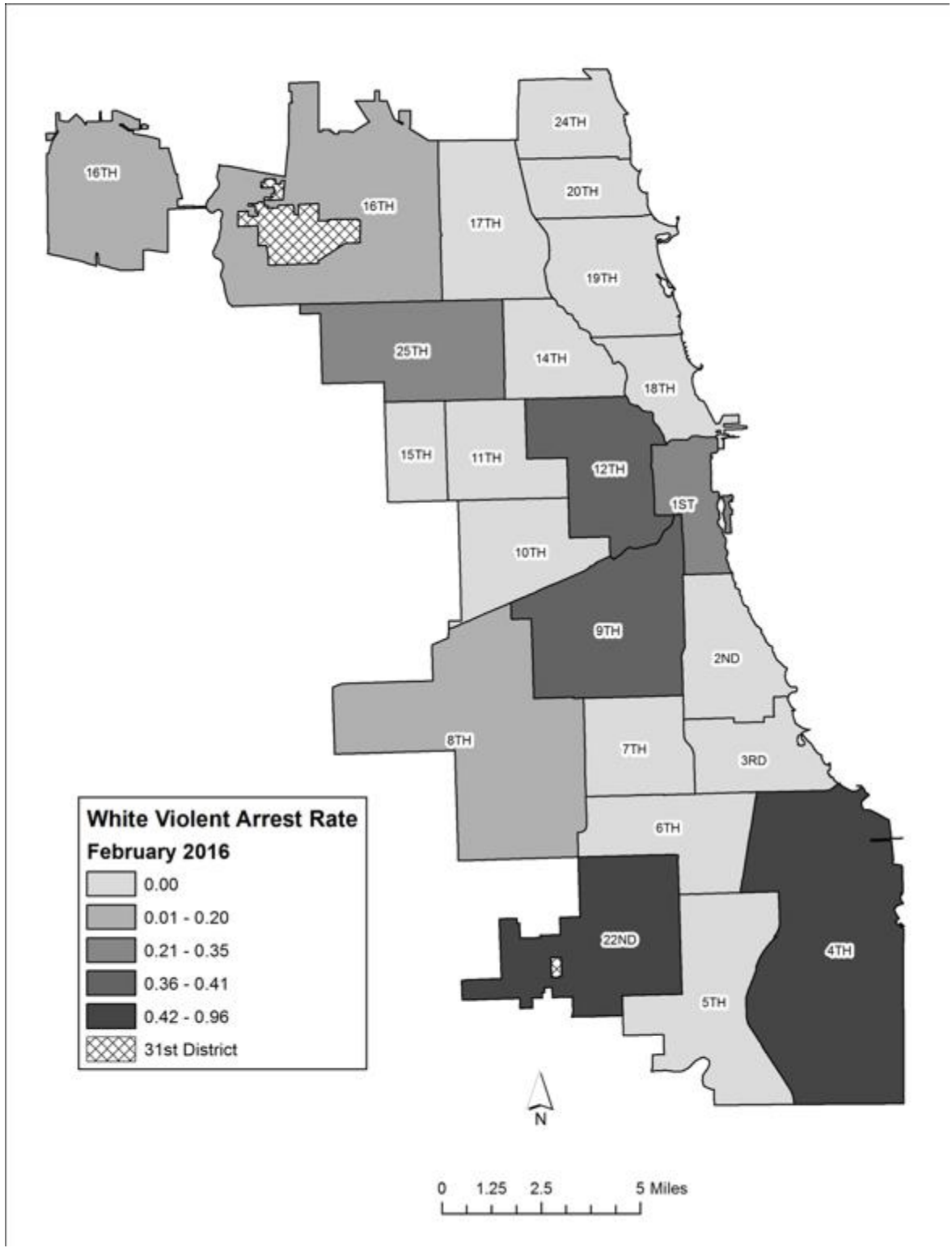
APPENDIX N: White Violent Arrest Rate, April 2014



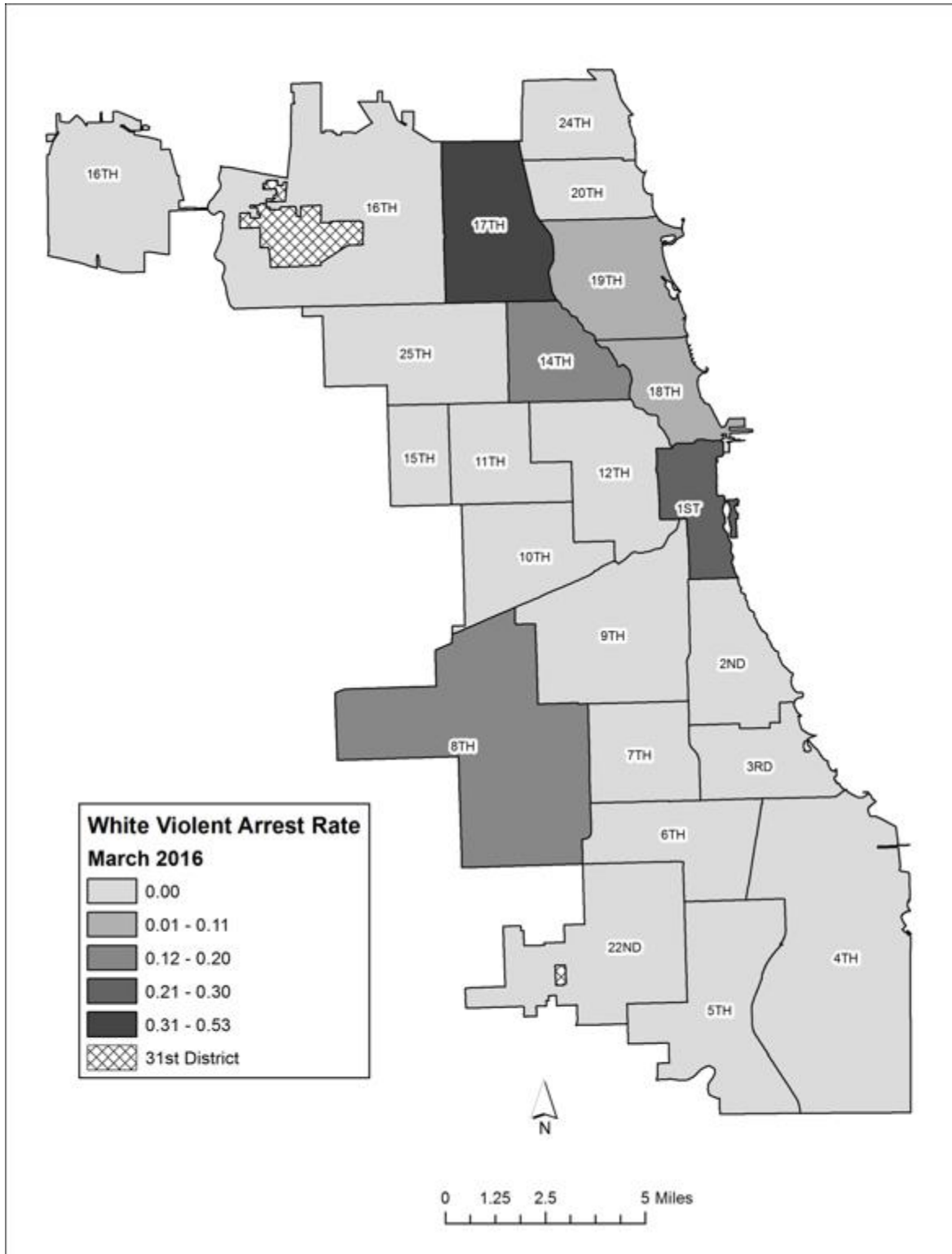
APPENDIX O: White Violent Arrest Rate, January 2016



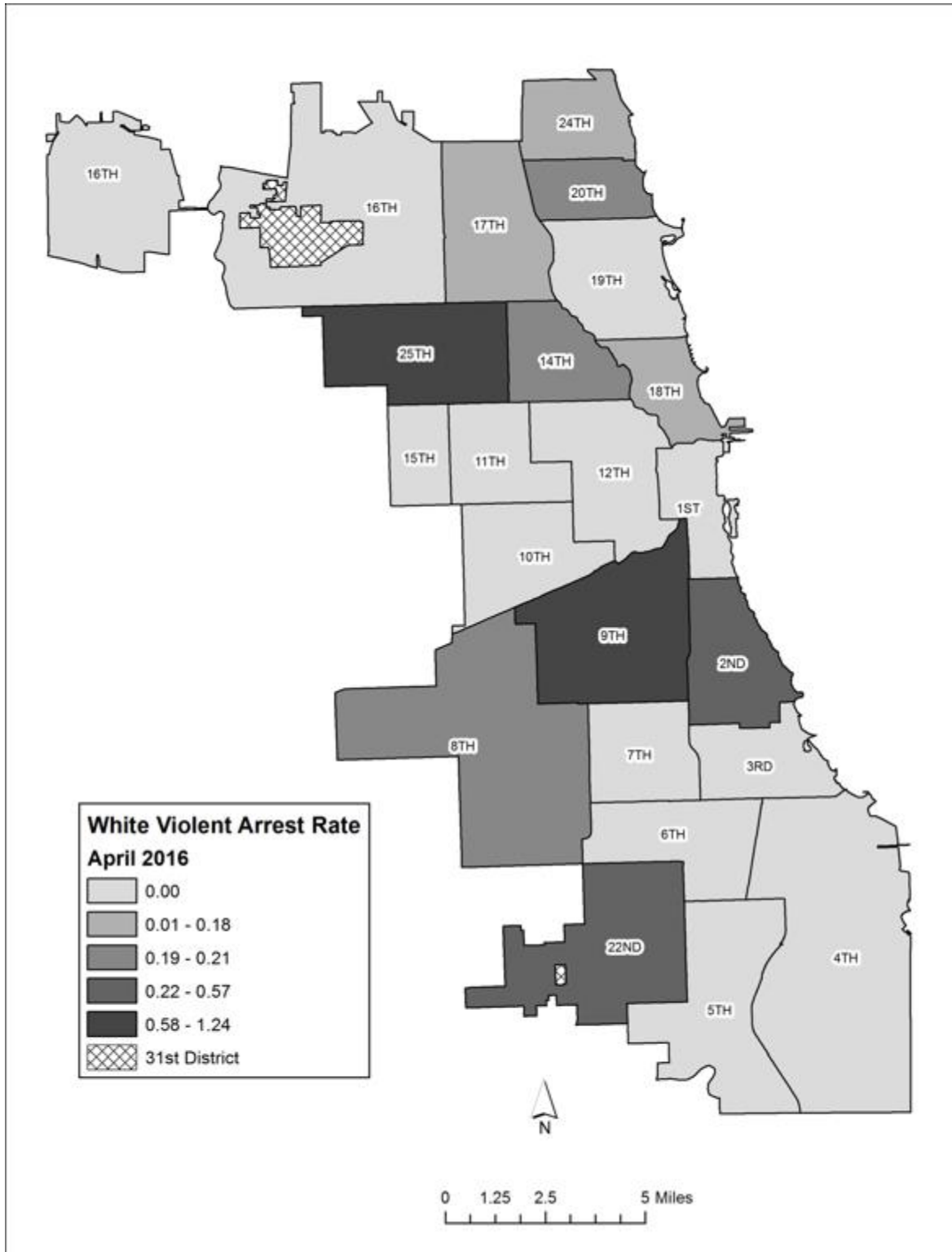
APPENDIX P: White Violent Arrest Rate, February 2016



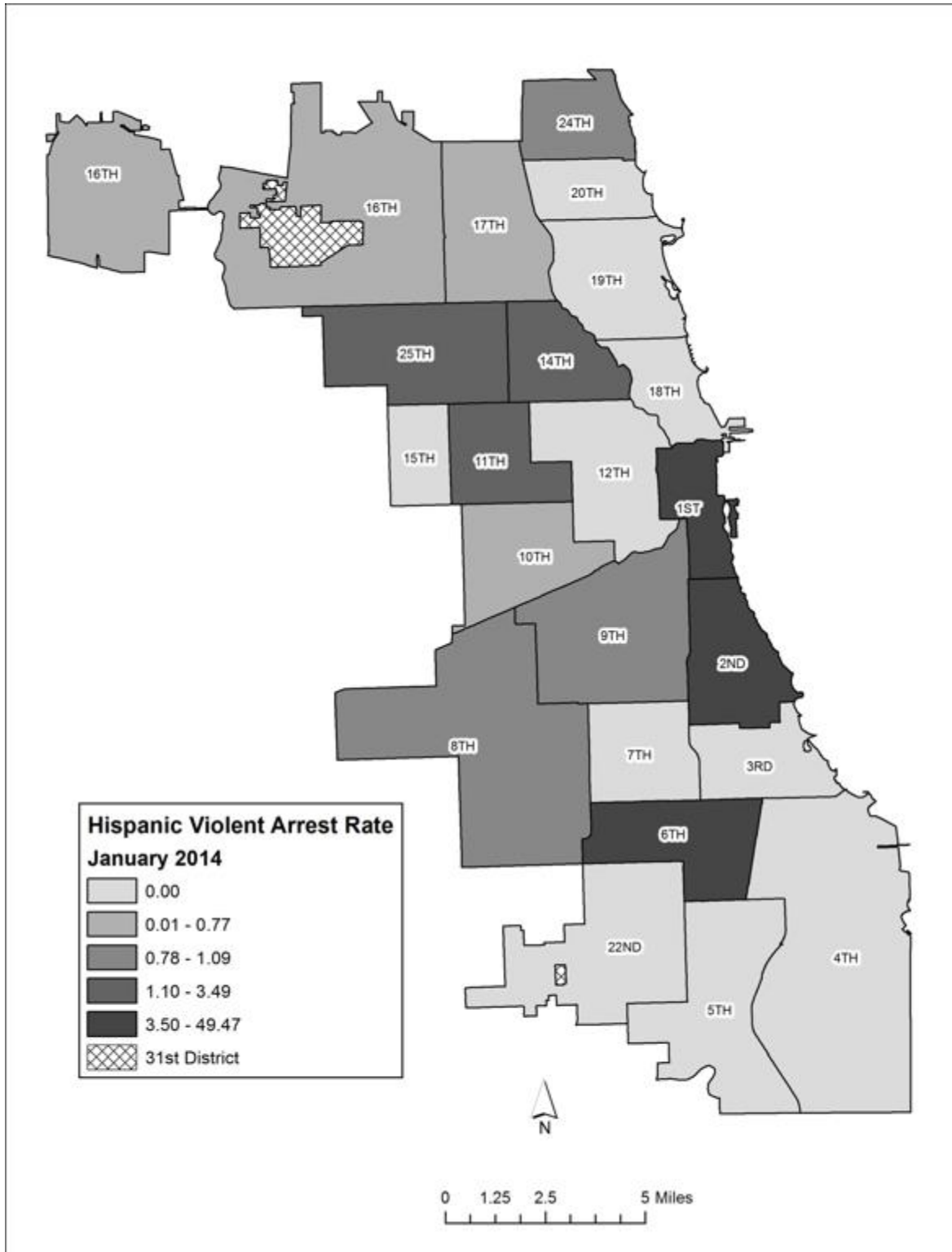
APPENDIX Q: White Violent Arrest Rate, March 2016



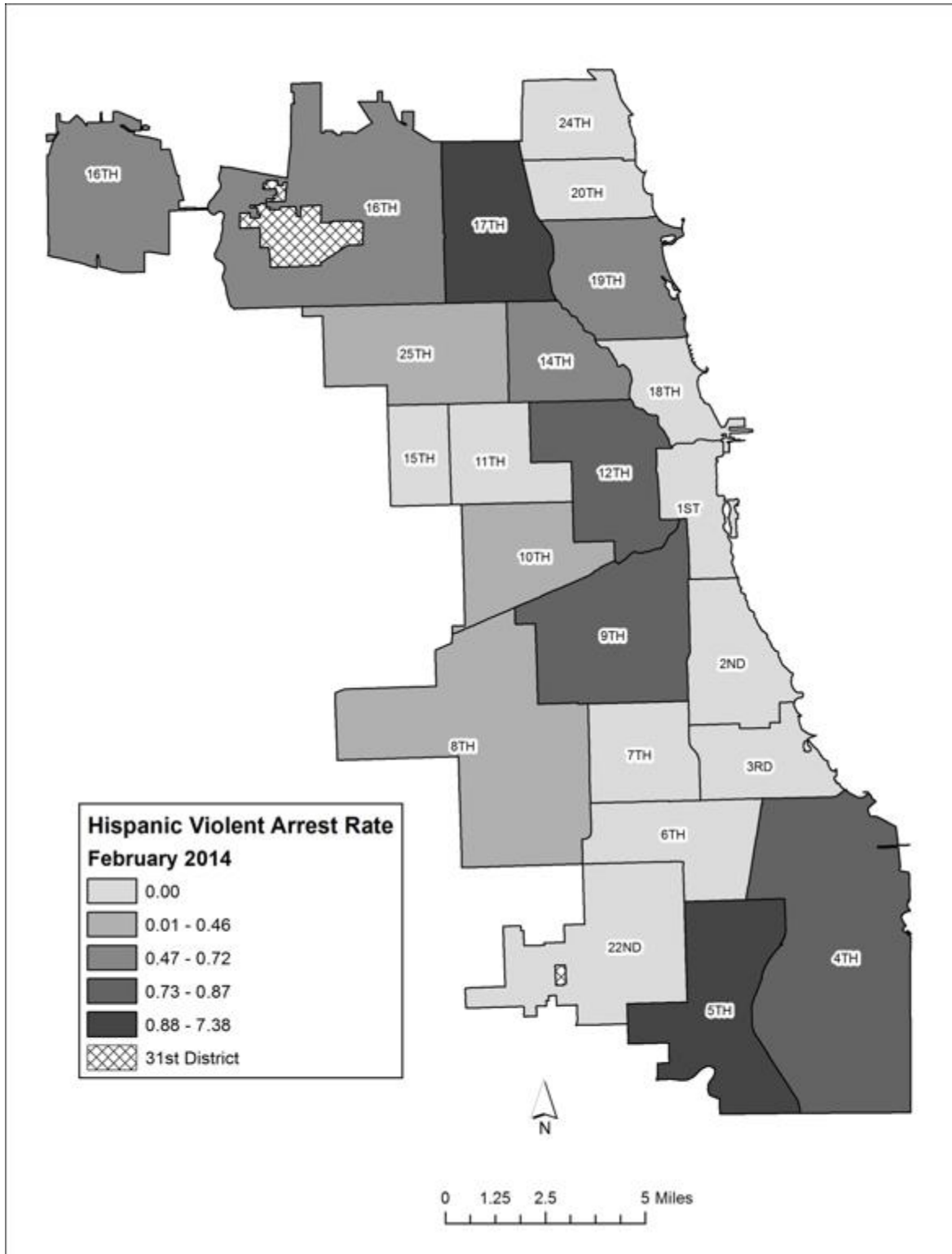
APPENDIX R: White Violent Arrest Rate, April 2016



APPENDIX S: Hispanic Violent Arrest Rate, January 2014

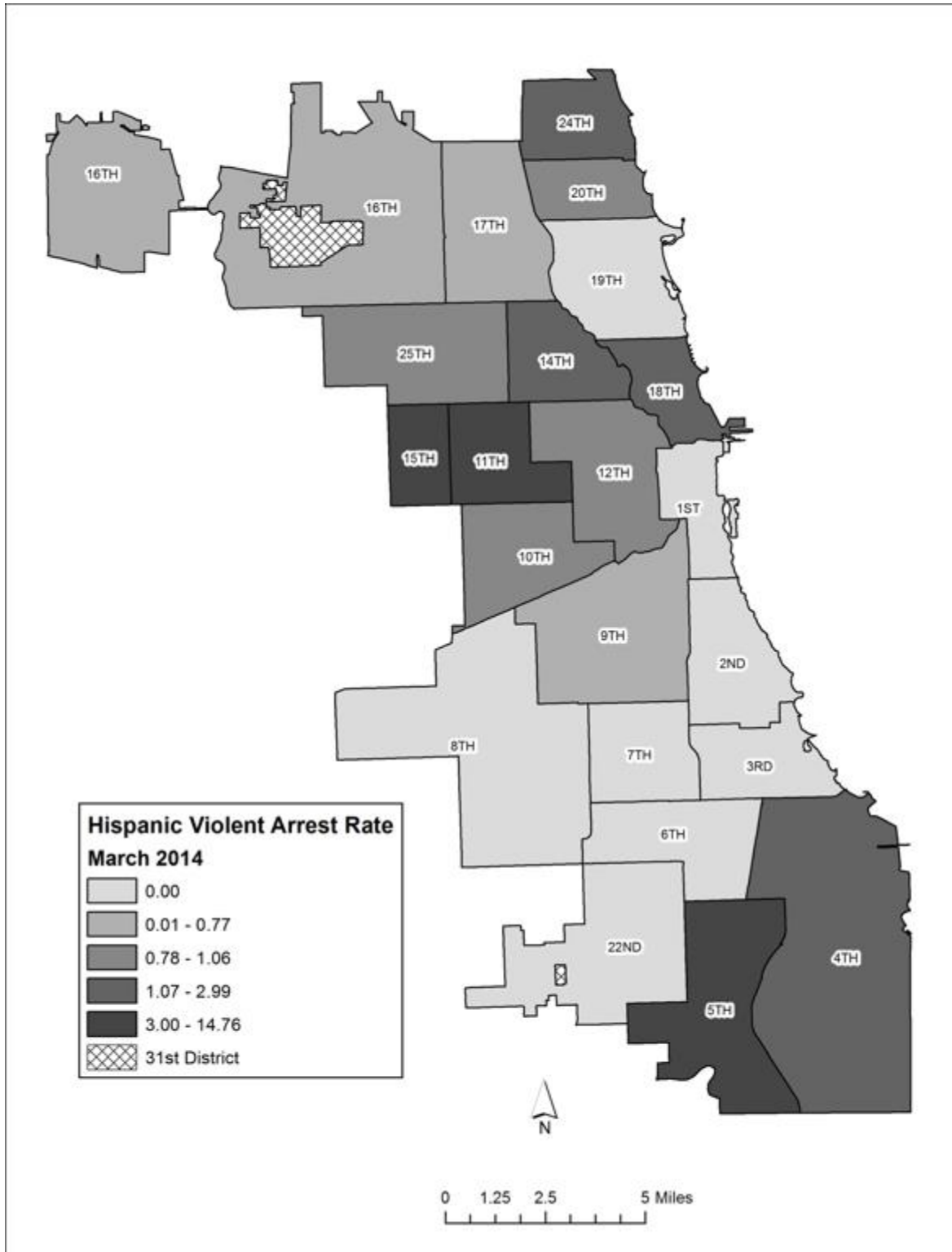


APPENDIX T: Hispanic Violent Arrest Rate, February 2014

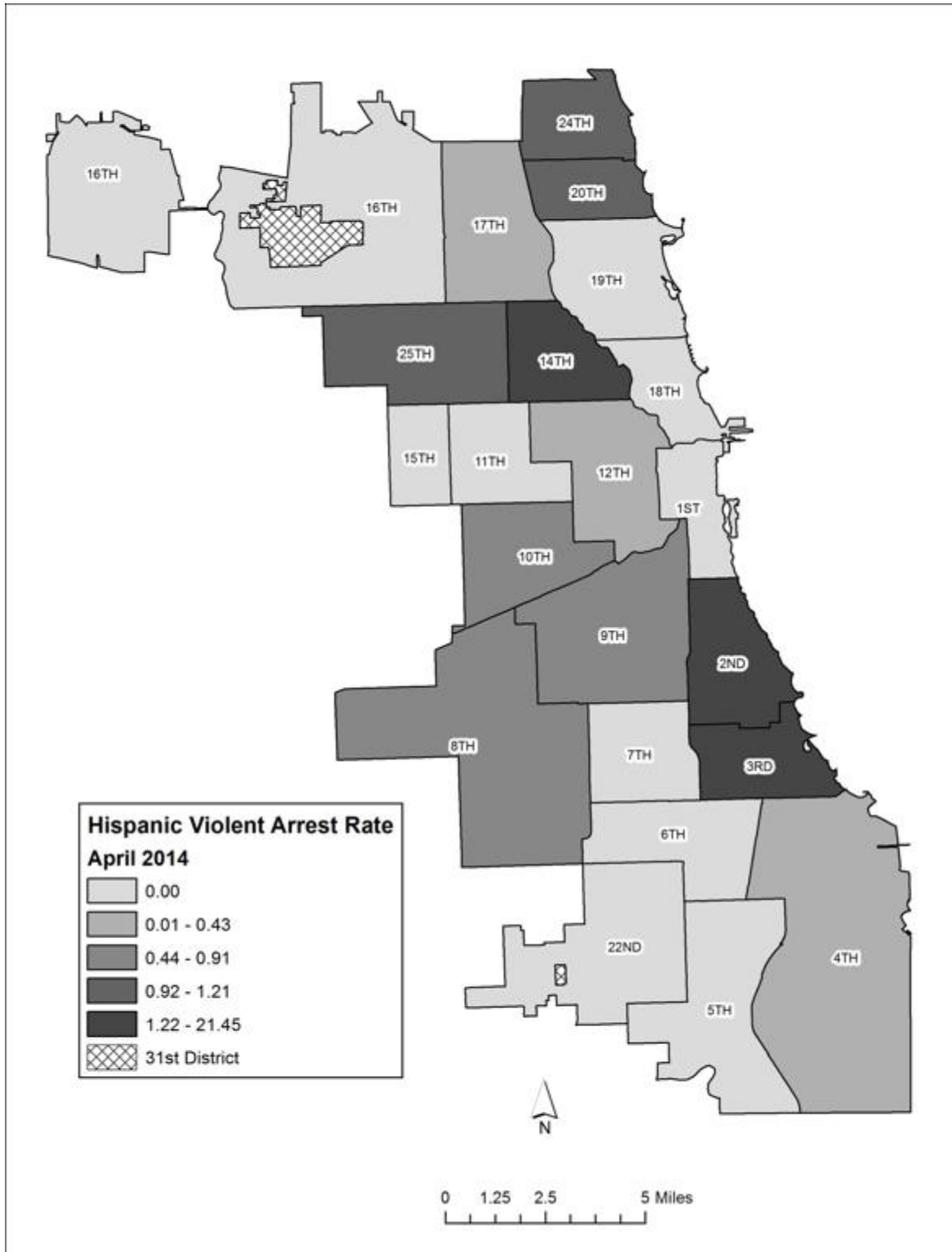




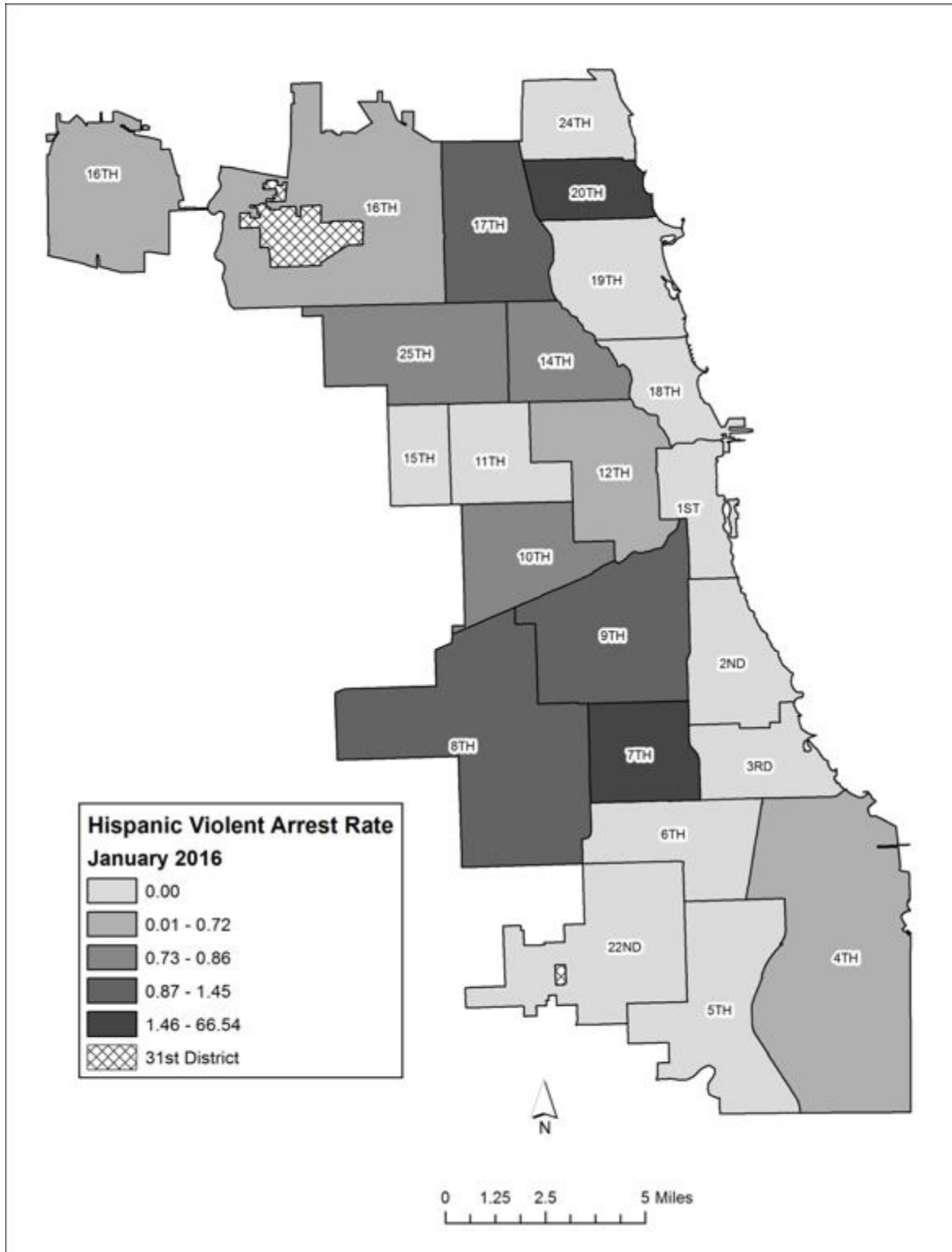
APPENDIX U: Hispanic Violent Arrest Rate, March 2014



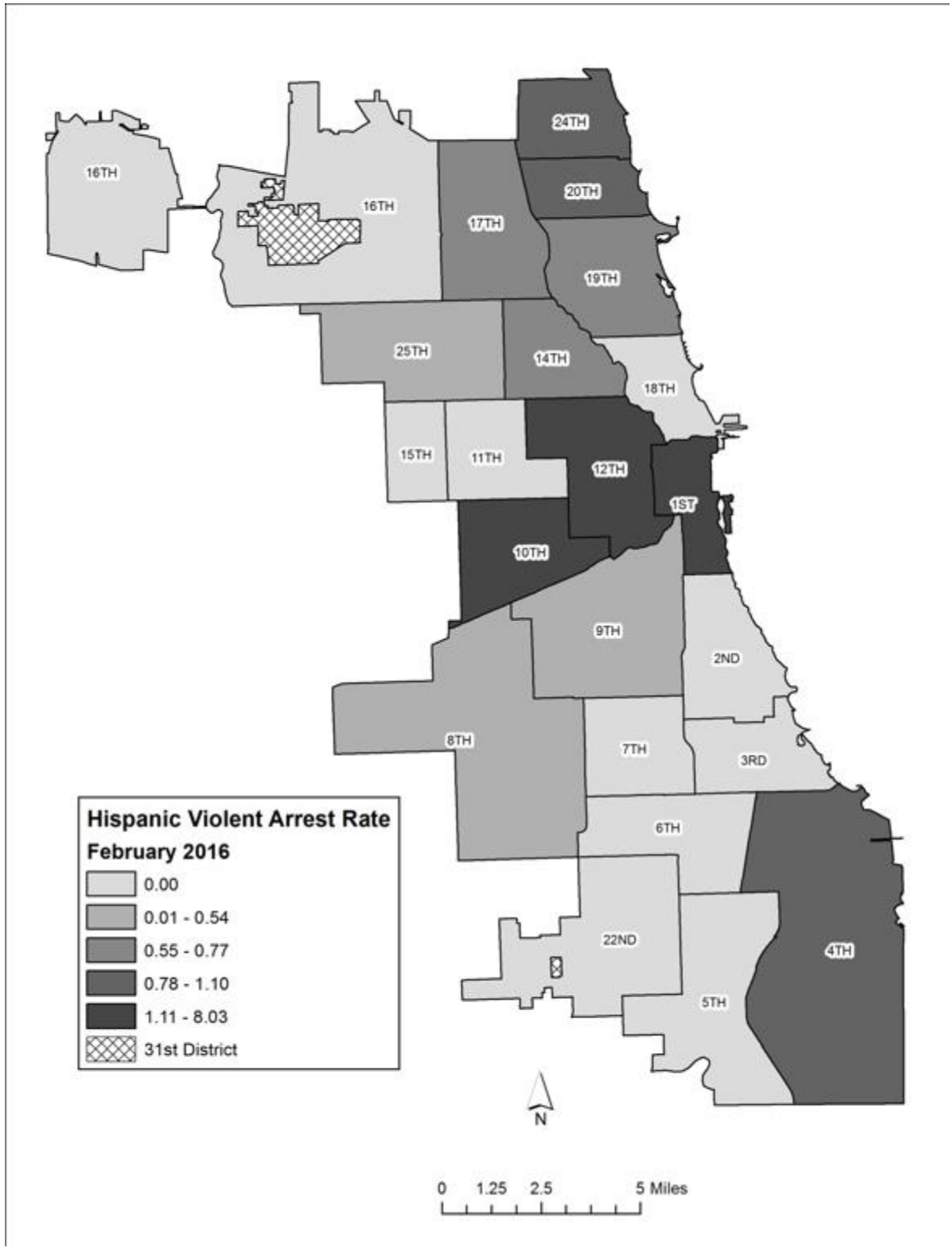
APPENDIX V: Hispanic Violent Arrest Rate, April 2014



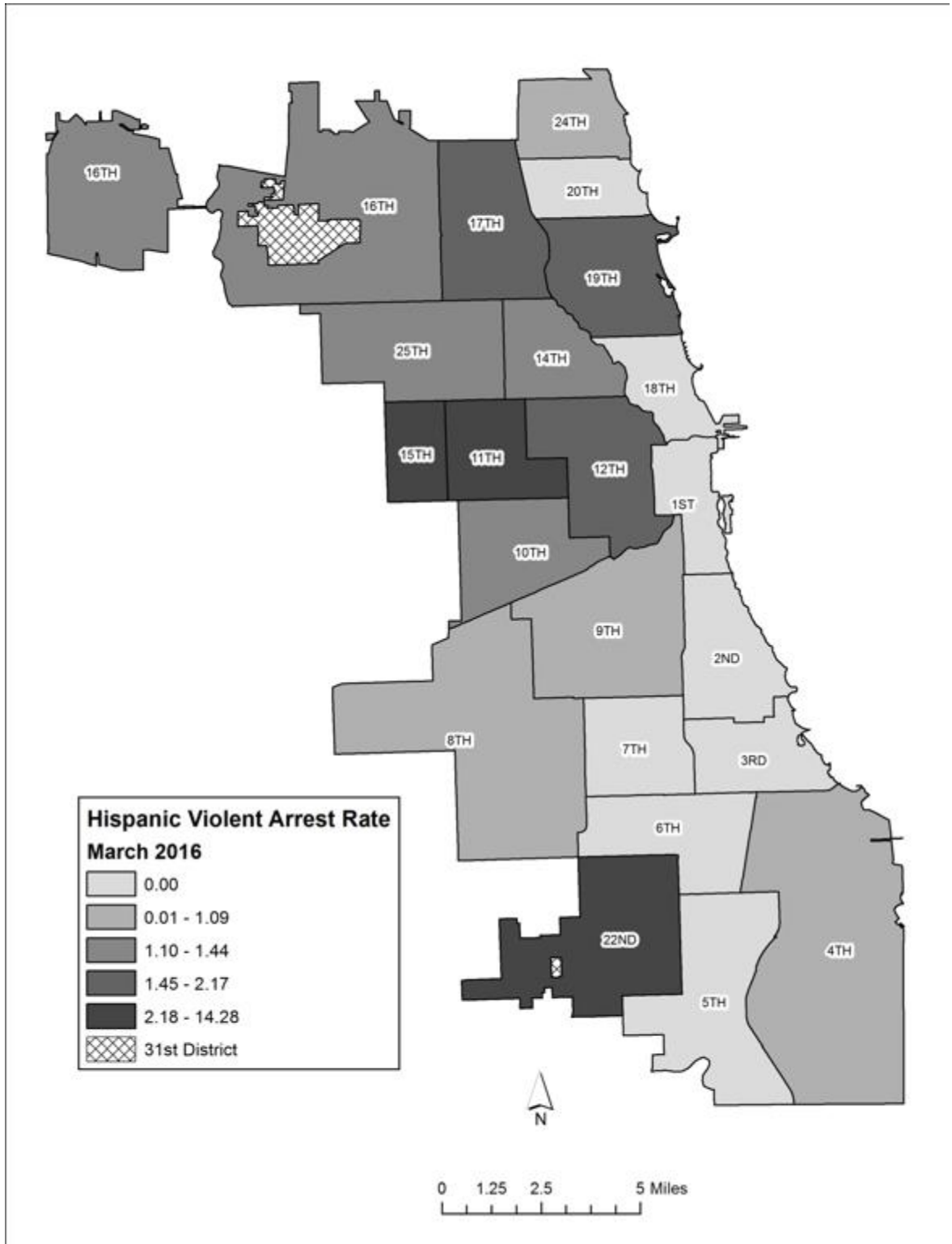
APPENDIX W: Hispanic Violent Arrest Rate, January 2016



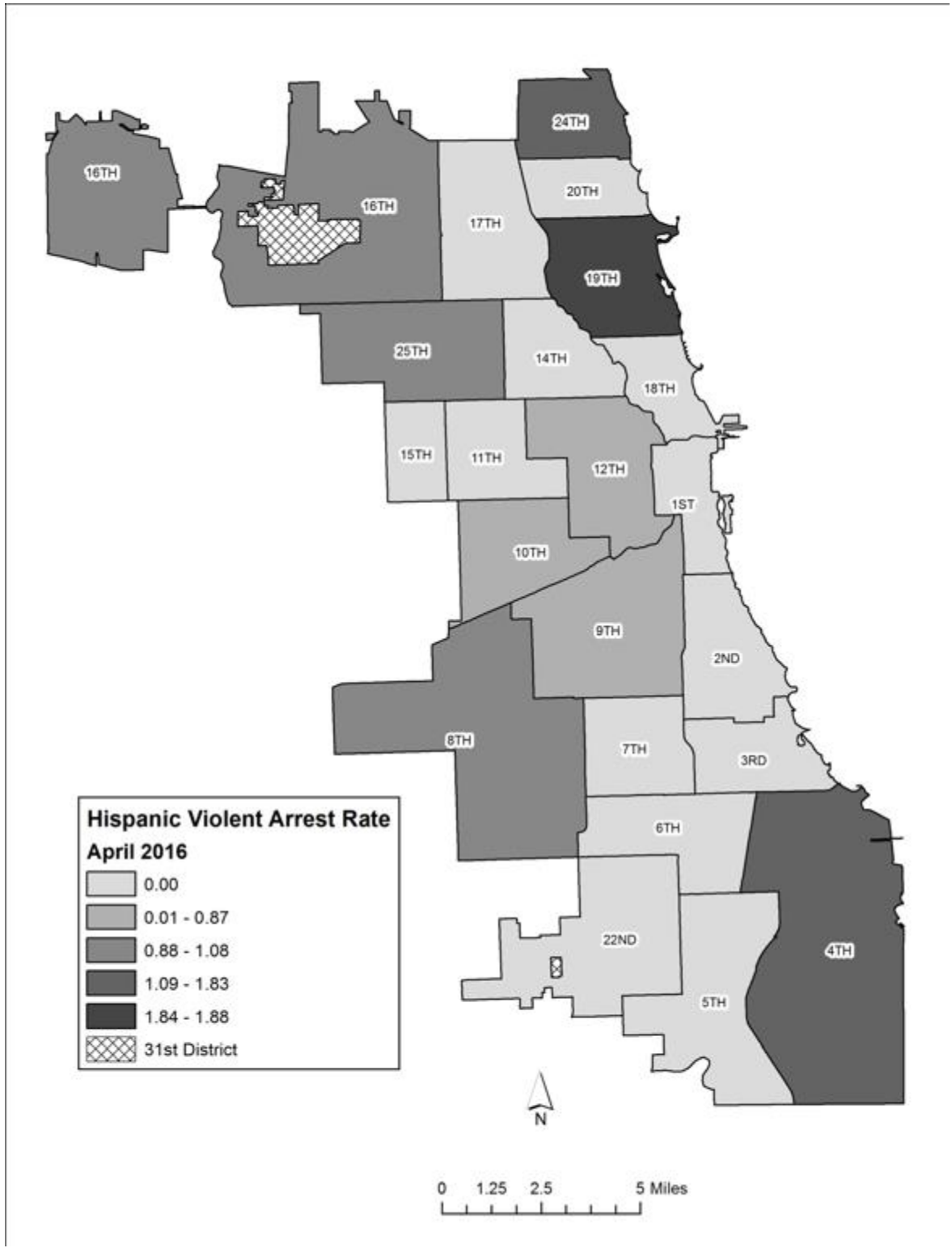
APPENDIX X: Hispanic Violent Arrest Rate, February 2016



APPENDIX Y: Hispanic Violent Arrest Rate, March 2016



APPENDIX Z: Hispanic Violent Arrest Rate, April 2016



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# Summary Report of Arrest Data, January 2014 – April 2016

Lallen T. Johnson and Ralph B. Taylor

VERSION: 20161213

This is a confidential document prepared under contract for the City of Chicago, to be released only to those specifically designated by the City of Chicago, ACLU-IL, the Chicago Police Department, or the Hon. Arlander Keys (Ret.).

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## Introduction

This report describes monthly total arrests or all arrests, city wide and at the police district levels, conducted by the Chicago Police Department from January of 2014 through April of 2016. Its format parallels another report focusing just on violent arrests (Summary Report of Violent Arrest Data, January 2014 – April 2016) The data compiled and presented provide a sketch of changes in total arrests levels over time.

This report's purpose is *purely* descriptive. It will not form inferences about any changes in violence rates. In other words, interpretations of data patterns offered here merely tell what *is* happening where and when, according to these indicators, as opposed *why*.

The data presented here will play a role in another report presenting ecological analyses of investigative stop reports (ISRs). Per discussion of the ecological analysis plans with the parties' experts, the data presented here serve as one of three denominators for those ecological analyses of investigative stop reports (ISRs).<sup>1</sup>

The report presents three major items: 1) city-level counts and rates of arrests for violent incidents for all races/ethnicities, by month, for 28 months (Jan 2014 – April 2016); 2) district-level counts and rates of arrests for violent incidents by race/ethnicity, by month, for the same period, for the three largest race/ethnicity combinations in the city (Non-Hispanic Blacks, Non-Hispanic Whites, and Hispanic Whites); and 3) quantile thematic maps of arrest rates for Non-Hispanic Blacks, Non-Hispanic Whites, and Hispanic Whites for the first four months of 2014, and the first four months of 2016. All analyses exclude arrests associated with the 31<sup>st</sup> district, as these events occurred outside of the Chicago city limits.<sup>2</sup>

## Monthly Arrest Counts and Rates

---

<sup>1</sup> Those experts were Sharad Goel, Aziz Huq, Jens Ludwig and Justin McCrary. Technically, the variables noted here will serve as exposure variables in count models.

<sup>2</sup> A total of 273,022 arrests occurred in Chicago from January 2014 to April 2016. Of those, 2,185 arrest records, or .8% were missing data on the district variable. Our communication with Officer Joseph Candella of the Chicago Police Department, indicated that cases with missing district information are arrests that occurred outside of the city and should be associated with the 31<sup>st</sup> District. He also indicated that roughly 16 of those cases could not be geocoded due to address entry errors. We decided to exclude all cases with missing district information considering that they represent such a small fraction of the dataset, and because they are generally outside of the study area. This leaves a total arrest count of 270,837.

Table 1 displays monthly total arrest counts and rates for the city of Chicago from January 2014 to April of 2016 for all races/ethnicities, Non-Hispanic Blacks, Non-Hispanic Whites and Hispanic Whites. Arrest rates are calculated as the ratio of race/ethnicity-specific arrests to the race/ethnicity-specific population. After, that ratio is multiplied by 10,000. As such, total arrest rates can be interpreted as the number of expected arrests, normalized for every 10,000 residents of said racial/ethnic group. The respective population denominators are derived from the American Community Survey.<sup>3</sup>

To be clear, the columns labeled “All” are for the entire city-wide or district-wide population, regardless of race or ethnicity. The columns labeled “Black,” “White,” and “Hispanic” have both numerators (total arrest counts) and, as relevant, denominators (population counts), that are each specific to one of three race/ethnic combinations: Non-Hispanic Blacks, Non-Hispanic Whites, and Hispanic Whites.

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<sup>3</sup> More specifically, the following 2010-2014 ACS variables were used as denominators.

For Non-Hispanic Blacks: B03002004 (ACS total = 880,066)

For Non-Hispanics Whites: B03002003 (ACS total = 980,789)

For Hispanic Whites: B03002013 (ACS total = 469,978)

Table 1: City-Level Total Arrest Counts and Rates

Month and Year	Counts				Rates per 10,000 Population			
	All	Black	White	Hispanic	All	Black	White	Hispanic
Jan-14	9,480	6,975	750	1,635	34.99	81.90	8.61	37.13
Feb-14	9,406	6,946	785	1,564	34.72	81.56	9.02	35.52
Mar-14	11,242	8,202	940	1,968	41.50	96.31	10.80	44.70
Apr-14	11,024	8,106	863	1,920	40.69	95.18	9.91	43.61
May-14	12,013	8,594	973	2,290	44.34	100.91	11.17	52.01
Jun-14	11,673	8,464	984	2,063	43.09	99.38	11.30	46.86
Jul-14	12,073	8,789	1,000	2,128	44.57	103.20	11.48	48.33
Aug-14	11,746	8,420	1,094	2,076	43.36	98.86	12.56	47.15
Sep-14	10,888	7,809	919	1,993	40.19	91.69	10.55	45.27
Oct-14	10,959	8,055	891	1,858	40.45	94.58	10.23	42.20
Nov-14	9,394	6,746	823	1,694	34.68	79.21	9.45	38.47
Dec-14	8,670	6,294	763	1,485	32.00	73.90	8.76	33.73
Jan-15	9,945	7,243	823	1,758	36.71	85.04	9.45	39.93
Feb-15	8,181	5,882	709	1,492	30.20	69.06	8.14	33.89
Mar-15	10,939	7,811	905	2,068	40.38	91.71	10.39	46.97
Apr-15	9,788	7,153	777	1,727	36.13	83.99	8.92	39.22
May-15	10,099	7,264	839	1,845	37.28	85.29	9.64	41.90
Jun-15	9,713	6,837	873	1,857	35.85	80.28	10.03	42.18
Jul-15	10,068	7,176	885	1,873	37.16	84.26	10.16	42.54
Aug-15	10,081	7,290	889	1,769	37.21	85.60	10.21	40.18
Sep-15	9,568	6,881	830	1,733	35.32	80.79	9.53	39.36
Oct-15	9,558	7,001	821	1,589	35.28	82.20	9.43	36.09
Nov-15	8,273	5,963	728	1,483	30.54	70.02	8.36	33.68
Dec-15	6,720	4,766	600	1,228	24.81	55.96	6.89	27.89
Jan-16	6,749	4,877	587	1,193	24.91	57.26	6.74	27.10
Feb-16	6,892	5,001	594	1,214	25.44	58.72	6.82	27.57
Mar-16	7,918	5,716	625	1,464	29.23	67.12	7.18	33.25
Apr-16	7,777	5,635	619	1,404	28.71	66.16	7.11	31.89

Across all races and ethnicities, a total of 270,837 arrests occurred in the city from January 2014 – April 2016. Annually, 128,568 occurred in 2014, 112,933 in 2015, and 29,336 from January to April 2016. The all arrests count dropped 12.2 percent from 2014 to 2015. (This compares to an 18.2 percent drop in the violent arrest count comparing the same two periods.) Looking just at the first four months of 2016, and comparing all arrests counts for that period to the same months in 2015, all arrests dropped from 38,853 to the aforementioned 29,336, for a 24.4 percent reduction.

Among the three specific races and ethnicities examined in this report, for each year and the four-month period of 2016, Non-Hispanic Blacks demonstrated the highest within-year average monthly all arrests rates per 10,000 resident population: 91.39 in 2014, 79.52 in 2015, and 62.32 in 2016. Considering unweighted monthly arrest rates for this group, the average monthly all arrests rate dropped 13 percent from 2014 to 2015. Looking just at the first four months of 2015 and the same period in 2016, the unweighted monthly all arrests rates dropped from 82.45 to the above mentioned 62.32, representing a drop of 24.4 percent.

The within-year average monthly all arrests rate of Non-Hispanic Whites was the lowest of the three groups being considered: 10.32 in 2014, 9.26 in 2015, and 6.96 in 2016. Using unweighted monthly averages this represented a drop of 10.2 percent from 2014 to 2015. Numbers show a drop of 24.5 percent from the first four months of 2015 (rate = 9.22) to the first four months of 2016 (rate=6.96) using unweighted monthly averages.

The Hispanic within-year average monthly all arrests rates fell in the middle (42.91 in 2014, 38.65 in 2015, and 29.95 in 2016) of the three racial/ethnic groups being considered. The Hispanic average, unweighted monthly all arrests rate dropped 9.9 percent from 2014 to 2015. This is closely comparable to the all arrests rate drop seen for Non-Hispanic Whites over the same period. The Hispanic all arrests unweighted monthly average dropped from 40 for the first four months of 2015 to the aforementioned 29.95 for the first four months of 2016. This represented a 25 percent drop between these two time frames. This, again, is closely comparable to the percentage drop seen for the other two groups, when comparing these two periods.

Figures 1 and 2 display line graphs of monthly arrest counts and rates per 10,000, respectively. Considering everyone (solid black line), visual inspection of Figure 2 suggests an overall slightly decreasing trend in the all arrests rates between January 2014 to April of 2016.

More specifically, the all arrests rate including everyone in January 2014 was at 34.99 per 10,000 residents. It then peaked at 44.57 in July 2014 before beginning a downward trend through December of the same year. The following year (2015) saw rates peak in March at 40.38 per 10,000 residents. That was followed by a slightly downward trend over the next few months followed by sharp decreases in November and December. The overall arrest rate remained stable through February 2016 before elevating again in March and April.

The all arrests rate of Hispanic Whites (dash-dot line) most closely resembles the above-described overall all arrests rate trend. In most months the rate for this group was very slightly above the rate for everyone.

The Non-Hispanic Black all arrests rate also resembles the above trends over time: initial increases in early 2014, followed by generally declining rates in the last half of the year, followed by an increase in early 2015, followed by a slow decline in the latter months of the year and sharper decreases by early 2016. But the levels for this group proved markedly

different. The Non-Hispanic Black all arrest rates generally exceeded the general rate based on total population by about twofold.

Non-Hispanic Whites demonstrate the lowest and most stable arrest rates over time, only fluctuating between about 7 and 13 arrests per 10,000.

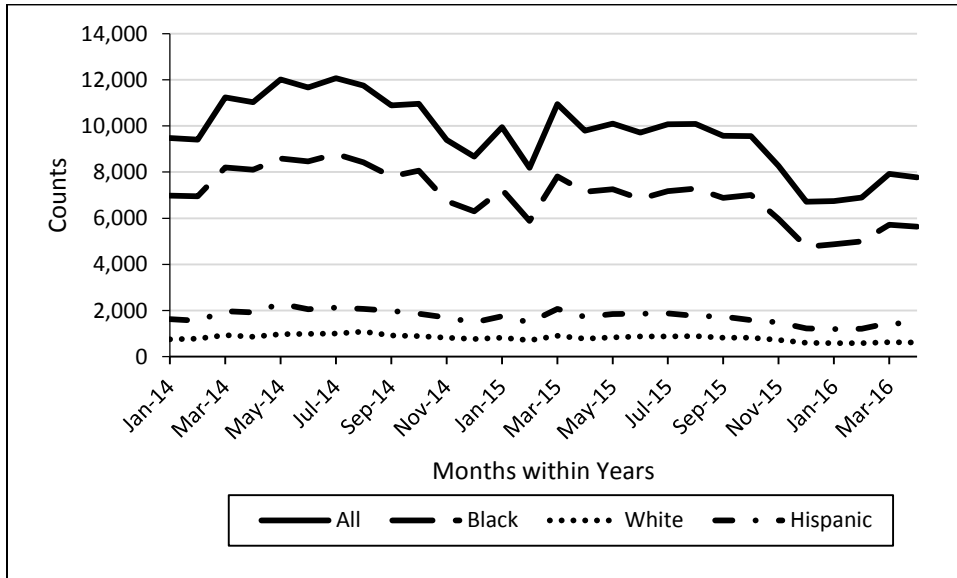


Figure 1: Chicago Monthly All Arrests Counts, Jan 2014 - Apr 2016

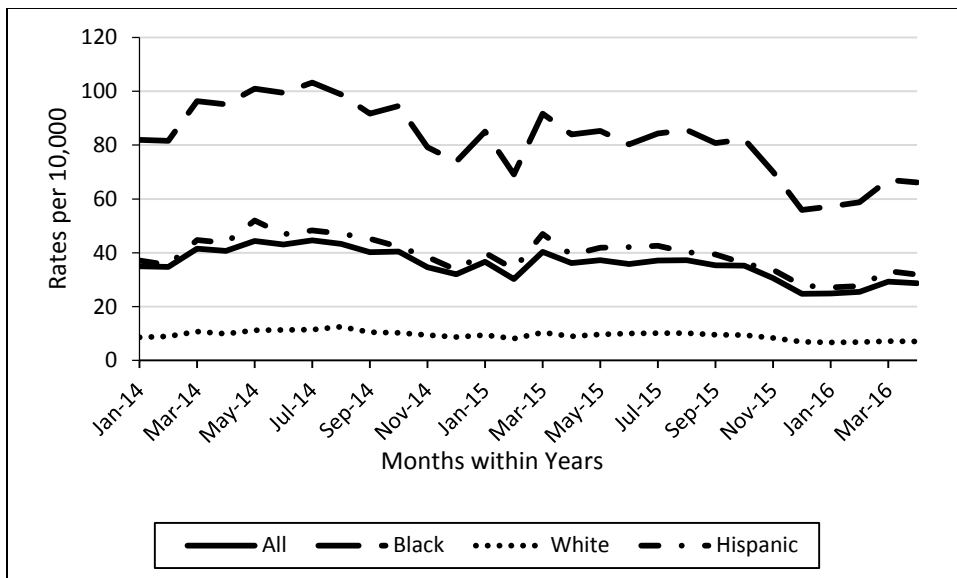


Figure 2: Chicago Monthly All Arrests Rates, Jan 2014 - Apr 2016

District-level monthly arrest counts and rates by race and ethnicity are shown in APPENDIX A.

## Maps of District-level Monthly Arrest Rates

Thematic maps are used to display data associated with places—in this case, police districts. Each map reveals district-level arrest rates per 10,000 for a given month, organized by five quantiles. Each quantile includes 20 percent of Chicago's 22 police districts. The lowest quantile, indicated by the lightest gray shading on each map, denotes districts with an arrest rate for the specified month falling within the lowest 20 percent. The highest quantile, indicated by the darkest shading on each map, identifies districts with arrest rates falling in the highest 20 percent. We exclude the 31<sup>st</sup> district (denoted by the cross-hatched features in each map), since arrests in these areas occurred outside of the Chicago city limits. We provide arrest rate maps for the first four months of 2014, and the first four months of 2016 for Non-Hispanic Blacks, Non-Hispanic Whites and Hispanic Whites in APPENDICES B - Y.

Looking at these maps suggests the highest monthly all arrests rates for Non-Hispanic Blacks cluster in the districts surrounding The Loop, West Side, and the 16<sup>th</sup> district. In the western section, such areas include the 11<sup>th</sup> district, as well as the 9<sup>th</sup>, 10<sup>th</sup>, and 12<sup>th</sup> which also score in the higher quantiles. The lowest rates of arrest can be found, at times, in the South Side (22<sup>nd</sup>, 4<sup>th</sup>, 5<sup>th</sup>, and 2<sup>nd</sup> districts) and North Side (24<sup>th</sup>, 20<sup>th</sup>, 17<sup>th</sup>, and 14<sup>th</sup>).

Non-Hispanic White arrest rates in the top two quantiles cluster in West and South Side districts of Chicago. Districts with relatively lower rates (below the 40<sup>th</sup> percentile) align Lake Michigan, and also include the 22<sup>nd</sup> district in the South Side.

Arrest rates in the highest quantile for Hispanic Whites are clustered districts in the West Side and South Side. On the West Side these include the 15<sup>th</sup> and 11<sup>th</sup> districts. On the South Side they include the 7<sup>th</sup> and 6<sup>th</sup> districts. Those two hot spots of arrest are consistent throughout all months of data mapped, save February and March 2016 for the 15<sup>th</sup> district, and February 2014 for the 6<sup>th</sup> district. The lowest arrest rates cluster in the North Side and most commonly in the 22<sup>nd</sup> district in the South Side.

APPENDIX A: District-Level Monthly Arrest Counts and Rates

District	Month and Year	Counts				Rates per 10,000 Population			
		All	Black	White	Hispanic	All	Black	White	Hispanic
01	Jan-14	468	362	62	33	69.96	257.81	18.48	132.51
01	Feb-14	482	374	56	42	72.06	266.36	16.69	168.65
01	Mar-14	558	427	80	41	83.42	304.10	23.85	164.64
01	Apr-14	441	335	66	24	65.93	238.58	19.67	96.37
01	May-14	445	342	63	30	66.52	243.57	18.78	120.47
01	Jun-14	418	301	68	31	62.49	214.37	20.27	124.48
01	Jul-14	432	314	77	30	64.58	223.63	22.95	120.47
01	Aug-14	433	312	81	29	64.73	222.20	24.14	116.45
01	Sep-14	427	324	66	30	63.83	230.75	19.67	120.47
01	Oct-14	433	326	70	27	64.73	232.17	20.86	108.42
01	Nov-14	366	258	63	30	54.71	183.74	18.78	120.47
01	Dec-14	334	230	70	23	49.93	163.80	20.86	92.36
01	Jan-15	394	304	51	29	58.90	216.50	15.20	116.45
01	Feb-15	304	226	53	22	45.45	160.95	15.80	88.34
01	Mar-15	407	304	58	37	60.84	216.50	17.29	148.58
01	Apr-15	347	257	63	21	51.87	183.03	18.78	84.33
01	May-15	356	249	60	40	53.22	177.33	17.88	160.62
01	Jun-15	338	235	69	28	50.53	167.36	20.57	112.44
01	Jul-15	354	227	74	42	52.92	161.67	22.06	168.65
01	Aug-15	371	258	64	33	55.46	183.74	19.08	132.51
01	Sep-15	393	291	71	27	58.75	207.25	21.16	108.42
01	Oct-15	415	291	72	36	62.04	207.25	21.46	144.56
01	Nov-15	387	265	87	26	57.85	188.73	25.93	104.41
01	Dec-15	361	254	64	32	53.97	180.90	19.08	128.50
01	Jan-16	341	253	48	35	50.98	180.18	14.31	140.55
01	Feb-16	304	186	60	45	45.45	132.47	17.88	180.70
01	Mar-16	321	235	41	34	47.99	167.36	12.22	136.53
01	Apr-16	312	221	46	36	46.64	157.39	13.71	144.56
02	Jan-14	393	375	7	9	41.10	56.48	4.02	64.34
02	Feb-14	422	405	7	9	44.13	61.00	4.02	64.34
02	Mar-14	475	453	11	8	49.67	68.23	6.31	57.19
02	Apr-14	430	411	10	9	44.96	61.90	5.74	64.34
02	May-14	511	485	10	13	53.43	73.05	5.74	92.93
02	Jun-14	471	458	5	7	49.25	68.98	2.87	50.04
02	Jul-14	529	510	7	11	55.32	76.81	4.02	78.64
02	Aug-14	443	424	11	7	46.32	63.86	6.31	50.04
02	Sep-14	333	315	7	8	34.82	47.44	4.02	57.19
02	Oct-14	322	303	6	11	33.67	45.64	3.44	78.64
02	Nov-14	231	216	5	10	24.16	32.53	2.87	71.49



02	Dec-14	248	241	3	4	25.93	36.30	1.72	28.59
02	Jan-15	292	271	13	5	30.53	40.82	7.46	35.74
02	Feb-15	213	194	7	12	22.27	29.22	4.02	85.78
02	Mar-15	323	309	7	5	33.78	46.54	4.02	35.74
02	Apr-15	261	247	7	5	27.29	37.20	4.02	35.74
02	May-15	303	278	9	13	31.68	41.87	5.16	92.93
02	Jun-15	265	245	5	12	27.71	36.90	2.87	85.78
02	Jul-15	304	284	10	6	31.79	42.77	5.74	42.89
02	Aug-15	259	241	7	11	27.08	36.30	4.02	78.64
02	Sep-15	291	281	5	4	30.43	42.32	2.87	28.59
02	Oct-15	375	348	16	10	39.21	52.41	9.18	71.49
02	Nov-15	297	279	7	10	31.06	42.02	4.02	71.49
02	Dec-15	229	221	3	5	23.95	33.29	1.72	35.74
02	Jan-16	207	194	7	5	21.65	29.22	4.02	35.74
02	Feb-16	226	215	4	5	23.63	32.38	2.29	35.74
02	Mar-16	280	256	10	11	29.28	38.56	5.74	78.64
02	Apr-16	266	253	8	5	27.82	38.11	4.59	35.74
03	Jan-14	494	483	7	4	63.36	68.27	22.46	83.84
03	Feb-14	478	463	4	9	61.30	65.44	12.84	188.65
03	Mar-14	595	583	6	5	76.31	82.40	19.25	104.80
03	Apr-14	581	570	5	3	74.51	80.57	16.04	62.88
03	May-14	580	572	4	3	74.39	80.85	12.84	62.88
03	Jun-14	603	598	3	2	77.34	84.52	9.63	41.92
03	Jul-14	626	619	4	2	80.29	87.49	12.84	41.92
03	Aug-14	586	580	4	1	75.16	81.98	12.84	20.96
03	Sep-14	565	554	5	3	72.46	78.30	16.04	62.88
03	Oct-14	556	549	4	1	71.31	77.60	12.84	20.96
03	Nov-14	473	464	3	5	60.66	65.58	9.63	104.80
03	Dec-14	497	485	6	5	63.74	68.55	19.25	104.80
03	Jan-15	463	458	3	1	59.38	64.74	9.63	20.96
03	Feb-15	389	386	2	1	49.89	54.56	6.42	20.96
03	Mar-15	527	521	3	0	67.59	73.64	9.63	0.00
03	Apr-15	465	458	3	2	59.64	64.74	9.63	41.92
03	May-15	415	408	4	1	53.22	57.67	12.84	20.96
03	Jun-15	434	425	5	3	55.66	60.07	16.04	62.88
03	Jul-15	459	443	4	10	58.87	62.61	12.84	209.61
03	Aug-15	509	500	5	2	65.28	70.67	16.04	41.92
03	Sep-15	400	394	2	3	51.30	55.69	6.42	62.88
03	Oct-15	372	364	1	3	47.71	51.45	3.21	62.88
03	Nov-15	375	369	3	2	48.09	52.16	9.63	41.92
03	Dec-15	315	305	2	6	40.40	43.11	6.42	125.76
03	Jan-16	280	273	2	4	35.91	38.59	6.42	83.84
03	Feb-16	302	296	2	3	38.73	41.84	6.42	62.88

03	Mar-16	374	366	4	2	47.97	51.73	12.84	41.92
03	Apr-16	363	360	2	1	46.56	50.88	6.42	20.96
04	Jan-14	439	365	21	52	36.69	50.88	20.22	19.06
04	Feb-14	477	394	13	65	39.86	54.92	12.51	23.82
04	Mar-14	501	397	21	78	41.87	55.34	20.22	28.59
04	Apr-14	652	526	25	92	54.48	73.32	24.07	33.72
04	May-14	659	520	15	116	55.07	72.48	14.44	42.52
04	Jun-14	696	582	25	86	58.16	81.12	24.07	31.52
04	Jul-14	682	553	27	96	56.99	77.08	25.99	35.19
04	Aug-14	794	686	23	82	66.35	95.62	22.14	30.05
04	Sep-14	775	653	18	100	64.76	91.02	17.33	36.65
04	Oct-14	673	560	16	85	56.24	78.06	15.40	31.15
04	Nov-14	616	515	18	80	51.48	71.78	17.33	29.32
04	Dec-14	532	435	16	79	44.46	60.63	15.40	28.95
04	Jan-15	652	543	22	82	54.48	75.69	21.18	30.05
04	Feb-15	541	440	23	75	45.21	61.33	22.14	27.49
04	Mar-15	676	569	20	79	56.49	79.31	19.25	28.95
04	Apr-15	552	470	15	65	46.13	65.51	14.44	23.82
04	May-15	551	456	20	67	46.04	63.56	19.25	24.56
04	Jun-15	558	439	14	99	46.63	61.19	13.48	36.28
04	Jul-15	568	462	17	83	47.47	64.40	16.36	30.42
04	Aug-15	613	503	20	84	51.23	70.11	19.25	30.79
04	Sep-15	552	431	17	102	46.13	60.08	16.36	37.38
04	Oct-15	507	419	14	66	42.37	58.40	13.48	24.19
04	Nov-15	488	398	23	64	40.78	55.48	22.14	23.46
04	Dec-15	377	297	13	60	31.50	41.40	12.51	21.99
04	Jan-16	398	328	8	59	33.26	45.72	7.70	21.62
04	Feb-16	400	331	11	54	33.43	46.14	10.59	19.79
04	Mar-16	430	361	8	58	35.93	50.32	7.70	21.26
04	Apr-16	341	278	15	46	28.50	38.75	14.44	16.86
05	Jan-14	432	409	9	13	59.51	60.00	67.75	95.93
05	Feb-14	427	413	5	7	58.82	60.58	37.64	51.66
05	Mar-14	528	511	5	10	72.73	74.96	37.64	73.80
05	Apr-14	528	508	10	8	72.73	74.52	75.27	59.04
05	May-14	713	694	6	13	98.22	101.80	45.16	95.93
05	Jun-14	650	621	15	13	89.54	91.10	112.91	95.93
05	Jul-14	595	566	12	13	81.96	83.03	90.33	95.93
05	Aug-14	535	519	8	8	73.70	76.13	60.22	59.04
05	Sep-14	507	492	6	7	69.84	72.17	45.16	51.66
05	Oct-14	546	526	8	8	75.21	77.16	60.22	59.04
05	Nov-14	509	489	8	10	70.11	71.73	60.22	73.80
05	Dec-14	479	455	12	8	65.98	66.74	90.33	59.04
05	Jan-15	568	541	16	9	78.24	79.36	120.44	66.42

05	Feb-15	431	417	4	8	59.37	61.17	30.11	59.04
05	Mar-15	482	463	6	12	66.40	67.92	45.16	88.55
05	Apr-15	442	422	7	11	60.89	61.90	52.69	81.17
05	May-15	475	449	15	9	65.43	65.86	112.91	66.42
05	Jun-15	430	413	13	3	59.23	60.58	97.86	22.14
05	Jul-15	464	450	6	8	63.92	66.01	45.16	59.04
05	Aug-15	398	386	10	2	54.82	56.62	75.27	14.76
05	Sep-15	372	358	9	3	51.24	52.52	67.75	22.14
05	Oct-15	336	325	8	2	46.28	47.67	60.22	14.76
05	Nov-15	334	318	6	9	46.01	46.65	45.16	66.42
05	Dec-15	289	275	6	6	39.81	40.34	45.16	44.28
05	Jan-16	311	302	3	4	42.84	44.30	22.58	29.52
05	Feb-16	294	284	6	4	40.50	41.66	45.16	29.52
05	Mar-16	372	362	2	6	51.24	53.10	15.05	44.28
05	Apr-16	403	391	7	4	55.51	57.36	52.69	29.52
06	Jan-14	542	530	6	4	59.49	60.06	107.53	197.89
06	Feb-14	581	563	13	3	63.77	63.80	232.99	148.42
06	Mar-14	744	726	10	8	81.66	82.27	179.22	395.78
06	Apr-14	642	631	3	4	70.47	71.50	53.77	197.89
06	May-14	650	638	6	4	71.34	72.30	107.53	197.89
06	Jun-14	659	654	5	0	72.33	74.11	89.61	0.00
06	Jul-14	691	677	7	4	75.84	76.72	125.46	197.89
06	Aug-14	628	613	8	6	68.93	69.46	143.38	296.83
06	Sep-14	630	621	6	2	69.15	70.37	107.53	98.94
06	Oct-14	627	614	5	5	68.82	69.58	89.61	247.36
06	Nov-14	507	486	13	5	55.65	55.07	232.99	247.36
06	Dec-14	489	479	9	1	53.67	54.28	161.30	49.47
06	Jan-15	615	600	10	5	67.50	67.99	179.22	247.36
06	Feb-15	504	491	9	1	55.32	55.64	161.30	49.47
06	Mar-15	636	625	5	5	69.81	70.82	89.61	247.36
06	Apr-15	621	615	1	4	68.16	69.69	17.92	197.89
06	May-15	641	631	5	4	70.36	71.50	89.61	197.89
06	Jun-15	612	591	13	7	67.17	66.97	232.99	346.31
06	Jul-15	602	587	9	5	66.08	66.52	161.30	247.36
06	Aug-15	588	574	9	2	64.54	65.04	161.30	98.94
06	Sep-15	652	642	9	1	71.56	72.75	161.30	49.47
06	Oct-15	593	589	2	2	65.09	66.74	35.84	98.94
06	Nov-15	536	526	6	4	58.83	59.60	107.53	197.89
06	Dec-15	446	437	4	3	48.95	49.52	71.69	148.42
06	Jan-16	409	394	8	5	44.89	44.65	143.38	247.36
06	Feb-16	423	404	14	4	46.43	45.78	250.91	197.89
06	Mar-16	573	556	12	5	62.89	63.00	215.07	247.36
06	Apr-16	525	511	10	0	57.62	57.90	179.22	0.00

07	Jan-14	598	584	2	7	91.15	94.27	29.37	155.27
07	Feb-14	588	569	8	9	89.63	91.85	117.48	199.63
07	Mar-14	676	657	3	15	103.04	106.05	44.06	332.72
07	Apr-14	725	704	10	10	110.51	113.64	146.85	221.81
07	May-14	692	678	7	5	105.48	109.44	102.80	110.91
07	Jun-14	732	714	7	9	111.58	115.25	102.80	199.63
07	Jul-14	715	704	5	6	108.98	113.64	73.43	133.09
07	Aug-14	662	642	9	8	100.91	103.63	132.17	177.45
07	Sep-14	640	622	7	8	97.55	100.40	102.80	177.45
07	Oct-14	712	703	2	5	108.53	113.48	29.37	110.91
07	Nov-14	594	572	7	12	90.54	92.33	102.80	266.18
07	Dec-14	609	596	4	9	92.83	96.21	58.74	199.63
07	Jan-15	666	647	4	14	101.52	104.44	58.74	310.54
07	Feb-15	491	475	6	9	74.84	76.68	88.11	199.63
07	Mar-15	688	676	5	6	104.87	109.12	73.43	133.09
07	Apr-15	851	833	5	10	129.71	134.46	73.43	221.81
07	May-15	1,012	992	5	12	154.25	160.13	73.43	266.18
07	Jun-15	849	829	5	11	129.41	133.82	73.43	243.99
07	Jul-15	825	811	8	5	125.75	130.91	117.48	110.91
07	Aug-15	825	807	7	7	125.75	130.27	102.80	155.27
07	Sep-15	744	724	5	13	113.40	116.87	73.43	288.36
07	Oct-15	804	789	8	7	122.55	127.36	117.48	155.27
07	Nov-15	627	608	8	10	95.57	98.14	117.48	221.81
07	Dec-15	448	438	5	3	68.29	70.70	73.43	66.54
07	Jan-16	521	508	3	10	79.41	82.00	44.06	221.81
07	Feb-16	473	457	7	8	72.10	73.77	102.80	177.45
07	Mar-16	553	540	6	5	84.29	87.17	88.11	110.91
07	Apr-16	578	565	3	7	88.10	91.20	44.06	155.27
08	Jan-14	537	278	34	215	21.33	53.99	6.84	32.78
08	Feb-14	536	280	52	202	21.29	54.38	10.46	30.80
08	Mar-14	628	332	61	231	24.94	64.48	12.27	35.22
08	Apr-14	740	405	71	261	29.39	78.66	14.28	39.80
08	May-14	750	354	70	319	29.79	68.75	14.08	48.64
08	Jun-14	696	380	63	247	27.64	73.80	12.67	37.66
08	Jul-14	698	374	79	237	27.72	72.64	15.89	36.14
08	Aug-14	752	383	87	275	29.87	74.39	17.50	41.93
08	Sep-14	677	336	60	273	26.89	65.26	12.07	41.63
08	Oct-14	630	336	72	214	25.02	65.26	14.48	32.63
08	Nov-14	606	288	61	248	24.07	55.94	12.27	37.81
08	Dec-14	446	214	64	164	17.71	41.56	12.87	25.01
08	Jan-15	570	291	70	204	22.64	56.52	14.08	31.11
08	Feb-15	480	240	56	180	19.06	46.61	11.26	27.45
08	Mar-15	679	334	59	278	26.97	64.87	11.87	42.39

08	Apr-15	556	303	46	205	22.08	58.85	9.25	31.26
08	May-15	574	262	59	251	22.80	50.89	11.87	38.27
08	Jun-15	599	325	62	209	23.79	63.12	12.47	31.87
08	Jul-15	539	250	59	227	21.41	48.55	11.87	34.61
08	Aug-15	627	324	70	225	24.90	62.93	14.08	34.31
08	Sep-15	541	269	50	218	21.49	52.25	10.06	33.24
08	Oct-15	599	326	54	211	23.79	63.32	10.86	32.17
08	Nov-15	457	238	43	172	18.15	46.22	8.65	26.23
08	Dec-15	357	143	44	166	14.18	27.77	8.85	25.31
08	Jan-16	331	158	33	136	13.15	30.69	6.64	20.74
08	Feb-16	330	169	25	136	13.11	32.82	5.03	20.74
08	Mar-16	416	203	48	160	16.52	39.43	9.66	24.40
08	Apr-16	398	182	42	168	15.81	35.35	8.45	25.62
09	Jan-14	466	217	28	212	28.26	120.05	11.57	38.51
09	Feb-14	441	179	49	205	26.74	99.03	20.25	37.24
09	Mar-14	485	195	44	235	29.41	107.88	18.18	42.68
09	Apr-14	554	257	38	247	33.60	142.18	15.70	44.86
09	May-14	571	222	58	285	34.63	122.82	23.97	51.77
09	Jun-14	534	221	62	241	32.38	122.26	25.62	43.77
09	Jul-14	598	228	56	295	36.27	126.14	23.14	53.58
09	Aug-14	562	239	46	268	34.08	132.22	19.01	48.68
09	Sep-14	521	206	54	255	31.60	113.97	22.31	46.32
09	Oct-14	536	228	58	240	32.51	126.14	23.97	43.59
09	Nov-14	421	190	38	186	25.53	105.11	15.70	33.78
09	Dec-14	376	174	20	174	22.80	96.26	8.26	31.60
09	Jan-15	493	212	45	227	29.90	117.29	18.60	41.23
09	Feb-15	441	216	27	195	26.74	119.50	11.16	35.42
09	Mar-15	545	230	51	256	33.05	127.24	21.07	46.50
09	Apr-15	527	203	52	263	31.96	112.31	21.49	47.77
09	May-15	505	218	56	216	30.63	120.61	23.14	39.23
09	Jun-15	464	182	43	229	28.14	100.69	17.77	41.59
09	Jul-15	536	212	51	263	32.51	117.29	21.07	47.77
09	Aug-15	487	215	47	221	29.53	118.95	19.42	40.14
09	Sep-15	483	206	51	217	29.29	113.97	21.07	39.41
09	Oct-15	494	228	34	222	29.96	126.14	14.05	40.32
09	Nov-15	388	158	38	182	23.53	87.41	15.70	33.06
09	Dec-15	285	123	30	126	17.28	68.05	12.40	22.89
09	Jan-16	310	149	21	133	18.80	82.43	8.68	24.16
09	Feb-16	305	137	32	131	18.50	75.79	13.22	23.79
09	Mar-16	379	133	40	197	22.98	73.58	16.53	35.78
09	Apr-16	341	121	37	171	20.68	66.94	15.29	31.06
10	Jan-14	590	360	20	205	54.55	102.62	56.22	42.96
10	Feb-14	534	342	24	167	49.37	97.49	67.46	35.00

10	Mar-14	602	333	15	251	55.66	94.93	42.16	52.60
10	Apr-14	622	385	16	218	57.51	109.75	44.97	45.69
10	May-14	788	444	33	305	72.85	126.57	92.76	63.92
10	Jun-14	753	449	20	279	69.62	127.99	56.22	58.47
10	Jul-14	714	428	25	256	66.01	122.01	70.27	53.65
10	Aug-14	724	422	34	264	66.94	120.30	95.57	55.33
10	Sep-14	678	382	22	263	62.68	108.89	61.84	55.12
10	Oct-14	734	481	30	219	67.86	137.11	84.33	45.90
10	Nov-14	623	408	21	192	57.60	116.30	59.03	40.24
10	Dec-14	487	299	13	173	45.02	85.23	36.54	36.26
10	Jan-15	497	309	14	169	45.95	88.08	39.35	35.42
10	Feb-15	409	276	11	118	37.81	78.68	30.92	24.73
10	Mar-15	664	392	26	239	61.39	111.74	73.08	50.09
10	Apr-15	616	374	27	206	56.95	106.61	75.90	43.17
10	May-15	612	382	17	210	56.58	108.89	47.79	44.01
10	Jun-15	555	348	9	194	51.31	99.20	25.30	40.66
10	Jul-15	605	360	15	226	55.93	102.62	42.16	47.36
10	Aug-15	638	367	30	240	58.99	104.62	84.33	50.30
10	Sep-15	630	398	23	205	58.25	113.45	64.65	42.96
10	Oct-15	561	361	17	170	51.87	102.91	47.79	35.63
10	Nov-15	508	292	16	196	46.97	83.24	44.97	41.08
10	Dec-15	440	273	10	152	40.68	77.82	28.11	31.86
10	Jan-16	357	224	9	122	33.01	63.85	25.30	25.57
10	Feb-16	381	228	18	132	35.22	64.99	50.60	27.66
10	Mar-16	604	406	18	177	55.84	115.73	50.60	37.09
10	Apr-16	552	326	14	206	51.03	92.93	39.35	43.17
11	Jan-14	1,348	1,185	67	85	187.17	195.77	328.00	296.89
11	Feb-14	1,250	1,122	47	74	173.56	185.36	230.09	258.47
11	Mar-14	1,515	1,350	76	80	210.36	223.03	372.06	279.43
11	Apr-14	1,413	1,256	78	69	196.20	207.50	381.85	241.01
11	May-14	1,391	1,236	70	75	193.14	204.19	342.68	261.96
11	Jun-14	1,372	1,226	77	59	190.50	202.54	376.95	206.08
11	Jul-14	1,434	1,283	72	68	199.11	211.96	352.47	237.51
11	Aug-14	1,450	1,262	88	89	201.33	208.49	430.80	310.86
11	Sep-14	1,302	1,156	75	62	180.78	190.98	367.16	216.56
11	Oct-14	1,368	1,218	76	68	189.95	201.22	372.06	237.51
11	Nov-14	1,217	1,088	50	71	168.98	179.74	244.77	247.99
11	Dec-14	1,161	1,030	40	76	161.21	170.16	195.82	265.46
11	Jan-15	1,301	1,145	53	98	180.64	189.16	259.46	342.30
11	Feb-15	1,021	901	51	60	141.77	148.85	249.67	209.57
11	Mar-15	1,482	1,340	55	78	205.78	221.38	269.25	272.44
11	Apr-15	1,260	1,137	49	69	174.95	187.84	239.88	241.01
11	May-15	1,134	1,019	45	59	157.46	168.34	220.30	206.08

11	Jun-15	1,098	965	74	50	152.46	159.42	362.26	174.64
11	Jul-15	1,250	1,090	79	69	173.56	180.07	386.74	241.01
11	Aug-15	1,231	1,083	74	67	170.92	178.92	362.26	234.02
11	Sep-15	1,139	1,009	61	61	158.15	166.69	298.62	213.06
11	Oct-15	1,305	1,148	72	73	181.20	189.66	352.47	254.98
11	Nov-15	1,140	1,009	63	61	158.29	166.69	308.41	213.06
11	Dec-15	918	824	39	50	127.46	136.13	190.92	174.64
11	Jan-16	946	865	30	46	131.35	142.90	146.86	160.67
11	Feb-16	990	902	33	49	137.46	149.02	161.55	171.15
11	Mar-16	965	852	38	72	133.99	140.76	186.03	251.48
11	Apr-16	1,075	953	46	66	149.26	157.44	225.19	230.53
12	Jan-14	331	202	39	83	25.42	86.42	7.19	35.99
12	Feb-14	337	200	30	106	25.88	85.57	5.53	45.96
12	Mar-14	385	220	47	111	29.57	94.12	8.67	48.13
12	Apr-14	359	205	37	110	27.57	87.70	6.82	47.69
12	May-14	351	175	52	118	26.95	74.87	9.59	51.16
12	Jun-14	335	188	46	96	25.73	80.43	8.48	41.62
12	Jul-14	286	171	35	78	21.96	73.16	6.45	33.82
12	Aug-14	340	174	61	101	26.11	74.44	11.25	43.79
12	Sep-14	282	141	37	94	21.66	60.32	6.82	40.76
12	Oct-14	314	164	38	107	24.11	70.16	7.01	46.39
12	Nov-14	244	141	27	72	18.74	60.32	4.98	31.22
12	Dec-14	233	134	37	56	17.89	57.33	6.82	24.28
12	Jan-15	234	137	39	52	17.97	58.61	7.19	22.55
12	Feb-15	223	101	37	82	17.13	43.21	6.82	35.55
12	Mar-15	303	155	38	101	23.27	66.31	7.01	43.79
12	Apr-15	295	171	43	78	22.65	73.16	7.93	33.82
12	May-15	322	174	44	99	24.73	74.44	8.11	42.92
12	Jun-15	396	211	49	129	30.41	90.27	9.04	55.93
12	Jul-15	306	174	40	89	23.50	74.44	7.38	38.59
12	Aug-15	318	179	41	91	24.42	76.58	7.56	39.46
12	Sep-15	299	171	42	80	22.96	73.16	7.75	34.69
12	Oct-15	298	193	32	68	22.88	82.57	5.90	29.48
12	Nov-15	272	172	23	72	20.89	73.59	4.24	31.22
12	Dec-15	227	126	42	53	17.43	53.91	7.75	22.98
12	Jan-16	236	149	31	53	18.12	63.75	5.72	22.98
12	Feb-16	226	143	33	48	17.36	61.18	6.09	20.81
12	Mar-16	247	138	32	74	18.97	59.04	5.90	32.08
12	Apr-16	228	137	34	53	17.51	58.61	6.27	22.98
14	Jan-14	152	32	24	91	12.72	36.73	4.41	26.11
14	Feb-14	135	40	22	72	11.30	45.91	4.04	20.66
14	Mar-14	221	53	35	128	18.50	60.83	6.43	36.73
14	Apr-14	230	70	34	120	19.25	80.34	6.24	34.43

14	May-14	268	87	42	126	22.43	99.85	7.71	36.15
14	Jun-14	291	66	40	170	24.36	75.75	7.34	48.78
14	Jul-14	293	86	48	146	24.53	98.70	8.81	41.89
14	Aug-14	267	59	43	154	22.35	67.71	7.89	44.19
14	Sep-14	238	58	43	123	19.92	66.57	7.89	35.29
14	Oct-14	220	69	32	108	18.41	79.19	5.88	30.99
14	Nov-14	149	27	37	77	12.47	30.99	6.79	22.09
14	Dec-14	149	44	25	74	12.47	50.50	4.59	21.23
14	Jan-15	157	40	31	81	13.14	45.91	5.69	23.24
14	Feb-15	152	35	32	81	12.72	40.17	5.88	23.24
14	Mar-15	202	36	43	120	16.91	41.32	7.89	34.43
14	Apr-15	183	55	35	85	15.32	63.12	6.43	24.39
14	May-15	206	48	24	119	17.24	55.09	4.41	34.14
14	Jun-15	207	45	37	113	17.33	51.65	6.79	32.42
14	Jul-15	204	49	31	116	17.08	56.24	5.69	33.28
14	Aug-15	236	60	49	116	19.75	68.86	9.00	33.28
14	Sep-15	197	52	33	108	16.49	59.68	6.06	30.99
14	Oct-15	190	32	32	118	15.90	36.73	5.88	33.86
14	Nov-15	150	41	25	79	12.56	47.06	4.59	22.67
14	Dec-15	139	41	22	66	11.63	47.06	4.04	18.94
14	Jan-16	136	24	29	78	11.38	27.54	5.32	22.38
14	Feb-16	141	45	17	76	11.80	51.65	3.12	21.81
14	Mar-16	173	33	37	93	14.48	37.87	6.79	26.68
14	Apr-16	186	40	33	104	15.57	45.91	6.06	29.84
15	Jan-14	589	555	21	10	99.46	101.00	174.36	142.78
15	Feb-14	552	504	28	17	93.21	91.72	232.48	242.73
15	Mar-14	724	681	23	18	122.25	123.93	190.97	257.00
15	Apr-14	754	703	29	19	127.32	127.93	240.79	271.28
15	May-14	843	777	35	25	142.35	141.40	290.60	356.95
15	Jun-14	763	720	23	14	128.84	131.02	190.97	199.89
15	Jul-14	887	835	30	21	149.77	151.95	249.09	299.84
15	Aug-14	925	861	30	34	156.19	156.68	249.09	485.45
15	Sep-14	823	774	25	20	138.97	140.85	207.57	285.56
15	Oct-14	753	699	23	29	127.15	127.20	190.97	414.06
15	Nov-14	661	619	23	15	111.61	112.64	190.97	214.17
15	Dec-14	589	546	28	14	99.46	99.36	232.48	199.89
15	Jan-15	677	624	31	20	114.32	113.55	257.39	285.56
15	Feb-15	565	521	23	19	95.40	94.81	190.97	271.28
15	Mar-15	686	651	15	20	115.83	118.47	124.54	285.56
15	Apr-15	633	603	15	12	106.89	109.73	124.54	171.34
15	May-15	605	572	11	20	102.16	104.09	91.33	285.56
15	Jun-15	587	558	13	16	99.12	101.54	107.94	228.45
15	Jul-15	655	619	17	18	110.60	112.64	141.15	257.00



15	Aug-15	656	617	22	16	110.77	112.28	182.67	228.45
15	Sep-15	677	635	21	19	114.32	115.56	174.36	271.28
15	Oct-15	543	514	13	14	91.69	93.54	107.94	199.89
15	Nov-15	502	460	15	24	84.77	83.71	124.54	342.67
15	Dec-15	362	343	9	10	61.13	62.42	74.73	142.78
15	Jan-16	343	325	5	11	57.92	59.14	41.51	157.06
15	Feb-16	445	427	12	6	75.14	77.70	99.64	85.67
15	Mar-16	513	500	7	6	86.62	90.99	58.12	85.67
15	Apr-16	493	460	16	16	83.25	83.71	132.85	228.45
16	Jan-14	239	56	103	72	11.63	232.15	7.47	25.90
16	Feb-14	220	22	108	83	10.71	91.20	7.84	29.86
16	Mar-14	295	37	122	125	14.36	153.39	8.85	44.97
16	Apr-14	234	31	104	91	11.39	128.51	7.54	32.74
16	May-14	300	42	138	110	14.60	174.11	10.01	39.58
16	Jun-14	285	47	124	104	13.87	194.84	9.00	37.42
16	Jul-14	265	41	123	96	12.90	169.97	8.92	34.54
16	Aug-14	263	41	134	74	12.80	169.97	9.72	26.62
16	Sep-14	265	33	145	74	12.90	136.80	10.52	26.62
16	Oct-14	271	52	121	86	13.19	215.57	8.78	30.94
16	Nov-14	219	42	95	72	10.66	174.11	6.89	25.90
16	Dec-14	226	35	117	64	11.00	145.09	8.49	23.03
16	Jan-15	287	60	131	90	13.97	248.73	9.50	32.38
16	Feb-15	238	37	115	83	11.59	153.39	8.34	29.86
16	Mar-15	293	56	127	97	14.26	232.15	9.21	34.90
16	Apr-15	250	40	111	89	12.17	165.82	8.05	32.02
16	May-15	306	56	143	91	14.90	232.15	10.37	32.74
16	Jun-15	258	29	134	83	12.56	120.22	9.72	29.86
16	Jul-15	245	33	130	75	11.93	136.80	9.43	26.98
16	Aug-15	232	41	104	78	11.29	169.97	7.54	28.06
16	Sep-15	237	37	118	68	11.54	153.39	8.56	24.46
16	Oct-15	233	42	115	70	11.34	174.11	8.34	25.18
16	Nov-15	207	41	99	57	10.08	169.97	7.18	20.51
16	Dec-15	174	31	75	58	8.47	128.51	5.44	20.87
16	Jan-16	177	41	83	51	8.62	169.97	6.02	18.35
16	Feb-16	178	39	84	46	8.66	161.68	6.09	16.55
16	Mar-16	190	33	72	76	9.25	136.80	5.22	27.34
16	Apr-16	168	38	65	59	8.18	157.53	4.72	21.23
17	Jan-14	144	21	33	80	9.68	45.95	5.81	20.62
17	Feb-14	173	33	57	71	11.63	72.21	10.03	18.30
17	Mar-14	187	23	56	100	12.57	50.33	9.85	25.78
17	Apr-14	181	29	43	100	12.17	63.46	7.57	25.78
17	May-14	238	31	58	138	16.00	67.84	10.21	35.58
17	Jun-14	192	29	55	98	12.91	63.46	9.68	25.27

17	Jul-14	205	26	55	112	13.78	56.90	9.68	28.87
17	Aug-14	195	35	58	91	13.11	76.59	10.21	23.46
17	Sep-14	185	33	49	92	12.44	72.21	8.62	23.72
17	Oct-14	165	39	27	89	11.09	85.34	4.75	22.95
17	Nov-14	166	23	47	84	11.16	50.33	8.27	21.66
17	Dec-14	164	43	30	81	11.02	94.10	5.28	20.88
17	Jan-15	165	36	51	70	11.09	78.78	8.98	18.05
17	Feb-15	183	34	49	88	12.30	74.40	8.62	22.69
17	Mar-15	211	33	57	108	14.18	72.21	10.03	27.84
17	Apr-15	192	31	48	101	12.91	67.84	8.45	26.04
17	May-15	167	34	33	93	11.23	74.40	5.81	23.98
17	Jun-15	187	31	48	94	12.57	67.84	8.45	24.23
17	Jul-15	167	26	47	85	11.23	56.90	8.27	21.91
17	Aug-15	151	20	41	81	10.15	43.77	7.22	20.88
17	Sep-15	158	16	39	87	10.62	35.01	6.86	22.43
17	Oct-15	173	30	37	95	11.63	65.65	6.51	24.49
17	Nov-15	144	25	34	84	9.68	54.71	5.98	21.66
17	Dec-15	143	31	36	67	9.61	67.84	6.34	17.27
17	Jan-16	149	28	46	68	10.02	61.27	8.10	17.53
17	Feb-16	132	27	39	64	8.87	59.08	6.86	16.50
17	Mar-16	134	22	41	65	9.01	48.14	7.22	16.76
17	Apr-16	147	35	37	69	9.88	76.59	6.51	17.79
18	Jan-14	267	167	73	22	22.08	172.81	8.12	45.04
18	Feb-14	300	213	54	21	24.81	220.41	6.01	42.99
18	Mar-14	356	259	65	16	29.44	268.01	7.23	32.75
18	Apr-14	311	229	55	25	25.72	236.97	6.12	51.18
18	May-14	362	247	73	37	29.94	255.60	8.12	75.75
18	Jun-14	351	256	57	33	29.03	264.91	6.34	67.56
18	Jul-14	369	278	45	42	30.52	287.68	5.01	85.98
18	Aug-14	349	214	84	44	28.86	221.45	9.35	90.08
18	Sep-14	304	208	44	43	25.14	215.24	4.90	88.03
18	Oct-14	322	219	63	33	26.63	226.62	7.01	67.56
18	Nov-14	251	166	65	15	20.76	171.78	7.23	30.71
18	Dec-14	280	206	46	24	23.16	213.17	5.12	49.13
18	Jan-15	293	198	51	36	24.23	204.89	5.67	73.70
18	Feb-15	203	145	35	15	16.79	150.05	3.89	30.71
18	Mar-15	287	184	72	24	23.73	190.40	8.01	49.13
18	Apr-15	248	165	46	29	20.51	170.74	5.12	59.37
18	May-15	277	179	54	36	22.91	185.23	6.01	73.70
18	Jun-15	276	185	57	30	22.82	191.44	6.34	61.42
18	Jul-15	295	210	54	23	24.40	217.31	6.01	47.09
18	Aug-15	306	202	61	35	25.31	209.03	6.79	71.65
18	Sep-15	244	170	40	24	20.18	175.92	4.45	49.13

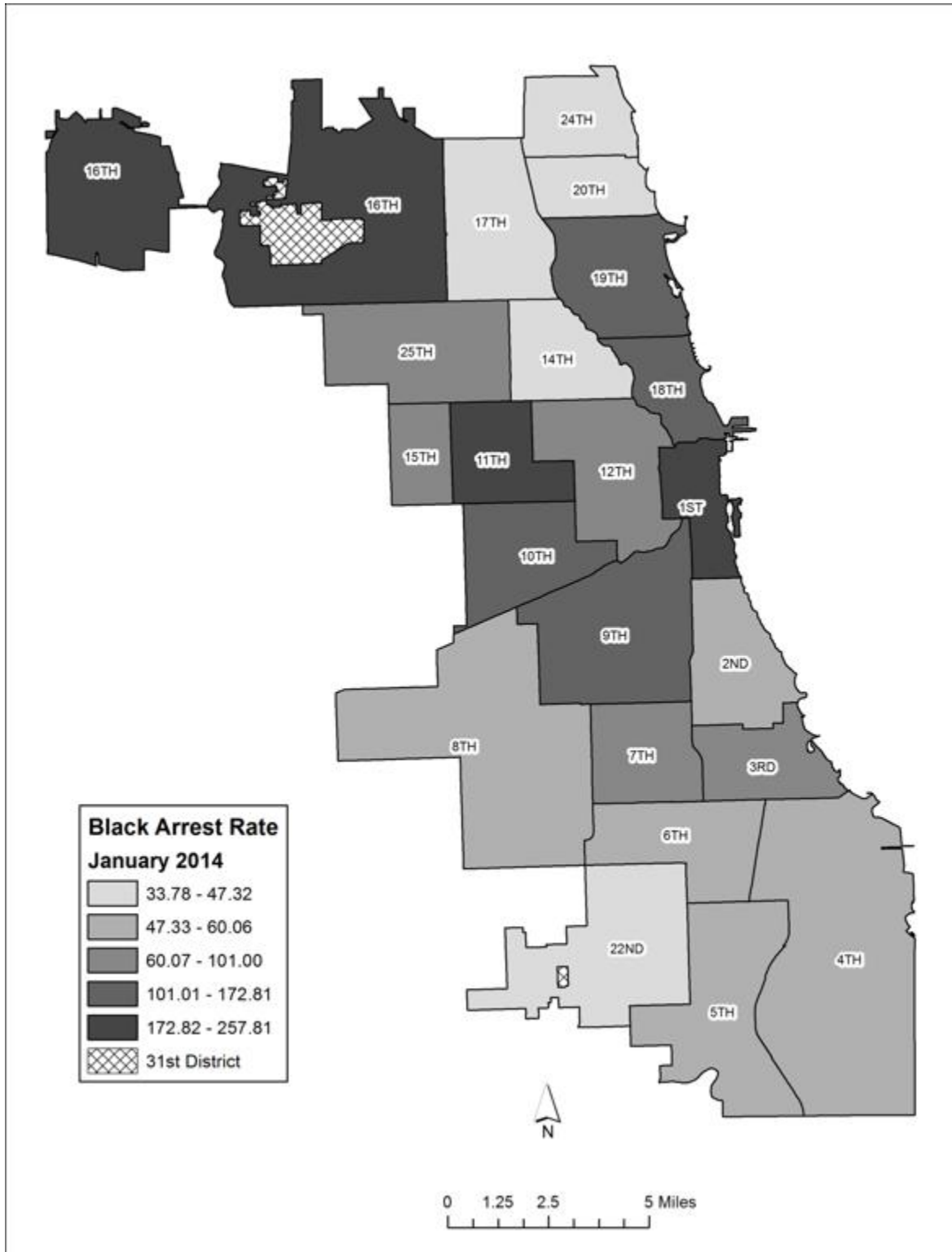
18	Oct-15	224	157	46	19	18.52	162.46	5.12	38.90
18	Nov-15	231	167	43	17	19.10	172.81	4.78	34.80
18	Dec-15	214	144	40	17	17.70	149.01	4.45	34.80
18	Jan-16	187	106	51	24	15.46	109.69	5.67	49.13
18	Feb-16	205	137	41	22	16.95	141.77	4.56	45.04
18	Mar-16	195	109	55	25	16.13	112.79	6.12	51.18
18	Apr-16	192	138	31	20	15.88	142.80	3.45	40.94
19	Jan-14	284	161	77	38	13.71	129.87	4.95	23.86
19	Feb-14	287	156	70	48	13.85	125.84	4.50	30.14
19	Mar-14	346	157	111	68	16.70	126.64	7.13	42.70
19	Apr-14	354	173	95	70	17.08	139.55	6.10	43.95
19	May-14	357	195	79	74	17.23	157.29	5.08	46.46
19	Jun-14	469	232	124	93	22.63	187.14	7.97	58.39
19	Jul-14	449	231	118	90	21.67	186.33	7.58	56.51
19	Aug-14	424	230	100	80	20.46	185.53	6.43	50.23
19	Sep-14	350	177	96	65	16.89	142.77	6.17	40.81
19	Oct-14	367	191	94	62	17.71	154.07	6.04	38.93
19	Nov-14	251	112	72	63	12.11	90.34	4.63	39.56
19	Dec-14	261	109	78	62	12.60	87.92	5.01	38.93
19	Jan-15	229	115	66	41	11.05	92.76	4.24	25.74
19	Feb-15	221	108	65	42	10.67	87.12	4.18	26.37
19	Mar-15	326	147	93	67	15.73	118.58	5.98	42.07
19	Apr-15	273	127	75	58	13.17	102.44	4.82	36.42
19	May-15	269	128	77	56	12.98	103.25	4.95	35.16
19	Jun-15	356	164	103	77	17.18	132.29	6.62	48.35
19	Jul-15	287	144	81	56	13.85	116.16	5.21	35.16
19	Aug-15	307	166	76	59	14.82	133.90	4.88	37.05
19	Sep-15	319	158	94	64	15.39	127.45	6.04	40.19
19	Oct-15	301	139	102	49	14.53	112.12	6.55	30.77
19	Nov-15	216	89	75	45	10.42	71.79	4.82	28.26
19	Dec-15	159	76	45	32	7.67	61.30	2.89	20.09
19	Jan-16	184	85	61	33	8.88	68.56	3.92	20.72
19	Feb-16	172	84	51	33	8.30	67.76	3.28	20.72
19	Mar-16	209	96	54	53	10.09	77.44	3.47	33.28
19	Apr-16	227	102	60	56	10.95	82.28	3.86	35.16
20	Jan-14	92	46	16	30	10.56	47.32	3.30	31.81
20	Feb-14	119	55	29	32	13.66	56.58	5.97	33.93
20	Mar-14	111	56	23	25	12.75	57.61	4.74	26.51
20	Apr-14	113	38	36	35	12.97	39.09	7.41	37.11
20	May-14	142	63	28	41	16.30	64.81	5.77	43.47
20	Jun-14	147	53	45	39	16.88	54.52	9.27	41.35
20	Jul-14	173	79	35	52	19.86	81.27	7.21	55.13
20	Aug-14	156	64	43	41	17.91	65.84	8.86	43.47

20	Sep-14	137	63	41	25	15.73	64.81	8.44	26.51
20	Oct-14	158	68	33	50	18.14	69.95	6.80	53.01
20	Nov-14	153	59	43	44	17.57	60.69	8.86	46.65
20	Dec-14	113	46	26	33	12.97	47.32	5.35	34.99
20	Jan-15	103	40	22	32	11.83	41.15	4.53	33.93
20	Feb-15	88	38	24	22	10.10	39.09	4.94	23.33
20	Mar-15	127	49	34	37	14.58	50.41	7.00	39.23
20	Apr-15	102	52	20	23	11.71	53.49	4.12	24.39
20	May-15	131	70	22	29	15.04	72.01	4.53	30.75
20	Jun-15	128	60	20	37	14.70	61.72	4.12	39.23
20	Jul-15	142	70	31	36	16.30	72.01	6.38	38.17
20	Aug-15	133	66	38	27	15.27	67.89	7.83	28.63
20	Sep-15	122	69	26	19	14.01	70.98	5.35	20.14
20	Oct-15	119	54	29	32	13.66	55.55	5.97	33.93
20	Nov-15	93	42	19	28	10.68	43.20	3.91	29.69
20	Dec-15	108	29	34	37	12.40	29.83	7.00	39.23
20	Jan-16	111	53	26	29	12.75	54.52	5.35	30.75
20	Feb-16	99	39	21	35	11.37	40.12	4.32	37.11
20	Mar-16	102	39	15	39	11.71	40.12	3.09	41.35
20	Apr-16	101	42	23	27	11.60	43.20	4.74	28.63
22	Jan-14	248	225	17	6	24.13	36.26	4.85	28.79
22	Feb-14	260	244	14	2	25.30	39.32	4.00	9.60
22	Mar-14	289	264	19	6	28.12	42.54	5.42	28.79
22	Apr-14	287	276	9	1	27.93	44.47	2.57	4.80
22	May-14	375	339	29	6	36.49	54.63	8.28	28.79
22	Jun-14	318	293	20	5	30.95	47.21	5.71	23.99
22	Jul-14	344	325	16	3	33.48	52.37	4.57	14.40
22	Aug-14	252	225	26	1	24.52	36.26	7.42	4.80
22	Sep-14	264	249	14	1	25.69	40.12	4.00	4.80
22	Oct-14	309	290	16	1	30.07	46.73	4.57	4.80
22	Nov-14	216	199	13	3	21.02	32.07	3.71	14.40
22	Dec-14	187	172	12	2	18.20	27.72	3.43	9.60
22	Jan-15	276	254	12	8	26.86	40.93	3.43	38.39
22	Feb-15	217	209	5	1	21.12	33.68	1.43	4.80
22	Mar-15	327	311	13	2	31.82	50.11	3.71	9.60
22	Apr-15	246	226	15	3	23.94	36.42	4.28	14.40
22	May-15	258	233	22	2	25.11	37.54	6.28	9.60
22	Jun-15	237	224	10	3	23.06	36.09	2.85	14.40
22	Jul-15	219	196	16	5	21.31	31.58	4.57	23.99
22	Aug-15	271	244	18	7	26.37	39.32	5.14	33.59
22	Sep-15	246	227	15	3	23.94	36.58	4.28	14.40
22	Oct-15	260	234	21	5	25.30	37.71	5.99	23.99
22	Nov-15	202	179	21	2	19.66	28.84	5.99	9.60

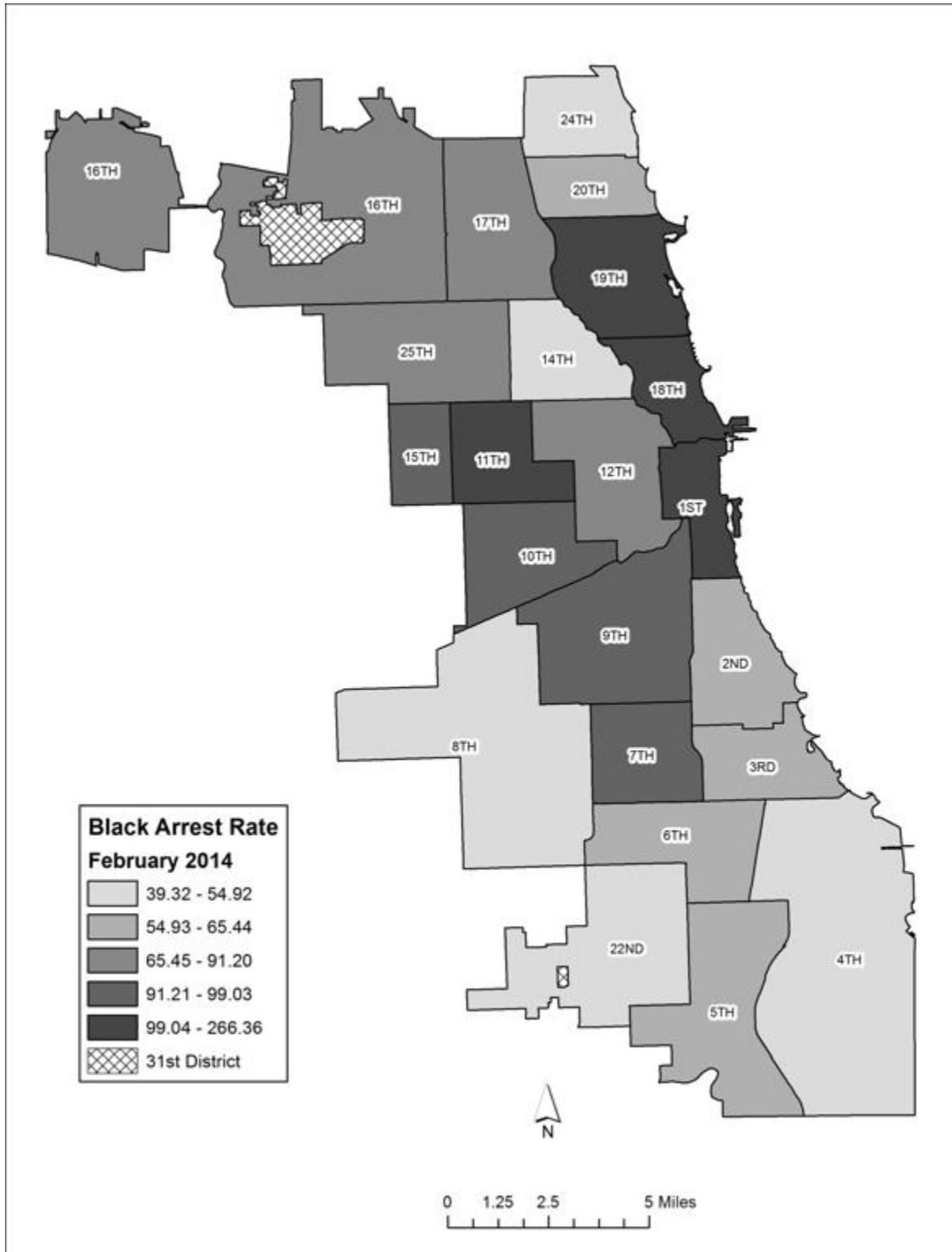
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22	Feb-16	201	181	18	2	19.56	29.17	5.14	9.60
22	Mar-16	239	219	16	4	23.26	35.29	4.57	19.19
22	Apr-16	220	206	11	3	21.41	33.19	3.14	14.40
24	Jan-14	160	83	22	50	11.32	33.78	3.53	24.91
24	Feb-14	185	111	27	42	13.08	45.17	4.34	20.92
24	Mar-14	229	148	31	47	16.20	60.23	4.98	23.41
24	Apr-14	204	137	26	36	14.43	55.76	4.17	17.93
24	May-14	235	135	38	53	16.62	54.94	6.10	26.40
24	Jun-14	221	124	35	48	15.63	50.47	5.62	23.91
24	Jul-14	241	132	43	54	17.04	53.72	6.90	26.90
24	Aug-14	241	126	39	61	17.04	51.28	6.26	30.39
24	Sep-14	226	138	36	43	15.98	56.16	5.78	21.42
24	Oct-14	159	98	22	34	11.24	39.88	3.53	16.94
24	Nov-14	190	97	41	45	13.44	39.48	6.58	22.42
24	Dec-14	157	74	36	39	11.10	30.12	5.78	19.43
24	Jan-15	198	104	26	57	14.00	42.33	4.17	28.39
24	Feb-15	161	100	19	29	11.39	40.70	3.05	14.45
24	Mar-15	222	120	44	49	15.70	48.84	7.06	24.41
24	Apr-15	166	81	35	44	11.74	32.97	5.62	21.92
24	May-15	241	160	45	31	17.04	65.12	7.23	15.44
24	Jun-15	200	104	32	53	14.14	42.33	5.14	26.40
24	Jul-15	239	131	41	48	16.90	53.31	6.58	23.91
24	Aug-15	232	128	38	55	16.41	52.09	6.10	27.40
24	Sep-15	212	133	33	37	14.99	54.13	5.30	18.43
24	Oct-15	208	129	36	32	14.71	52.50	5.78	15.94
24	Nov-15	187	99	36	44	13.22	40.29	5.78	21.92
24	Dec-15	137	82	22	27	9.69	33.37	3.53	13.45
24	Jan-16	164	90	26	36	11.60	36.63	4.17	17.93
24	Feb-16	173	105	22	38	12.23	42.73	3.53	18.93
24	Mar-16	163	95	23	32	11.53	38.66	3.69	15.94
24	Apr-16	168	101	28	31	11.88	41.11	4.50	15.44
25	Jan-14	667	279	62	314	33.31	85.15	21.96	54.31
25	Feb-14	622	264	68	278	31.06	80.57	24.08	48.09
25	Mar-14	792	340	76	362	39.55	103.77	26.92	62.62
25	Apr-14	669	227	63	368	33.41	69.28	22.31	63.65
25	May-14	792	318	59	394	39.55	97.06	20.90	68.15
25	Jun-14	717	252	65	389	35.81	76.91	23.02	67.29
25	Jul-14	847	329	81	416	42.30	100.41	28.69	71.96
25	Aug-14	765	309	77	358	38.20	94.31	27.27	61.92
25	Sep-14	759	274	63	402	37.90	83.63	22.31	69.54
25	Oct-14	784	322	75	376	39.15	98.28	26.56	65.04

25	Nov-14	731	287	73	355	36.51	87.59	25.85	61.41
25	Dec-14	653	247	71	320	32.61	75.39	25.15	55.35
25	Jan-15	815	314	62	428	40.70	95.83	21.96	74.03
25	Feb-15	706	292	56	349	35.26	89.12	19.83	60.37
25	Mar-15	846	306	74	448	42.25	93.39	26.21	77.49
25	Apr-15	702	283	59	344	35.06	86.37	20.90	59.50
25	May-15	739	266	69	387	36.91	81.18	24.44	66.94
25	Jun-15	679	229	58	377	33.91	69.89	20.54	65.21
25	Jul-15	803	348	65	378	40.10	106.21	23.02	65.38
25	Aug-15	693	309	58	310	34.61	94.31	20.54	53.62
25	Sep-15	660	210	66	370	32.96	64.09	23.37	64.00
25	Oct-15	648	289	60	285	32.36	88.20	21.25	49.30
25	Nov-15	532	188	38	295	26.57	57.38	13.46	51.03
25	Dec-15	440	136	41	251	21.97	41.51	14.52	43.42
25	Jan-16	480	169	47	250	23.97	51.58	16.65	43.24
25	Feb-16	492	165	44	273	24.57	50.36	15.58	47.22
25	Mar-16	486	162	46	270	24.27	49.44	16.29	46.70
25	Apr-16	493	175	51	256	24.62	53.41	18.06	44.28

APPENDIX B: Non-Hispanic Black All Arrests Rate, January 2014

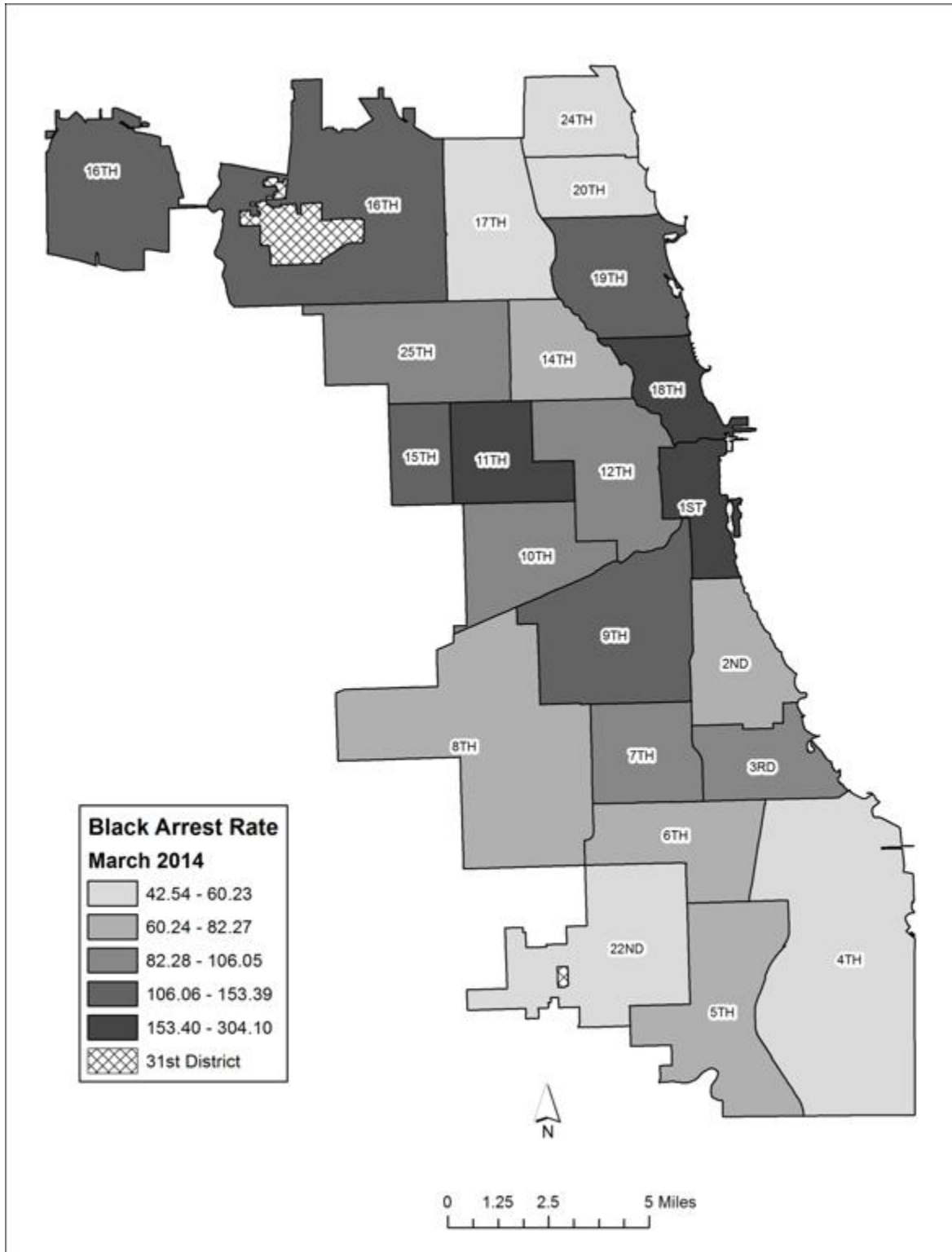


APPENDIX C: Non-Hispanic Black All Arrests Rate, February 2014

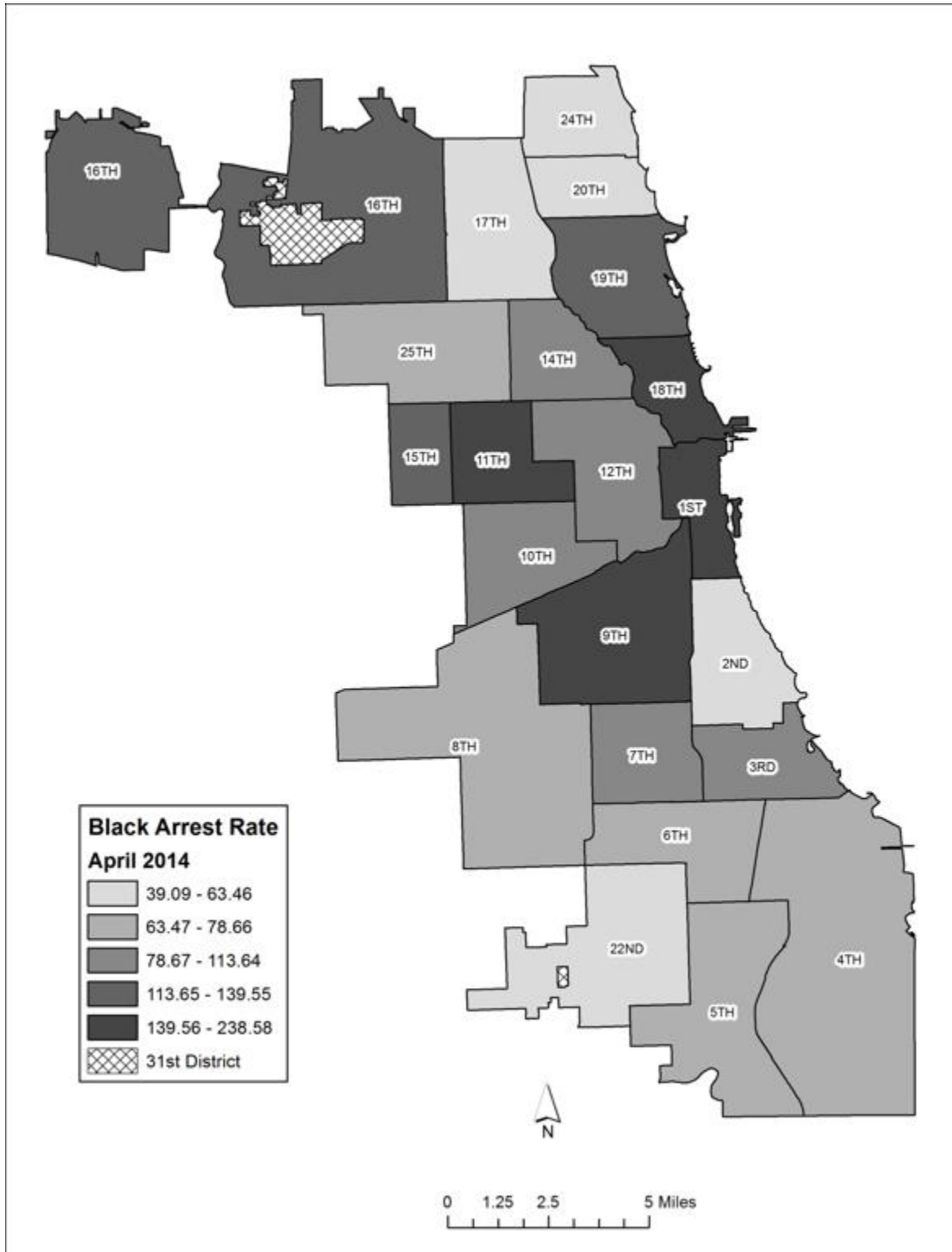




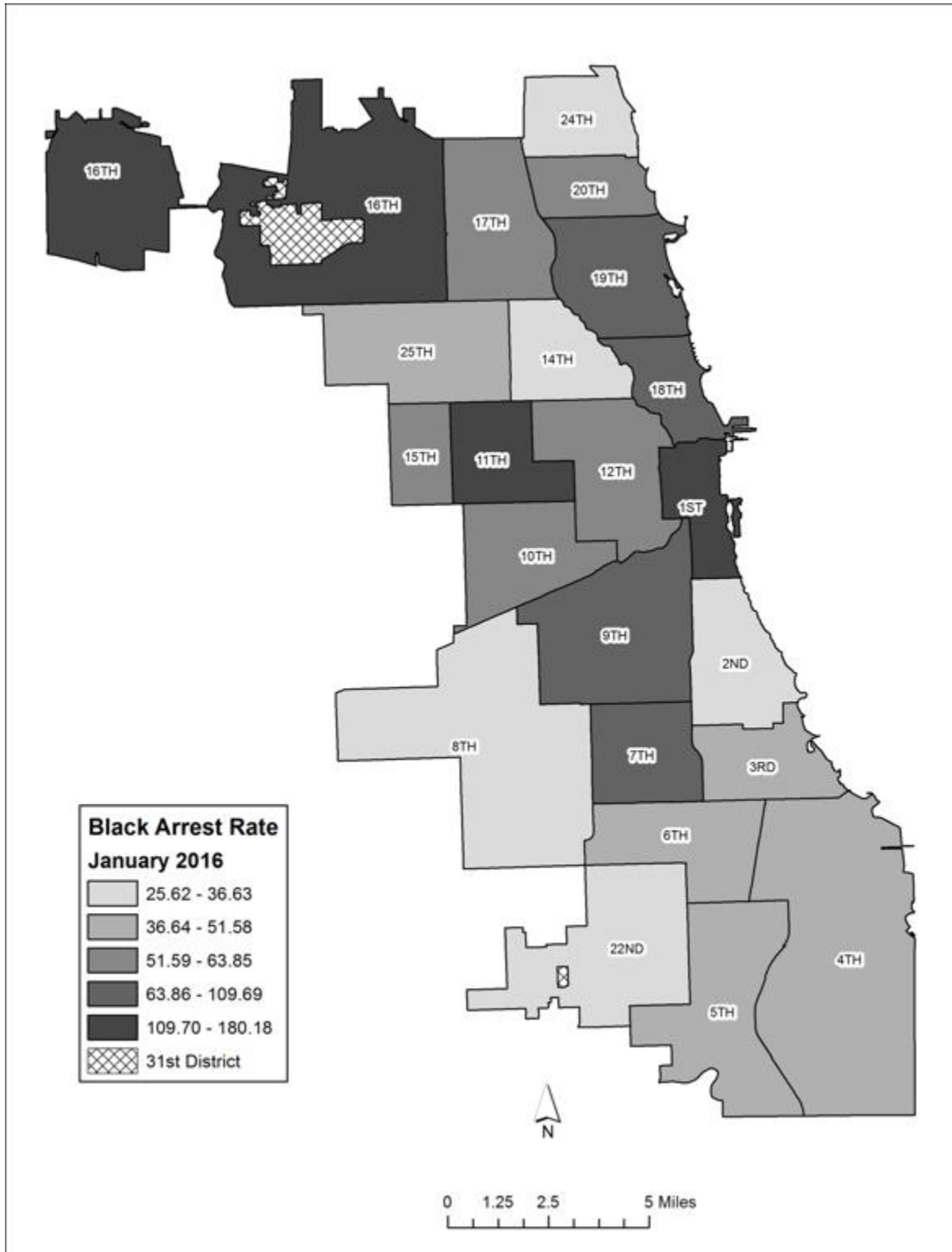
APPENDIX D: Non-Hispanic Black All Arrests Rate, March 2014



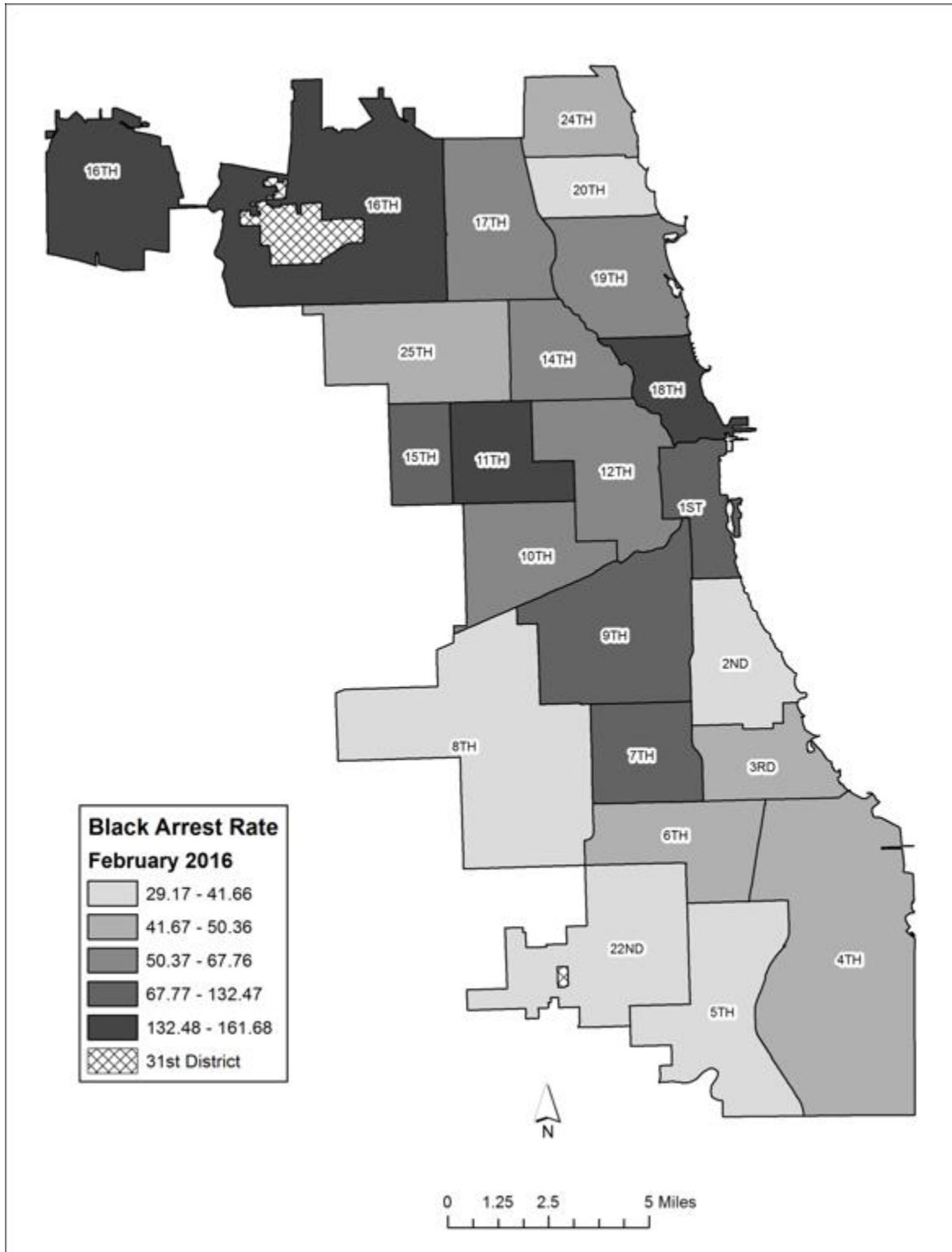
APPENDIX E: Non-Hispanic Black All Arrests Rate, April 2014



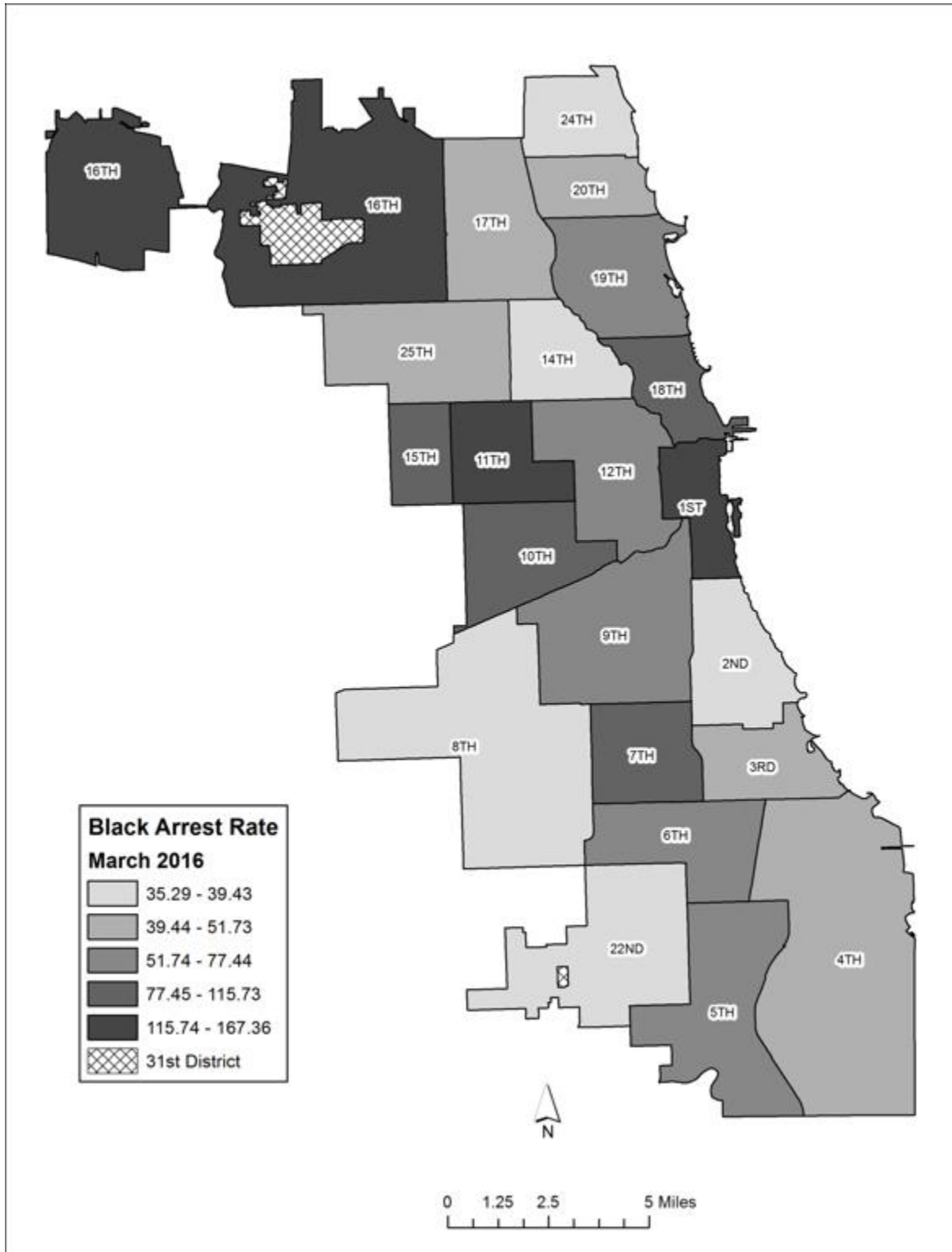
APPENDIX F: Non-Hispanic Black All Arrests Rate, January 2016



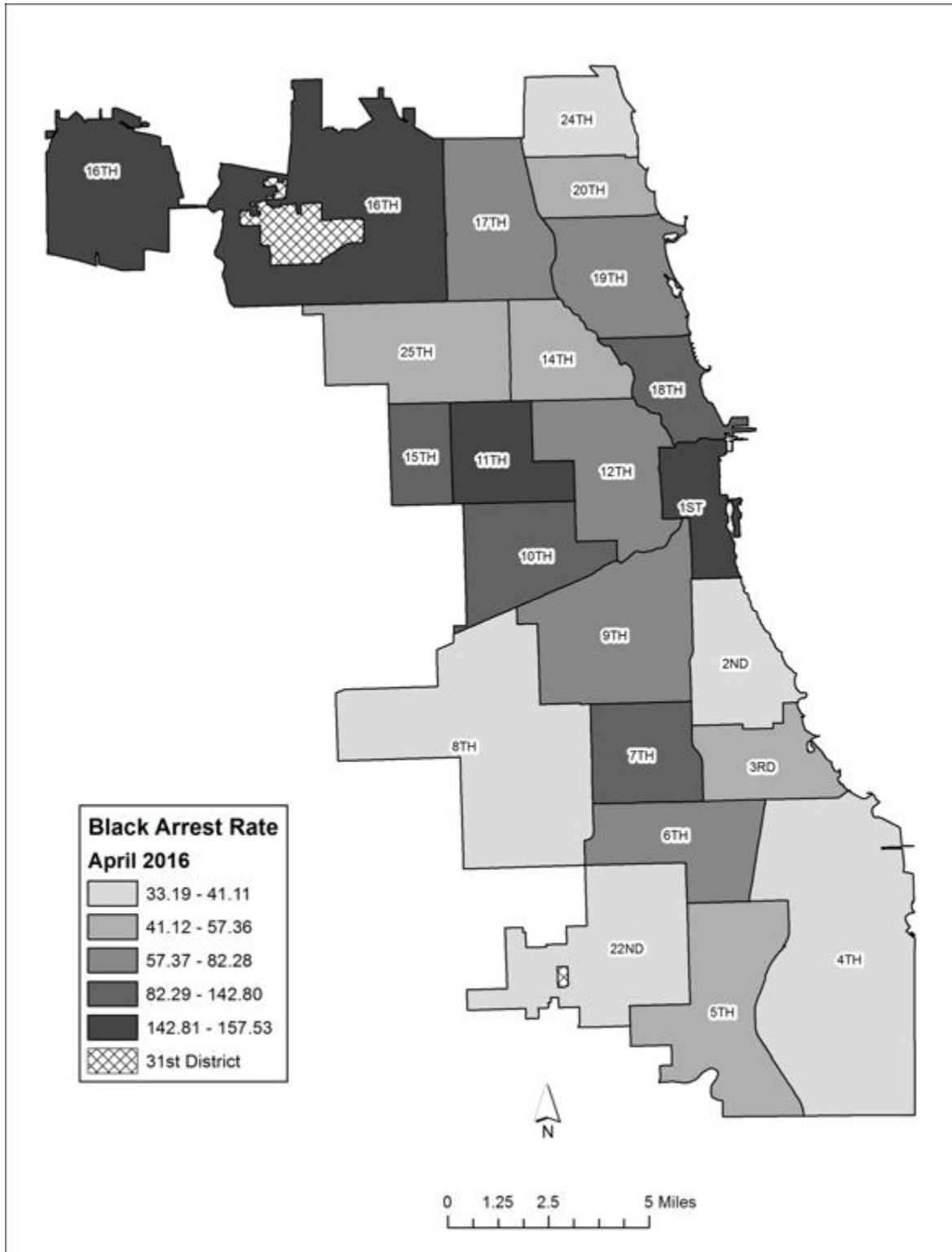
APPENDIX G: Non-Hispanic Black All Arrests Rate, February 2016



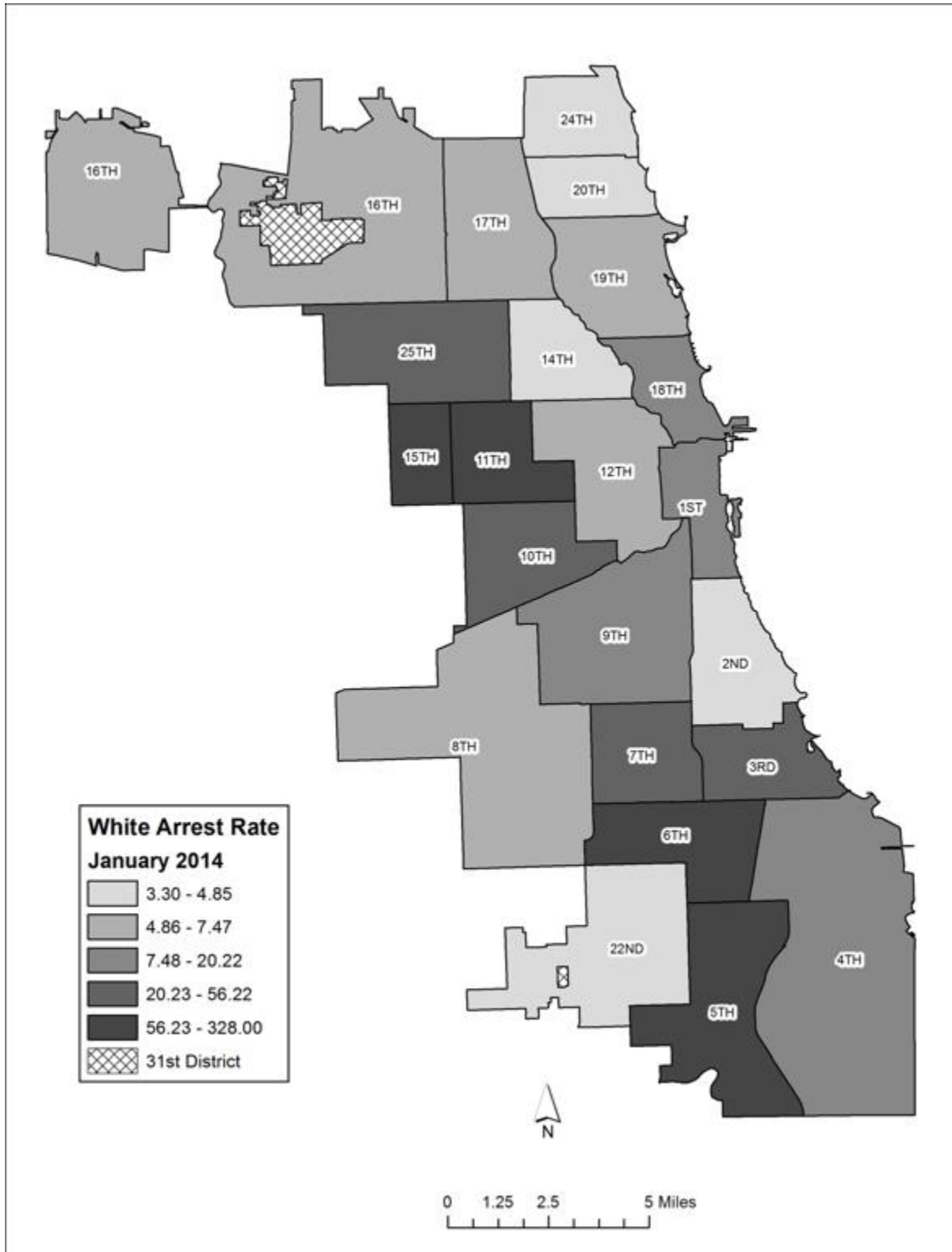
APPENDIX H: Non-Hispanic Black All Arrests Rate, March 2016



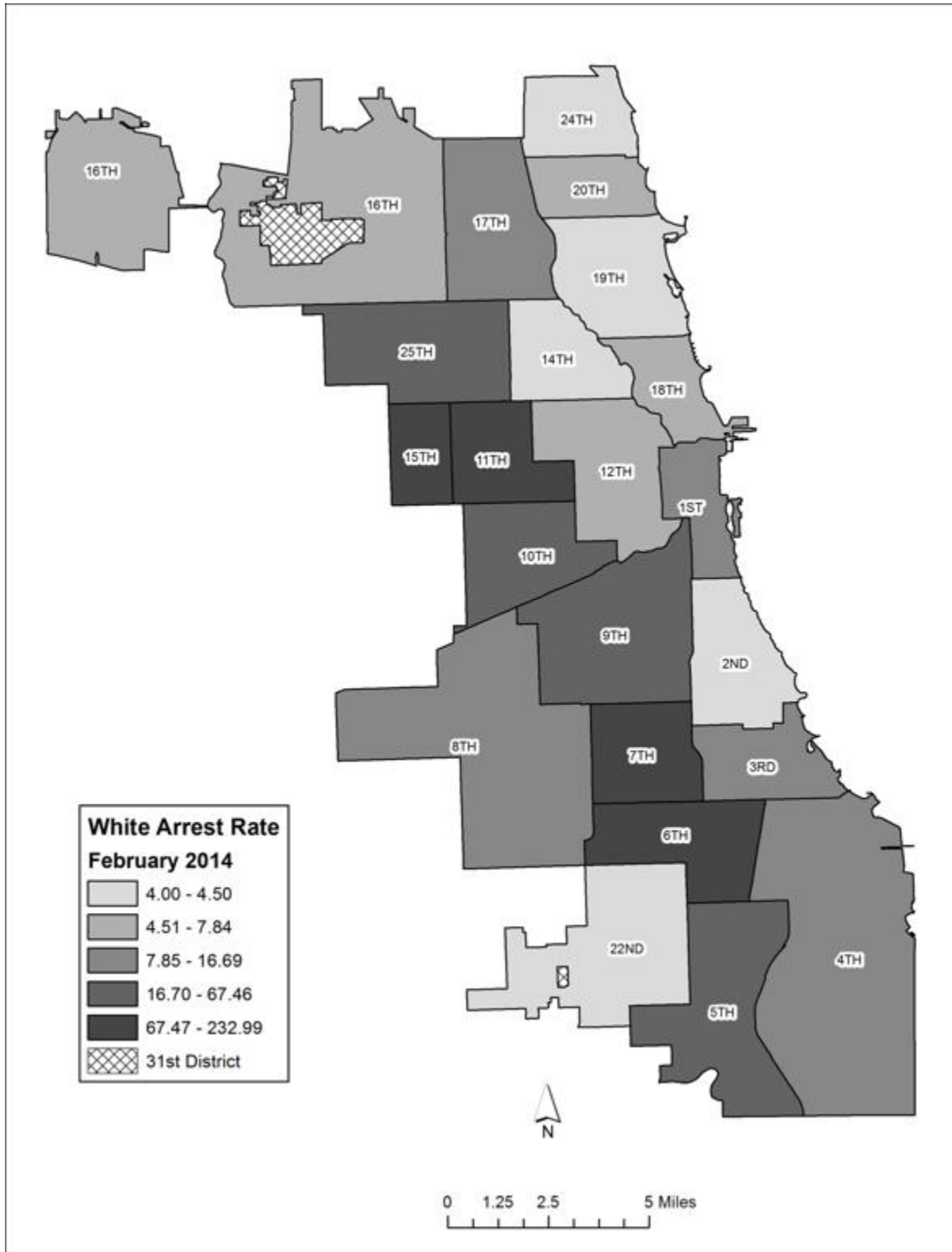
APPENDIX I: Non-Hispanic Black All Arrests Rate, April 2016



APPENDIX J: Non-Hispanic White All Arrests Rate, January 2014

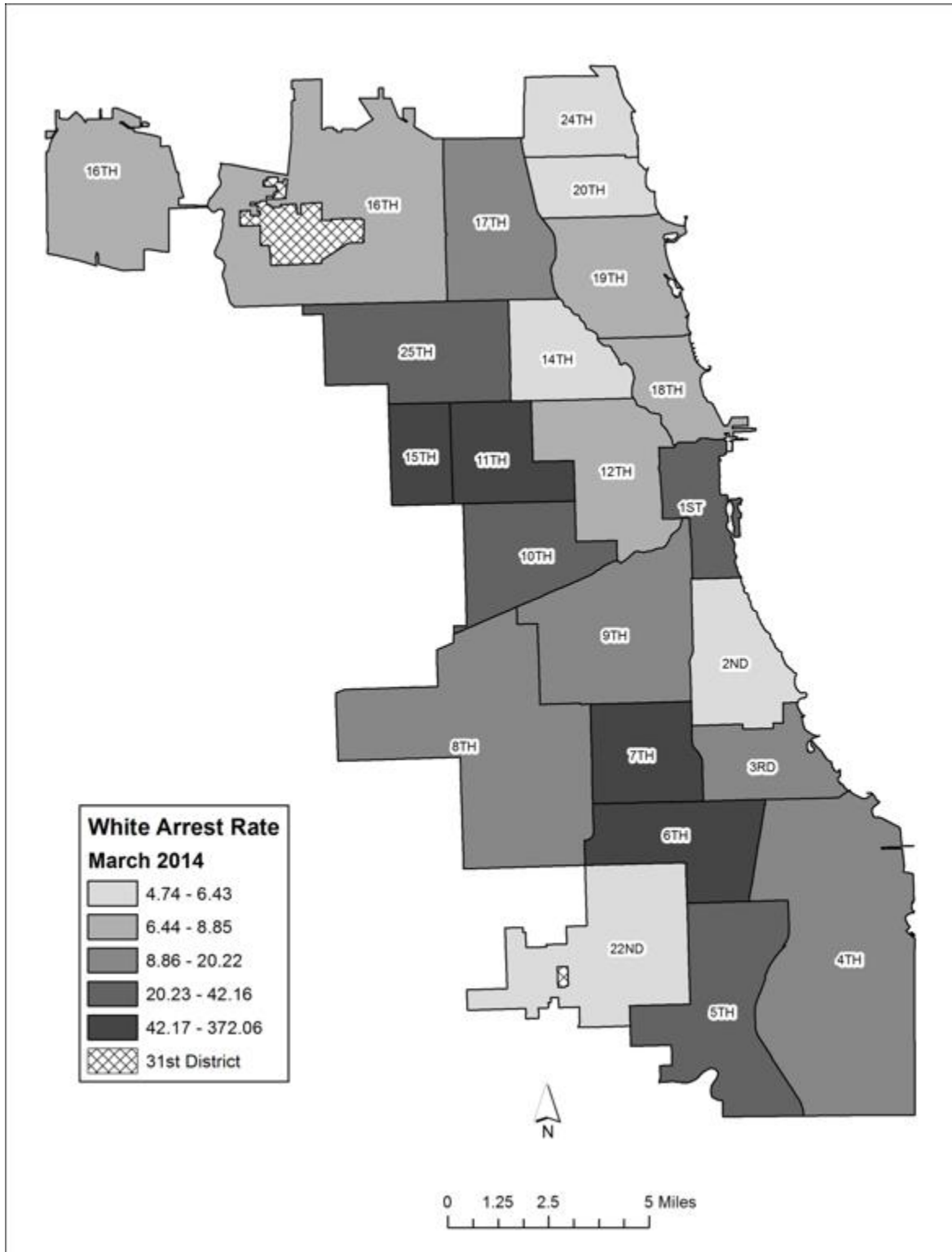


APPENDIX K: Non-Hispanic White All Arrests Rate, February 2014

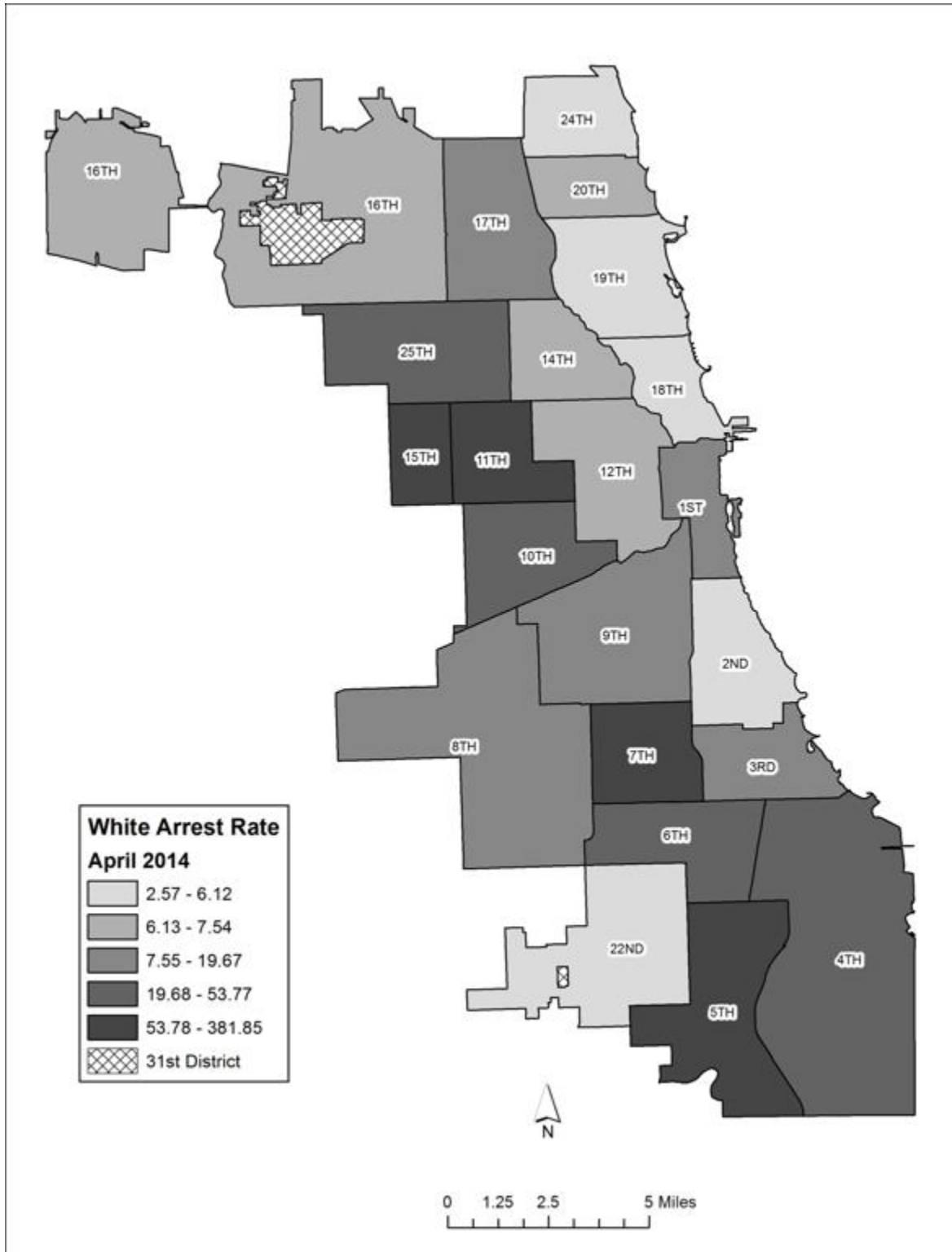




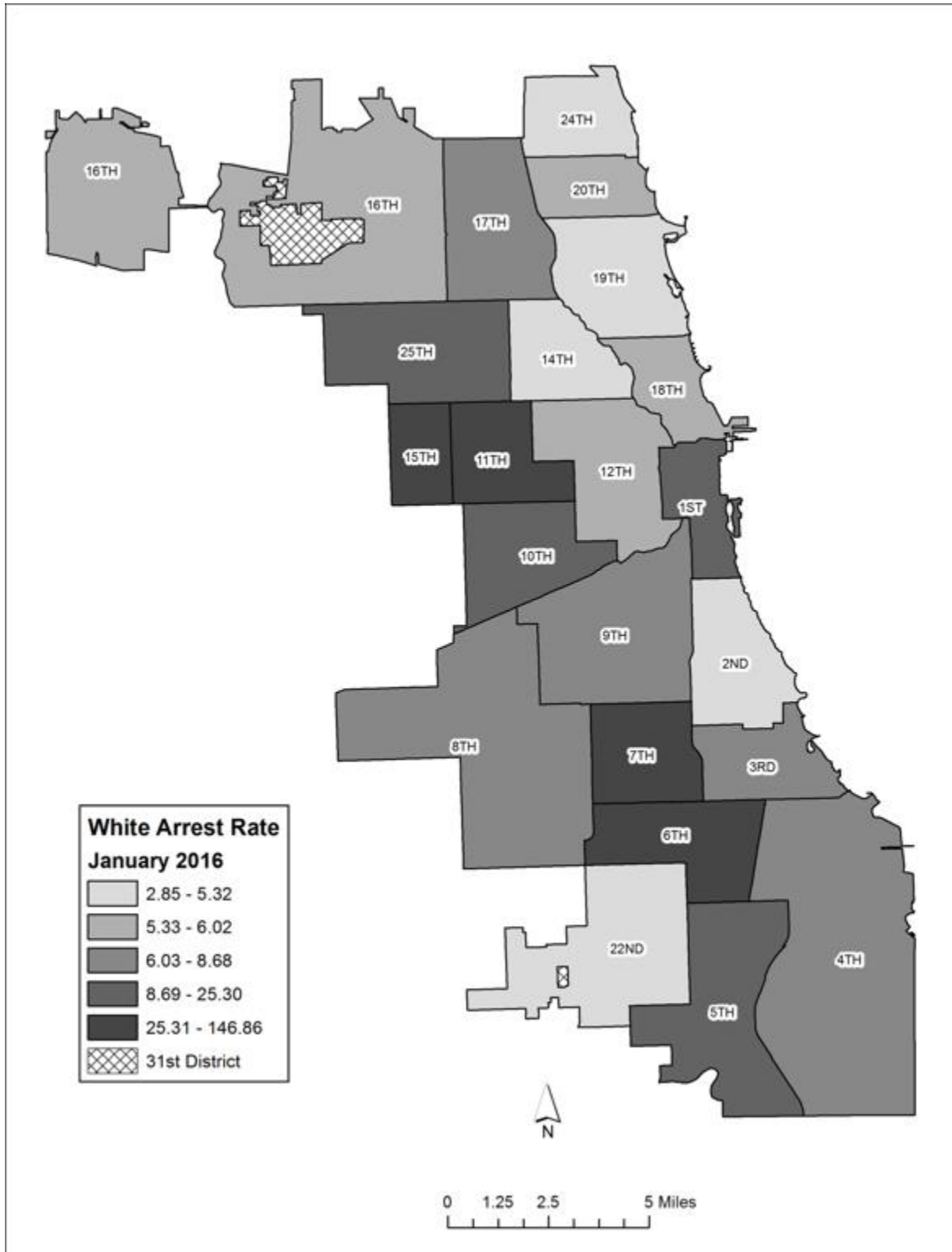
APPENDIX L: Non-Hispanic White All Arrests Rate, March 2014



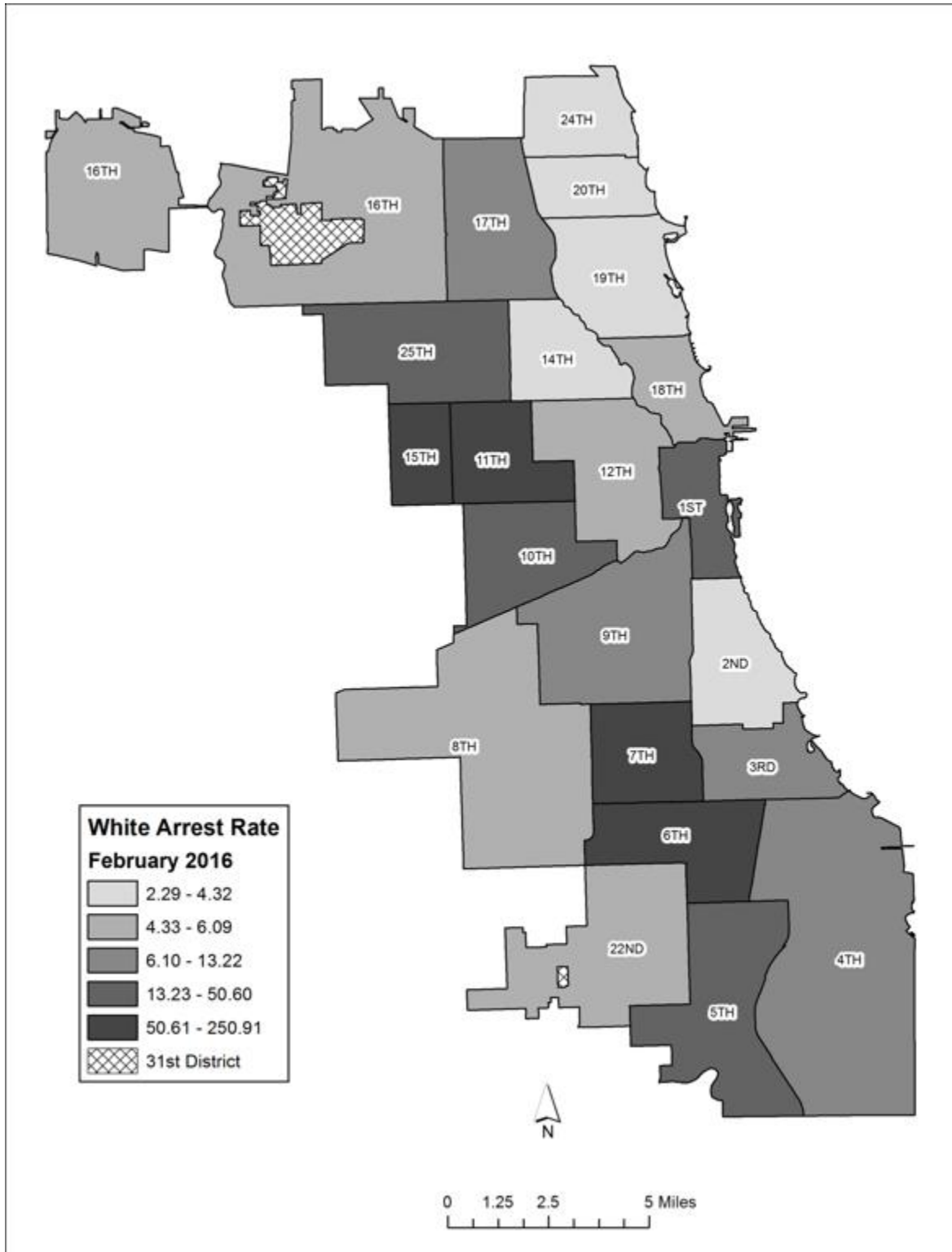
APPENDIX M: Non-Hispanic White All Arrests Rate, April 2014



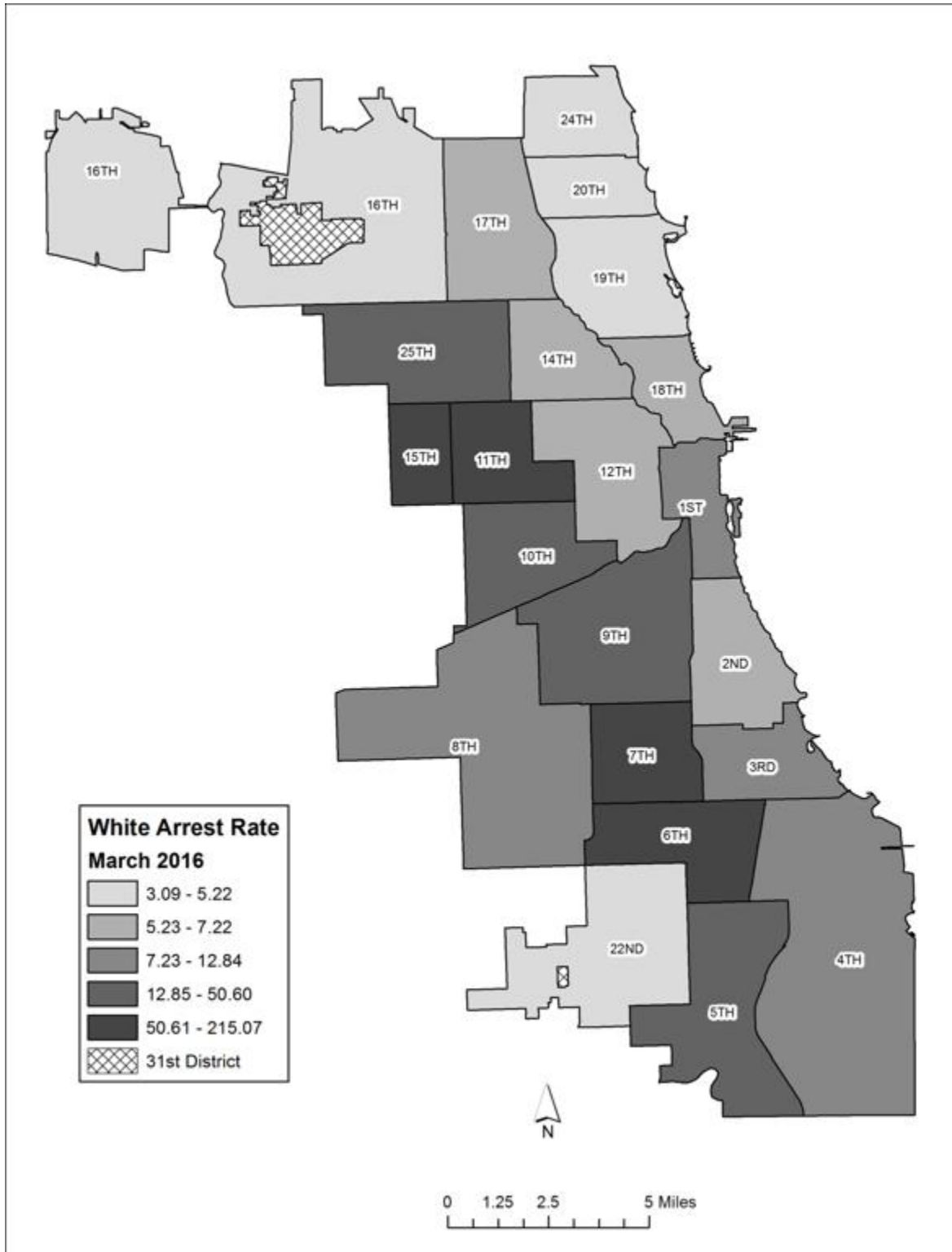
APPENDIX N: Non-Hispanic White All Arrests Rate, January 2016



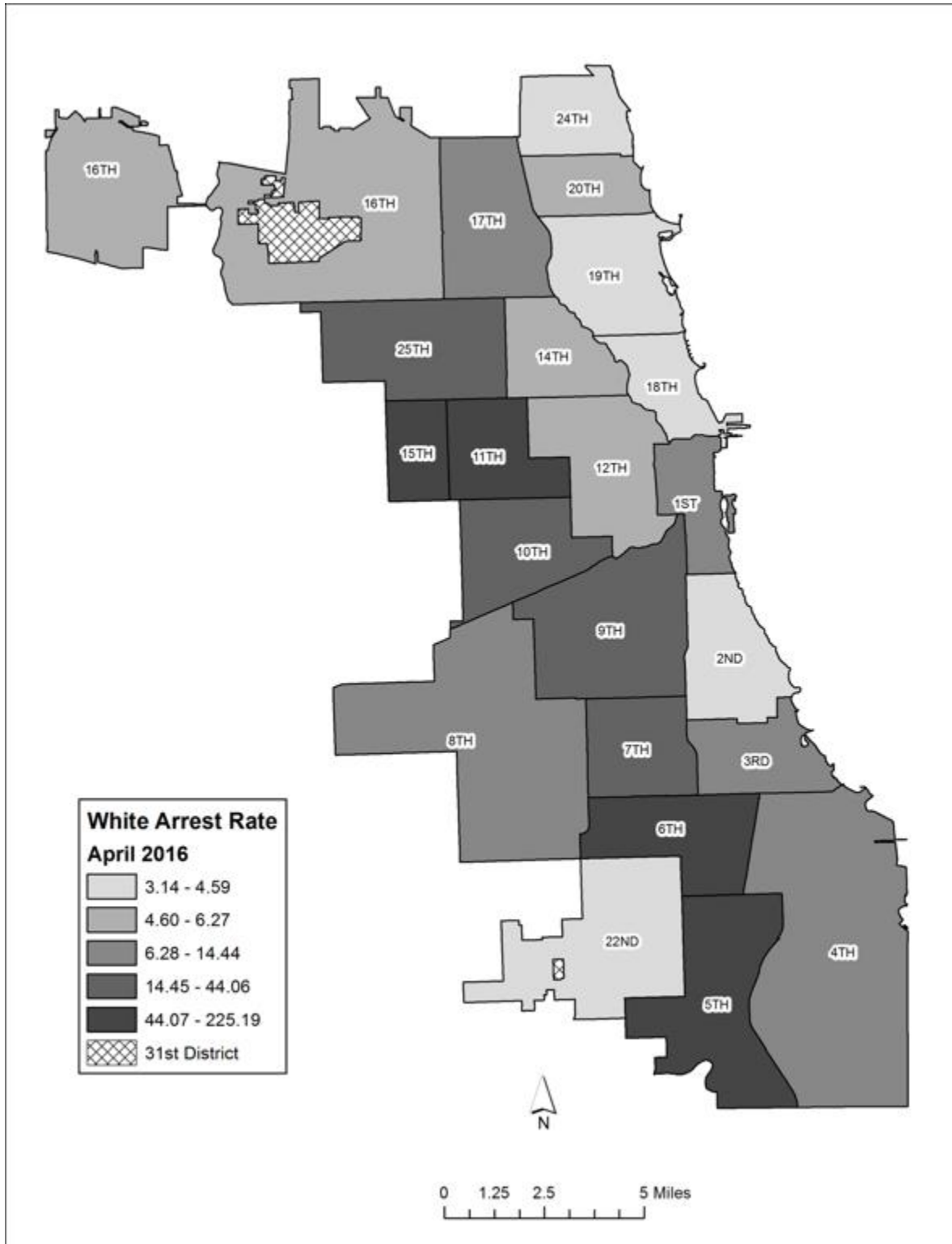
APPENDIX O: Non-Hispanic White All Arrests Rate, February 2016



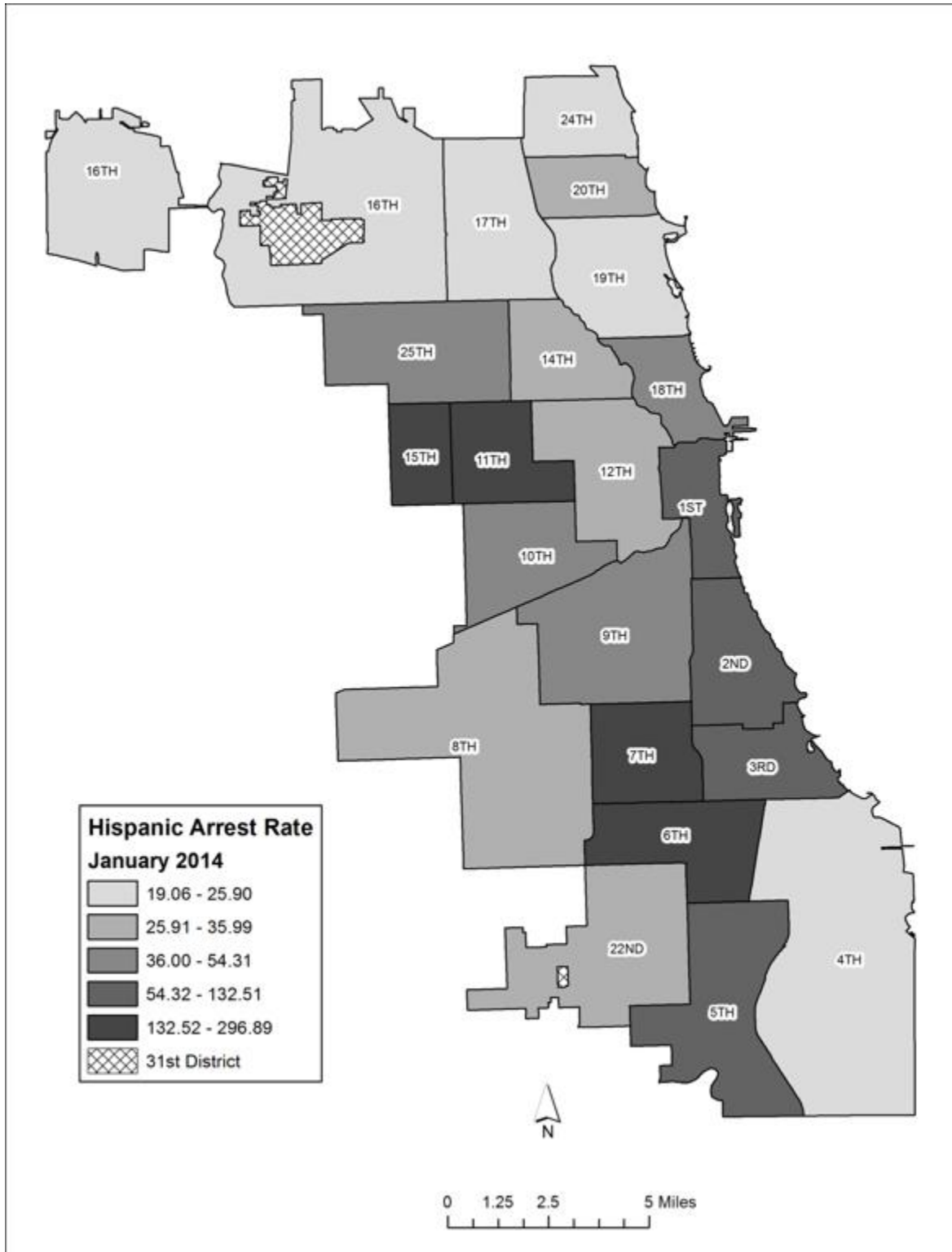
APPENDIX P: Non-Hispanic White All Arrests Rate, March 2016



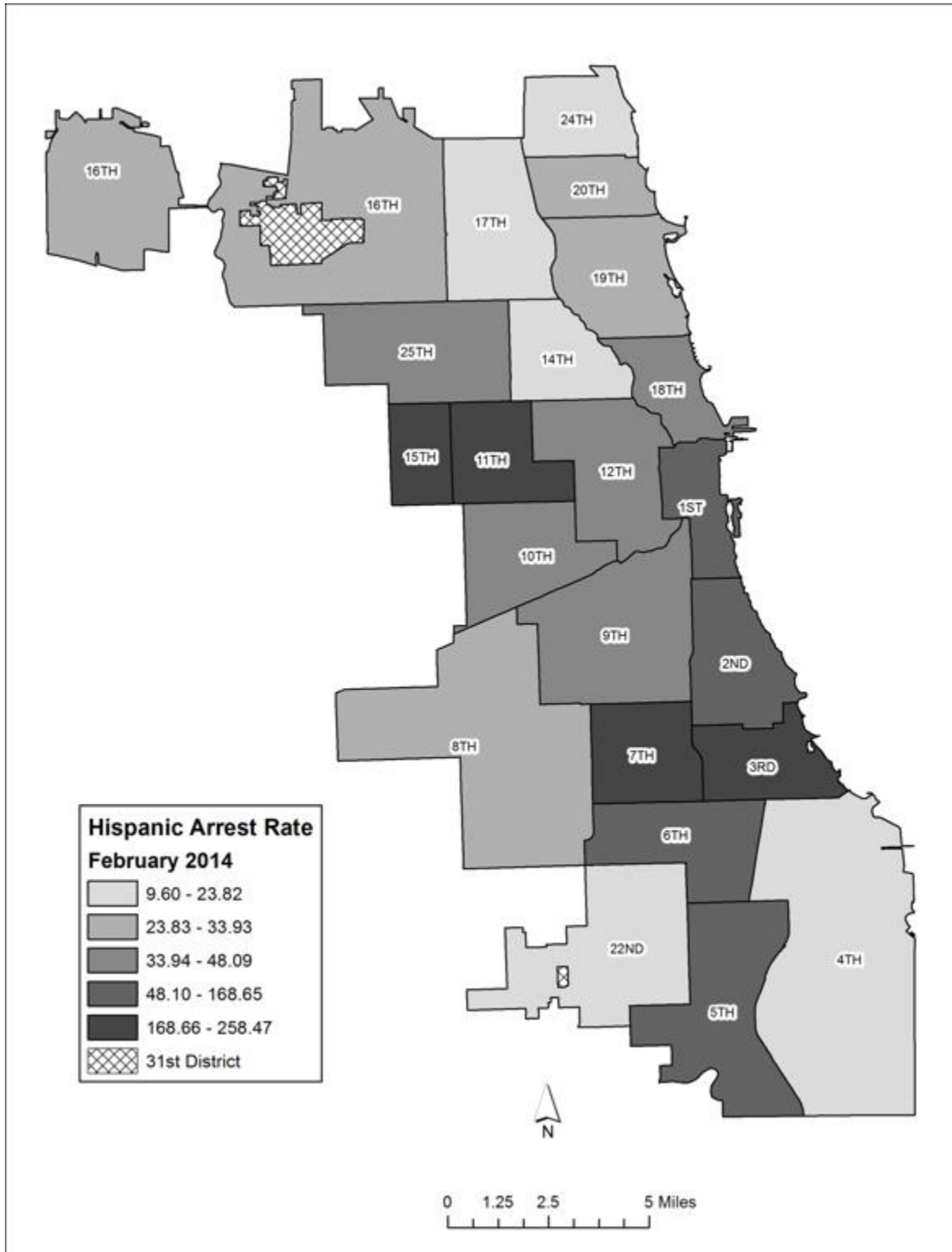
APPENDIX Q: Non-Hispanic White All Arrests Rate, April 2016



APPENDIX R: Hispanic All Arrests Rate, January 2014

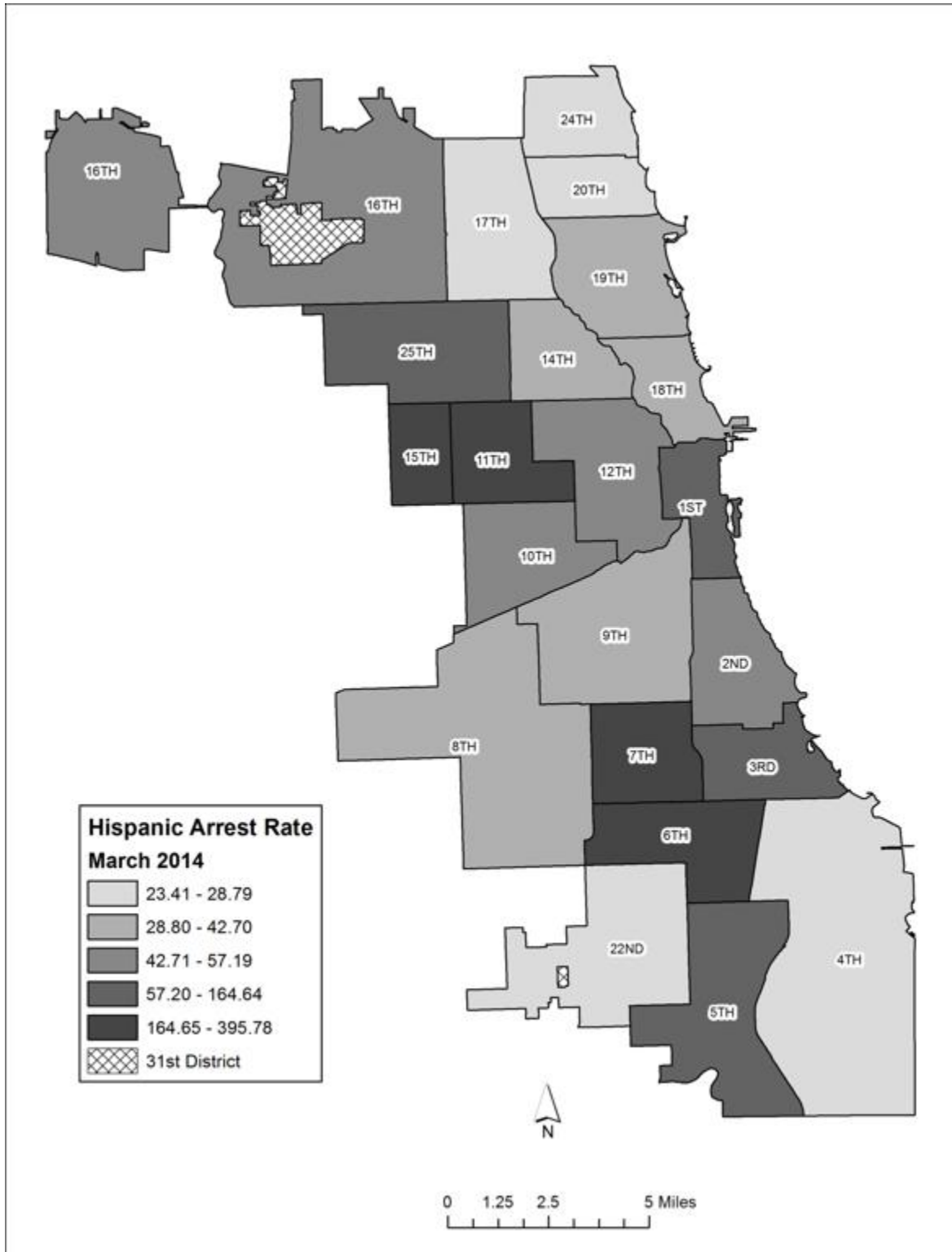


APPENDIX S: Hispanic All Arrests Rate, February 2014

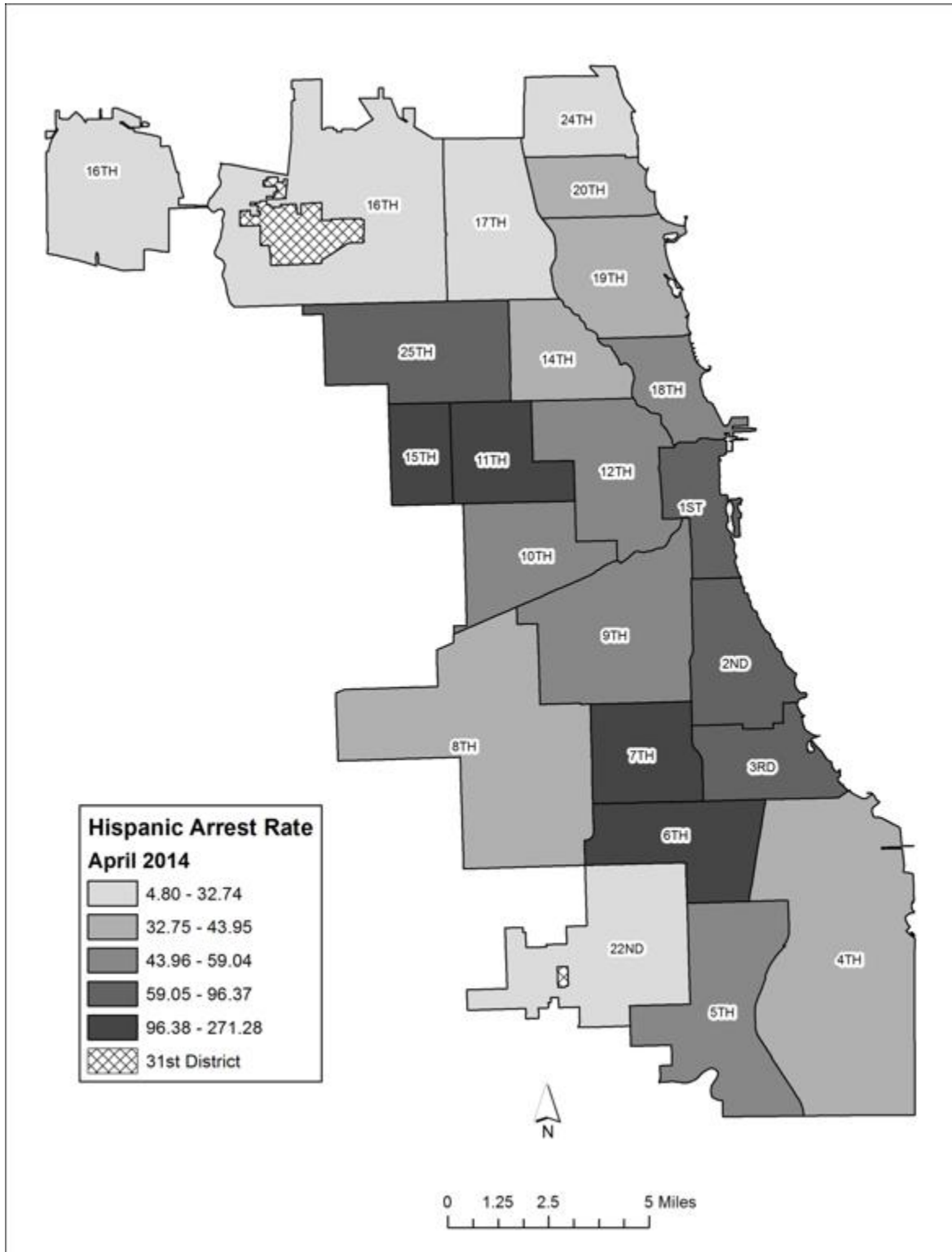




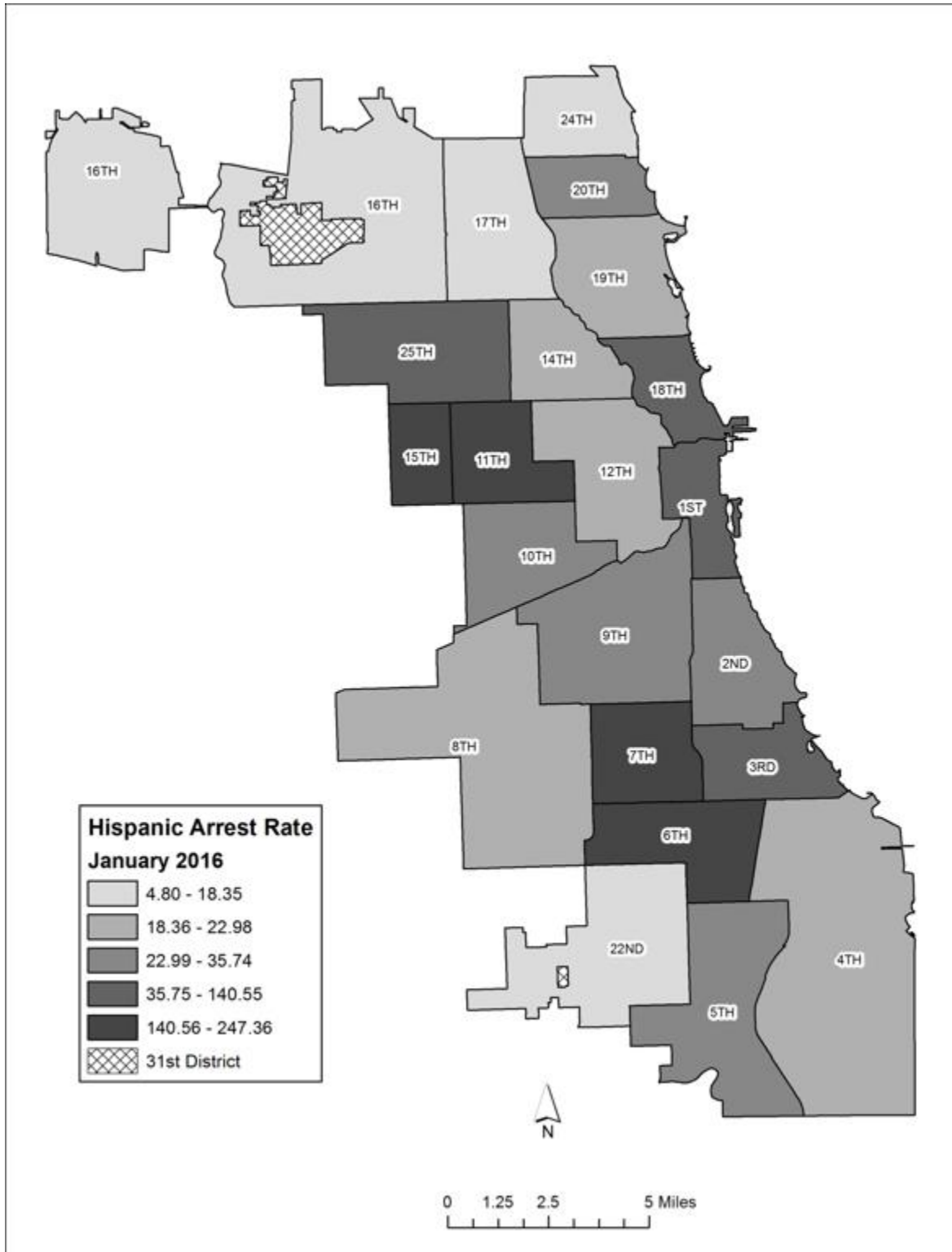
APPENDIX T: Hispanic All Arrests Rate, March 2014



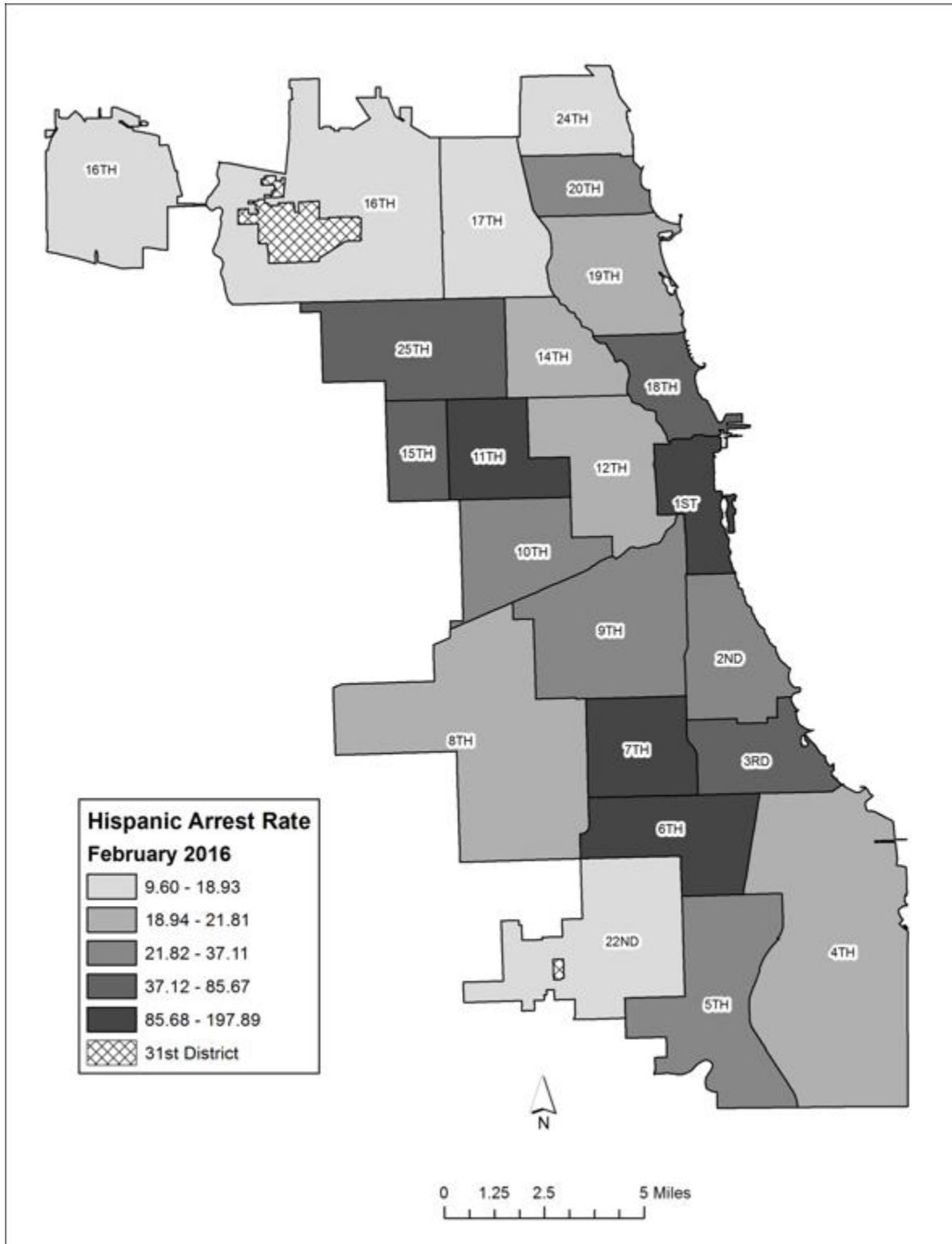
APPENDIX U: Hispanic All Arrests Rate, April 2014



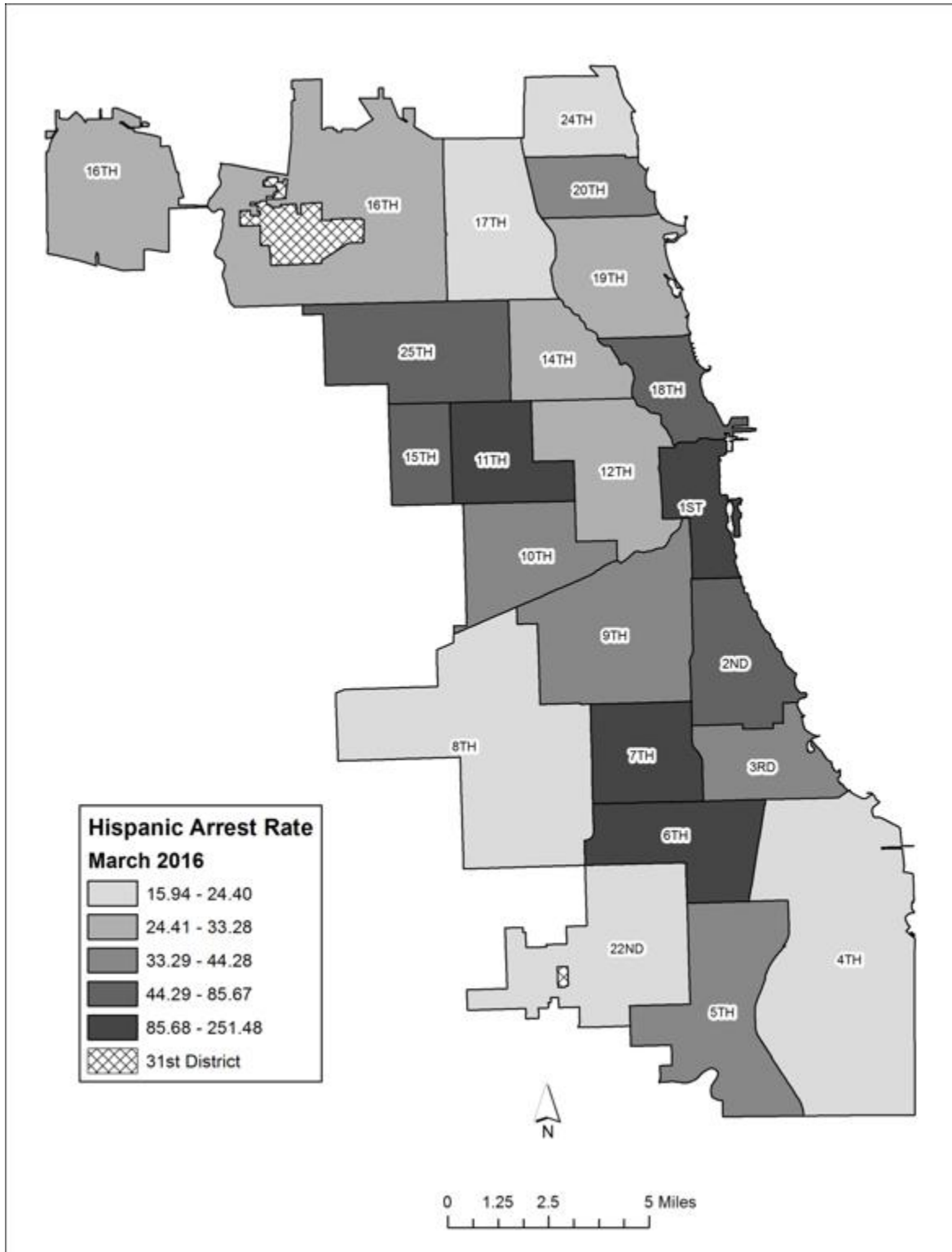
APPENDIX V: Hispanic All Arrests Rate, January 2016



APPENDIX W: Hispanic All Arrests Rate, February 2016



APPENDIX X: Hispanic All Arrests Rate, March 2016



APPENDIX Y: Hispanic All Arrests Rate, April 2016

