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

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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 ethnoracial categories. **Statistical testing** procedures are applied to reveal which ethnoracial 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 ethnoracial-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 ethnoracial categories are of central interest *both* the numerator and denominator need to be *specific* to the ethnoracial 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 ethnoracial 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. [1]

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. [2]

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[1] 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.

[2] 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." [3]

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.

[3] 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. [4] To blindly assume that a community level connection or difference applies to individuals represents a mistake in scientific reasoning.

[4] 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 [5] 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.

[5] 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? [6] 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).[7] 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 leadings 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

[6] 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.

[7] 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. [8] 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 [9] 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. [10]

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[8] 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).

[9] 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.

[10] 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. [11]

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-

[11] 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.[12] 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).

[12] 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.

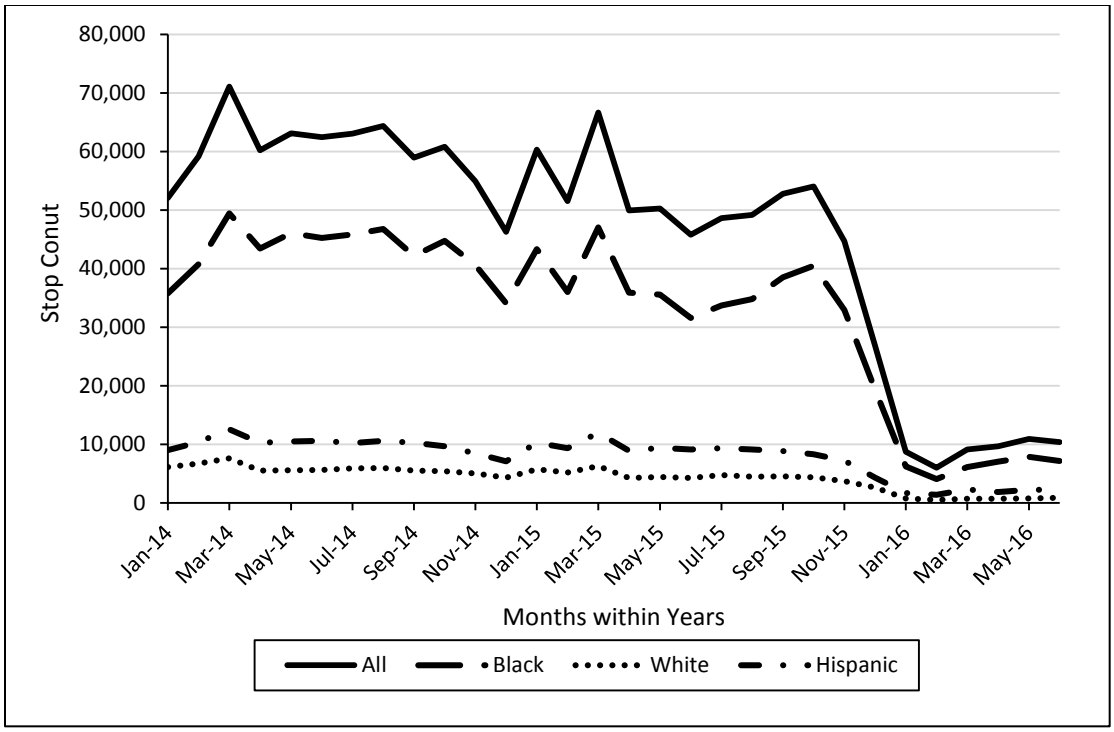


Figure 1: City-Level Stop Counts, Jan 2014 - Jun 2016

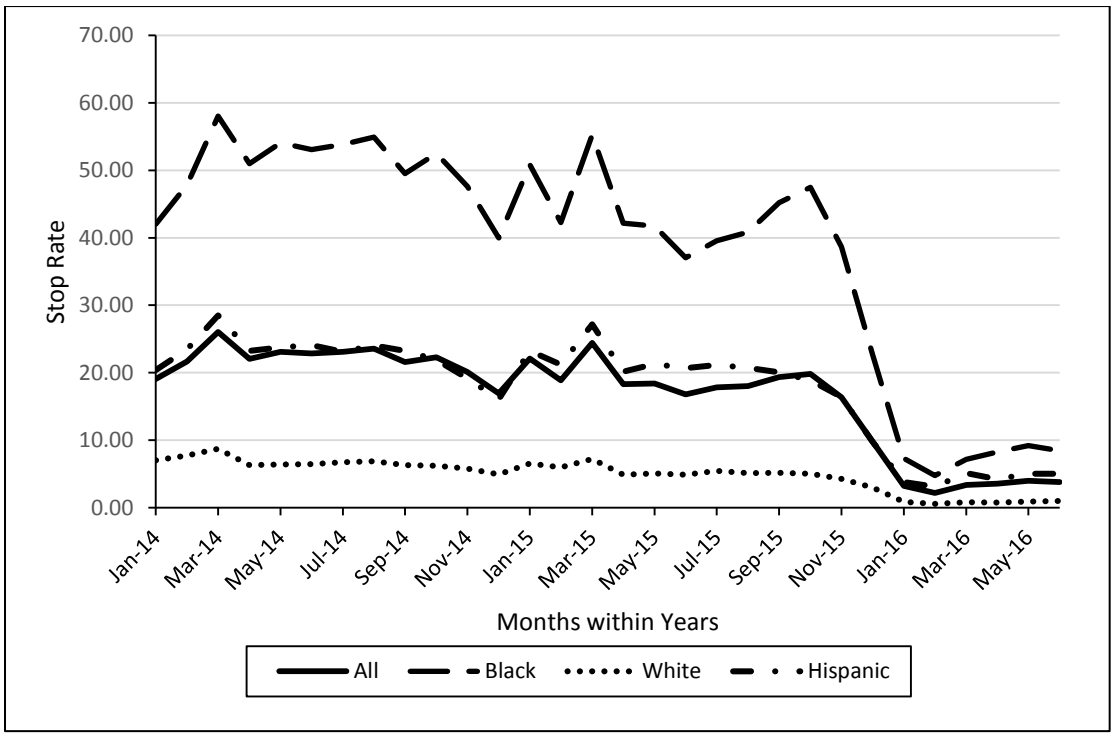


Figure 2: City-Level Stop Rates by 1,000 Population

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.

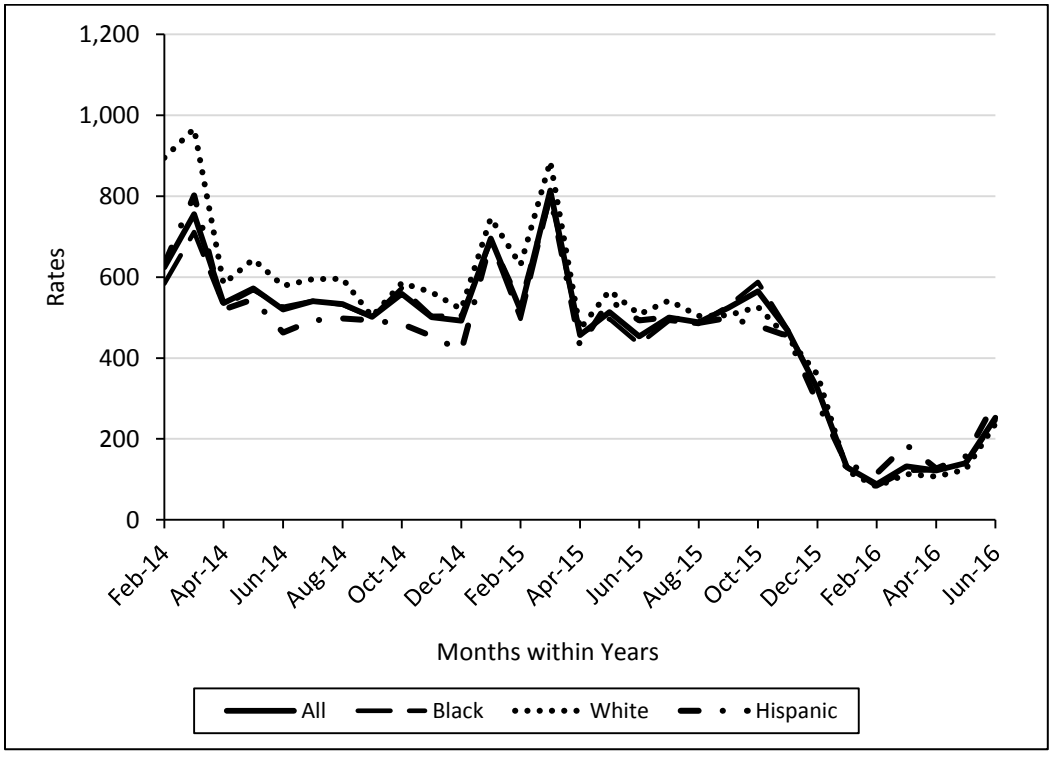


Figure 3: City-Level Stops per 100 Previous Month's Total Arrests

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.

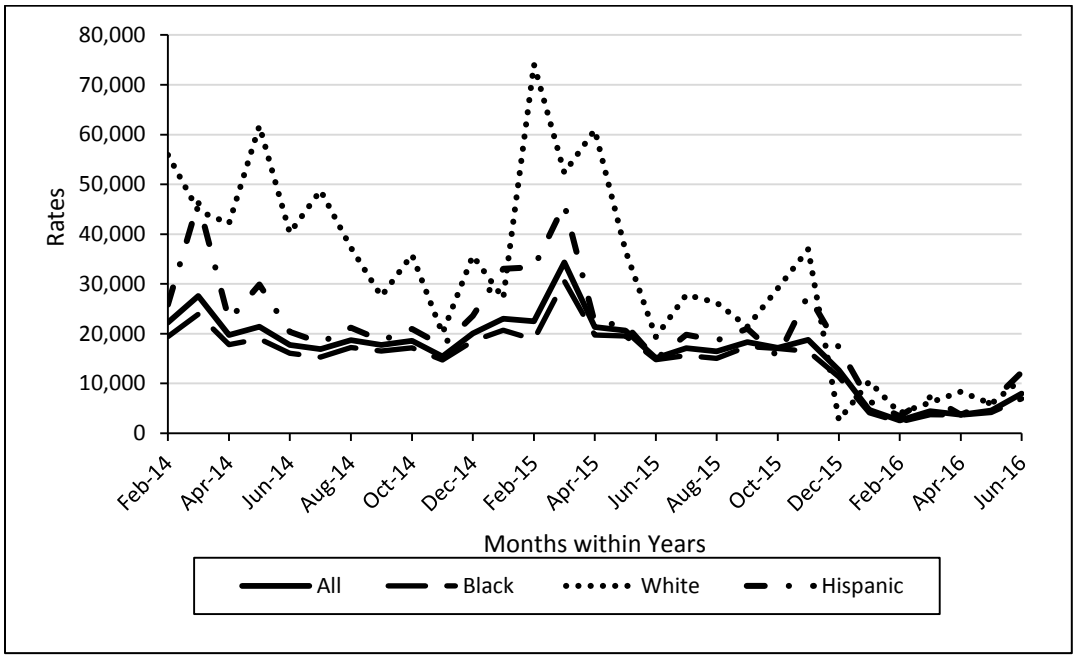


Figure 4: City-Level Stops per 100 Previous Month's Violent Arrests

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. [13] 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. [14] The highest quantile, indicated by the darkest shading on each map, identifies districts with stop rates falling in the highest 20 percent. The 31st 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 16th district, and in the districts located around The Loop and Near North (1st and 18th). 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 9th, 10th, and 11th districts; and, at times the 12th and 15th 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 17th, 25th, and 14th districts collectively score in the lowest quantile for Black stops relative to their population there. On the South Side these include the 6th, 22nd, 5th, and 8th districts from March-April 2016. The 2nd 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 24th, 17th, 19th, and 14th districts. To a lesser extent, the 16th and 25th 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 8th, 4th, 22nd, 5th, 3rd, and 2nd 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 15th, 11th, 7th, and 6th districts. The 9th and 10th 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 22nd 2nd, 19th, 18th, and 14th districts remained within the lowest two quantiles of the distribution. On the other hand,

[13] The denominator, ethnoracial-specific population, includes residents of all ages, not just young residents.

[14] 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 11th and 15th districts in the West Side, and the 6th and 7th districts in the South Side. Spatially situated between these two highest rate subregions are the 8th, 9th, 10th, 12th, and 1st districts with rates clustering above the 20th and below the 80th 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: [15]

- 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.

[15] 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. [16] 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.[17]

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-

[16] Adding in district-level predictors with only 22 districts is potentially problematic from a modeling perspective (Bryan & Jenkins, 2016; Schmidt-Catran & Fairborther, 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.

[17] 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	*	-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 (χ^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. * 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 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 2 Variance	0.189				0.097			
Likelihood Ratio χ^2	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). [18]

Model E introduces yearly and monthly dummy measures in lieu of the temporal linear and curvilinear trends, and demographics.[19] 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

[18] But see fn. 10.

[19] Variance inflation factor (VIF) value of 4.33 suggests some evidence of multicollinearity in Model E.

counts. Yet, a socioeconomic status effect emerges. For every 1-unit increase in the district socioeconomic status measure, predicted stop counts decreased 42 percent.

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 2 Variance	0.173	0.056			0.183	0.058			0.065	0.022		
Likelihood Ratio (X2	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 2 Variance	0.185	0.058			0.065	0.022		
Likelihood Ratio (χ2	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 measure is 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 2 Variance	0.084	0.027			0.088	0.027			0.070	0.022		
Likelihood Ratio (χ^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 2 Variance	0.087	0.027			0.070	0.022		
Likelihood Ratio (χ2	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 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.

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 [20]
B. April 1, 2014 to December 31, 2014[21]
C. January 1, 2015 to December 31, 2015 [22]
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,

[20] March 30 substituted for April 3
[21] April 1 substituted for April 4
[22] 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
Violent Arrests					
Black	+	+	+	+	+
Hispanic	-	-	-	-	+
Stability	-	-	-	-	+
SES	-	-	-	-	-
Percent Black	-	-	-	-	-
Young Population					
Black	+	+	+	+	+
Hispanic	+	+	+	+	+
Stability	-	-	-	-	-
SES	-	-	-	-	-
Percent Black	-	-	-	-	-
Total Arrests					
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.

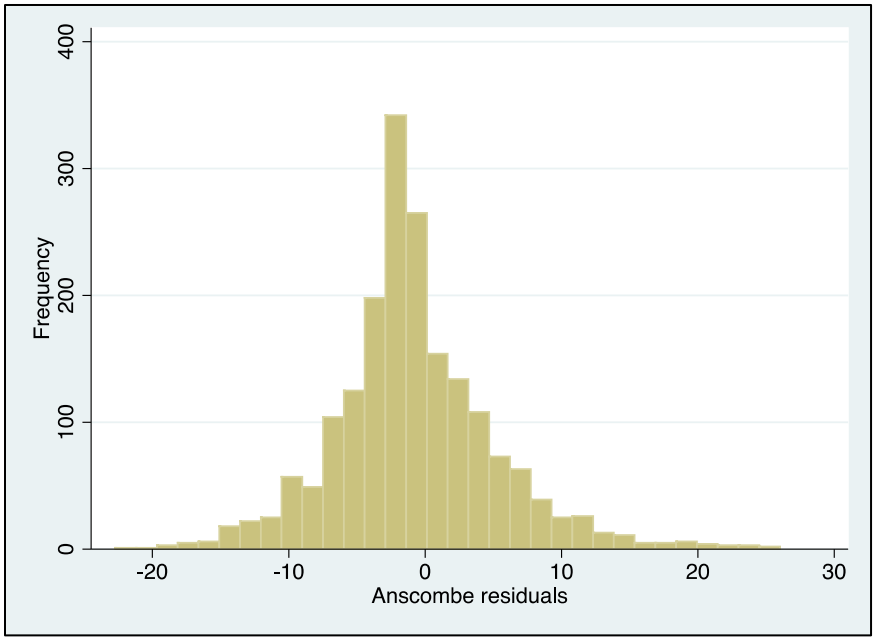


Figure 5: Standardized Residuals: Model E, Violent Arrest Exposure Variable

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.

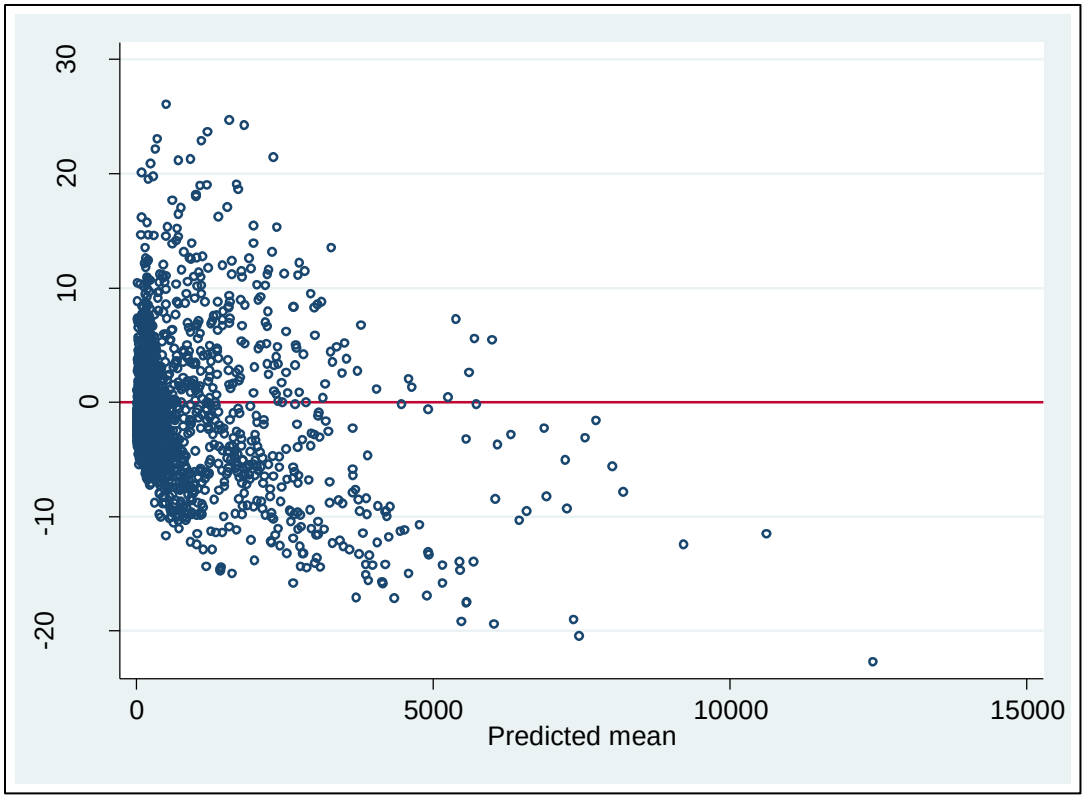


Figure 6: Predicted Stop Counts and Standardized Model E Residuals: Violent Arrest Exposure Variable

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.

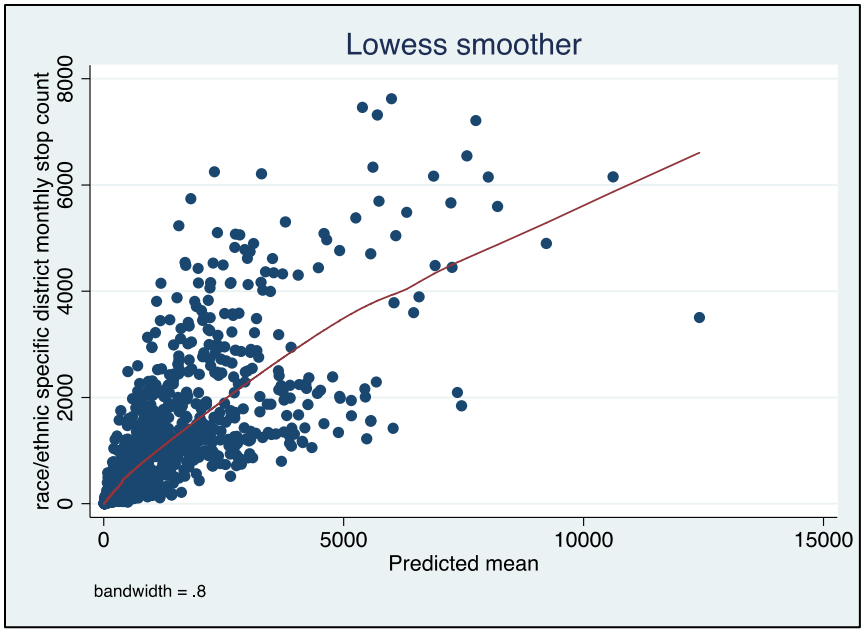


Figure 7: LOWESS Plot of Predicted to Observed Stop Counts

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.

	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 [1]	1,914	30,304.630	13,923.980	14,180.040	66,363.350

Note: [1]Not race-specific. Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, Investigatory Stop Reports, and arrest data.

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

Sources: 2010-2014 American Community Survey; 2014-2016 Chicago Police Department Contact Cards, and Investigatory Stop Reports.

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

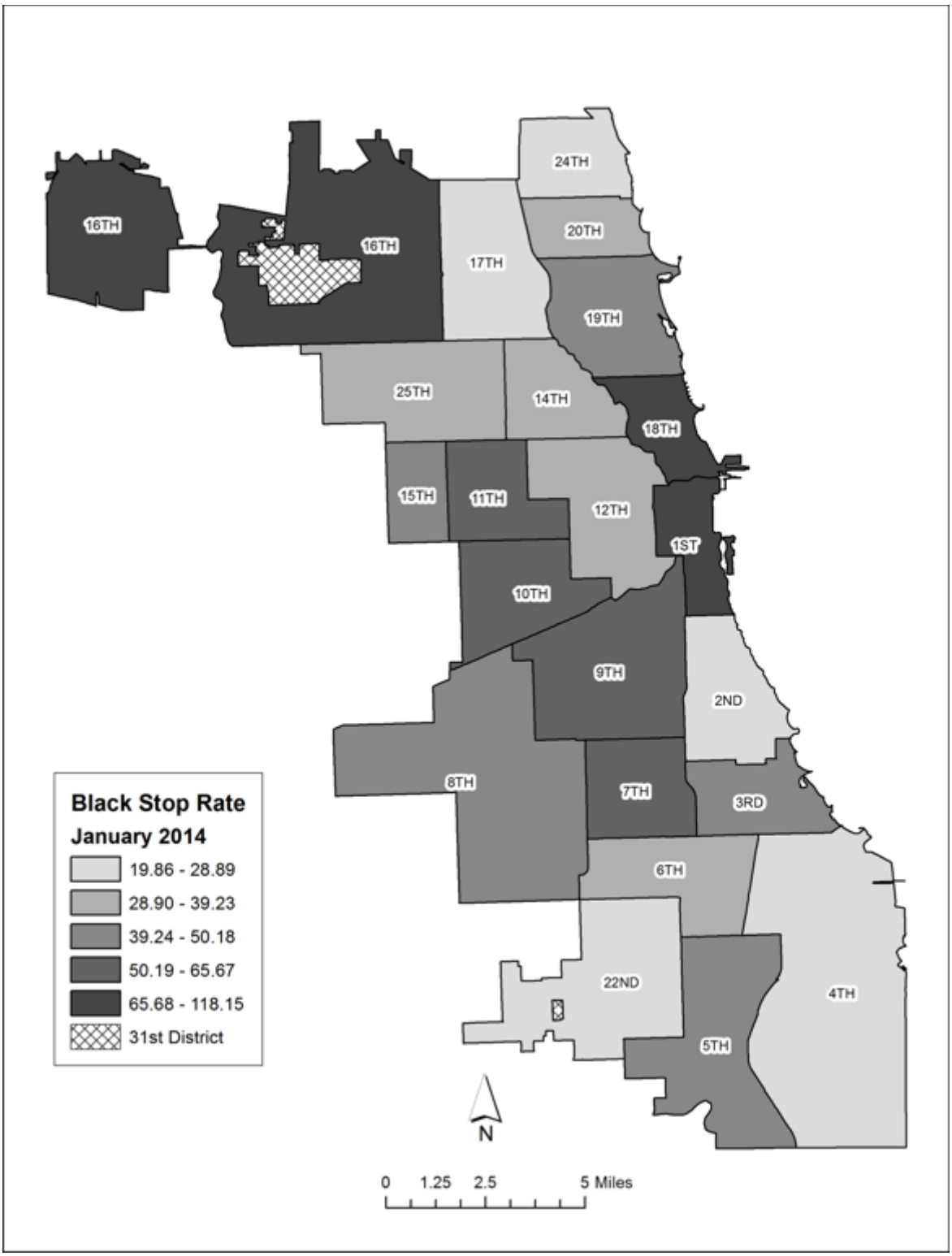
24	Jul-15	20,740.0	22,880.0	38,000.0	20,400.0	1,037.0	1,100.0	1,187.5	769.8
24	Aug-15	11,937.5	8,100.0	0.0	17,850.0	799.2	865.6	729.3	743.8
24	Sep-15	25,587.5	23,680.0	0.0	12,800.0	882.3	925.0	871.1	698.2
24	Oct-15	24,300.0	19,500.0	0.0	29,900.0	917.0	879.7	1,066.7	808.1
24	Nov-15	36,025.0	41,050.0	26,200.0	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.

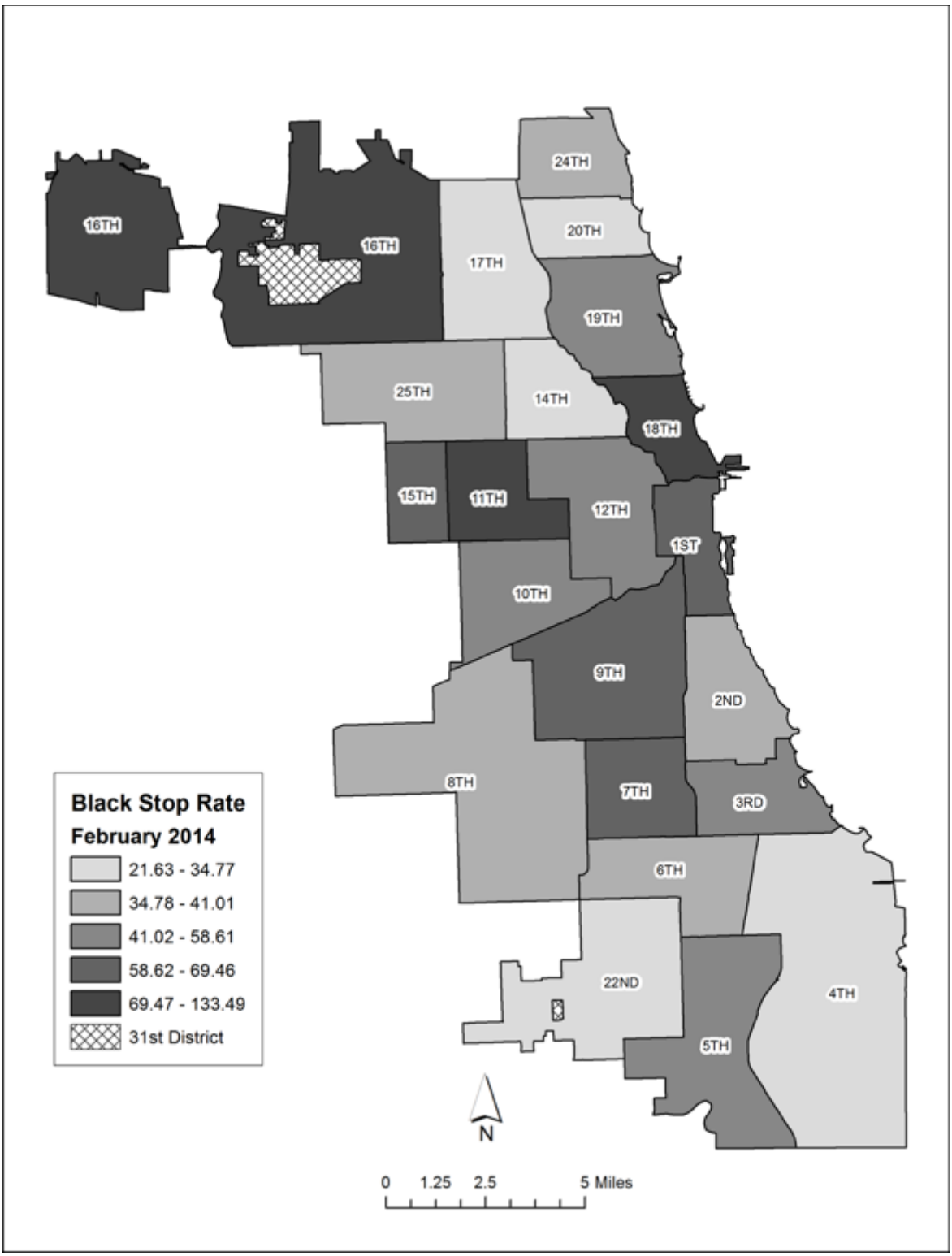
	Violent Arrests				Young Population				Total Arrests			
	b	SE	IRR		b	SE	IRR		b	SE	IRR	
Intercept	4.814	0.084	123.171	***	-3.858	0.175	0.021	***	1.586	0.064	4.882	***
Ln(Exposure)	1.000				1.000				1.000			
Ln(alpha)	-0.305	0.030		***	0.419	0.028		***	-0.945	0.032		***
Level 2 Variance	0.146	0.047			0.657	0.203			0.085	0.027		
Likelihood Ratio χ2	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.

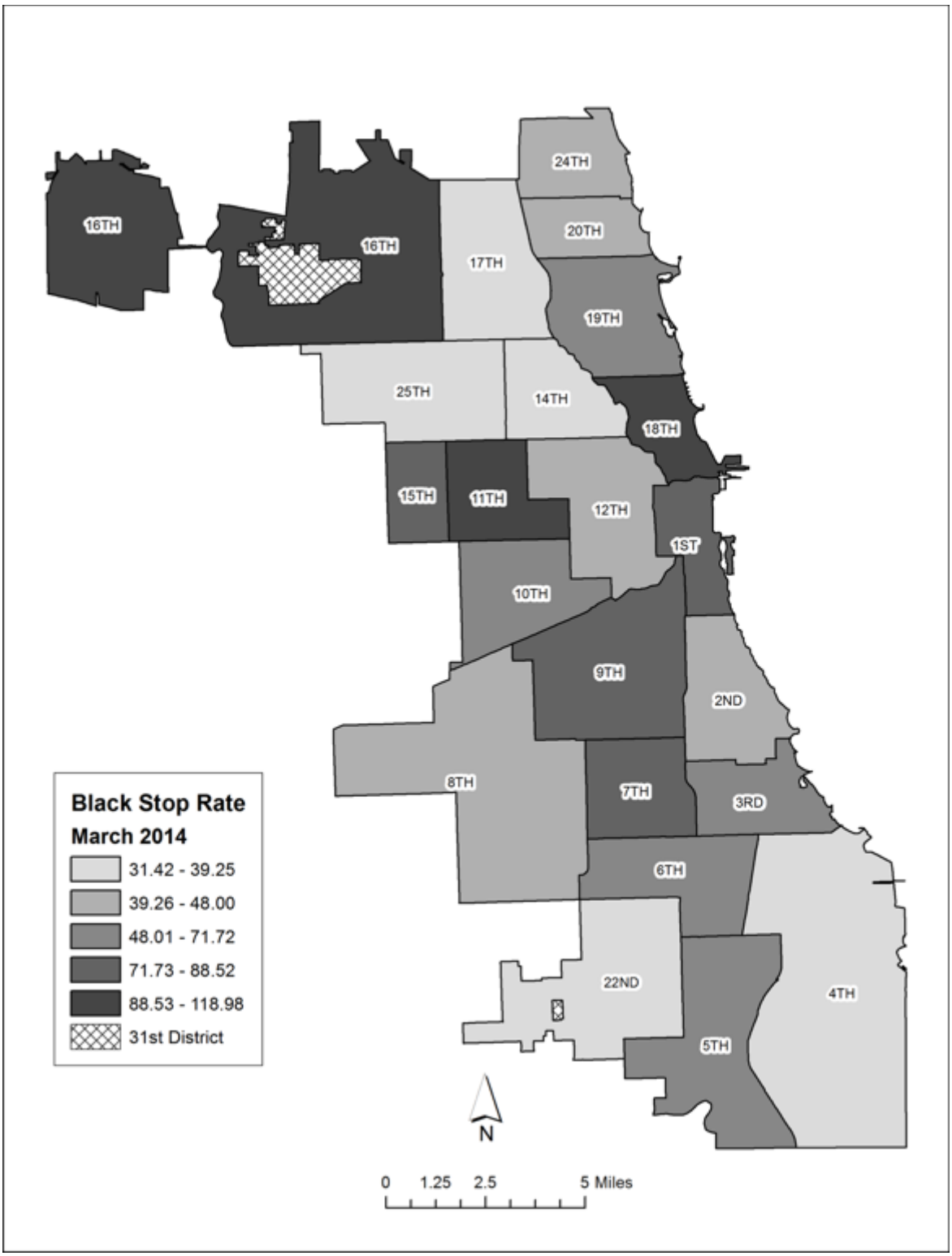
APPENDIX E: Non-Hispanic Black Stop Rate (per 1,000 race/ethnic specific population), January 2014



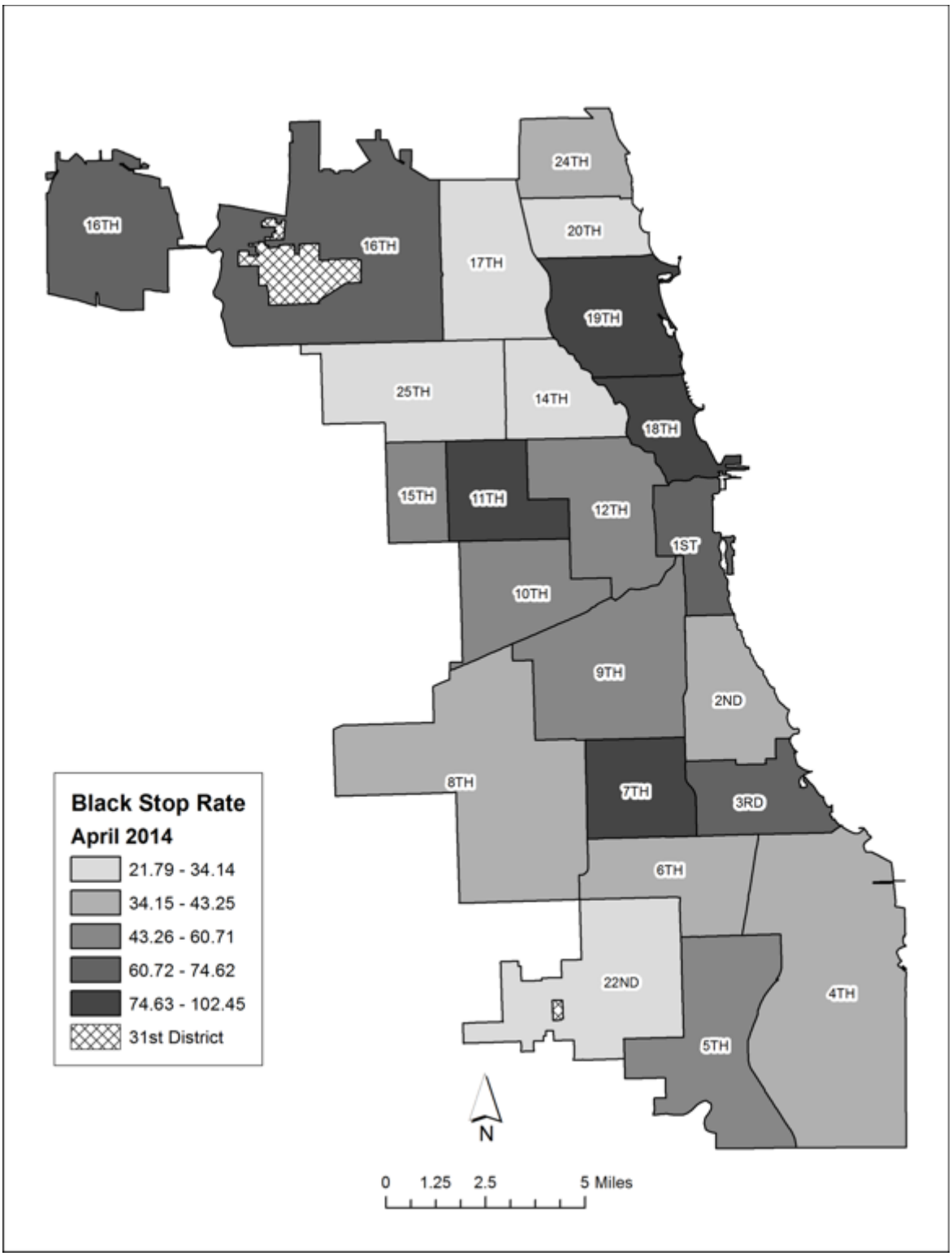
APPENDIX F: Non-Hispanic Black Stop Rate (per 1,000), February 2014



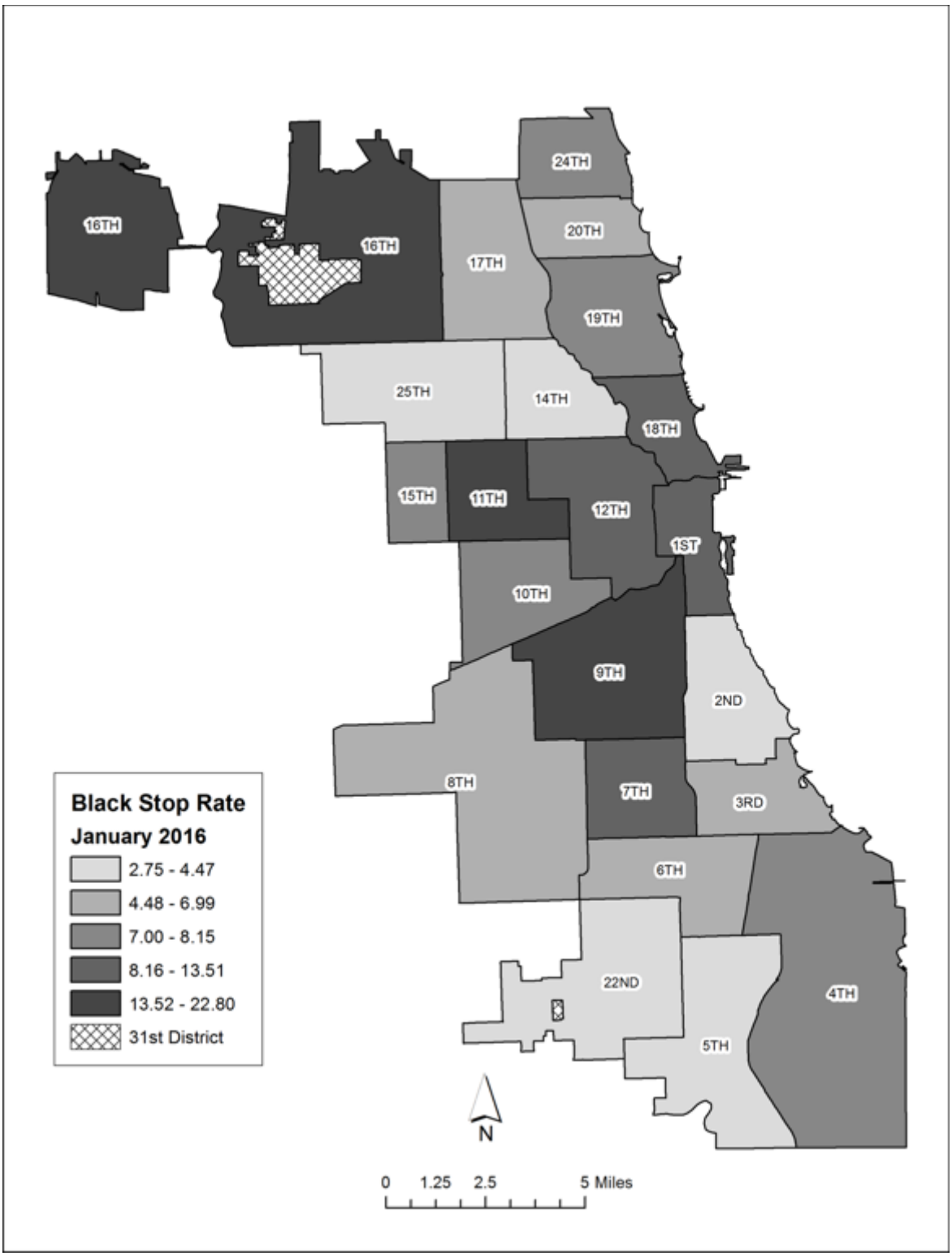
APPENDIX G: Non-Hispanic Black Stop Rate (per 1,000), March 2014



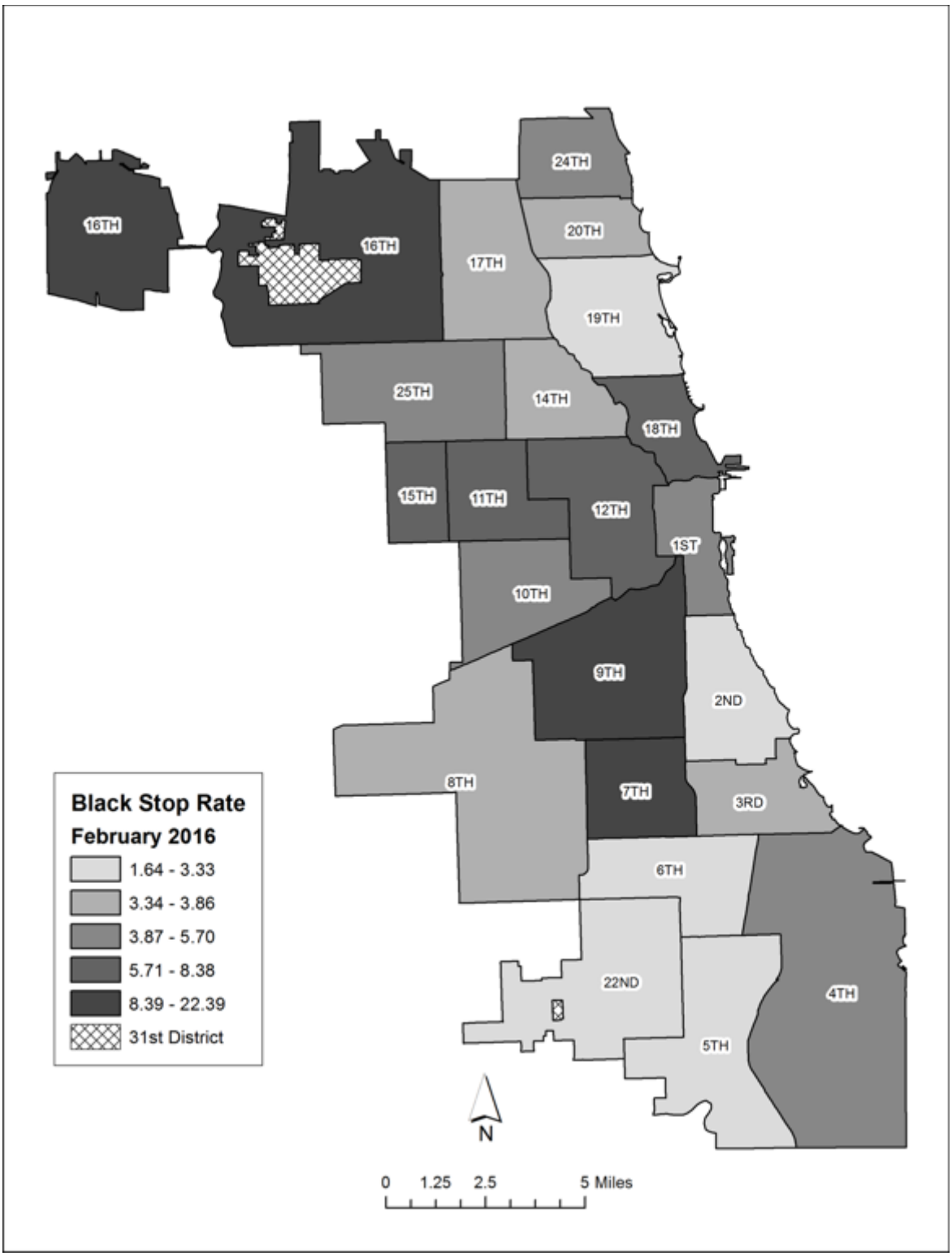
APPENDIX H: Non-Hispanic Black Stop Rate (per 1,000), April 2014



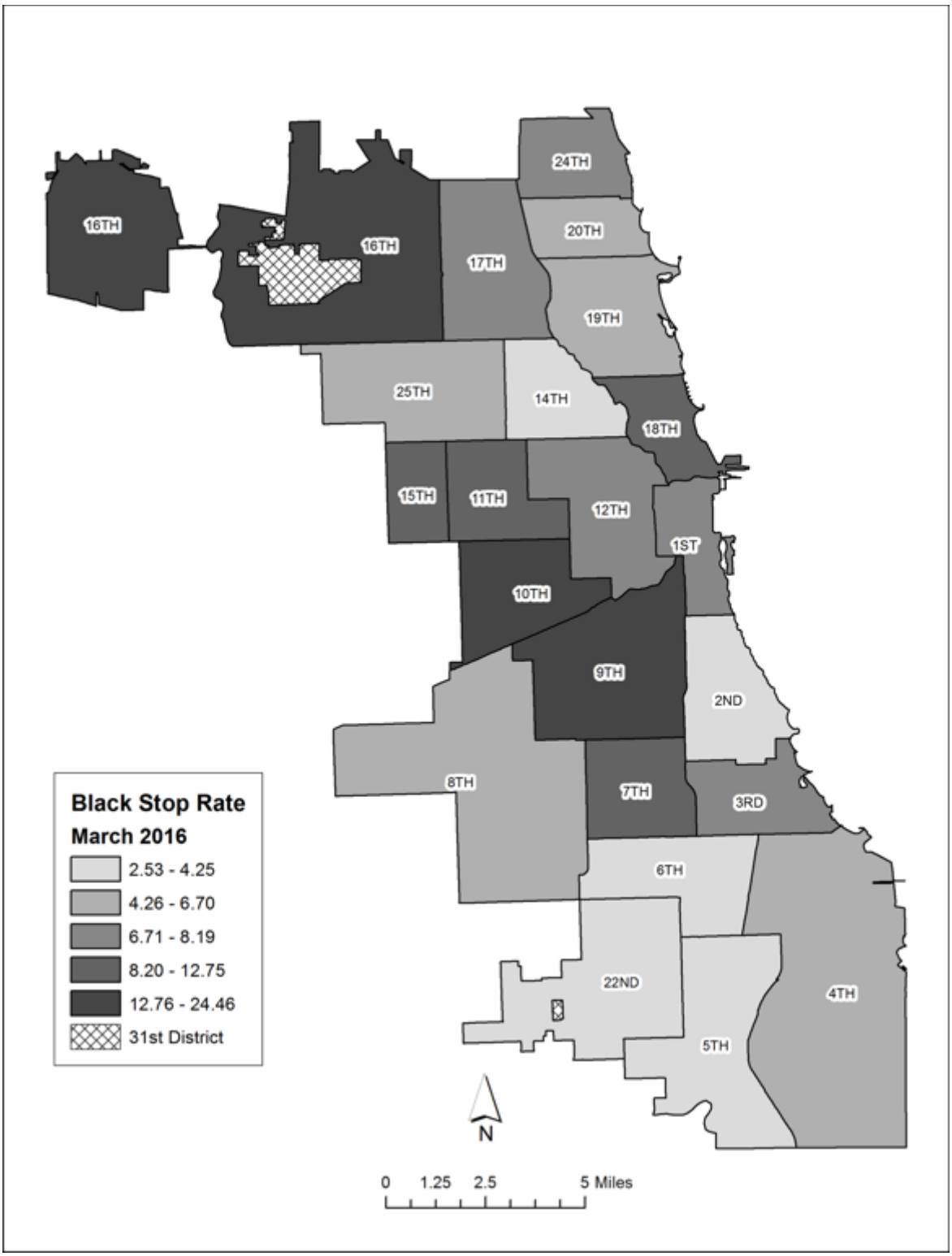
APPENDIX I: Non-Hispanic Black Stop Rate (per 1,000), January 2016



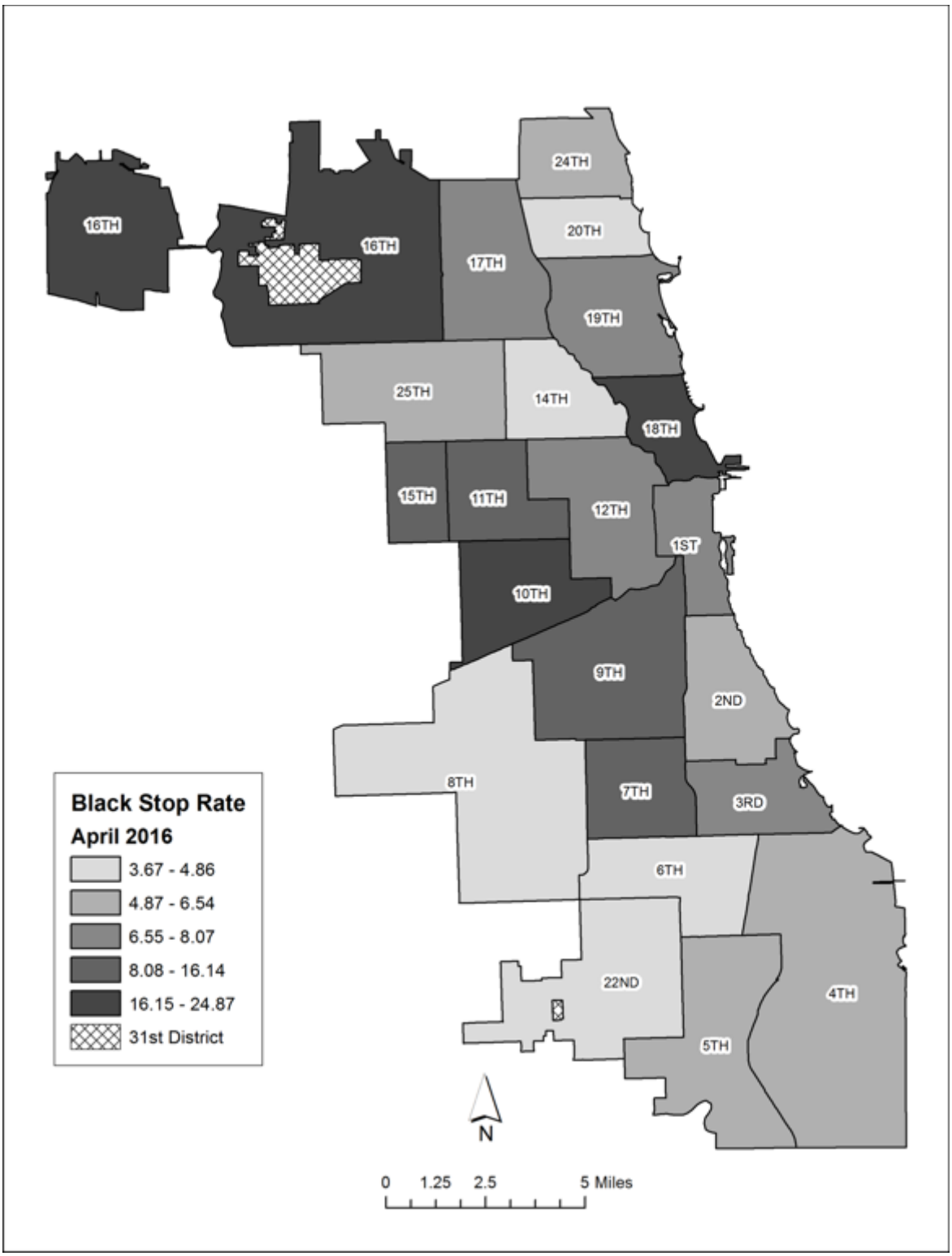
APPENDIX J: Non-Hispanic Black Stop Rate (per 1,000), February 2016



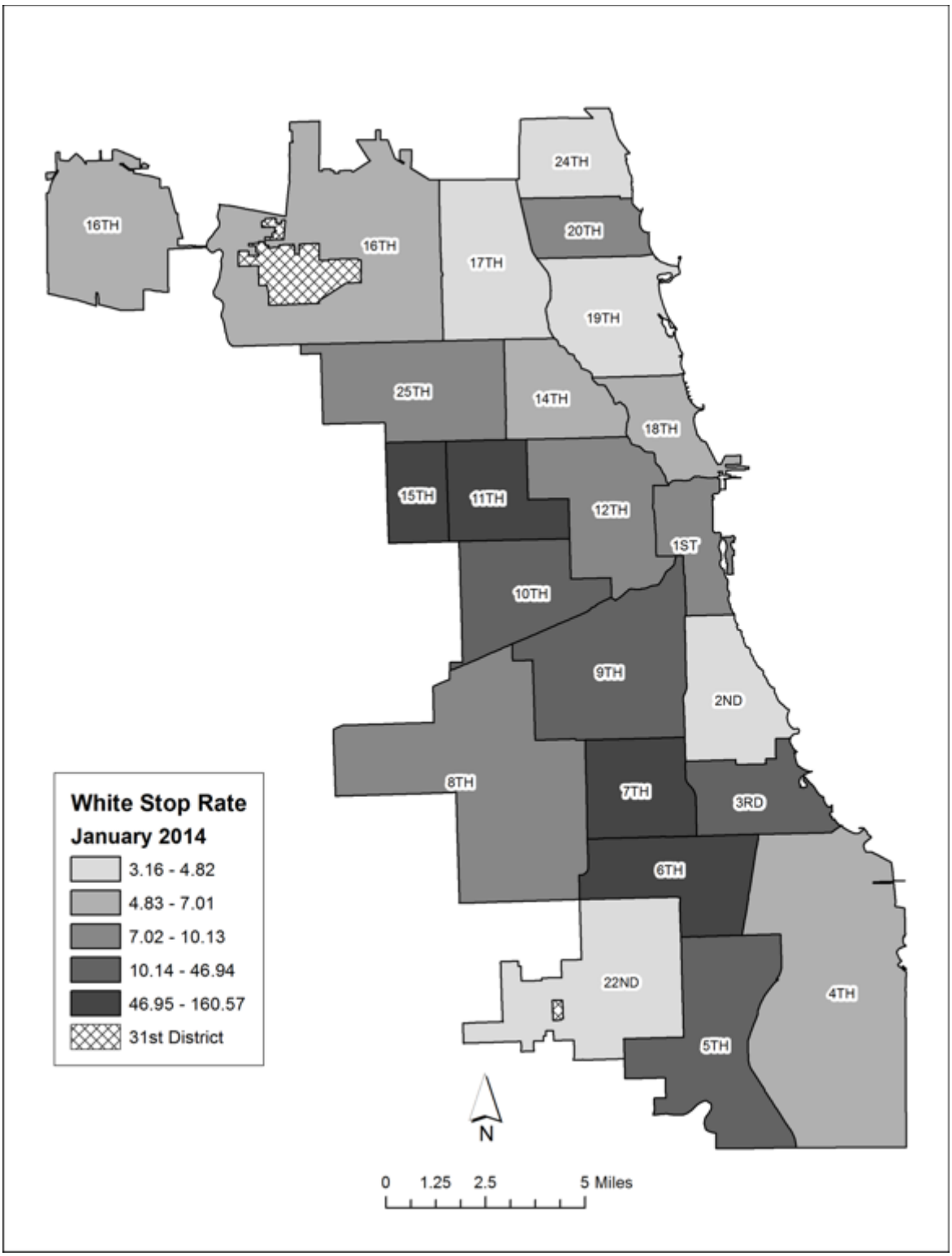
APPENDIX K: Non-Hispanic Black Stop Rate (per 1,000), March 2014



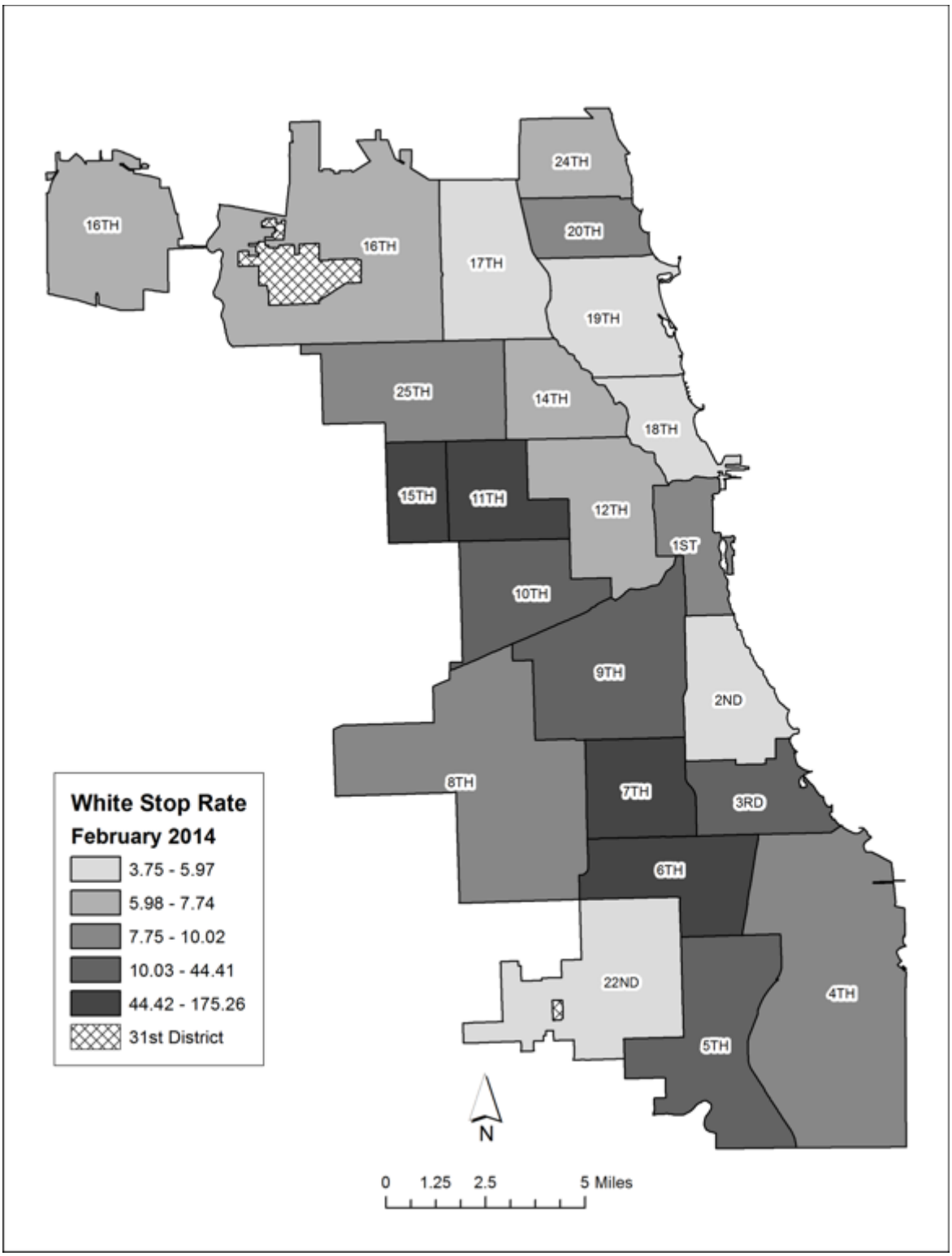
APPENDIX L: Non-Hispanic Black Stop Rate (per 1,000), April 2016



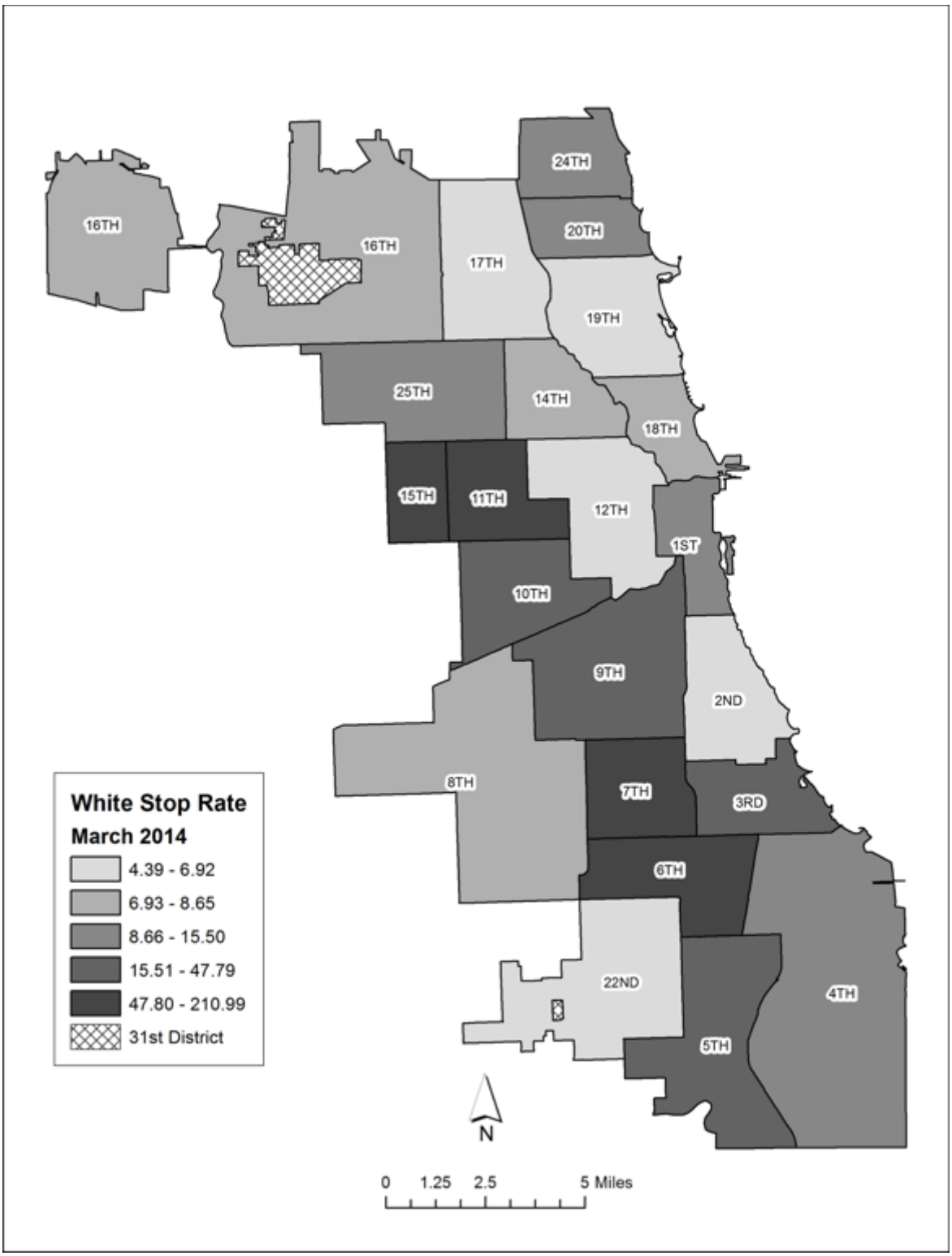
APPENDIX M: Non-Hispanic White Stop Rate (per 1,000), January 2014



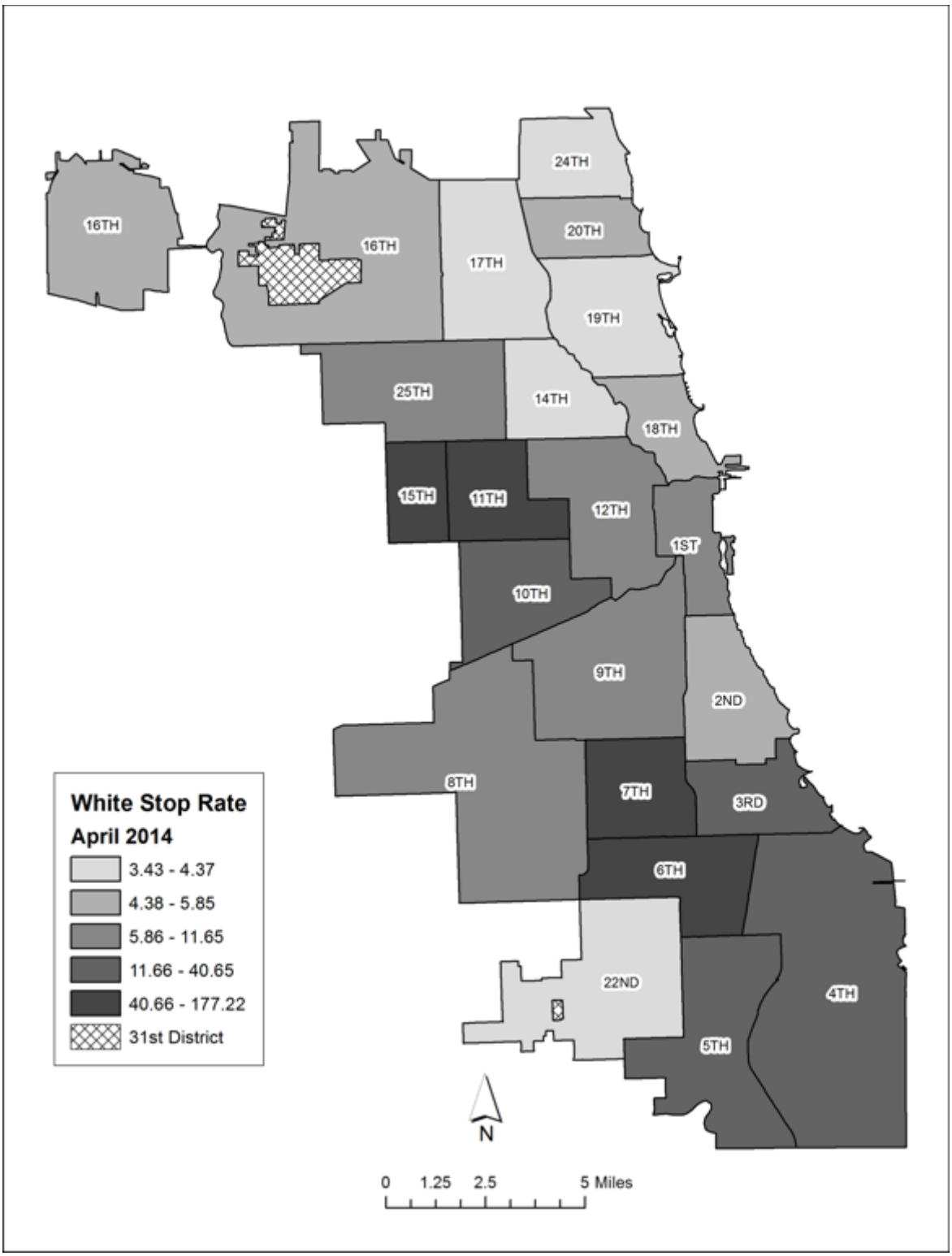
APPENDIX N: Non-Hispanic White Stop Rate (per 1,000), February 2014



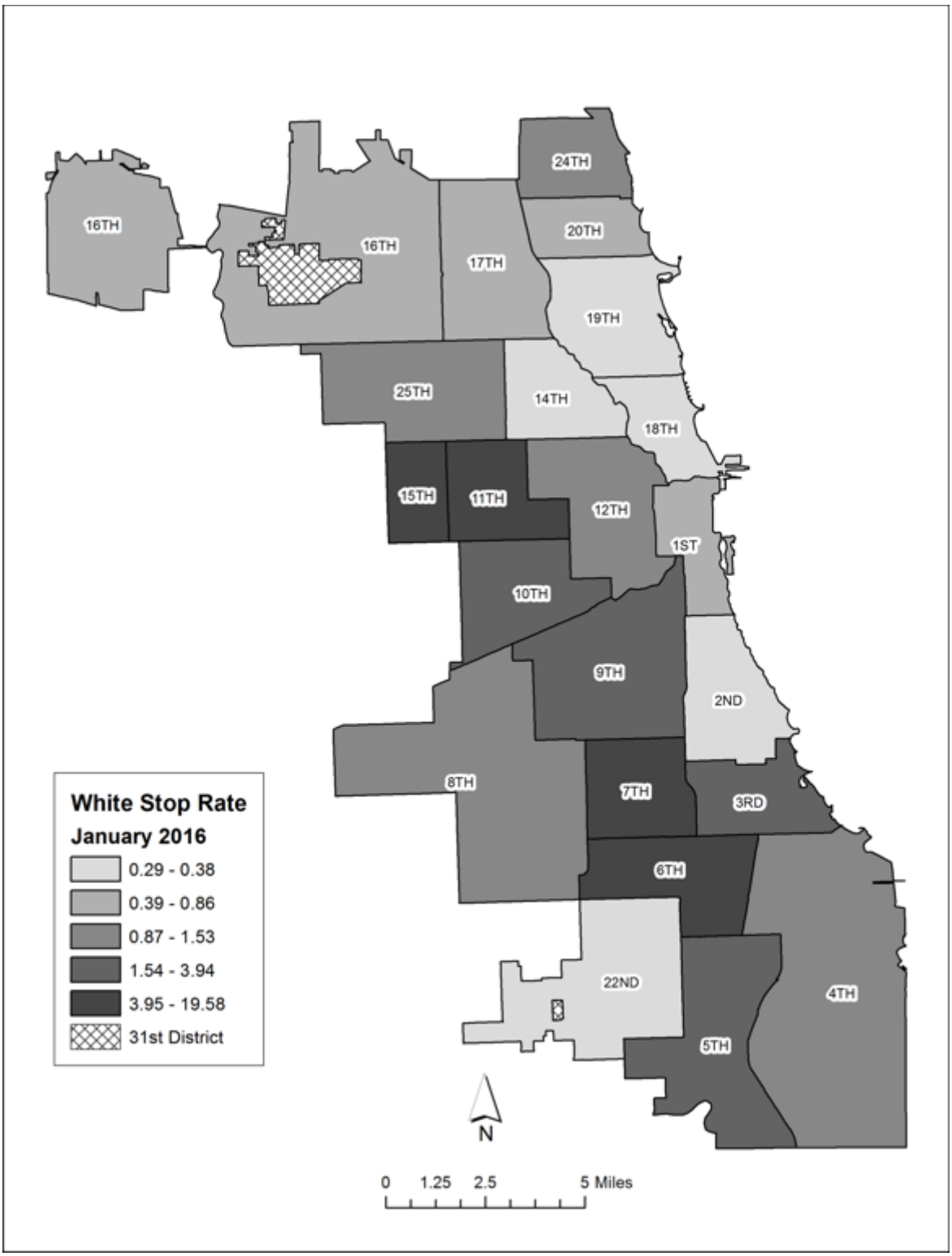
APPENDIX O: Non-Hispanic White Stop Rate (per 1,000), March 2014



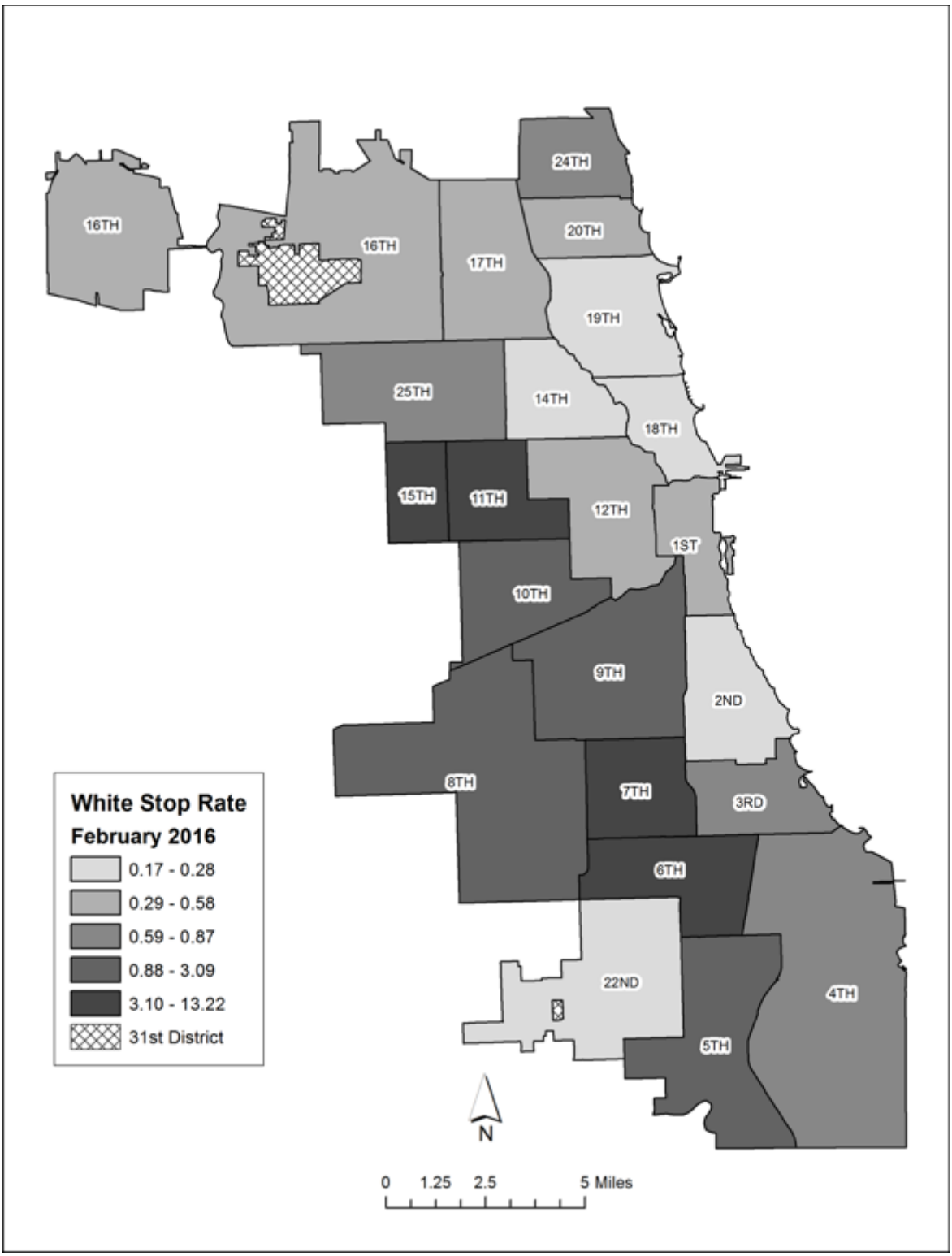
APPENDIX P: Non-Hispanic White Stop Rate (per 1,000), April 2014



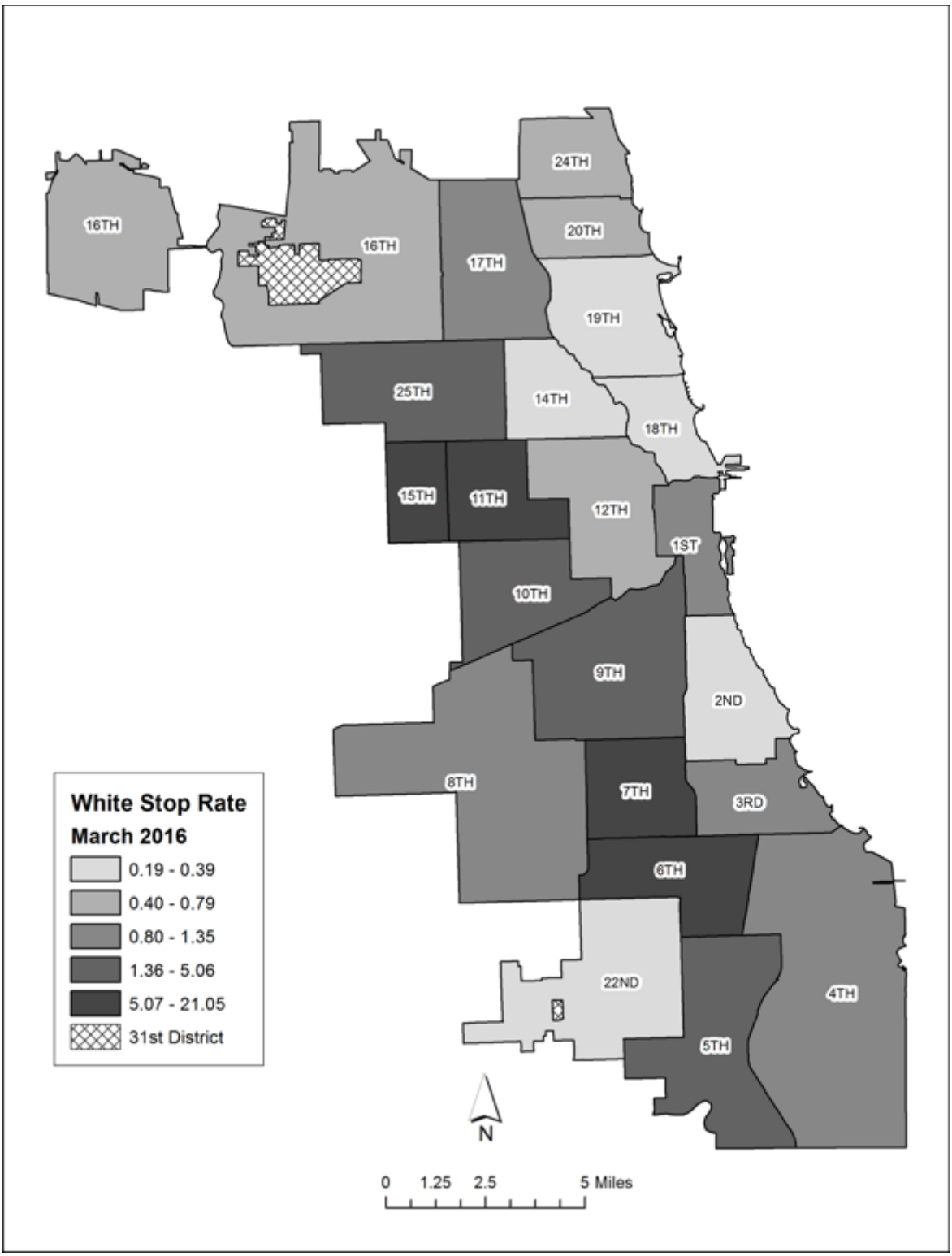
APPENDIX Q: Non-Hispanic White Stop Rate (per 1,000), January 2016



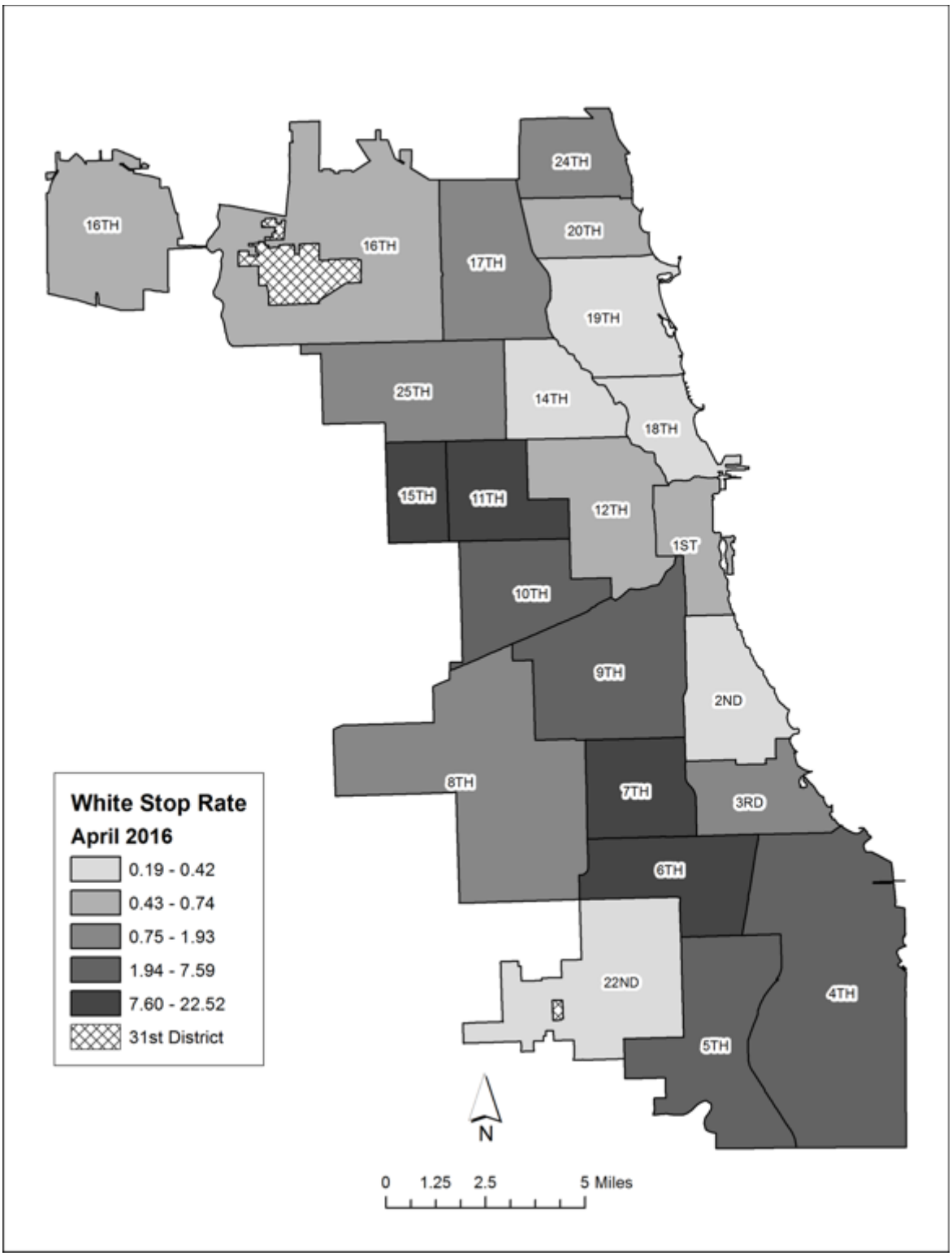
APPENDIX R: Non-Hispanic White Stop Rate (per 1,000), February 2016



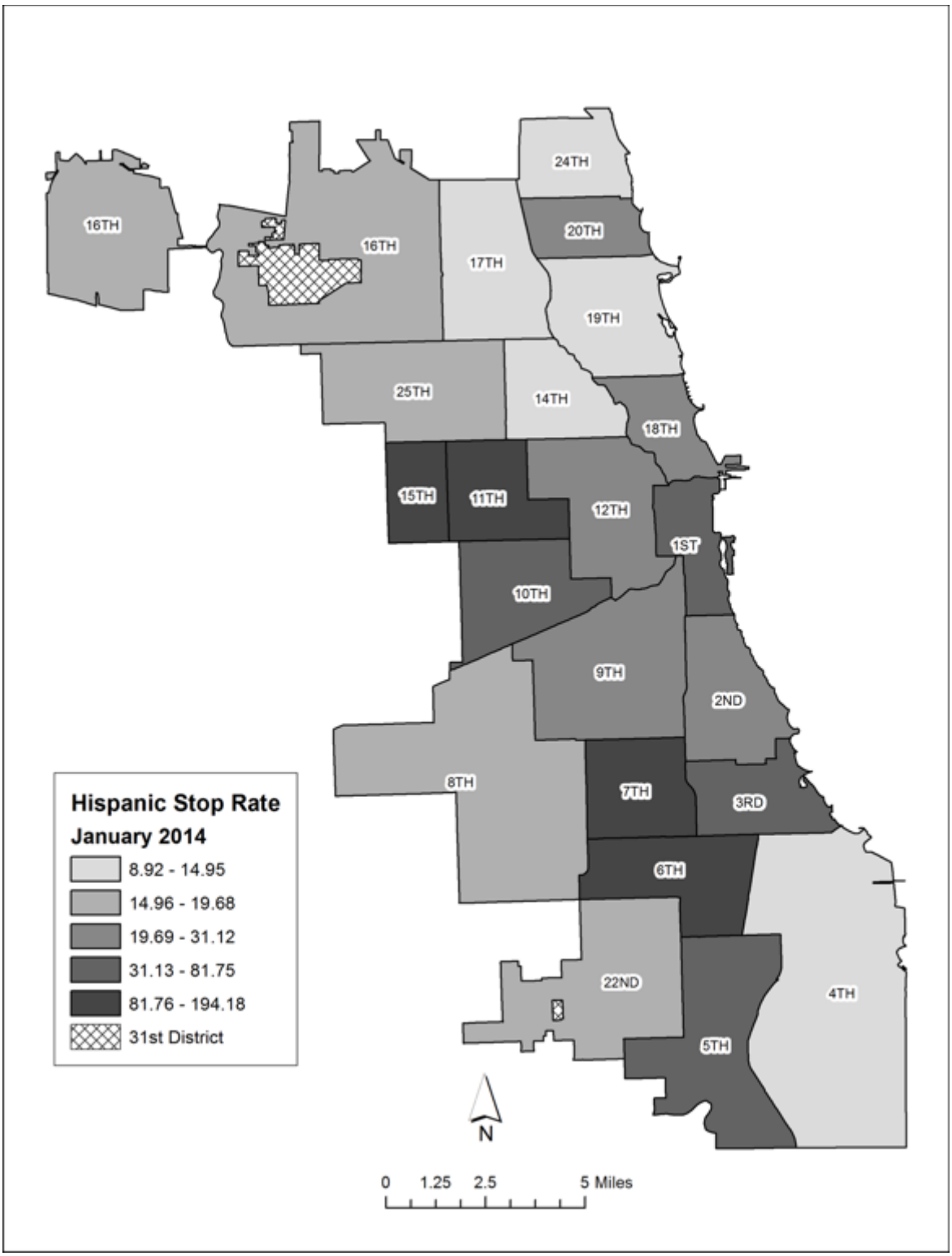
APPENDIX S: Non-Hispanic White Stop Rate (per 1,000), March 2016



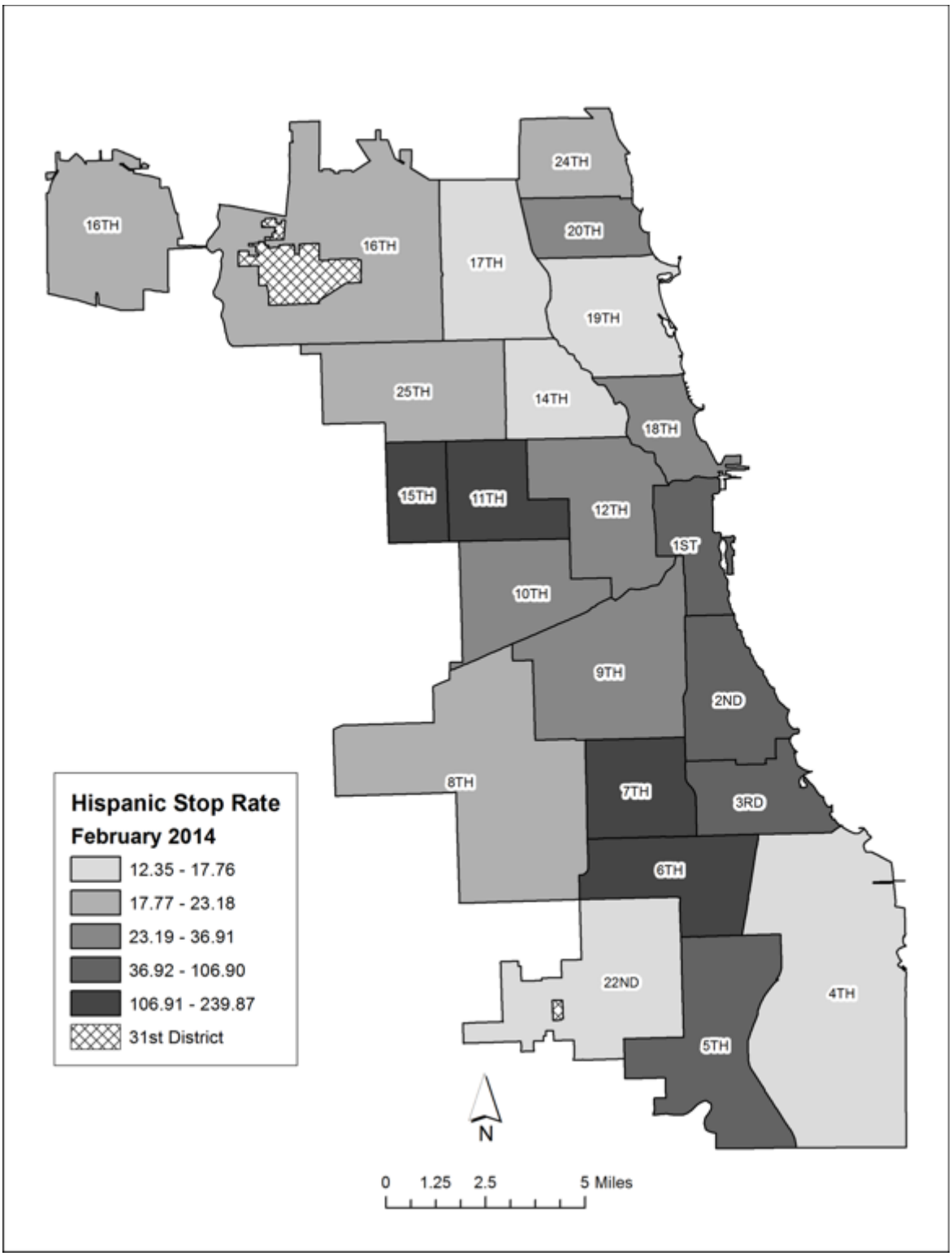
APPENDIX T: Non-Hispanic White Stop Rate (per 1,000), April 2016



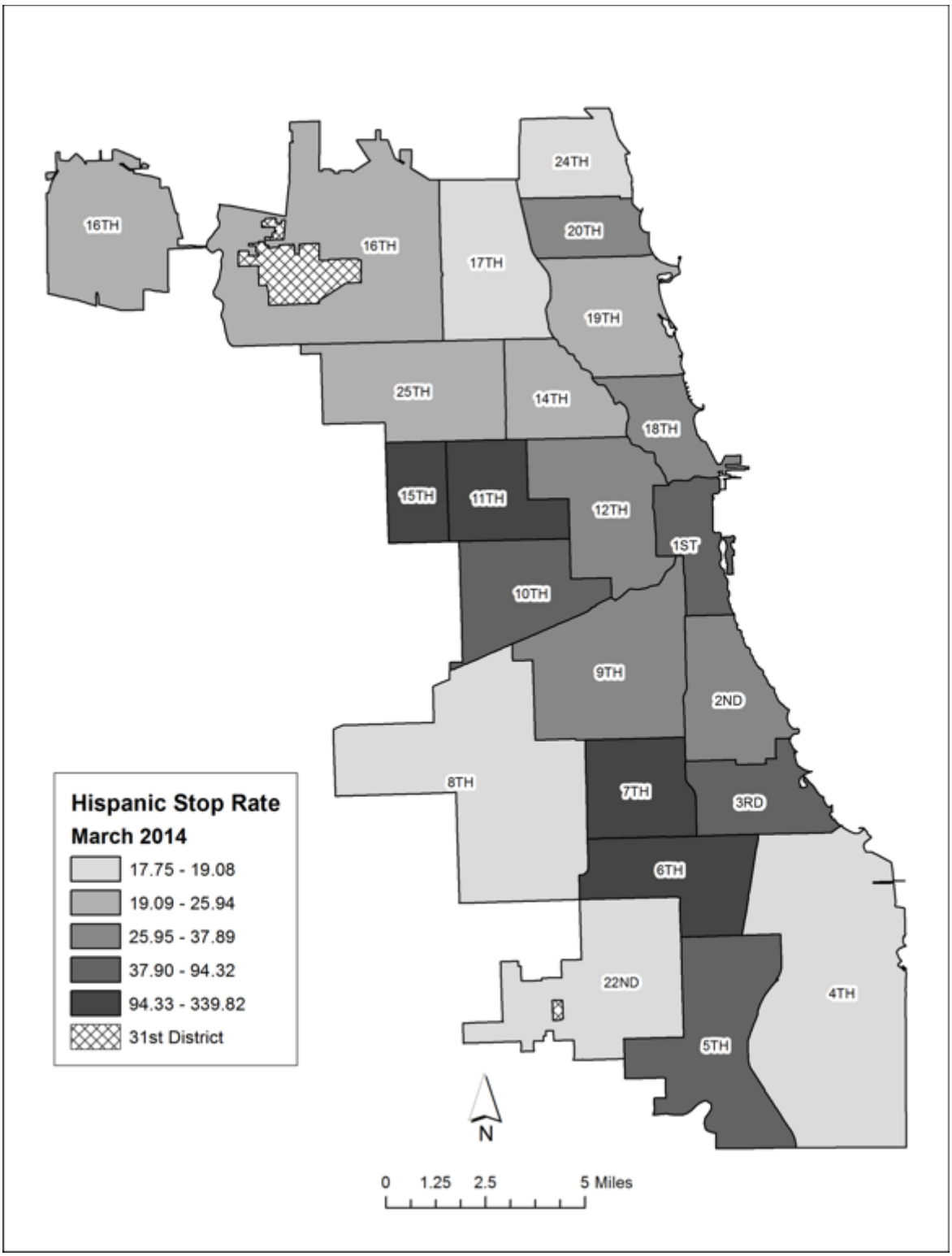
APPENDIX U: Hispanic White Stop Rate (per 1,000), January 2014



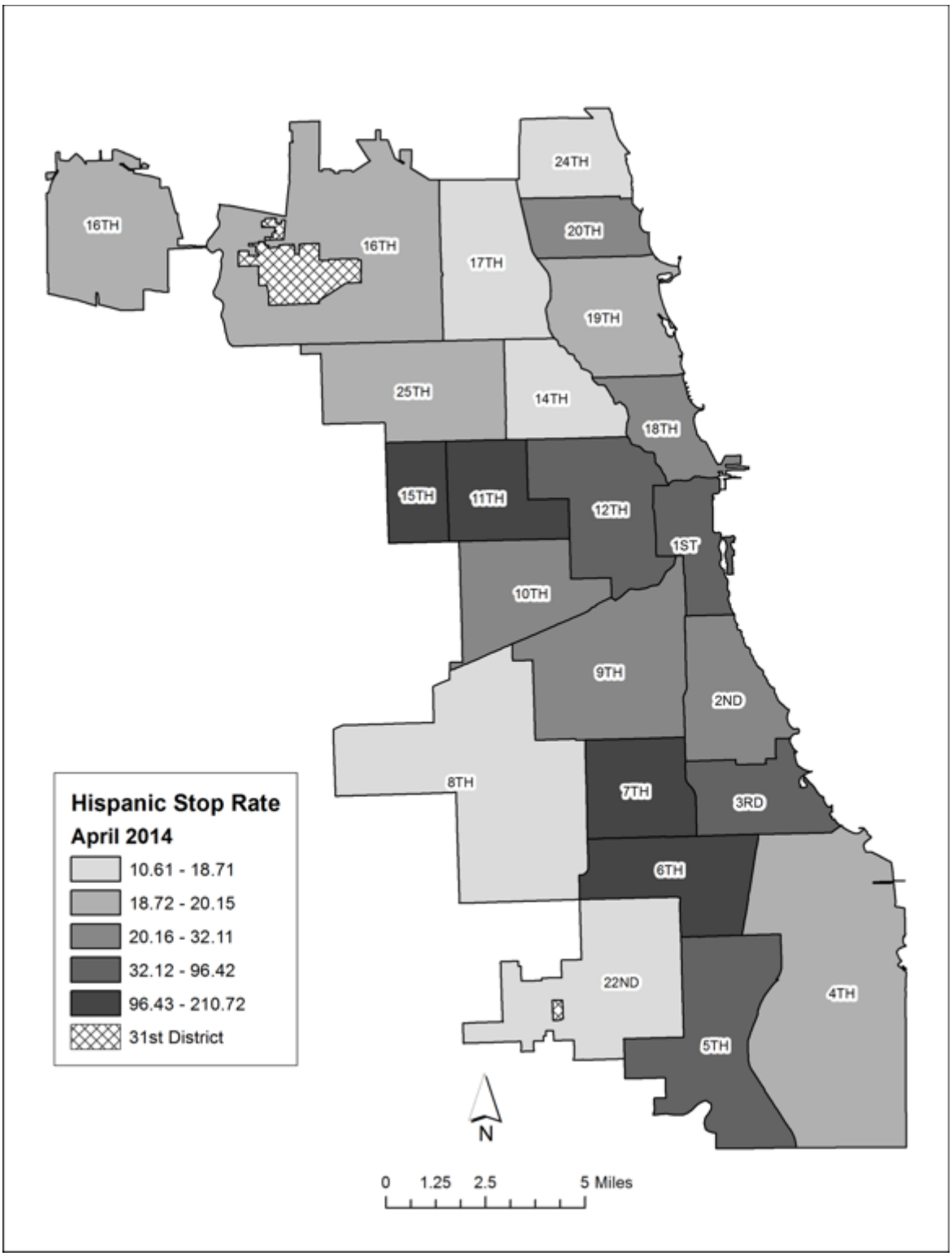
APPENDIX V: Hispanic White Stop Rate (per 1,000), February 2014



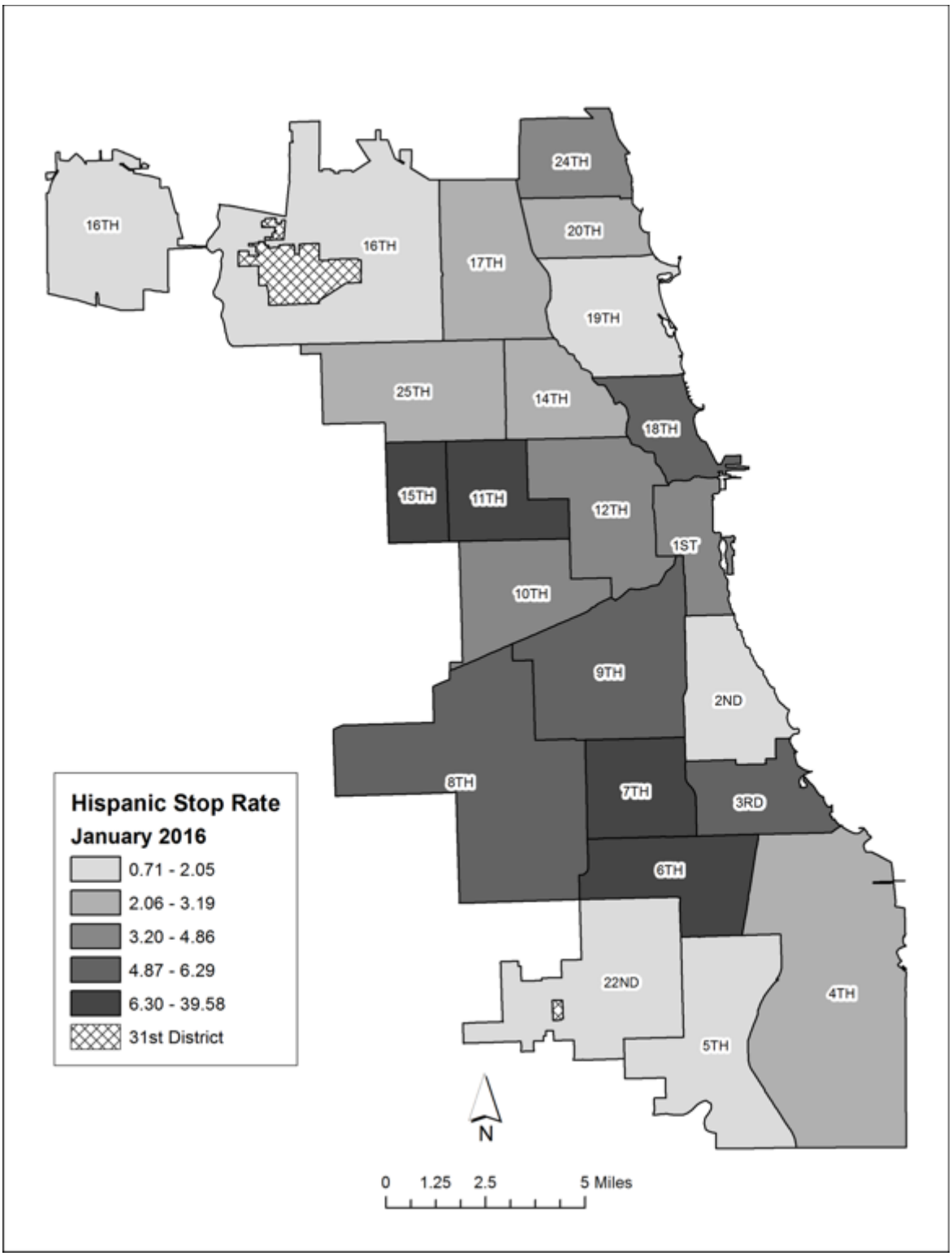
APPENDIX W: Hispanic White Stop Rate (per 1,000), March 2014



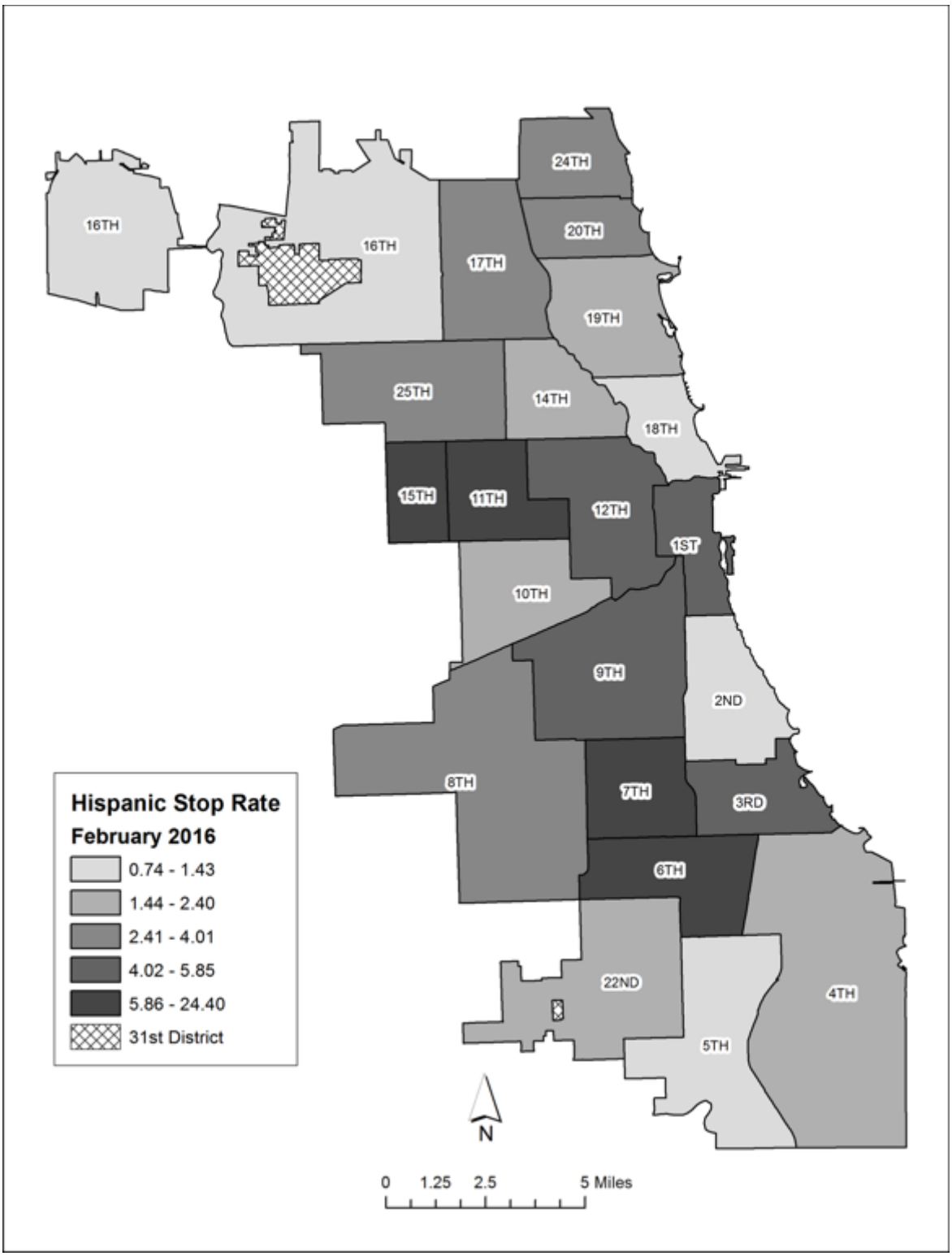
APPENDIX X: Hispanic White Stop Rate (per 1,000), April 2014



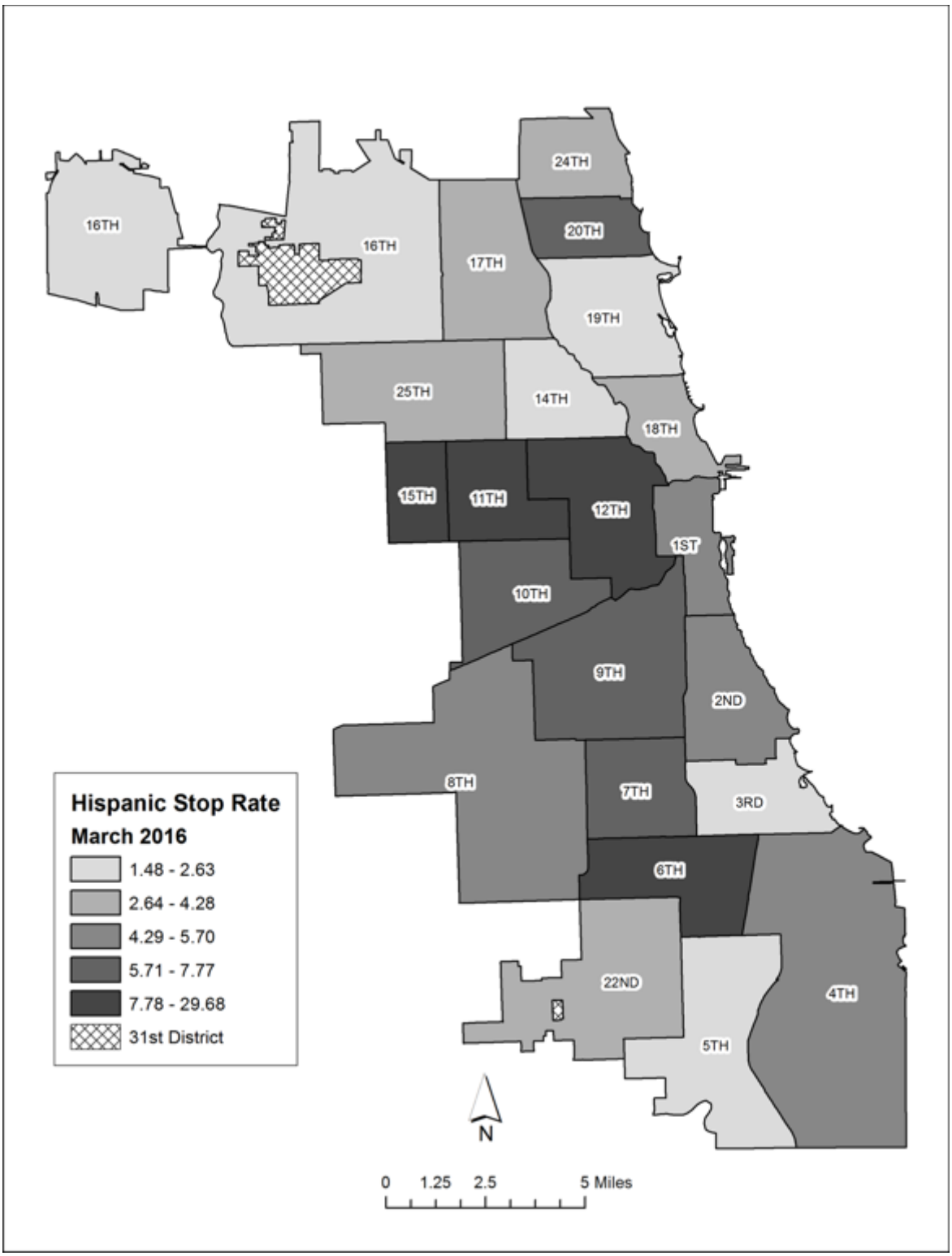
APPENDIX Y: Hispanic White Stop Rate (per 1,000), January 2016



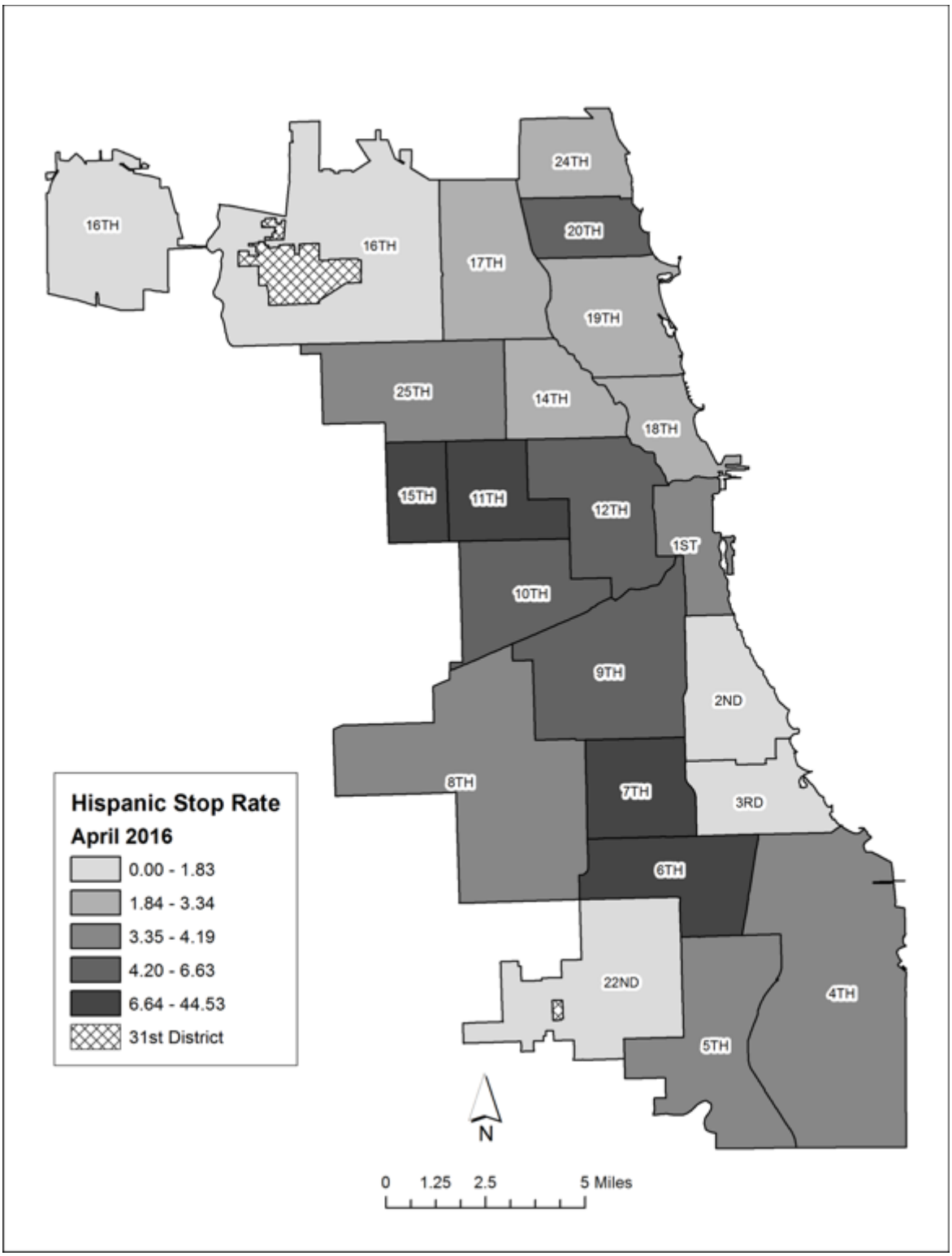
APPENDIX Z: Hispanic White Stop Rate (per 1,000), February 2016



APPENDIX AA: Hispanic White Stop Rate (per 1,000), March 2016



APPENDIX BB: Hispanic White Stop Rate (per 1,000), April 2016



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