Security Without Equity? The Effect of Secure Communities on Racial Profiling by Police

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Abstract

Anecdotal and circumstantial evidence suggest that the implementation of Secure Communities, a federal program that allows police officers to more easily identify illegal immigrants, has increased racial bias by police. The goal of this analysis is to empirically evaluate the effect of Secure Communities on racial bias by police using motor vehicle stop and search data from the North Carolina State Bureau of Investigation. This objective differs from most previous research, which has largely attempted to quantify racial profiling for a moment in time rather than looking at how an event influences racial profiling. I examine the effects of Secure Communities on police treatment of Hispanics vs. whites with an expanded difference-in-difference approach that looks at outcomes in motor vehicle search success rate, search rate conditional on a police stop, and police action conditional on stop. Statistical analyses yield no evidence that the ratification of Secure Communities increased racial profiling against Hispanics by police. This finding is at odds with the anecdotal and circumstantial evidence that has led many to believe that the ratification of Secure Communities led to a widespread increase in racial profiling by police, a discrepancy that should caution policy makers about making decisions driven by stories and summary statistics.

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1 Introduction

Secure Communities, a federal program that allows local police to quickly and easily identify illegal immigrants, has come under intense scrutiny since its inception in 2008. Under Secure Communities, all arrested individuals who receive a criminal background check are cross-referenced against a Department of Homeland Security database to identify if they have violated immigration laws. In response, critics have argued that police may be arbitrarily arresting individuals whom they suspect to be illegal immigrants in order to initiate deportation proceedings. The idea that police may be engaging in racial profiling against Hispanics is reinforced by anecdotal and circumstantial evidence. For example, Latinos comprise 93% of all people arrested through Secure Communities while they only make up 77% of the U.S. undocumented population[1]. While these arguments are effective at generating national attention, they lack the statistical robustness necessary to assert that Secure Communities has, in fact, precipitated an increase in racial profiling against Hispanics by police. This analysis will attempt to empirically test the effect of Secure Communities on racial profiling.

Previous literature related to the economic analysis of racial profiling has overwhelmingly attempted to quantify racial profiling at a given moment in time, rather than to evaluate how an event may have influenced racial profiling. The theory developed in this paper builds on literature that attempts to differentiate justifiable statistical discrimination from unproductive racial bias in its development of a model to identify the effects of Secure Communities on racial profiling. The resulting models indicate what inferences of racial profiling can be drawn from differential changes in three outcome variables associated with motor vehicle stops: the change for whites vs. Hispanics from before to after the implementation of Secure Communities in 1) rate of possession of contraband conditional on search, 2) rate of search conditional on stop, and 3) police action taken against stopped motorists. The empirical analysis will build on previous work in two subsets of existing litera-

[1]Kohli, Markowitz, & Chavez (2011).

ture. First, it will utilize an expanded difference-in-difference methodology similar to two previous empirical attempts to determine how events affected racial profiling. Second, it will employ empirical tests developed in the subset of previous studies that also had access to microdata, rather than merely summary data. The models will be fit with data from the North Carolina State Bureau of Investigation to quantify the effect of Secure Communities on racial profiling. Statistical analyses yield no evidence that the ratification of Secure Communities increased racial profiling against Hispanics by police.

The results are significant in two ways. First, this paper fills a current void in the critical evaluation of Secure Communities. With immigration law and policy a pressing issue in present day America, Secure Communities has been and will continue to be widely scrutinized. Many people have argued for its repeal, in part because of their belief in the program's tendency to increase racial bias by police against Hispanics. In its empirical investigation of the effect of Secure Communities on racial bias by police, this analysis should contribute to any thorough assessment of the true value of Secure Communities. Secondly, this analysis should warn policy makers to limit the extent to which they allow anecdotal and circumstantial evidence to enter into their decision making. This paper's finding that Secure Communities has not increased racial bias by police is at odds with the prevailing anecdotal and circumstantial evidence that has, to this point, predominantly shaped opinions about the program. Such a discrepancy should serve as a reminder that stories often do not tell the entire story of a policy's impact, and that without proper context, summary statistics can be misleading. While this analysis does not attempt to answer the question of absolute levels of racial bias, it could be the case that police are racially biased, but that their bias is unaffected by Secure Communities. If anecdotes and circumstantial evidence compel policy makers to limit Secure Communities, communities may feel like they have addressed the problem causing racial bias by police when they actually have not, which would disadvantage the people whom police are biased against. Such was likely the case in Alamance County, NC, which repealed 287g due to statistically suspect claims of its exacerbating influence on racial bias by police.

With nearly 4 million observations in the data, the analysis is well representative of the state of North Carolina. One potential limitation, however, is its generalizability to the rest of the USA. According to the US Census Bureau, North Carolina is the ninth largest state in the USA and is similar to the rest of the USA in age and gender composition and education levels, with its average income just slightly lower than the rest of the nation. However, North Carolina has a smaller population of Hispanics than the national average: its ratio of Hispanics to whites is about 1:8.1, while in the USA as a whole it is about 1:4.5. Removing California, Florida, and Texas from the calculation, the ratio of Hispanics to whites in the USA drops to 1:7.4, which is roughly comparable to North Carolina's demographic. Similarly, 7.6% of North Carolina's population is foreign born, while 12.9% of the population of the USA is foreign born; after removing California, Florida, and Texas, the share of the US population that is foreign born drops to just 9.3%, which is still higher but satisfyingly similar to North Carolina's share of foreign born persons. Results may therefore not be generalizable to states with large shares of Hispanics in their population, such as those on the southern border of the USA, but are conceivably generalizable to the rest of the nation.

The study proceeds as follows: Section 2 discusses relevant background information, Section 3 provides a brief summary of background literature, section 4 builds a theoretical model, section 5 introduces the data used, section 6 explains the empirical model, section 7 describes the results of analysis, and section 8 concludes.

2 Background Information

Secure Communities is a federal program designed to involve local police in the Department of Homeland Security's (DHS) fight against immigration violations. Prior to its implementation the identification of illegal immigrants was time and resource intensive, and usually required on-site interviews by federal Immigration and Customs Enforcement (ICE) officers.[2] When police arrest individuals, standard procedure is to take the fingerprint of the arrestee and submit it to the Federal Bureau of Investigation (FBI) for a criminal background check; Secure Communities mandates that all fingerprints sent to the FBI for criminal background checks are forwarded to the DHS, where they are run through a database that flags known violators of immigration laws. A flagged individual's identity is then sent to ICE for review, after which ICE determines whether it wants to issue a detainer on the arrestee. Detainers result in the arrestee being held in jail for up to 48 hours, during which an ICE officer will interview the individual and determine if he/she will be deported. The individual does not need to be found guilty of the crime for which he/she was arrested in order to be deported, and deportation verdicts are often found prior to the conclusion of parallel proceedings through the criminal justice system; through May 2013, 63,665 of the 306,662 people (21%) deported under secure communities had a spotless criminal record.[3] If ICE deems the individual deportable, he/she is placed in a detaining facility until the event of his/her deportation. Secure Communities was gradually rolled out in all local police jurisdictions in the USA from 2008 to 2013.

Prior to the implementation of Secure Communities, local police officers from select jurisdictions could aid in immigration enforcement through provisions outlined in section 287(g) of the Immigration and Nationality Act enacted in 1996 (287g). Under 287g, local police jurisdictions and the Federal Government may enter agreements that allow police officers, after a baseline level of training and under the supervision of trained ICE officers, to identify and detain illegal immigrants they encounter while on duty. Jurisdictions that had previously enacted 287g were still mandated to implement Secure Communities, but the ease with which they could identify illegal immigrants increased less than in jurisdictions that had not previously enacted 287g. While considerable literature exists on the effect of Secure Communities and 287g on crime, citizens' rights, and police relations with their

[2] This and the following descriptions of Secure Communities and 287(g) come from *http://www.ice.gov*, the subset of the DHS website devoted to ICE.

[3] Immigration and Customs Enforcement (2013).

local community,[4] to my knowledge, no formal economic model has been built to quantify the effect of Secure Communities on racial bias police.

3 Literature Review

Two prevailing, competing definitions of racial discrimination have emerged in previous literature: 1) racial discrimination is the use of race as an input in police' decisions, and 2) racial discrimination is the use of race as an input in police' decisions that results in suboptimal decision-making. There is a subtle but important distinction between the two that lies in the acknowledgment of statistical discrimination as a positive force. Under the assumption that racial discrimination is undesirable, the first definition advocates that police should be color-blind at all times, regardless of its effect on their ability to do their jobs, while the second advocates that police do their jobs to the best of their abilities independent of race. This analysis will subscribe to the second definition, which parallels the notion of taste-based discrimination first introduced by Becker (1957). This will allow for statistical discrimination in which police can use information about race as they would other signals, like age, gender, type of car being driven, location, etc., to improve their performance as police officers.

The absence of racial discrimination yields an equilibrium in which the marginal motorist of all races should have an equal probability of being guilty, which here is defined as carrying contraband, conditional on being searched. Unfortunately, data only provide each race's average probability of carrying contraband conditional on search, which is known as its "hit rate." The challenge that most relevant previous literature attempts to address is how to extrapolate from average to marginal hit rates, which is known as the "infra-marginality problem." Knowles, Persico, & Todd (2001) address this problem with a model that describes an equilibrium in which all motorists will have the same probability of carrying contraband. This work paved

[4]Examples include Kohli, Markowitz, & Chavez (2011), Weinstein (2012), Kang (2012), Gill (2013), Cox & Miles (2014).

the way for continued research that attempts to differentiate between statistical and taste-based discrimination, similar to the model built in this paper. While this analysis will rely on a theoretical model that parallels and builds on Knowles, Persico & Todd (2001), it will not be subject to their key assumptions, because the goal of this analysis differs from most previous literature. Previous literature has overwhelmingly focused on identifying the presence of racial discrimination by police at a given moment in time, but this analysis seeks to identify how an event affected racial discrimination by police.[5]

At least two other studies, to my knowledge, have similarly attempted to determine how an event effects racial profiling rather than to assess the existence of racial profiling at a given point in time. Warren & Tomaskovic-Devey (2009) sought to determine if increased social and political scrutiny on racial profiling affected racial profiling levels of police. Using data from the North Carolina Highway Traffic Study, Warren & Tomaskovic-Devey examined whether the timing of changes in search and hit rates is correlated with media references and legislative changes. They do not, however, include a control group, which subjects their analysis to potential confounding.

Heaton (2010) extends their study to assess the effects of police agency or government programs aimed at reducing racial bias by police. Heaton focuses on the state police department of New Jersey, which experienced a racial profiling scandal in 1998-9 in which white police officers shot four African-American and Hispanic motorists on the NJ Turnpike. The scandal precipitated an investigation that identified racial profiling by NJ state police officers and implemented reforms to decrease racial profiling. Heaton uses an expanded difference-in-difference specification that controls for location and crime type in its evaluation of how motor vehicle crime rates changed for whites vs. minorities from before to after the scandal. He uses data from neighboring states as a control to evaluate changes in racial profiling spe-

[5]For generalizations and extensions of the Knowles, Persico & Todd (2001) model, see Hernández-Murillo & Knowles (2004), Dharmapala & Ross (2004), Dominitz & Knowles (2006), Anwar & Fang (2006), Persico & Todd (2006), Bjerk (2007), Antonovics & Knight (2009), and Sanga (2009).

cific to New Jersey. While Heaton's methodology provides a good starting point, he only has access to summary data that provide yearly averages of crimes by race and location, and therefore cannot control for individual level observables, like gender and time of day, that are available in microdata.

Another aspect of previous literature that relates to the current study is empirical research based on microdata rather than summary data. Pickerill, Mosher, & Pratt (2009) provide a good explanation of the importance of using microdata in quantifying racial bias. They argue that the outcomes that suggest racial inequality may not be indicative of intentional racial bias if there exist observable signals, like gender or time of day of the stop, that correlate with the race of a motorist and a police officer's decision to stop or search the person. Many studies fail to account for these signals in their use of summary data. Pickerill, Mosher, & Pratt use microdata from the state of Washington to control for observable motorist characteristics and attempt to isolate the racial bias that is truly due to race. They control for characteristics of the driver, police officer, and the stop in general. Importantly, they differentiate between the amount of discretion that officers have in different types of stops and searches, arguing that searches precipitated by a high level of police discretion are more prone to intentional racially motivated bias than those in which the police officer has no choice in whether or not to make the search. This analysis will borrow the insight of Pickerill, Mosher, & Pratt (2009) and incorporate the discretion level of a search into its empirical model.

Grogger & Ridgeway (2006) similarly argue that intentional racially motivated bias will be more prevalent during daylight hours when police can more easily identify the race of a motorist. They test their hypothesis by examining the difference in discrepancies in the rate at which police stop whites vs. blacks for stops that occur during the day vs. after sundown. While this analysis will not infer racial bias from stop rates, it will control for daylight for completeness. Finally, Antonovics & Knight (2009) recognize that if racial inequality is due purely to statistical discrimination, then levels of racial inequality should not vary depending on the race of the police officer for a given group of motorists (e.g., white police officers should search black motorists at the same success rate as black police officers search black motorists). Unfortunately, the data used in this analysis does not contain information on the race of the police officer, so this test is not presently feasible, which is a limitation in the analysis.

4 Theoretical Methodology

4.1 Overall Theory and Assumptions

A. Convergence of expected value of making a search

From 2004 to 2012, North Carolina police searched roughly 7% of stopped motorists; police determine which stopped motorists to search by attempting to maximize the expected value of their searches under the stated goal of protecting and serving the citizens in their jurisdiction. Let {contraband $= C$, search $= S$, punishment $= P$, race $= R$, white $= W$, and Hispanic $= H$}. The expected value of a search is the product of the probability of the searched motorist carrying contraband and its value conditional on the carrying of contraband:

$$E[S \mid R] = \Pr(C \mid S, R) \times \mathrm{E}[P \mid C, R]$$

The probability of a motorist to carry contraband is inferred by the police officer based on the signals he/she observes when stopping a motorist. Some of the these observed signals are known to the data analyst and the police officer, like the gender, race and age of the driver, or the time of day, while others are known only to the officer, like the shiftiness of the drivers eyes or the smell of the car. All probabilities used in building theory will be conditional on the signals observable to the police officer unless otherwise noted.

Assume that prior to a search, police cannot perceive which searches would be of higher expected value conditional on finding contraband:[6]

$$\mathrm{E}[P \mid C, W] = \mathrm{E}[P \mid C, H] \tag{1}$$

Assumption 1 implies that the expected value of a search is proportional to the probability that a motorist is carrying contraband:

$$E[S \mid R] \propto \Pr(C \mid S, R)$$

To maximize the expected value of their searches, police search the motorists with the highest perceived probability of carrying contraband, and then proceed to search motorists in descending order of probability up to some break-even threshold. The break-even perceived search probability, above which police search motorists and below which they do not, would vary by police officer depending on his/her individual-specific value of wrongly searching innocent motorists vs. failing to search guilty ones, but its value would be the same for whites and Hispanics for non-racially biased police officers. Thus, absent racial profiling and on the margin, the probability of finding contraband, which can be defined as the "hit rate," will converge across races for every police officer, and therefore for police as a whole:

$$\Pr(C \mid S, W) = \Pr(C \mid S, H) \tag{2}$$

B. Expected value of carrying contraband

The expected profit of carrying contraband (π) to a motorist depends on the motorist's expected benefit if not caught with contraband (b); the cost (c), which is

[6]This assumption could break down if increased racial bias led to increased deportation, and deporting criminals is better for society than imprisoning them, then this assumption could wrongly accuse detected racial bias as unjustified. Since no increased racial bias is detected, exploration of this possibility is not necessary. That probabilities of a search yielding contraband conditional on its presence and search costs may vary by race do not harm the model since the model employs difference-in-difference, as described in Section 4.2.

a sum of financial costs (e.g., gas, tolls), opportunity costs (e.g., forgone wages at a legitimate job), and the mental anguish associated with transporting contraband; and the expected value of the penalty of being caught. The expected penalty of being caught while carrying contraband equals the product of the probability of being searched (p_1), the probability of a police officer finding contraband conditional on search and its presence (p_2), and the expected penalty conditional on being searched and contraband found. The expected penalty conditional on being caught with contraband is a weighted average of expected penalty issued by the criminal justice system conditional on a finding of contraband and no deportation (j), which includes fines and jail time, and the negative value placed on being deported conditional on deportation (d), weighted by the probability of being deported conditional on contraband being found ($q \mid C$). For U.S. citizens, $(q \mid C) = 0$. The expected value of committing a crime is:

$$\pi = (1 - p_1 \times p_2) \times b - c - p_1 \times p_2 \times ((1 - (q \mid C)) \times j + (q \mid C) \times d) \tag{3}$$

After implementation of Secure Communities, the expected value of carrying contraband would change:

$$\Delta\pi = \Delta\left((1 - p_1 \times p_2) \times b\right) - \Delta c - \Delta\left(p_1 \times p_2 \times (j + (q \mid C) \times (d - j))\right) \tag{4}$$

Assume that for the subset of crimes included in the analysis, which is confined to the presence of contraband in a motor vehicle, deportation is viewed by illegal immigrants as worse than prosecution through the judicial system, so that $d - j > 0$. Also assume that the implementation of Secure Communities does not affect the expected benefit or costs associated with carrying contraband, the probability of a search finding contraband conditional on its presence, or the expected judiciary outcome or negative value people place on deportation conditional on the presence of contraband.[7] Therefore, $\Delta b = \Delta c = \Delta p_2 = \Delta j = \Delta d = 0$. Equation 4 simplifies

[7]The validity of this assumption is a potential limitation of the study. It is conceivable that after the ratification of Secure Communities, the contraband carrying market will adjust and a

to:

$$\begin{aligned}\Delta\pi =& - b \times p_2 \times \Delta p_1 - p_2 \times (d-j) \times \Delta\left(p_1 \times (q \mid C)\right) - j \times p_2 \times \Delta p_1 \\ =& - (b+j) \times p_2 \times \Delta p_1 - p_2 \times (d-j) \times \Delta\left(p_1 \times (q \mid C)\right)\end{aligned}$$

Since $0 \leq p_2 \leq 1$; $0 \leq \min\{b,\ c,\ d,\ j\}$, the values of p_2, d, b, c, and j affect the magnitude of the change in motorists' incentives to commit crimes, but the values cannot change its sign. Decomposing by whites (W) and Hispanics (H), the theoretical changes in propensity to commit a crime are:

$$\begin{aligned}\Delta\pi^W &= -[\gamma \times \Delta p_1^W + \delta \times \Delta\left(p_1^W \times (q \mid C)^W\right))] \\ \Delta\pi^H &= -[\gamma \times \Delta p_1^H + \delta \times \Delta\left(p_1^H \times (q \mid C)^H\right))]\end{aligned}$$

where $\gamma = (b+j) \times p_2$ and $\delta = p_2 \times (d-j)$ are positive constants. The primary effect of Secure Communities is to increase the probability of being deported conditional on contraband being found, or $\Delta\left(q \mid C\right) > 0$. Since there is a much higher fraction of Hispanics than whites who are in the USA illegally,[8] $\Delta\left(q \mid C\right)^H > \Delta\left(q \mid C\right)^W$. Next, police would require some time, even if only a very brief amount, to realize that Hispanics are no longer committing as many crimes and update their statistical discrimination. Therefore, the most likely change in p_1^R that may result from the implementation of Secure Communities is a perceived increase in expected value of punishment conditional on finding contraband by searching more Hispanics, so that $\Delta p_1^H \geq 0$. Taken together, theory indicates that $\Delta\pi^H < 0$, $\Delta\pi^H < \Delta\pi^W$, and that $\Delta\pi^W$ is theoretically ambiguous since there is a small effect on punishment $\Delta\left(q \mid C\right)^W > 0$ which may be counteracted by a shift in police resources from searching whites to Hispanics, or $\Delta p_1^W < 0$. In plain language, the implementation

new general equilibrium will arise in which carrying contraband is costlier to illegal immigrants but more well compensated due to the increased risk, so that the the value of carrying contraband increases, or $\Delta b > 0$. For simplicity, assume that there is an inelastic enough supply of contraband carriers that their compensation does not change after the ratification of Secure Communities.

[8]Pew Hispanic Center (2006) estimates that approximately 78% of undocumented people in the U.S. are Hispanic, while the CIA Fact Book estimates that the total population of the U.S. is only 15.1% Hispanic

of Secure Communities yields an equilibrium in which Hispanics are incentivized to commit fewer crimes, both absolutely and relative to whites.

4.2 Methods and method specific theory

Method 1: Hit rates

As explained in section 4.1, part A, when the expected value conditional on finding contraband is equal for both races of motorists, the marginal searched white and Hispanic motorists will have the same probability of carrying contraband, or hit rates of marginal motorists will be equal:

$$\Pr(C \mid S, W) = \Pr(C \mid S, H)$$

This marginal rate is unknown to the data analyst, however, since only the average hit rate is deducible from recorded statistics. Furthermore, looking simply at the observed average hit rates fails to account for the infra-marginality problem, which acknowledges the potential difference between average and marginal hit rates. While hit rates should be equal across races on the margin, if there exist strong signals that indicate the presence of contraband more reliably for one race compared to another, so that the probability of carrying contraband is higher for non-marginal individuals of one race, then the average hit rates will not be equal absent racial profiling. As the goal of this analysis is to determine how an exogenous event affects racial profiling, rather than to quantify the level of racial discrimination in a community at a point in time, it has the unique ability to use difference-in-difference to remedy the infra-marginality problem without requiring stronger claims about the convergence of behavior at equilibrium.

Police officers' searches can be ordered by their perceived probability of success, which is known by the police but not by the analyst, in order to establish which searches should be considered "on the margin." Define that the α percentile of searches with the lowest perceived success rate are on the margin, and the $(1-\alpha)$

percentile of searches with the highest probability are not on the margin, with $(1-\alpha) >> \alpha$. The average success rate for the non-marginal searches is θ, and the average success rate for the marginal searches is λ, with $\theta \geq \lambda$. The observed contraband hit rate, X, for race R is a weighted average of θ and λ:

$$X^R = (1-\alpha) \times \theta^R + \alpha \times \lambda^R$$

The difference in marginal probability of carrying contraband conditional on search between Hispanics, H, and whites, W, must be calculated using data that contain only the difference in average hit rates:

$$\begin{aligned} X^H - X^W &= (1-\alpha) \times \theta^H + \alpha \times \lambda^H - \left((1-\alpha) \times \theta^W + \alpha \times \lambda^W\right) \\ &= (1-\alpha) \times \left(\theta^H - \theta^W\right) + \alpha \times \left(\lambda^H - \lambda^W\right) \end{aligned}$$

Next, the difference-in-difference is calculated by subtracting the difference in hit rates for Hispanics vs. whites from before to after implementation of Secure Communities. The percentile at which "the margin" has been defined is held constant, so $\Delta\alpha = 0$. The difference-in-difference of hit rates will be:

$$\Delta\left(X^H - X^W\right) = (1-\alpha) \times \left(\Delta\theta^H - \Delta\theta^W\right) + \alpha \times \left(\Delta\lambda^H - \Delta\lambda^W\right)$$

To assert that Secure Communities increased racial bias by police against Hispanics requires determination that $\Delta\lambda^W - \Delta\lambda^H > 0$. Section 4.1, part B, showed that $\Delta\theta^H < \Delta\theta^W$, and by definition $0 < \alpha < (1-\alpha) < 1$. The above relationship can be rewritten:

$$\Delta\left(X^H - X^W\right) = \alpha \times \left(\Delta\lambda^H - \Delta\lambda^W\right) + \tau$$

where $\tau = (1-\alpha) \times \left(\Delta\theta^H - \Delta\theta^W\right) > 0$. Racial profiling exists against Hispanics only if the marginal searched Hispanic motorist is of lower probability of success than the marginal searched white motorist, or $\Delta\lambda^H < \Delta\lambda^W$. An observation of $\Delta\left(X^H - X^W\right) > 0$ would necessarily occur absent racial profiling against Hispanics, or when $\Delta\lambda^H \geq \Delta\lambda^W$, but could also occur in conjunction with racial profiling if the decrease in the probability of the non-marginal motorists is sufficiently high, or $\alpha \times \left(\Delta\lambda^{H} - \Delta\lambda^{W}\right) < \tau = (1-\alpha) \times (\Delta\theta H - \Delta\theta W)$. Therefore, a finding of $\Delta\left(X^{H} - X^{W}\right) > 0$ is inconclusive, while a finding of $\Delta\left(X^{H} - X^{W}\right) < 0$ indicates the presence of racial bias against Hispanics. Therefore, in order to reject that racial bias exists, it is necessary but not sufficient that $\Delta\left(X^{H} - X^{W}\right) > 0$.

Method 2: Search given stop rates

Define {contraband = C, search = S, punishment = P, race = R, white = W, and Hispanic = H}. Here, a model will be built to infer racial bias from police' decision to search a motorist given a stop has already occurred. In determining which motorists to search given a stop, police maximize the value of their actions under time and financial constraints, as outlined in section 4.1, part A. Section 4.1, part A shows that when the expected value conditional on finding contraband is equal for both races of motorists, the marginal searched white and Hispanic motorists have the same probability of carrying contraband, or the hit rate of the marginal motorists is equal:

$$\Pr(C \mid S, W) = \Pr(C \mid S, H) \tag{5}$$

Two iterations of Bayes Rule and some straightforward simplification yield that equilibrium absent bias requires a constant marginal "hit rate" across races:

$$\Pr(C \mid W)/\Pr(S \mid W) = \Pr(C \mid H)/\Pr(S \mid H) \tag{6}$$

Equation 6 can be rewritten:

$$\Pr(C \mid W)/\Pr(C \mid H) = \Pr(S \mid W)/\Pr(S \mid H)$$

After implementation of Secure Communities, absent racial profiling this relationship yields:

$$\Delta[\Pr(C \mid W)/\Pr(C \mid H)] = \Delta[\Pr(S \mid W)/\Pr(S \mid H)]$$

As demonstrated in section 4.1, part B, theory predicts that Secure Communities will cause Hispanics to decrease their propensity to carry contraband more than it will for whites:

$$\Delta[\Pr(C \mid W)/\Pr(C \mid H)] > 1$$

which implies that absent racial bias,

$$\Delta[\Pr(S \mid W)/\Pr(S \mid H)] > 1$$

Equivalently, the rate of search conditional on stop should decrease for Hispanics relative to whites absent racial bias by police, which is empirically testable. Furthermore, whites should be searched increasingly more than Hispanics, or $\Delta[\Pr(S \mid W)/\Pr(S \mid H)]$ should increase, as time passes after implementation of Secure Communities if police update their beliefs regarding relative propensity to carry contraband of whites vs. Hispanics, or $\Delta[\Pr(C \mid W)/\Pr(C \mid H)]$, with a lag. This possibility is discussed further in section 4.3. Finally, search costs do not enter the model because they are independent of Secure Communities and will therefore be negated in the difference-in-difference methodology.

Method 3: Stop Outcome

The final method will employ a categorical outcome variable that denotes the outcome of a stop to determine whether the ratification of Secure Communities prompted a change in the distribution of stop outcomes for whites vs. Hispanics in a meaningful way. After a motorist has been stopped, police can take no action; issue a written warning, verbal warning, or citation; or arrest the stopped motorist. In parallel with the model of taste based discrimination developed by Becker (1957), the utility of a police officer is a function of the action they take against the stopped motorist, or decision d, and the race of the motorist, which for this purpose is either white or Hispanic, $R = \{W, H\}$. Assume police receive utility from doing their job well, $U_j(d)$, and from their treatment of people depending on their race. The decision that maximizes the quality with which a police officer does his/her job is defined as d^*. Therefore, a police officer's full utility function is:

$$U(d|R, d^*) = \alpha \times [U_j(d) - U_j(d^*)] + U(d|H) \times \mathbb{1}(R = H)$$

With $\alpha > 0$ and

$$\mathbb{1}(R = H) = \begin{cases} 1 & \text{if R = H} \\ 0 & \text{if R = W} \end{cases}$$

The outcome space for police officers can be reduced to three decisions for simplicity: arrest ($d = a$), citation ($d = c$), or no action ($d = n$), where n also includes both verbal and written warnings. Therefore a police officer chooses between three actions to maximize his/her utility:

$$U(a|R, d^*) = \alpha \times [U_j(a) - U_j(d^*)] + U(a|H) \times (1 \mid R = H)$$
$$U(c|R, d^*) = \alpha \times [U_j(c) - U_j(d^*)] + U(c|H) \times (1 \mid R = H)$$
$$U(n|R, d^*) = \alpha \times [U_j(n) - U_j(d^*)] + U(n|H) \times (1 \mid R = H)$$

Here, $U(d|H)$ represents the utility achieved by a police officer taking action d against a Hispanic motorist versus a white motorist. The ratification of Secure Communities has no effect on the utility police officers derive from doing their job well, or $\Delta U_j(d) = 0$. Furthermore, since Secure Communities only results in the identification of illegal immigrants when an arrest is made, its ratification would not affect the utility police receive from issuing warnings or citations to members of different races, or $\Delta U(c|R) = \Delta U(n|R) = 0$. Therefore, $\Delta U(c|R, d^*) = \Delta U(n|R, d^*) = 0$. Secure Communities would plausibly result in $\Delta U(a|H) > 0$ for a racially biased police officer who seeks to use arrests to increase deportation of Hispanics, and therefore $\Delta U(a|H, d^*) \neq 0$ for racially biased officers who favor whites to Hispanics. Furthermore, $(U_j(a) - U_j(c))^2 < (U_j(a) - U_j(n))^2$, or for a given motorist, arrest is a closer substitute to citation than no action taken. Since arresting someone who would otherwise be cited incurs a lower cost to police

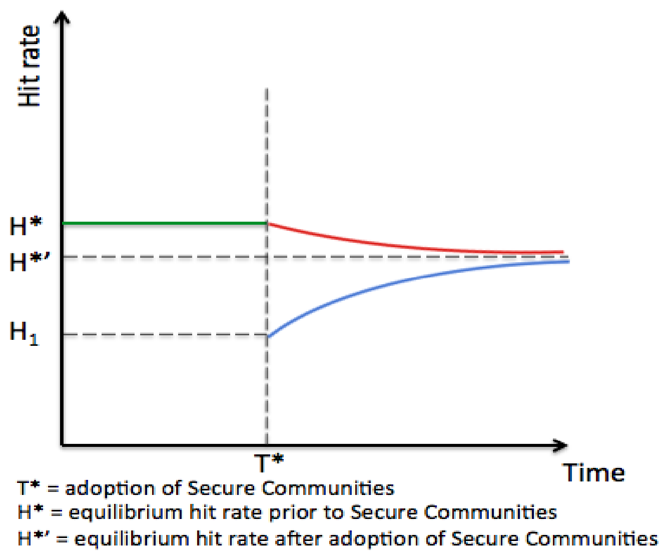

Figure 1: Change in hit rates absent racial profiling with ratification of Secure Communities. Blue = Hispanics' hit rates; red = whites'.

officer's utility derived from doing his/her job well than arresting someone who would otherwise be given a warning, racially biased police are expected to decrease the share of Hispanics they cite relative to offer a warning. Together, racially biased police would be expected to arrest a larger share of Hispanics after the ratification of Secure Communities relative to the amount they cite, while the share of Hispanics against whom no action is taken should remain roughly constant.

4.3 Specification 2: The lagged effect of Secure Communities

Police may engage in productive profiling, or searching people who have a higher probability of being guilty more often, in order to maximize their success rates of searches. Just as officers might search stopped motorists whose vehicles smell like alcohol or drugs more often than those that do not, they may justifiably search people based on, for example, their gender or race if doing so results in an improvement in their search success rates. Police determine the rate at which they will statistically discriminate through a learning process from working on the job; from realizing that their success rate in searching one race is higher than that of another, the hit rate maximizing officer would adjust his decision accordingly such that on the margin, each search would have an equal expected value. The statistical updating that prompts alteration of police search calculations takes time to be realized, even if only a very short amount of time. Assuming the ratification of Secure Communities results in Hispanics carrying contraband less frequently in order to avoid deportation, as described in section 4.2 part B, police should update their search decisions to reflect the change, but police behavior may not necessarily change immediately. Therefore, police might over-search Hispanics immediately after the ratification of Secure Communities if there is a lag between their perception of behavior changes and the time it takes for people to change behaviors. As a result, the Hispanic hit rate would decrease immediately after the ratification of Secure Communities, but ultimately reach a new equilibrium with whites after police officers have had the opportunity to update their statistical discrimination. This process is illustrated in Figure 1.[9]

To investigate the presence of potential statistical updating, the Secure Communities binary ratification variable was broken into a categorical variable that reflects how long prior to or after Secure Communities' ratification a police stop takes place. These timing variables are described in the data section. While average hit rates might decrease for Hispanics after the ratification of Secure Communities, this might not be an indication of racial bias if the decreased average is caused by an immediate decrease in hit rate that later returns to equilibrium, as illustrated in Figure 1. The timing variables will help identify how racial profiling reacts to the ratification of Secure Communities and how long, if not instantaneously, police take to complete the statistical updating necessary for continued hit rate maximization.

[9]The equilibrium hit rate is drawn lower after the ratification of Secure Communities than it was before because there will be fewer Hispanics carrying contraband, and therefore an equal number of total searches will yield fewer successes. In reality, general equilibrium suggests that with fewer Hispanics willing to carry contraband, the value of carrying contraband will increase and therefore others will take the place of the vacated Hispanics. Therefore, the change in equilibrium hit rate is theoretically ambiguous.

5 Data

The data used come from Stop, Search, and Contraband datasets collected by the North Carolina State Bureau of Investigation, and include all motor vehicle stops in the state of North Carolina between January 1, 2004 and December 31, 2012. I restricted the data to observations with either a coded race of $White$ or ethnicity of *Hispanic*. People who are listed as both white and Hispanic are considered *Hispanic* in the analysis, so the only people considered $White$ are those who are both white and non-Hispanic. All people who are neither white nor Hispanic are excluded from analysis.

The SC, or Secure Communities, variable indicates the timing of the stop relative to the local implementation of Secure Communities. In specification 1, the Secure Communities variable, SC, indicates whether a stop takes place in a jurisdiction that has previously ratified Secure Communities at the time of the stop:

$$SC = \begin{cases} 1 & \text{if stop occurs in county that has previously ratified Secure Communities} \\ 0 & \text{otherwise} \end{cases}$$

In specification 2, Secure Communities will be categorical rather than binary and will reflect the time that has passed since Secure Communities was ratified in the jurisdiction where the stop was made. For these specifications, $SecureCommunities$ implies that the stop took place during the stated 6 month interval after the ratification of Secure Communities, with stops in the control group taking place before the ratification of Secure Communities. The final Secure Communities timing variable uses data confined to stops within a period of three months prior to Secure Communities and 6 months after its ratification, and indicates the month in which the stop occurred. Relatedly, the variable $287g$ will mark whether the jurisdiction in which a stop takes place has previously adopted 287g, a provision that, as described in the background information section, allows local police involvement in immigration enforcement.

The level of discretion that the police have in making a search is reflected in the data by the type for the search, with higher discretion searches denoted by the binary variable $HighDiscSearch$. Following Pickerill, Mosher, and Pratt (2009), searches are marked as high discretion if their search type is consent or protective frisk, and low discretion if the type is due to a search warrant, probable cause, or a search incident to arrest.

The time of a stop is marked at night if it is between the hours of 20:00 and 5:00. Additionally, 99 binary variables were created to add fixed effects for the 100 counties in which stops take place, and year fixed effects were added to the empirical model. The data also contains information on the age and gender of the motorist. The data is confined to stops made by local police within a named county, because the ratification date of Secure Communities is unclear for highway stops not made within county lines. The purpose of a stop is also recorded and will be controlled for:

$$StopPurpose = \begin{cases} 1 & \text{If stop due to speed limit violation} \\ 2 & \text{If stop due to stop light/sign violation} \\ 3 & \text{If stop due to expected DWI} \\ 4 & \text{If stop due to sage movement violation} \\ 5 & \text{If stop due to vehicle equipment violation} \\ 6 & \text{If stop due to vehicle regulatory violation} \\ 7 & \text{If stop due to seat belt violation} \\ 8 & \text{If stop part of an investigation} \\ 9 & \text{If stop due to "other" motor vehicle violation} \\ 10 & \text{If stop occurs at checkpoint} \end{cases}$$

The level of discretion for a stop is not reliably able to be inferred from its stated purpose, and there is therefore no measure of discretion for stops. All other variables are will be used without manipulation. There are 3,837,247 motor vehicle stops and 268,372 motorist searches in the final dataset.

6 Empirical Methodology

As outlined in the theory section, three methods will be used to jointly determine whether the implementation of Secure Communities increased racial bias by police. Each method will use an extended difference-in-difference approach to isolate the effect of Secure Communities on the policing of whites vs. Hispanics. The methods will be distinguished by the unique outcome variable that each employs, and they will largely share covariates and controls in the difference-in-differences; each model will include available covariates to prevent confoundedness to the extent possible. There will likely still exist signals observable to the police but not the data analyst, like the smell of the car or the shiftiness of its driver's eyes, but assuming these signals are not correlated with both the outcome variable and the race/ethnicity of the driver, these omitted variables will not bias the results. Covariates used include the age, gender, and ethnicity of the driver; the county, time of day, and year of the stop; whether the stop was made in a jurisdiction that previously ratified 287g; whether the stop was before or after the implementation of Secure Communities (and in Specification 2 how long before or after the ratification of Secure Communities it was made); and the level of discretion associated with the search. The potential existence of omitted signals that correlate with both the outcome variable and race is a natural limitation of this analysis.

Method 1: Hit rates

The first model will measure hit rates, which is the proportion of motor vehicle searches that yield contraband findings. To do so, it will employ a binary dependent variable of search outcome that indicates whether the motor vehicle search successfully uncovered contraband:

$$Contraband = \begin{cases} 1 & \text{if contraband is found in a motor vehicle search} \\ 0 & \text{if no contraband is found} \end{cases}$$

Since the outcome variable is whether or not police find contraband in a search, the data used to fit this model will be confined to the subset of stopped motorists who are searched.

The model will be fitted using a difference-in-difference-in-difference-in-difference (DDDD) methodology to attempt to isolate the effect of Secure Communities on the hit rates of whites vs. Hispanics. The first difference is whether the search occurs before vs. after the implementation of Secure Communities. By differencing before and after Secure Communities, pre-existing, baseline variation in the propensity of whites vs. Hispanics to carry contraband can be controlled for to isolate the effect of the implementation. The next difference will be Hispanics vs. whites. This difference prevents the influence of exogenous changes that affect the entire population over time (e.g., police budget cuts), and allows determination of how police behavior changed toward Hispanics relative to whites.

The third difference is whether the search occurred in jurisdictions that had previously ratified 287g vs. those that had not. Prior to the implementation of Secure Communities, 287g jurisdictions already provided local police the ability to aid in immigration enforcement and initiate the deportation process for illegal immigrants, so the ratification of Secure Communities in those jurisdictions did not change police incentives as much as in non-287g jurisdictions. Therefore, non-287g jurisdictions in which police incentives changed more dramatically are expected to experience greater effects of racial bias stemming from Secure Communities. The adoption of 287g requires an agreement between ICE and a local police jurisdiction, and is therefore self-selected by police jurisdictions, making it prone to confounding. It is likely that the jurisdictions that enacted 287g were, if anything, more predisposed to racial profiling against Hispanics, and therefore using them as a control group would, if anything, understate the effect that Secure Communities would have had on increasing racial bias in police absent the existence of 287g. This is another potential limitation of the analysis.

The final difference used will be the discretion level of a search, as employed in

Pickerill, Mosher, & Pratt (2009):

$$HighDiscSearch = \begin{cases} 1 & \text{if Search Type is consensual or initiated by protective frisk by a police officer} \\ 0 & \text{if Search Type is due to a search warrant, probable cause, or a search incident to arrest} \end{cases}$$

Police will be more able to exhibit racial bias in searches associated with higher discretion levels, so the effect of Secure Communities on racial bias should be more apparent for high relative to low discretion searches. The results of high and low discretion searches will be differenced to determine if this holds empirically.

Racial bias will be determined by the subset of variables that contain the interaction $Hispanic{:}SC$, which is the effect of Secure Communities on search success rates of Hispanics relative to whites, controlling for the presence of 287g, search discretion, and available covariates.

<u>Method 2: Search rates</u>

The second model will measure the rate at which motorists of different ethnicities are searched conditional on their being stopped. To do so, it will use a binary outcome variable of whether someone is searched:

$$Search = \begin{cases} 1 & \text{if a stopped motorist is searched} \\ 0 & \text{if a stopped motorist is not searched} \end{cases}$$

Since the outcome variable is whether or not police search a vehicle conditional on stop, the data used to fit this model will be include all stopped motorists. The DDD methodology used in Method 2 is similar to that described in Method 1, but search discretion cannot be used since the outcome variable predicts search, and only searched motorists have a value of search discretion. Stop purpose will be controlled for but different purposes are not reliably correlated with police discretion.

Again, racial bias will be determined by the subset of variables that contain the interaction $Hispanic{:}SC$, which is the effect of Secure Communities on police propensity to search Hispanics relative to whites, controlling for the presence of 287g and available covariates.

Method 3: Stop outcome

The fourth model will measure the change in probability of different stop outcomes for whites vs. Hispanics after the ratification of Secure Communities. It will employ a categorical outcome variables:

$$Stop\ Action = \begin{cases} 1 & \text{if stopped motorist is given a verbal warning} \\ 2 & \text{if stopped motorist is given a written warning} \\ 3 & \text{if stopped motorist is given a citation} \\ 4 & \text{if stopped motorist is arrested} \\ 5 & \text{if no action is taken against stopped motorist} \end{cases}$$

This model will again employ a DDD methodology to determine the rate at which Hispanics experience different motor vehicle stop outcomes vs. whites controlling for, similar to Models 1 & 2, the implementation of Secure Communities and the presence of 287g. Here, racial bias will be inferred from the effect of the subset of variables that contain interactions of $Hispanic{:}SC$, which measures the effect of Secure Communities on the propensity of police to perform different actions on stopped Hispanic relative to white motorists, controlling for the presence of 287g and available covariates.

Specification 2: The lagged effect of Secure Communities

All empirical models will be fit for specification 1, which treats Secure Communities as a binary variable, and models for methods 1 & 2 will also be fit for Specification 2, which employs information about the timing of the stop relative to the ratification of Secure Communities.

7 Results

The regressions modeled above as specification 1, methods 1 & 2, were run using the full dataset, the results of which are in Table 1. Analysis was done in R using the *bigglm* function from the *biglm* package. Due to memory constraints, a subset of 100,000 randomly sampled observations from the full dataset was used to fit the models corresponding to method 3 and methods 1 & 2, specification 2. These statistical analyses were also done in R. For all regressions, year and county fixed effects were included but are not reported. Gender, age, and stop purpose are also controlled for but not reported. Night and 287g were controlled for but not reported in Specification 2.

Method 1, which quantifies the effect of the ratification of Secure Communities on police search hit rates of white vs. Hispanic motorists, indicates no effect of Secure Communities on racial bias (Table 1, Column 2). The variable of interest, $Hispanic{:}SC$, is not significant at conventional levels. Classification as a high discretion search, which should be less successful than searches that are low discretion and prompted by events more indicative of the presence of contraband, is correctly associated with significantly lower hit rates than low discretion searches. Furthermore, Hispanics are searched with higher probability in high discretion searches vs. low discretion searches after the ratification of Secure Communities, further rejecting increased racial bias due to Secure Communities. Similarly, 287g was found to be associated with higher hit rates for Hispanics vs. whites, indicating that it also did not prompt increased racial bias by police. There was no significant difference in the effect of Secure Communities in areas that had and had not previously adopted 287g. With 268,372 observations, any lack of significance is not due to a lack of precision or power in the analysis, but rather a lack of measurable effect.

Method 2, which quantifies the effect of ratification of Secure Communities on the rate at which Hispanics vs. whites are searched conditional on being stopped, also provides no evidence that Secure Communities affected racial bias by police (Table 1, Column 1). The variable of interest, $Hispanic{:}SC$, is significantly negative, indicating that fewer fewer Hispanics were searched conditional on being stopped after the ratification of Secure Communities. Therefore, while Hispanics may be searched significantly more in general, this disparity is not exacerbated by Secure Communities. The decrease in search rate that occurs after ratification of Secure Communities is partially reversed for stops made in areas that had previously adopted 287g, but the net effect of Secure Communities still does not suggest an increase in racial bias.

Method 3, which assess the propensity of police to take different actions against stopped motorists, also suggests no evidence of racial bias by police, the results of which are presented in Table 2. There is no significant effect of Secure Communities on the rate at which Hispanics vs. whites are arrested relative to given warnings or cited. Hispanics are arrested relatively more after the ratification of Secure Communities in places that had previously adopted 287g, but less at night, the aggregation of which yield no evidence that Secure Communities caused an increase in racial bias by police.

The regressions fit to model Methods 1 & 2 were re-run with Secure Communities timing indicator variables to assess the existence of a lagged effect of Secure Communities, the results of which are included in tables 3-4. The month indicators do not help to tell a story that indicates racial bias or updating of statistical discrimination as outlined in section 4.3. There are no discernible trends of the measured effect of the ratification of Secure Communities on racial bias of police against Hispanics vs. whites. This result provides compelling evidence against the theory that Secure Communities led to increased racial bias by police. The results also suggest that either there was no change in the behavior of whites vs. Hispanics after the implementation of Secure Communities, or that police behavior adjusted instantaneously in response to any changes in the behavior of Hispanics vs. whites.

Table 1: Regression Results, Specification 1, Methods 1 & 2

	Dependent variable:	
	Search (Method 2)	Contraband (Method 1)
SC	-0.137^{***}	0.186^{***}
	(0.011)	(0.025)
Night	0.319^{***}	0.060^{**}
	(0.013)	(0.027)
287g	0.010	0.191^{***}
	(0.010)	(0.020)
SC:287g	-0.141^{***}	-0.176^{***}
	(0.013)	(0.026)
Night:SC	-0.064^{***}	0.014
	(0.011)	(0.022)
Hispanic	0.091^{***}	-0.834^{***}
	(0.026)	(0.060)
Hispanic:SC	-0.353^{***}	-0.048
	(0.021)	(0.059)
Hispanic:287g	-0.014	0.121^{***}
	(0.358)	(0.033)
Hispanic:Night	-0.364^{***}	0.343^{***}
	(0.012)	(0.027)
Hispanic:SC:287g	0.152^{***}	-0.108^{*}
	(0.027)	(0.059)
Hispanic:Night:SC	-0.013	0.184^{***}
	(0.025)	(0.057)
HighDiscSearch		-0.502^{***}
		(0.011)
SC:HighDiscSearch		-0.142^{***}
		(0.022)
Hispanic:HighDiscSearch		-0.119^{***}
		(0.027)
Hispanic:SC:HighDiscSearch		0.159^{***}
		(0.056)
Observations	3,837,247	268,372
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 2: Regression Results, Specification 1, Method 3. Baseline is Verbal Warning ($N_{\text{Verbal Warning}} = 24,124$; $N_{\text{All}} = 100,000$)

	Dependent variable:			
	Written Warn	Citation	Arrest	No Action
SC	0.071	-0.132^{***}	0.035	0.886^{***}
	(0.055)	(0.042)	(0.124)	(0.136)
Hispanic	0.063	0.366^{***}	1.119^{***}	0.700^{***}
	(0.109)	(0.073)	(0.144)	(0.168)
287g	-0.066	0.035	-0.136^{*}	-0.218^{**}
	(0.055)	(0.037)	(0.082)	(0.090)
Night	-0.263^{***}	-0.436^{***}	0.008	-0.114
	(0.068)	(0.050)	(0.117)	(0.121)
Hispanic:SC	-0.011	0.075	-0.249	0.294
	(0.194)	(0.132)	(0.270)	(0.237)
SC:287g	-0.309^{***}	-0.345^{***}	-0.323^{***}	0.084
	(0.067)	(0.047)	(0.114)	(0.110)
Night:SC	-0.038	-0.039	-0.066	-0.173^{*}
	(0.056)	(0.040)	(0.102)	(0.094)
Hispanic:Night	0.199^{**}	0.277^{***}	0.240^{**}	0.041
	(0.099)	(0.065)	(0.118)	(0.153)
Hispanic:287g	-0.085	0.005	-0.098	0.113
	(0.132)	(0.079)	(0.141)	(0.172)
Hispanic:SC:287g	-0.262	-0.051	0.543^{**}	-0.109
	(0.200)	(0.116)	(0.240)	(0.239)
Hispanic:Night:SC	-0.163	-0.263^{**}	-0.660^{***}	0.039
	(0.178)	(0.107)	(0.224)	(0.223)
Observations	12,384	57,187	3,462	2,843
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 3: Regression Results, Specification 2a, Methods 1 & 2
(baseline is prior to ratification of Secure Communities)

	Dependent variable:	
	Search (Method 2)	Contraband (Method 1)
SC(0-6mo)	−0.112	0.260***
	(0.183)	(0.072)
SC(6-12mo)	−0.074	0.393***
	(0.199)	(0.073)
SC(>12mo)	−0.160	0.227***
	(0.128)	(0.047)
Hispanic:SC(0-6mo)	−0.387	0.216
	(0.427)	(0.182)
Hispanic:SC(6-12mo)	0.152	0.121
	(0.406)	(0.189)
Hispanic:SC(>12mo)	−0.162	−0.105
	(0.277)	(0.126)
Hispanic:SC(0-6mo):287g	0.425	−0.017
	(0.320)	(0.173)
Hispanic:SC(6-12mo):287g	0.503*	−0.053
	(0.301)	(0.185)
Hispanic:SC(>12mo):287g	0.228	−0.179
	(0.210)	(0.123)
Hispanic:Night:SC(0-6mo)	−0.110	−0.068
	(0.316)	(0.171)
Hispanic:Night:SC(6-12mo)	−0.128	0.097
	(0.302)	(0.185)
Hispanic:Night:SC(>12mo)	−0.184	0.252**
	(0.204)	(0.121)
Hispanic:SC(0-6mo):HighDiscSearch		0.143
		(0.170)
Hispanic:SC(6-12mo):HighDiscSearch		0.039
		(0.181)
Hispanic:SC(>12mo):HighDiscSearch		0.139
		(0.119)
Observations	100,000	100,000
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 4: Regression Results, Specification 2b, Methods 1 & 2
(baseline is 2-3 months prior to ratification of Secure Communities)

	Dependent variable:	
	Search (Method 2)	Contraband (Method 1)
SC(1-2mo prior)	0.122	0.091
	(0.219)	(0.158)
SC(0-1mo prior)	0.414**	−0.011
	(0.204)	(0.154)
SC(0-1mo after)	0.283	0.068
	(0.206)	(0.151)
SC(1-2mo after)	0.294	0.114
	(0.202)	(0.151)
SC(2-3mo after)	0.110	0.222
	(0.208)	(0.148)
SC(3-4mo after)	−0.003	−0.278*
	(0.212)	(0.154)
SC(4-5mo after)	0.228	−0.638***
	(0.206)	(0.153)
SC(5-6mo after)	0.205	0.111
	(0.208)	(0.152)
Hispanic:SC(1-2mo prior)	−1.283**	−0.309
	(0.499)	(0.391)
Hispanic:SC(0-1mo prior)	−0.578	−0.393
	(0.400)	(0.414)
Hispanic:SC(0-1mo after)	−0.969**	−0.402
	(0.452)	(0.395)
Hispanic:SC(1-2mo after)	−0.937**	0.021
	(0.445)	(0.368)
Hispanic:SC(2-3mo after)	−0.610	−0.623
	(0.432)	(0.387)
Hispanic:SC(3-4mo after)	−0.302	−0.102
	(0.423)	(0.389)
Hispanic:SC(4-5mo after)	−1.314***	−0.348
	(0.469)	(0.417)
Hispanic:SC(5-6mo after)	−0.642	−0.614
	(0.424)	(0.403)
Observations	100,000	21,731
Note:	*p<0.1; **p<0.05; ***p<0.01	

8 Conclusions

Three methodologies were developed using different outcome variables to identify if the ratification of Secure Communities, a federal program that allows police officers to more easily identify illegal immigrants, affected racial bias by police against Hispanics vs. whites. Given the assumptions behind the models, analysis of motor vehicle stop and search data from the North Carolina State Bureau of Investigation from 2004 to 2012 indicates no compelling evidence of an increase in racial profiling by police officers due to Secure Communities. The lack of evidence to support the claim that Secure Communities has prompted racial profiling by police against Hispanics is at odds with the numerous anecdotes of seemingly clear examples of racial profiling and the descriptive statistics that some have used to infer widespread racial bias by police; if racial bias does exist among police officers, it must have existed before the ratification of Secure Communities and was not exacerbated by its implementation. Furthermore, the prospect of policy founded on conclusions from anecdotes and other circumstantial evidence threatens societal well-being. One illustrative example can be found in a closer examination of the scandal that surrounded the adoption and existence of 287g in Alamance County, NC.

In 2012, 5 years after Alamance County adopted 287g in 2007, the Alamance County Sheriff's Office was taken to court and found guilty on the grounds that the program promoted racial profiling by police, which precipitated the repeal of 287g in Alamance County.[10] The case largely rested on the analysis of "experts," who used statistics to demonstrate the existence of racial bias by police that was due to the 287g program.[11,12] Contrary to their findings, running the models developed above with the dataset confined to observations from exclusively Alamance County from January 1, 2004, to December 31, 2012, yields no convincing statistical evidence of a change in racial profiling, as evidenced by tables 5-6. In Table 5, results from method 2 indicate that there is no change in the rate at which stopped motorists are

[10] Perez, *Re: United States' Investigation of the Alamance County Sherriff 's Office.*

[11] MacDonald, *Expert Report on the Alamance County Sheriff 's Office.*

[12] Lamberth, *Expert Report on the Alamance County Sheriff 's Office.*

searched for Hispanics vs. whites coinciding with the adoption and repeal of 287g. Furthermore, hit rates did not significantly change with the adoption of 287g, as evidenced by the results from method 1. Finally, results from method 3 suggest that the adoption of 287g is associated with an increase in the rate at which Hispanics are arrested relative to given verbal warnings, which provides suggestive evidence of racial bias by police. In context with the unaffected hit rates, however, it seems that the increase in arrests has not been detrimental to police ability to do their jobs as well as possible, and therefore not indicative of negative racial bias.

Taken together, there is little compelling evidence that the adoption of 287g had any incremental effect on racial bias by police against Hispanics in Alamance County. These results should alert people to the risks of taking summary statistics at face value. It is possible that police in Alamance County may disadvantage Hispanics relative to whites, but the level of bias appears unaffected by 287g; since the statistics used by the experts did not control for the ratification of 287g, their analysis could have confused racial bias that has always existed in the Alamance police force with an effect of the 287g program. For example, results from method 2 reveal that Hispanics in Alamance County are significantly more likely to be searched conditional on stop relative to whites, and results from method 3 indicate that police are significantly more likely to arrest or give a citation to a stopped Hispanic motorist relative to a white motorist. If this illustrative conjecture is true, citizens may be appeased by the repeal of 287g, but it would not actually solve the problem of racial bias in Alamance County. A better policy would allow 287g to continue but attempt to decrease racial bias in the Alamance County Police by putting officers through training programs, hiring a more diverse police force, or relieving the officers with the most severe record of bias of their duties.

Table 5: Regression Results for Alamance, Methods 1 & 2, Spec. 1

	Dependent variable:	
	Search (Method 2)	Contraband (Method 1)
SC	0.082	0.088
	(0.108)	(0.178)
287g	-0.164^{**}	0.648^{***}
	(0.071)	(0.173)
Night	0.410^{***}	0.318
	(0.101)	(0.220)
Night:SC	0.429^{***}	0.154
	(0.099)	(0.194)
Night:287g	-0.211^{**}	-0.364^{*}
	(0.094)	(0.189)
Hispanic	1.026^{***}	-0.201
	(0.160)	(0.337)
Hispanic:SC	0.032	0.674^{**}
	(0.203)	(0.343)
Hispanic:g	0.014	-0.414
	(0.115)	(0.302)
Hispanic:Night	-0.334^{***}	-0.314
	(0.098)	(0.211)
Hispanic:Night:SC	-0.036	-0.534
	(0.177)	(0.399)
Hispanic:Night:287g	-0.116	0.223
	(0.158)	(0.361)
HighDiscSearch		-0.545^{***}
		(0.118)
SC:HighDiscSearch		-0.462^{**}
		(0.187)
287g:HighDiscSearch		-0.285
		(0.185)
Hispanic:HighDiscSearch		-0.137
		(0.213)
Hispanic:SC:HighDiscSearch		0.434
		(0.437)
Hispanic:287g:HighDiscSearch		-0.310
		(0.406)
Observations	61,066	6,021
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$	

Table 6: Regression Results in Alamance, Specification 1, Method 3. Baseline is Verbal Warning ($N_{\text{Verbal Warning}} = 22,207; N_{\text{All}} = 61,066$)

	Dependent variable:			
	Written Warn	Citation	Arrest	No Action
287g	-0.313^{***}	-0.325^{***}	-0.506^{***}	-0.217^{**}
	(0.068)	(0.035)	(0.106)	(0.093)
SC	-0.122^{**}	0.037^{*}	0.034	-0.076
	(0.049)	(0.023)	(0.080)	(0.104)
Hispanic	0.010	0.447^{***}	1.511^{***}	0.675^{***}
	(0.147)	(0.070)	(0.140)	(0.196)
Night	-0.326^{***}	-0.606^{***}	0.123	-0.342^{**}
	(0.122)	(0.063)	(0.143)	(0.151)
SC:287g	-0.122^{**}	0.037^{*}	0.034	-0.076
	(0.049)	(0.023)	(0.080)	(0.104)
Hispanic:287g	-0.039	0.103	0.534^{***}	0.074
	(0.172)	(0.081)	(0.170)	(0.201)
Hispanic:SC	-0.014	-0.108^{*}	-0.190	-0.343^{**}
	(0.156)	(0.065)	(0.146)	(0.168)
Night:SC	0.386^{***}	-0.226^{***}	-0.105	-0.115
	(0.119)	(0.058)	(0.141)	(0.130)
Night:287g	-0.134	0.011	0.250^{*}	0.184
	(0.110)	(0.057)	(0.133)	(0.133)
Hispanic:Night	-0.103	-0.117	-0.281^{*}	0.283
	(0.156)	(0.080)	(0.145)	(0.189)
Hispanic:SC:287g	-0.014	-0.108^{*}	-0.190	-0.343^{**}
	(0.156)	(0.065)	(0.146)	(0.168)
Hispanic:Night:SC	-0.978^{***}	-0.124	-0.045	0.369
	(0.320)	(0.126)	(0.241)	(0.288)
Hispanic:Night:287g	-0.084	0.287^{**}	-0.203	-0.522^{*}
	(0.269)	(0.121)	(0.219)	(0.279)
Observations	3,023	30,197	3,380	2,259
Note:				*p<0.1; **p<0.05; ***p<0.01

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