

# Residency Blues: The Unintended Consequences of Police Residency Requirements <sup>\*</sup>

Julia Payson<sup>†</sup>

Srinivas Parinandi<sup>‡</sup>

June 2023

## Abstract

Do residency requirements change bureaucratic performance? We study the case of municipal police departments. While residency rules were popular in the 1970s, many cities and states abolished these policies in the 1990s and early 2000s. Drawing from an original survey and local archival sources, we hand collect data on the police residency laws of nearly 800 of the largest municipalities in the U.S. over the past three decades. We then test competing theoretical predictions about how these rules impact the racial composition of city police forces and the probability of fatal police-civilian encounters. Using a two-way fixed effects design, we find that residency requirements modestly improve police diversity, but fatal encounters are actually more likely when residency requirements are in place. This study provides the most credible evidence to date that residency rules do little to improve police performance and may not offer a particularly fruitful avenue for reform.

---

<sup>\*</sup>Authors contributed equally. For helpful comments and suggestions, we thank David Fortunato, Liz Gerber, Vlad Kogan, Zac Peskowitz, Kaylyn Schiff, Elisa Wirsching, Hye Young You, and participants at the APSA 2021 “Empirical Advances in Law Enforcement” panel. We also thank seminar and workshop participants at the Stanford Graduate School of Business Political Economy Seminar, the American University Government Department Speaker Series, and the NYU PIPER Lab. We gratefully acknowledge the hard work of our research assistants: Genevieve Andersen, Dinasha Ediriweera, Sebastian Kim, Wyatt Lowrie, Shaikh Mahsheeat, Sara McKinley, Khalia Reed, Jake Schneebaum, Reva Singh, and Seden Yilmazturk. We also acknowledge the invaluable financial support from the NYU Stephen Charney Vladeck Junior Faculty Fellowship.

<sup>†</sup>Assistant Professor, Department of Politics, University of California, Los Angeles (jpayson@ucla.edu).

<sup>‡</sup>Assistant Professor, Department of Political Science, UC Boulder (srinivas.parinandi@colorado.edu).

# 1 Introduction

After the 2020 killing of George Floyd by a Minneapolis police officer, protests against police brutality swept the country. Community-led movements called for policy changes ranging from defunding the police to ending chokeholds to investing in new social programs, and hundreds of cities and law enforcement departments across dozens of states pledged reform. Amid these debates, one proposal that gained traction was the idea of residency requirements, or mandating that police officers reside in the communities where they work. Supporters of these laws tout their ability to bolster the local tax base, increase the diversity of municipal employees, and foster deeper and richer ties between officers and their communities (Ostrom and Whitaker 1973; Hirsch and Rufolo 1985; Ogletree et al. 1995; Murphy and Worrall 1999). Opponents of these rules (including police unions) have argued that residency requirements limit the talent pool for officer recruitment and can create safety issues for officers during periods of increased tension between communities and law enforcement (Bouza 1978; Dorschner 1989; Schulz 2021).

In the 1970s, residency rules were the norm, and more than half of America’s largest cities required public safety officials to live within the boundaries of the cities they served as a condition of employment (Hirsch and Rufolo 1985). But over the past few decades, many cities and states across the country have rolled back their residency rules, often in response to political pressures. In Detroit, non-residency rates among police officers jumped from 20% to 75% following a new state law banning local residency requirements (Neavling 2017). The Minnesota State Legislature similarly overturned a municipal residency provision in Minneapolis in 1999, and by the time of George Floyd’s death only 8% of Minneapolis Police Department officers lived in the city (CBS Minnesota 2020).

Do residency requirements change the bureaucratic behavior of local police departments? A growing body of political science research explores the institutional determinants of police performance (Mummolo 2018*b*; Goldstein, Sances, and You 2020) and the consequences of

police-civilian contact on democratic participation and engagement (Lerman and Weaver 2014; Laniyonu 2019; Cohen et al. 2019). Scholars have offered theoretical arguments both for and against residency rules, but most empirical studies in this area date to the 1980s and 1990s and consist of cross-sectional analysis of a small number of cities (e.g. Smith 1980; Eisinger 1983; Murphy and Worrall 1999), making policy evaluation difficult. Despite the lack of evidence that residency requirements matter, state and local politicians continue to actively debate these measures. While places like Chicago and Buffalo have maintained police residency laws for decades, cities including Rochester, Milwaukee, and Baltimore are currently considering enacting new residency guidelines. Still other high-profile cities have just recently had their residency rules overturned by their state legislatures—including St. Louis in 2020.

In this article, we bring new data to bear on this question by conducting an original survey of nearly 800 cities to learn about their history of police residency requirements over the past three decades. We then test two key claims made by the existing literature about whether (1) residency rules promote bureaucratic diversity and (2) residency rules improve police-civilian contact. Using a within-city design that exploits the fact that a growing number of cities have dropped their residency rules in recent years, we find that local law enforcement agencies become whiter after these requirements are removed. However, we also find that civilian fatalities at the hands of police officers actually *decrease* after cities drop their residency requirements. This result is driven by cities that change their requirements locally—rather than via state mandate—and we uncover suggestive evidence that the choice to relax residency rules is often accompanied by other reforms that might more effectively reduce police violence.

This paper makes three primary contributions. First, our original panel dataset on changes in residency requirements for a large sample of U.S. cities represents the most comprehensive data collection effort on this topic that we are aware of. We hope that this database will inspire additional research in this area by scholars and policy practitioners

alike. Second, our results speak to important theoretical questions about how to achieve descriptive representation and encourage effective performance among local bureaucrats (e.g. Eisinger 1982; Selden, Brudney, and Kellough 1998; Meier, Wrinkle, and Polinard 1999; Theobald and Haider-Markel 2009). Finally, we add to a growing body of empirical literature both in the U.S. and in the comparative context showing that many reform efforts fail to meaningfully improve police performance (e.g. Mummolo 2018*a*; Gonzalez 2020; Blair et al. 2021; Eckhouse 2022). However, while our study provides some of the most systematic evidence to date on the effects of residency requirements, we also emphasize that one of the issues with current debates about these laws is that scholars face considerable obstacles to conducting robust and generalizable policy analysis given the available data, which is a challenge we highlight throughout the paper.

## 2 Competing Theoretical Predictions

Residency rules originated at the turn of the 20th century to facilitate patronage in industrialized cities. Aldermen commonly staffed municipal jobs with friends and loyal residents of their respective wards, and residency rules institutionalized this practice (Anderson 1925). During the Progressive Era, reformers took aim at these requirements, arguing that they hindered merit-based hiring and promoted corruption (Mosher, Kingsley et al. 1941; Wilson 1950). Many cities subsequently dropped their residency rules and adopted civil service reforms to govern their hiring. But after the Civil Rights movement, these laws regained popularity in the 1970s as urban scholars and reformers advocated for community-based approaches to policing. In 1977, two-thirds of cities with populations over 250,000 enforced police residency requirements (Eisinger 1983).

Two of the primary theoretical arguments in favor of residency requirements contend that these laws will promote descriptive representation among police officers and will improve police-community relationships through both selection and contact (e.g. Murphy and Worrall

1999; Smith and Holmes 2003; Trochmann and Gover 2016). In this paper, we assess these claims by studying how the removal of residency rules affects the racial composition of municipal police forces and the prevalence of fatal encounters between police and civilians—arguably the most extreme example of a negative outcome in terms of police-civilian contact. While existing theory and literature tends to focus on why cities might adopt these rules in the first place, we unfortunately lack outcome data from the period when most of these laws were institutionalized. Instead, we leverage the fact that an increasing number of cities have abandoned their residency requirements in recent decades. Observing how agency conditions evolve after this change can lend insight into what residency rules were achieving for cities when they were in place, as any benefits (or problems) attributable to these requirements should reverse when the law is removed.

## 2.1 Why Residency Rules Might Improve Police Performance

Proponents of residency requirements posit that these laws promote hiring and recruitment practices that lead police forces to more closely represent the communities they serve—particularly in terms of race. This was one of the most common rationales offered by cities that adopted residency requirements in the 1970s and 1980s (Eisinger 1983). During this time, police departments often turned to these rules to prevent white officers from moving to the suburbs and to encourage hiring and recruitment efforts among local residents of color (Livengood and Annalise 2020). Hinting at the relationship between race and residency, one study found that 49 percent of black police officers in the 75 largest municipal departments lived within the boundaries of the cities they served in 2014, while only 35 percent of white officers did (Silver 2014).<sup>1</sup>

There are a variety of reasons why racially diverse police forces might in turn lead to better relationships between police and communities and thus decrease the risk of fatal encounters. Scholars have demonstrated that more descriptively representative police departments are

---

<sup>1</sup>Throughout the manuscript, we use the term “white” to refer to officers self-identifying as non-Hispanic white.

associated with an increased sense of legitimacy among the public, which can facilitate community cooperation and lead to more effective policing (Skogan and Frydl 2004; Gau and Brunson 2010; Riccucci, Van Ryzin, and Lavena 2014). A growing body of empirical evidence also shows that non-white police officers behave differently and are more responsive to crime victimization reports from racial minorities (Harvey and Mattia 2022) and less likely to use excessive force in their encounters (Ba et al. 2021). Residency rules may thus indirectly improve the quality of police-civilian interactions by increasing department diversity.

Residency rules might also directly impact the relationship between police and the communities they serve by strengthening ties between officers and residents. This is theorized to happen both through selection—hiring officers who are from certain communities and therefore already have a stake in them—and through contact. Qualitative evidence suggests that officers who grew up in the cities where they work are better able to relate and identify with community members (Swank and Conser 1983). One officer explained in an interview, “I get to already kinda have a rapport with some people from the community. I’m socially embedded basically here. My church is here, my family is here, friends here since elementary school and up” (Headley 2022). Local officials often invoke similar arguments when justifying their support for residency requirements. As Philadelphia City Council Member Kenyatta Johnson stated, “It’s a plus if we have officers who live in the city, they grew up in the city, they have a stake in the city because it’s home. It goes a long way to building community trust” (Hauck and Nichols 2020).

Finally, even when officers aren’t long-time residents, residency requirements might also foster rapport through contact. The logic behind this expectation comes from a positive interpretation of the contact hypothesis (Allport 1954). A large empirical literature shows that, under certain conditions, regular interactions between group members can foster a sense of shared humanity, promote ties, and strengthen inter-group relationships (Pettigrew and Tropp 2006). In the case of police-civilian relations, the contact hypothesis predicts that regular engagement between police and communities should engender greater trust

between both groups and thus improve policing quality (Hennessy 1993). Police residency requirements, in turn, may institutionalize regular contact, helping to facilitate trust-building between police and the residents they serve (Ogletree et al. 1995).

In short, if residency rules encourage the recruitment of officers of color or limit the ability of white suburban officers to work in central cities, *we should expect these laws to be associated with a lower proportion of white officers on the force*. Supporters of these requirements further theorize that they will improve police-community relationships both indirectly (through diversification) and directly (through selection and contact). If this is the case, *we should observe fewer fatal encounters between police and civilians when residency laws are in place*, and the removal of these laws should increase the proportion of white officers and worsen contact-related outcomes.

## 2.2 Why Residency Rules Might Not Work As Intended

In contrast to the predictions outlined above, other scholars and policy practitioners have pointed out that these rules may not actually facilitate the desired goals and may even lead to unintended consequences. In terms of officer diversity, cities and police departments can engage in recruitment efforts to hire officers from different racial backgrounds whether or not a residency rule is in place (Bednar 2020). Chicago, a city with a residency requirement, recently experienced a drop in the percentage of black officers on the force following a hiring initiative in 2019 (Schulz 2021). If anything, these restrictions might limit the potential applicant pool—an argument frequently made by public safety unions (Bouza 1978; Dorschner 1989). For example, in an amicus curiae brief filed in support of an Ohio law banning local residency requirements, the Association of Professional Fire Fighters claimed that abolishing these rules “increases [a city’s] applicant pool and makes it more likely that it will be able to hire and retain qualified employees” (*City of Lima v. State of Ohio* 2008).

Even if residency rules do encourage departments to hire more diverse recruits, some research finds that officers of color are no less likely to kill civilians than white officers (Pao-

line, Gau, and Terrill 2018). Notably, Nicholson-Crotty, Nicholson-Crotty, and Fernandez (2017) find a positive correlation between fatal civilian encounters and the percentage of black officers on municipal police forces, which attenuates only when the number of black officers reaches a sufficiently high threshold. This result is consistent with scholarship arguing that minority group members are often “particularly zealous adherents to organizational or majority group norms” (Nicholson-Crotty, Nicholson-Crotty, and Fernandez 2017: pg. 212), especially when their numbers are low. The link between the racial composition of police officers and use of force is complex, and an improvement on the first dimension does not necessarily imply an improvement on the second.<sup>2</sup>

Residency laws also may not be particularly effective at promoting contact between police and communities if officers are able to live in a few concentrated enclaves within the cities they serve. From Warrendale in Detroit to Mount Greenwood in Chicago to Staten Island in New York City, cops and firefighters have long been known to cluster together in “copper canyons” that tend to be predominantly white and middle-class and located on the outskirts of the city (Livengood and Annalise 2020). Recent work by Ba et al. (2021) systematically demonstrates this pattern by drawing from voter file and census data for the country’s 100 largest agencies to show that police officers tend to live in whiter, richer, more Republican neighborhoods relative to other city residents. Leaving aside whether residency rules actually promote police-community contact, scholarship by Bertrand and Duflo (2017) and others has demonstrated that many of the positive outcomes attributed to contact can actually be explained by self-selection (e.g. more tolerant individuals may be more likely to seek out exposure to out-group members). An alternative hypothesis—conflict theory—predicts that physical proximity between members of different groups can actually lead to decreased trust and worse outcomes, including fatal encounters (Blumer 1958; Quillian 1995; Bobo 1999).

Finally, opponents of residency requirements have long argued that these laws are unpopular with police officers and can lead to morale problems (Swank and Conser 1983). Officers

---

<sup>2</sup>For a review of the criminology literature on the correlates of police use of force, see Brandl and Strohshine (2013).

living in cities with residency rules frequently claim that they fear for the safety of their families since people are more likely to know where they live (Chase 1979; Coleman 1983). If residency requirements cause cops to feel resentful and fear retaliation, they may be less well-equipped to perform their jobs. A variant of the morale problem could be that police officers feel politically marginalized in cities with residency requirements and embrace a fortress-like mentality to their jobs that might correlate with the use of deadly force (Beckett and Herbert 2008). In this case, removing the residency rule might reduce the risk of fatal encounters between police and civilians.

To sum up, if the arguments described in this section are correct, we *may or may not observe a difference in the proportion of white officers* when cities adopt (or drop) residency rules. Similarly, there are a variety of reasons why residency requirements might not improve police-civilian contact and reduce civilian deaths, including a restricted pool of qualified applicants, the presence of police enclaves, and lower officer morale. Under these conditions, *the rate of fatal encounters will be no better (and perhaps even worse)* when these requirements are in place, and dropping these rules might therefore *decrease* the risk of civilian deaths. In Table 1, we summarize the various predictions offered by the existing literature.

We note that the goal of this paper is not to introduce new theory to the lively debates surrounding residency requirements. Rather, we aim to test the competing claims introduced in Table 1 to help guide future theorizing and narrow down the possible mechanisms at work. One of the issues facing research on residency requirements is that existing empirical work is sparse, dated, and yields mixed results. Early work by Smith (1980) uncovered a positive correlation between the number of officers in residence and crime clearance rates as well as perceptions of police officers. Coleman (1983) similarly observed that police that lived in the cities they served claimed to be more interested in community welfare and more concerned about treating civilians fairly. However, Murphy and Worrall (1999) found that survey respondents living in cities with residency rules actually reported lower levels of trust in the police, and Smith and Holmes (2003) failed to detect a link between residency requirements

**Table 1: Competing Theoretical Predictions**

Theoretical Claim	Proposed Mechanisms	Observable Implication
Residency rules improve officer racial diversity	Rules prevent white suburban officers from working in urban agencies; agencies will more closely reflect demographics of cities	Dropping requirement → whiter police forces
Residency rules have no effect on racial diversity	Cities can prioritize diverse hiring with or without a residency law; residency laws do not guarantee racial diversity in recruitment	Dropping requirement → no change to racial composition of force
Residency rules improve police-civilian contact	More diverse police forces; selection of officers already invested in city; more contact between police and city residents	Dropping requirement → more fatal encounters
Residency rules do not improve (and may worsen) police-civilian contact	Officers enclaves can limit contact; restricted pool of qualified applicants; lower officer morale	Dropping requirement → no change or fewer fatal encounters

and complaints of police brutality. More recently, Allen and Parker (2013) surveyed officers in a midwestern city and found that residency status was weakly but positively linked with officers reporting more favorable opinions about the quality of police-civilian relations. But Trochmann and Gover (2016) found no effect of the percentage of officers living within city limits and use of force complaints reported in the 2003 or 2007 Law Enforcement Management and Administration Survey.

This research provides a helpful starting point for thinking about how residency requirements might affect police performance. However, these studies are also all cross-sectional and typically cover only a small number of the largest cities. Any correlation between residency rules and outcomes might therefore reflect underlying differences in the types of cities that adopt these laws in the first place. We build on this existing work by collecting data on hundreds of cities and exploiting within-city changes to residency rules over time, which allows us to more credibly study the effects of these policies.

### 3 New Data on Residency Requirements

For our analysis, we hand collected data on the residency requirements for the universe of municipal police agencies with at least 100 officers as well as a random sample of smaller agencies that appeared in at least one Law Enforcement and Administrative Statistics (LEMAS) survey between 1987 and 2020—nearly 800 agencies in total. The LEMAS survey has been administered by the Bureau of Justice Statistics roughly every three years since 1987 with the goal of obtaining information about the responsibilities, operations, and agency characteristics of local law enforcement agencies across the country. The LEMAS survey always requests information from agencies with at least 100 officers in addition to a sample of smaller agencies, and the response rate for municipal departments is around 80%.<sup>3</sup>

While there are over 18,000 law enforcement agencies across the U.S., the vast majority are small town police departments with 10 or fewer officers (Banks et al. 2016). In contrast, the cities that have historically experimented with residency rules tend to be large urban centers nestled among surrounding suburbs or exurbs within commuting distance of the central city (Eisinger 1983). Our sample of 787 agencies identified from the LEMAS data includes all of the 300 largest cities in the U.S. with municipal police departments.<sup>4</sup> We selected 100 sworn officers as our cutoff because below this threshold virtually no agencies have strict residency rules, and the theoretical arguments underpinning such laws typically focus on large and mid-sized cities with 1) sizeable police forces and 2) nearby suburbs where officers might choose to live in the absence of a residency requirement.

Although the LEMAS survey originally included a question about residency requirements, it stopped doing so in 1997. Because many of the high profile instances of state governments banning local residency requirements occurred during the 1990s and early 2000s (Wilson

---

<sup>3</sup><https://bjs.ojp.gov/data-collection/law-enforcement-management-and-administrative-statistics-lemas#methodology-0>

<sup>4</sup>The only exception is Las Vegas which is a city-county police department that filed as county rather than a municipal department for most of the LEMAS surveys. The handful of other cities that don't appear in the sample are places like Santa Clarita, California, that rely on county sheriff's departments or neighboring jurisdictions for local law enforcement.

et al. 2010), we opted to contact agencies directly to gather up-to-date information about their residency rules. In total, 452 cities responded to our survey, and we were able to obtain information on the history of residency requirements for the remaining 335 cities by relying on local newspapers, municipal codes, collective bargaining agreements, and other publicly available sources. The final panel consists of 787 municipal law enforcement agencies and runs from 1987 to 2020. For additional details about the data collection process, see the on-line appendix.

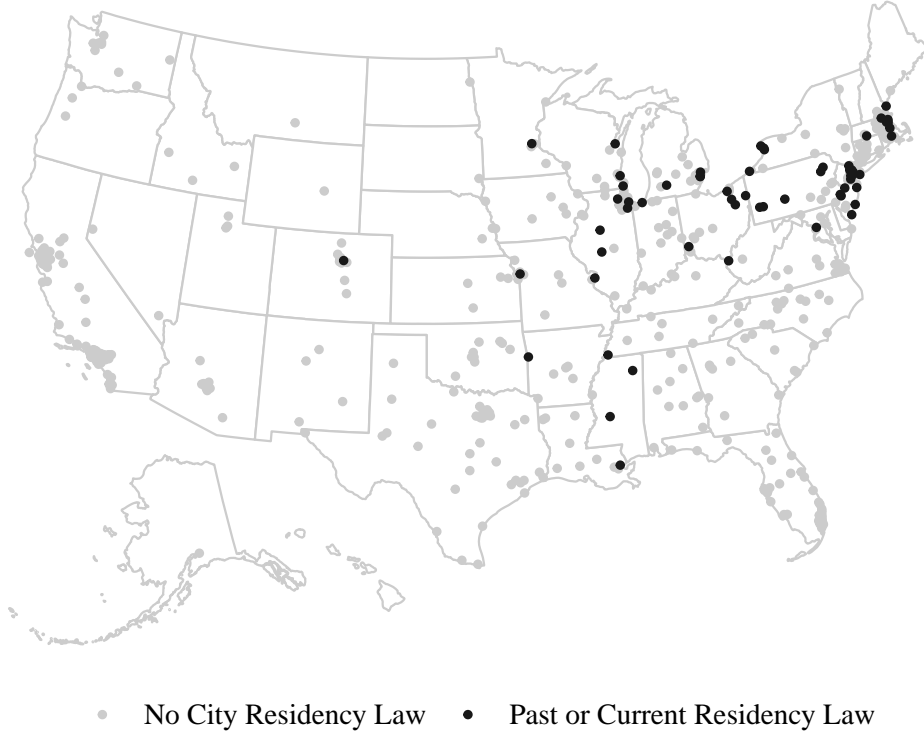
While many cities and states are currently contemplating changes to their residency laws, the data reveal that strict municipal residency requirements for police are relatively rare. In fact, only 46 cities in our sample (6%) had a city residency rule on the books in 2020. We define an agency as having a city residency rule if officers are required to live within city limits for at least five years as a condition of employment, as is the case in Philadelphia. Many departments mandate that officers live no farther than a neighboring county or within a certain distance from the city center, but this is primarily to ensure minimum response times in case of emergency (Maynard 2013). We focus specifically on requirements involving city boundaries, which is the type of law at the heart of most theoretical and policy debates.<sup>5</sup>

Figure 1 shows the location of cities that ever had a city residency requirement over the past three decades. Residency rules for police officers are especially common in the Northeast, which hints at the historical origins of these requirements in large industrial cities. Some of the cities in our sample maintained their residency rules over the entire course of the panel, including Chicago and Boston. Other cities like Denver, New Orleans, and Milwaukee experienced a change to their policy at some point. We note that although relatively few cities have ever enforced residency requirements during the period under study, the cities that have done so are among the largest and most important metropolitan centers in the country, with these laws collectively affecting over 15 million people.

---

<sup>5</sup> Among the cities in our sample, there were none that required residency in the city prior to employment. However, some cities offer special consideration in hiring (e.g. a salary bonus or more points on the entrance exam) to residents who have lived in the city for a certain length of time, and some states require in-state residency for a certain number of months prior to taking the civil service exam.

**Figure 1: Cities and Police Residency Requirements.** Shows the cities in our sample and whether they ever had a residency requirement between 1987 and 2020.



In Table A.1 in the appendix, we show descriptive statistics at the city level for each of the variables used in the following analyses. The median city in our sample has a population just under 100,000 and resembles a place like Kenosha, Wisconsin, or Youngstown, Ohio. We also conduct balance tests on each control variable and outcome for cities with and without residency rules (Table A.2). Cities with municipal residency requirements are larger than other cities but similar in terms of racial composition. They also have larger police forces. However, population adjusted crime rates are substantively similar across both types of cities, and the probability of a fatal encounter in a given year is virtually identical.

We estimate the effect of residency requirements on two primary outcomes. First, we examine whether police residency requirements increase the racial diversity of police forces by focusing on the *Percentage of non-Hispanic White Officers* in a police department, as reported in the LEMAS survey. One of the key arguments in favor of residency requirements is that they limit white officers from living in the suburbs while working within an inner city.

We note that in each analysis we control for a time-varying measure of the proportion of a city’s population that is white. The results are therefore identical if we instead define our outcome as the gap in percent white on the force relative to the percent white among city residents. We acknowledge that one weakness of the LEMAS survey is that it is conducted only periodically. However, after filing Public Records requests with dozens of cities in our dataset, it became clear that LEMAS was the only feasible source for documenting officer race over time. Of the 10 LEMAS surveys released from 1987 to 2020, the cities in our sample appear 7 times on average, and any measurement error introduced by occasional agency non-response should bias any effects we detect towards zero.

Next, we examine the number of *Fatal Encounters*, which captures the annual number of civilian fatalities that occur during interactions with police. These data come from the Fatal Encounters (2021) website, a comprehensive open-source system that tracks police-related deaths going back to the year 2000 (Finch et al. 2019). These data have been validated against other official sources of police killings (e.g. Comer and Ingram 2022) and represent the longest available panel of civilian deaths during police contact. While we would ideally want to study a variety of more nuanced measures of police-civilian contact, such as surveys of trust in the police, these data simply do not exist for the large number of municipalities in our sample over many decades. Although fatal encounters are a blunt measure, they are also important and include key events like the murders of George Floyd, Michael Brown, and Eric Garner—all of which led to renewed calls for residency requirements. Moreover, reducing police killings of civilians (particularly people of color) is one of the explicitly stated goals of various community groups including Communities United for Police Reform and Communities United Against Police Brutality. After presenting the main results, we draw from several years of LEMAS data on the number of citizen complaints sustained against police officers and show suggestive evidence that this alternative measure of police-civilian contact also improved in cities that dropped their residency rules.

## 4 Empirical Strategy

The choice to adopt residency rules is not random, and the small number of cities that employ such laws likely differ in ways both observable and unobservable from other cities. To account for this, our empirical strategy is a two-way fixed effects approach where we examine how outcomes evolve in cities that change their requirements relative to cities that don't. In the early 1990s, 80 cities enforced residency laws. Between 1990 and 2020, 48 cities changed their rules, with 41 cities dropping their requirements and 7 cities adding new requirements. Figure A.1 in the appendix shows this variation over time, and Table A.3 lists each treated city by the year and method of change. Unfortunately, given the small number of cities that changed their requirements over the course of the panel, credible causal identification is challenging. While we improve on existing cross-sectional analyses with our within-city design, one of the key takeaways of our study is that effectively evaluating the effects of residency policies is limited by the small number of treated units and issues surrounding outcome data availability and measurement error.

Because the vast majority of the variation in residency rules over the course of the panel comes from cities abolishing their requirements, we define our treatment as an indicator variable that takes a value of 1 in the absence of a residency requirement. In other words, this indicator switches from 0 to 1 when a city drops its residency rule. Note that if we inverted all 0s and 1s to specify the treatment as the presence or addition of a residency requirement, all results are identical but in the opposite direction. In some cases, the policy change is initiated locally via city council ordinance or collective bargaining agreement. Other times, state legislatures will mandate the change—often with the state supreme court later weighing in—as happened in Michigan in 1999 and Ohio in 2006. To maximize our sample size given the small number of treated units, we always begin by estimating an overall effect of the rule change. But because policies initiated by the state are arguably

more exogenous than locally driven changes, we also examine whether the effect of residency requirements varies depending on the method of the change.

We estimate equations of the form

$$y_{it} = \beta_1 \text{No Requirement}_{it} + \beta_2 X_{it} + \gamma_i + \delta_t + \varepsilon_{it}. \quad (4.1)$$

The coefficient of interest,  $\beta_1$ , captures the difference in outcomes in cities that drop their residency requirements relative to cities that don't change their policies in that year. The  $X_{it}$  contain several time varying controls including city population, city median income, and the percent of city residents that are white. The  $\gamma_i$  are city fixed effects that account for persistent characteristics like agency culture or professionalization, and the  $\delta_t$  are year fixed effects that control for temporal economic shocks that might broadly affect public sector employment or performance. Standard errors are always clustered at the city level.

Recent econometrics literature shows that standard difference-in-differences regressions can return biased estimates when the treatment switches on at different times for different units if treatment effects change over time (Xu 2017; De Chaisemartin and d'Haultfoeuille 2020; Goodman-Bacon 2021). We address this in two ways. First, following Cengiz et al. (2019), we create a set of clean control cities by constructing separate groups of cities that never change their residency requirement, one set for each year in which at least one treated city changed its status. We refer to each set of treated cities with their corresponding no-change cities as a "timing cohort." We can then compare treated cities only to cities that never changed their residency rules by including year-by-cohort fixed effects as follows:

$$y_{igt} = \beta_1 \text{No Requirement}_{igt} + \beta_2 X_{it} + \gamma_{ig} + \delta_{tg} + \varepsilon_{igt}. \quad (4.2)$$

Now,  $g$  identifies the timing group, and  $\delta_{tg}$  represents year-by-group fixed effects. We can interpret  $\beta_1$  as the effect of dropping a residency requirement on the outcome of interest under the assumption that treated and never treated cities would have been on the same average

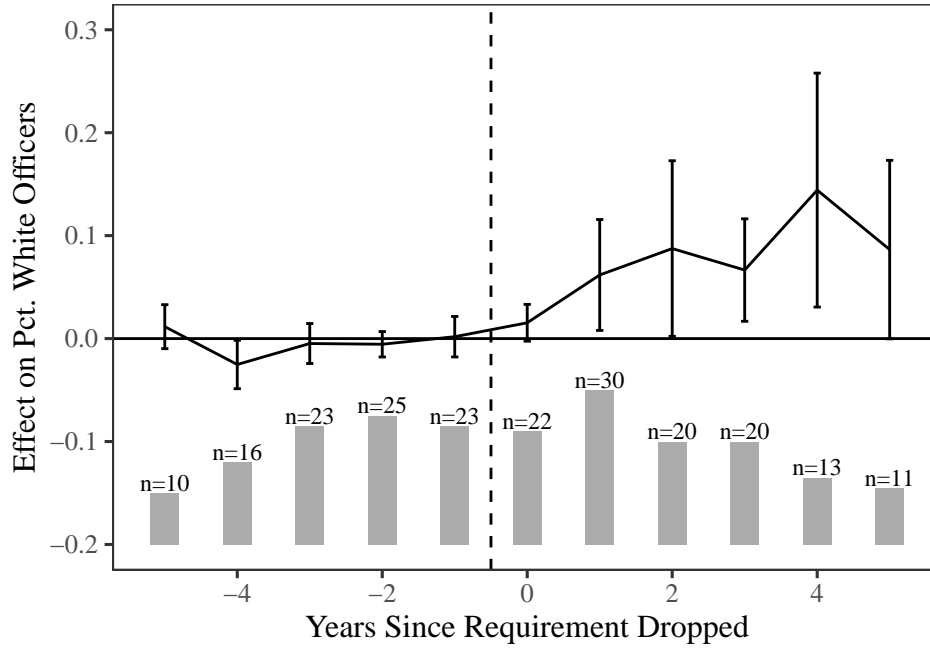
trajectory had neither changed their rules. Of course, the timing of reform is not random—cities might choose to drop their residency rule just as their police forces are starting to become more white or because police-community relationships are improving. To address this issue, we re-weight our data to ensure that treated and control cities match on the outcomes of interest in the pre-treatment years (Imai, Kim, and Wang 2021). Specifically, we employ entropy balancing use the `ebal` package in R (Hainmueller 2012), which we discuss in more detail in the next section.

## 5 Residency Rules Improve Officer Diversity

We begin by addressing the question of whether residency requirements help to increase the presence of officers of color on municipal police forces. First, we show an event study that plots the percentage of non-Hispanic white officers over time relative to when cities dropped their residency requirements. We use the counterfactual estimators framework introduced in Liu, Wang, and Xu (2021) and implemented via the `fact` package in R. This approach is particularly useful for designs involving multiple groups with staggered treatments and potentially heterogeneous treatment effects, which is likely the case with our data.

Figure 2 reveals that once a city drops its residency requirement, the percentage of white police officers increases in subsequent years. This effect kicks in almost immediately, which is consistent with case studies of individual cities suggesting that agencies begin hiring new officers right away upon changing their requirements (e.g. Neavling 2017; CBS Minnesota 2020). Although new officer training can take between 3 and 6 months, it is still feasible that an influx of new applicants would be able to join the force within the first year of the new policy being enacted—especially if these recruits are already sworn officers in a neighboring jurisdiction. We also note that although the five year period prior to the reform does not show any clear pre-trending among treated cities, there is a slight increase in the percentage of white officers in the years leading up the change in residency rules. However, we show

**Figure 2:** Event Study: Police Force Diversity



*Note:* Figure generated via `fect` in R. See Table 2 Column 2 for regression results.

that the results remain consistent and are even slightly larger when employing trajectory balancing to ensure parallel pre-trends between treated and untreated cities.

Table 2 displays the results generated via equations 4.1 and 4.2 described in the previous section. Column 1 shows the baseline model with city and year fixed effects and no controls, while Column 2 adjusts for time-varying city characteristics. Column 3 employs the “stacked approach” where (1) a new dataset is created for each year in which at least one city changed its residency requirement along with all pure control cities, (2) each dataset is assigned a cohort identifier, (3) the data are stacked, and (4) year-by-cohort fixed effects mean that we are restricting comparisons between treated cities and cities that never change their residency rules. In the period after a city abolishes its residency rule, we observe that the proportion of white police officers increases by around 4.4 percentage points relative to non-treated cities (Column 4). Note that this estimate includes both any increases in the proportion of white

**Table 2:** Residency Requirements and Pct. White on Police Force

	Original Data		Stacked Approach	
	(1)	(2)	(3)	(4)
Requirement Dropped	0.053*	0.049*	0.050*	0.044*
	(0.013)	(0.013)	(0.013)	(0.009)
City and Year FEs	Yes	Yes		
Controls		Yes	Yes	Yes
City and Cohort x Year FEs			Yes	Yes
Balancing Weights				Yes
Mean Outcome	0.79	0.79	0.79	0.79
Num. Agencies	785	785	785	785
Observations	5,480	5,480	113,756	113,756

Controls include city population (logged), median income (logged), % white residents. Robust standard errors clustered by city. \*p<0.05

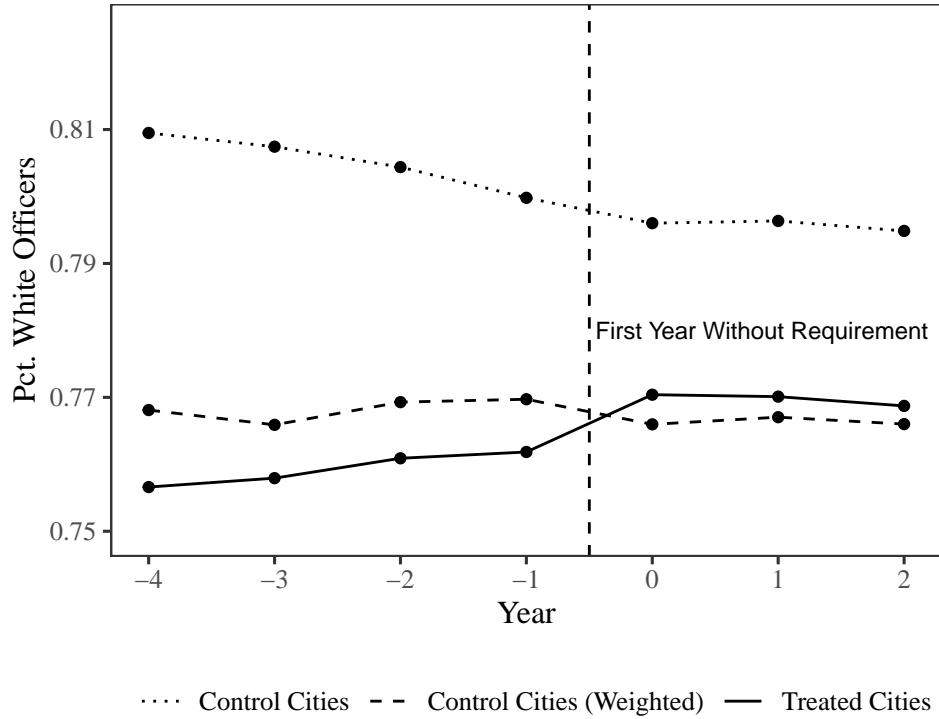
officers among cities that drop their rules as well as decreases in the proportion white among cities that don't change their rules.<sup>6</sup>

In the appendix, we also demonstrate that the results are robust to dropping each timing cohort from the analysis (Figure A.2). This helps us to rule out the possibility that reforms in one particular city or year are driving the results. For example, in our sample there were seven cities in Ohio that were forced to drop their residency rules in 2009 after the Ohio Supreme Court upheld state legislation banning municipal residency requirements. One of the concerns about two-way fixed effects models with multiple groups and treatment periods is that particular years might be contributing most of the weight toward the average estimated effect (e.g. Goodman-Bacon 2021). But Figure A.2 shows that the results are not sensitive to excluding any specific timing cohort.

Finally, Column 4 in Table 2 adds balancing weights to ensure that treated and control cities are following the same trajectory prior to the change in policy. To provide intuition

<sup>6</sup>While a 4 percentage point increase would be quite massive (e.g. a median sized force of 170 officers and 65% white would have to hire 20 new white officers to increase the proportion white to just under 69%), the raw data reveal that this effect is driven by both an increase in the number of white officers in treated cities and a decrease in control cities. We discuss the issue of mechanisms more in the next section.

**Figure 3:** Residency Rules and Police Force Diversity: Trajectory Balancing



*Note:* Figure plots raw data. Weights generated via `ebal` in R.

about what this balancing achieves, Figure 3 shows the average percentage of white officers for treated cities compared to control cities both with and without the balancing weights. Note that these are raw averages without adjusting for any other covariates. As suggested by Figure 3, we see an increase in the percentage of white police officers after treated cities eliminate their residency rules. However, note that among the original set of control cities (the dotted line), the pre-treatment trends are not completely parallel with those of the treated cities. After applying the balancing weights obtained via `ebal`, we can construct a set of control cities that much more closely resembles the treated cities both in terms of levels and trends in the percentage of white officers.

Over the course of the panel, many police departments across the country were actively working to diversify in terms of race. The results of these efforts are reflected in Figure 3, and the police agencies in our sample were generally becoming less white over time. However, the cities that drop their residency laws follow a noticeably different trajectory after changing

their policies. Across a range of specifications, the results are consistent with the idea that residency rules may aid in promoting racial diversity among police officers. In the next section, we further explore why this might be the case.

## 5.1 Why Do Agencies Become Whiter Without Residency Rules?

Why do police agencies become whiter after they abolish their residency requirements? To help us understand this result, we begin by showing the estimated effects for several key subsets of cities in Table 3. Column 1 compares only departments that were forced to drop their requirements via state mandate to non-treated departments, and Column 2 does the same for cities that changed their residency rules locally. The pattern is very similar in both cases, and in Table A.4 in the Appendix we confirm that the coefficients for both types of changes are not statistically distinguishable from each other. The fact that the effects are similar and robust whether the rule is dropped internally or imposed by the state suggests that this result is not driven by self-selection into treatment. For example, if agencies only became whiter when cities dropped their own requirements locally, we might be concerned that these cities changed their rules due to political pressure or some other unobservable dynamic, and it might be this political shift that led to an influx of white officers (rather than the change in residency rules). But agencies also become whiter even when the change is forced upon them by state law.

Columns 3 and 4 show the effects of dropping a residency requirement for two important subsets of agencies: large departments serving cities of at least 100,000 residents, and departments serving cities that are less than 70 percent non-Hispanic white (the sample average). We zoom in on these agencies because the types of cities currently debating residency laws tend to be both large and racially diverse. Strikingly, the effect is almost twice as big for agencies in cities with a large proportion of non-white residents (Column 4) compared to the estimates for the full sample introduced in Table 2. Among cities where fewer than 70

**Table 3:** Residency Requirements and Pct. White on Police Force

	State Change	Local Change	Pop > 100,000	City < 70% White
	(1)	(2)	(3)	(4)
Requirement Dropped	0.049* (0.013)	0.044* (0.016)	0.054* (0.012)	0.081* (0.019)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.79	0.79	0.74	0.69
Num. Cities	785	769	309	546
Observations	5,480	5,353	2,164	2,583

Controls include city population (logged), median income (logged), and % white residents.  
Robust standard errors clustered by city. \*p<0.05

percent of residents are white, eliminating residency rules leads to an 8.1 percentage point increase in the percentage of white officers relative to other cities.<sup>7</sup>

If residency rules are particularly helpful at promoting officer diversity in cities that themselves are racially diverse, this would be consistent with the argument that these policies are effective in part because they prevent white cops from living in the suburbs while working in the inner city. In this case, we should observe that the increase in the percentage of white officers on the force is driven by an influx of white hires following the change in requirements, rather than by officers of color leaving the force or being replaced by white colleagues. Qualitatively, one key reason why cities drop their residency requirements is often to expand the labor pool and aid with hiring (Schulz 2021). If these new hires are disproportionately white, this would help to explain the patterns uncovered in the previous section.

Unfortunately, the LEMAS data do not report when personnel are sworn in, so we can't directly examine whether the increase in the percentage of non-Hispanic white officers is driven by new hires. However, we can proxy for this indirectly by splitting the data into two samples: agencies that gain officers and grow over the course of the panel, and agencies that

<sup>7</sup>The results are also consistent if we allow the effect of residency rules to vary flexibly by the percentage of city residents that are white, but given the small number of treated units and the issues associated with continuous moderators identified by Hainmueller, Mummolo, and Xu (2019), we prefer this simple subsample approach.

**Table 4:** Residency Requirements and Pct. White on Police Force by Agency Growth

	Shrinking Agencies	Growing Agencies
	(1)	(2)
Requirement Dropped	0.015 (0.032)	0.055* (0.014)
City and Year FEs	Yes	Yes
Controls	Yes	Yes
Mean Outcome	0.77	0.79
Num. Cities	138	647
Observations	909	4,571

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

shrink. If abolishing residency rules leads to an increase in the percentage of white officers because more white officers subsequently join the force, we should observe the effects of the change being more pronounced in agencies that grow.

Table 4 demonstrates that this is exactly the case. When a city drops its residency requirement, its police force only becomes whiter if it hires new officers. In departments that stay the same size or lose officers over the course of the panel, residency laws have no effect on the racial composition of the agency. This analysis provides suggestive evidence that the observed increase in the percent white on the force is driven by new hires and hints that residency requirements might promote racial diversity in part because they limit the number of white officers who are able to work for the force while living outside city limits.

## 6 Fewer Fatal Encounters After Cities Drop Residency Rules

We now turn to the question of whether residency requirements improve contact outcomes between police and community members. Specifically, we rely on the Fatal Encounters dataset introduced in Section 4 to study how changes to residency rules impact the probability of civilian deaths that occur during police interactions. The fact that police departments

become whiter after dropping their residency laws—especially in diverse cities—might mean that police-civilian relations subsequently deteriorate. However, some research finds little change to policing performance after departments hire more officers of color (e.g. McCrary 2007). If relaxing these laws bolsters the quality of the talent pool or enhances officer morale, it’s possible that the rate of fatal encounters might instead improve.

To account for any changes in fatal encounters that might be due to a shift in the racial composition of the police force, we would ideally want to adjust for a time-varying measure of the percentage of white officers within a city. But because this variable is itself post-treatment, we choose to exclude it in the main analyses. However, in the Appendix we show that all of the following results remain unchanged if we control for the proportion of white officers on the force (Table A.6). After presenting the main results, we also perform several robustness checks to address some of the known issues associated with the Fatal Encounters data, including higher rates of under-reporting earlier in the sample.

Fatal encounters between police and civilians are relatively rare, happening on average slightly less than once a year in the cities in our sample. We therefore define our outcome as an indicator that takes a value of 1 if a city reports any fatal police encounters in a year, and estimates generated from the model described in equation 4.1 predict the probability of a fatal encounter conditional on treatment.<sup>8</sup> In Table 5, we observe a dramatic decrease in the probability that a civilian dies during an interaction with the police after cities drop their residency rules. This result is precisely estimated and robust across specifications, including the stacked approach with clean controls and the introduction of trajectory balancing weights discussed in the previous section (Columns 3 and 4). On average, the probability of a fatal encounter in any given year is around 35%. A 9-10 percentage point decrease in this probability is thus substantively quite large.

---

<sup>8</sup>Note that the results reveal the same pattern if we use the total number of fatal encounters as the outcome, which we show in Table A.5.

**Table 5:** Residency Requirement and Probability of Fatal Encounter

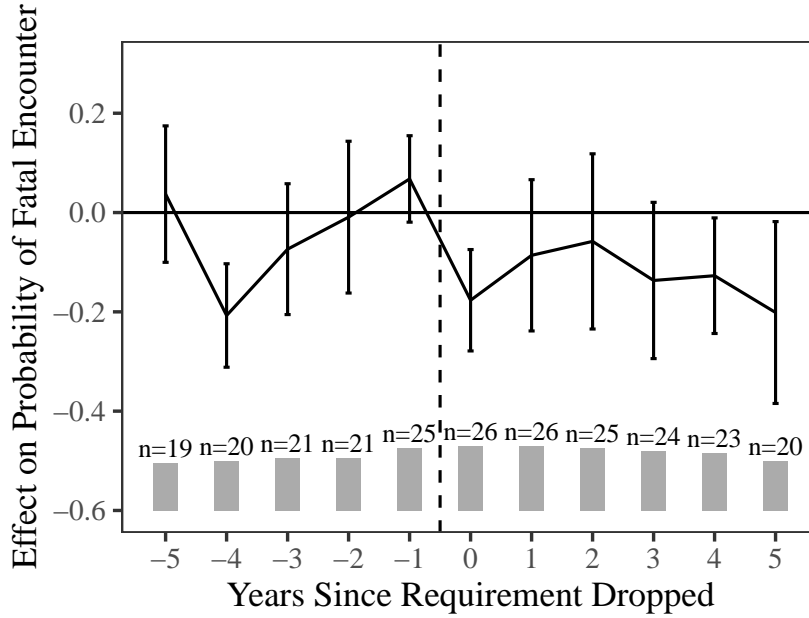
	Original Data		Stacked Approach	
	(1)	(2)	(3)	(4)
Requirement Dropped	-0.097*	-0.093*	-0.103*	-0.091*
	(0.037)	(0.036)	(0.039)	(0.037)
City and Year FEs	Yes	Yes		
Controls		Yes	Yes	Yes
City and Cohort x Year FEs			Yes	Yes
Balancing Weights				Yes
Mean Outcome	0.312	0.312	0.307	0.307
Num. Agencies	787	785	774	774
Observations	16,527	16,485	171,444	171,444

Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. \*p<0.05

An event study (again generated via the Liu, Wang, and Xu (2021) `fect` package in R) displays the probability of a fatal encounter in the years before and after treatment. The estimates displayed in Figure 4 are fairly imprecise, which makes sense given that civilian deaths are relatively rare events. The effects also jump around a bit and seem to be trending up before cities drop their residency requirements. As a result, we prefer the specification in Column 4 of Table 5 that employs balancing weights to generate control units that better match the treated cities in terms of pre-treatment fatalities. Figure A.3 in the Appendix shows that the use of weights makes the pre-treatment trends significantly more parallel. After cities abandon their rules, cities experience a noisy but noticeable decrease in the likelihood of a fatal encounter, especially after a few years have passed. We also note that the general negative trend post-treatment reflects a stark shift away from the pre-treatment period, where if anything the risk of fatal encounters seems to be increasing.

The pattern we uncover is consistent with the idea that residency requirements are not helpful in preventing civilian deaths during police interactions. Again, we urge caution in interpreting these results given how few cities actually dropped their requirements over the

**Figure 4:** Event Study: Fatal Encounters



*Note:* Figure generated via `fect` in R. See Table 5 Column 2 for regression results.

course of the panel and given that fatal encounters are a relatively rare event. At the same time, police killings can also serve as inflections points that lead to dramatic shifts in police-civilian relations. This outcome is thus meaningful from a policy perspective, and at the very least we can rule out with a high degree of confidence that fatal encounters increased in the years after residency rules were loosened. Moreover, in Figure A.4, we demonstrate that this effect is not driven by any particular treatment year by dropping each timing cohort one at a time. Instead, the results remain quite consistent across specifications.

## 6.1 Exploring Mechanisms

As we did when examining the racial composition of city police forces, we now decompose the effect of residency rules on fatal encounters in several ways. In Table 6, we find that the probability of a fatal encounter decreases even more markedly following a rule change when restricting the sample to large and racially diverse cities (Columns 3 and 4)—precisely

**Table 6:** Residency Requirement and Probability of Fatal Encounter

	State Change	Local Change	Pop > 100,000	City < 70% White
	(1)	(2)	(3)	(4)
Requirement Dropped	-0.031 (0.046)	-0.144* (0.050)	-0.101* (0.044)	-0.133* (0.048)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.311	0.308	0.572	0.389
Num. Agencies	757	771	315	596
Observations	15,897	16,191	5,300	8,095

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

the places that are at higher risk of civilian deaths at the hands of police officers. We also find that the method of the change matters a great deal. This result is driven by departments that change their policy locally (Column 2). While there is a modest decrease in the probability that a civilian dies during a police encounter when states overturn city residency requirements (around 3 percentage points), this effect is much smaller and not statistically distinguishable from zero.

Given that the reduction in civilian fatalities is most pronounced in cities that self-select into dropping their residency requirements, we need to be careful in how we interpret the results. However, even with the non-random assignment of the treatment, there are still several important things we can learn from the data. First, the fact that fatal encounters do not increase (and even slightly decrease) in the places where the change is imposed in a more exogenous manner by state law provides strong evidence against the claim that residency rules significantly reduce civilian deaths. Second, it is unlikely that the decrease in fatal encounters is caused by changes in officer morale. Whether the residency policy is changed locally or by the state, officers should be equally likely to reap the psychological benefits of enjoying greater flexibility in where they live.

Our data allow us to test four possible explanations for why civilian deaths at the hands of police drop after cities remove their residency rules internally. In the conclusion, we turn

to a discussion of other possible mechanisms that future work in this area might explore. First, we consider whether secular trends such as changes to department professionalism or culture lead cities both to drop their residency requirement while also improving civilian contact outcomes. We do this by adding city-specific linear trends to the regression models (in addition to the city fixed effects), which can account for slow-moving unobservable shifts that differ between treated and control units. Reassuringly, the results remain very consistent (Table A.7), ruling out the story that the agencies dropping their residency rules are simply becoming “better” or more professional over time.

Next, we consider whether departments are viewing residency rules as a substitute for formal training. For example, agency leaders might invest less time and money in officer training if they believe residency requirements are providing officers with additional local knowledge and cultural competency. They might then increase training requirements after relaxing the residency rule, which could be responsible for the reduction in fatal encounters. However, using LEMAS data on the total number of academy and field training hours mandated by each agency, we find no significant difference either across cities or within cities over time in terms of training requirements and the presence of residency rules (Table A.8).

Another possibility that opponents of residency laws regularly raise is the idea that these rules restrict the labor market and make it challenging for agencies to hire qualified applicants. If agencies are operating below their optimal capacity with residency rules in place, loosening these requirements might allow departments to bolster hiring and more easily meet staffing needs in places that are stretched thin. This could in turn improve contact-related outcomes like fatal encounters. The data reveal that agencies don’t always grow after the elimination of residency rules. Figure A.5 in the appendix shows that cities do gain some officers on average after dropping their requirements, but this effect is very noisy. Several high-profile agencies like the Detroit Police Department had their rules overturned but also lost thousands of officers over the course of the panel.

However, when we allow the effect to vary by the type of rule change (state vs. local), we see that agencies that drop their residency requirements internally do subsequently grow in size (Figure A.6). In other words, the data are consistent with the idea that cities might drop their residency rules when they are struggling to recruit enough applicants. Relaxing the residency requirement allows them to hire more officers and operate at full capacity, which could improve fatal encounters if officers were previously overworked.<sup>9</sup>

Finally, another possibility is that when an agency internally drops its residency requirement, it enacts other reforms at the same time. To unpack this possibility, we turn to several indicators of community involvement in policing available in the LEMAS survey. We uncover suggestive evidence that when cities drop their residency rules locally, they go on to adopt other practices that might indicate that agencies are trying to improve their performance. The three outcomes available in the LEMAS data are the presence of a civilian complaint oversight board, whether the police department reported meeting with community-based organizations that year, and whether the agency conducted a resident satisfaction survey. Each of these variables takes a value of either 0 or 1, and we can examine the probability that these measures were present before and after cities drop their residency rules. Because we are exploring why locally initiated policy changes might lead to better outcomes, we allow the effect to vary flexibly by the type of rule change (state vs. local).

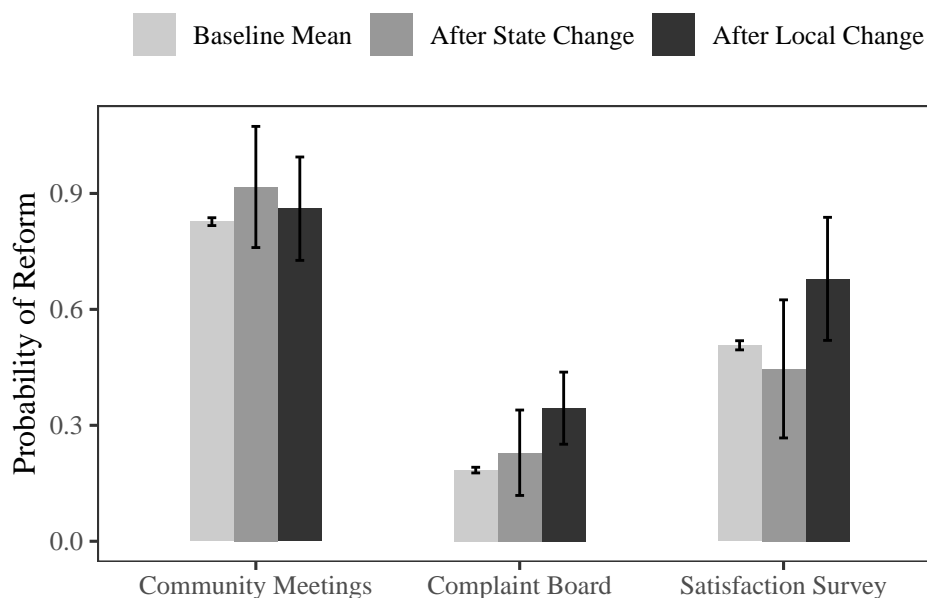
Figure 5 shows the baseline probabilities for each reform in the pre-treatment period along with predicted outcomes from two-way fixed effects models that vary by the source of the rule change. When cities are forced to drop their residency rule by state mandate, they are no more likely to adopt any of these reforms. But when a city internally drops its residency requirement, it is substantially more likely to institutionalize a civilian complaint board and to administer resident satisfaction surveys. These cities also adopt more total reforms relative to places that don't change their policies if the outcome is a summary variable that

---

<sup>9</sup>Interestingly, the number of officers in a city does not unambiguously correlate with fewer civilian killings. This makes sense given that more officers on the street might also indicate an overzealous policing culture, and larger agencies tend to be located in bigger cities where rates of civilian fatalities might already be higher. But in places that don't have enough recruits and that are operating at low capacity, increasing the size of the force to appropriate levels might plausibly improve outcomes.

ranges from 0 to 3. All of these results are shown formally in Table A.9 in the appendix. While tentative, this analysis is consistent with the idea that residency requirements may simply reflect blunt policies that are substituting for more meaningful reform.

**Figure 5:** Other Reforms After Dropping Residency Requirement



*Note:* See Table A.9 for regression results.

However, we also find that none of these other reforms reduces fatal encounters as much as dropping the residency requirement does. While the adoption of a complaint board does correlate with a reduction in civilian fatalities, the estimate is modest and not statistically distinguishable from zero. The effect of removing the residency rule also remains robust after including indicators for these other reforms as controls (Table A.10). We take this as evidence that the decrease in fatal encounters is not solely driven by other reforms. Rather, the presence of these new policies might signal that agencies are trying to improve the quality of police-civilian contact, and relaxing their residency rules was part of a broader effort to achieve this goal.

Of course, if we think the reason that fatal encounters decrease when cities drop their residency rules is because these agencies are changing in other ways—such as a new police

chief joining the force and implementing a variety of new policies—it becomes difficult to interpret the effect of the requirement causally. In this case, the results might better be thought of as a dynamic portrait of the general equilibrium effects of residency rules and contact outcomes. Even if the internal removal of a residency rule always happened to coincide with some other agency change that we are unable to observe, this would still mean that this “other change” was much more effective at reducing civilian deaths than the residency requirement was when it was in place. This type of descriptive inference can still prove useful to cities and states currently considering these rules, because the adoption of residency requirements is never actually randomly assigned in practice. The core conclusion remains: if cities are hoping to implement residency laws to reduce incidents of police violence, there is no evidence that these policies in and of themselves improve the risk of fatal police-civilian encounters.

## 7 Additional Robustness Checks

We conduct several additional robustness checks to examine the sensitivity of our two main results—namely, that when cities drop their residency requirements their police forces become whiter but police-civilian encounters are less likely to result in fatalities. Throughout the paper, the key source of the variation comes from 40 cities that drop their requirements and 7 cities that add new laws. To explore whether these effects are symmetrical, we drop the 7 cities that added requirements and show that the results are very similar to the overall effects (Table A.11).

To show robustness to additional control variables, in Table A.12 we adjust for two potential time-varying covariates that might influence the quality of police-community: city income inequality and levels of racial segregation. We uncover nearly identical results and describe these additional variables in more detail in the appendix. For transparency, in Table A.13 we also show the results of a simple model comparing our main outcomes only for cities

where the residency rule was overturned by state mandate—a plausibly more exogenous treatment—without control variables and using both the raw data and the stacked approach. This is the closest approximation to a standard difference-in-differences approach and can help to address concerns about self-selection. The results are consistent with the various subset analyses we have shown throughout the paper.

Next, in our main fatal encounters analysis, we include all civilian fatalities that occur during engagement with police officers. However, this measure includes accidental deaths including drownings and medical emergencies. When we restrict the analysis to officer-caused deaths including shootings, tasings, beatings, and asphyxiations, we uncover even larger effects. These results are shown in Table A.14. To address the fact that the Fatal Encounters dataset contains higher rates of missingness early in the sample, we also add city-specific linear trends to each specification to account for any secular changes in the reporting rate that might vary across treated and non-treated cities. Again, the results remain consistent (Table A.7).

Finally, we acknowledge that fatal encounters are a fairly blunt and extreme measure of police-civilian contact. In three years (2003, 2007, and 2020), one of the questions in the LEMAS survey was about the number of citizen complaints sustained against police officers. When we look at citizen complaints before and after cities drop their residency rules, we uncover a similar pattern to the fatal encounters result. This analysis is speculative given the small number of years for which data exist, but we uncover suggestive evidence that the number of citizen complaints decreases after cities drop their residency rule, which we show in Table A.15 in the appendix. This effect is driven by agencies that drop their residency requirement locally. Perhaps unsurprisingly, the number of citizen complaints also correlates with the number of fatal encounters. We find it reassuring that this alternative measure of police-civilian contact follows the same basic pattern as the main results, and we discuss other possible outcome measures and avenues for future research in the next section.

## 8 Discussion

Over the past few years, organizations like Communities United Against Police Brutality (CUAPB) have published reports detailing recommendations for improving police-civilian relations. These groups typically argue that residency requirements are a distraction from more substantial reforms. In one recent briefing, CUAPB emphatically declared that it has “never encountered a shred of evidence that requiring or incentivizing police officers to live in the communities in which they work has any positive effect on the quality of policing” (Communities United Against Police Brutality 2021). And yet, various theoretical arguments suggest that these laws should improve police performance, and state and local officials continue to actively debate the merit of these requirements. Our survey of the largest municipal police departments provides a much needed update to the literature on police residency rules. Using a within-city design that exploits changes to residency requirements over the past three decades, we uncover evidence that these rules may help to promote racial diversity among officers but are associated with higher rates of fatal civilian encounters.

Future work might expand on this research in several ways. Our outcome measure of civilian deaths during police encounters is fairly blunt and does not necessarily capture the complex ways in which police and communities interact. A deep dive into a few large cities might allow researchers to collect more granular and meaningful data on police contact with community members at the city level. Another possibility would be to extend our analysis to explore other more routinized forms of police-civilian interactions such as police stops of pedestrians and vehicular traffic, including racial disparities in police stops. Given the small number of cities that actually dropped their requirements over the course of the panel, it may also be worth considering a more qualitative approach to exploring the particular dynamics of local policing in this subsample of cities. This might take the form of case studies, interviews, or other ethnographic techniques.

Our results are consistent with recent research suggesting that reforms designed to increase police force diversity and strengthen community ties do not always work as intended (Paoline, Gau, and Terrill 2018). For example, incorporating community policing practices did not increase public trust in six countries in the Global South (Blair et al. 2021). Nicholson-Crotty, Nicholson-Crotty, and Fernandez (2017) do not uncover a straightforward relationship between the number of African-American police officers and the number of African-American citizens killed during police encounters in large U.S. cities, and McCrary (2007) finds that court-ordered quotas to increase hiring of candidates of color in municipal police departments did not affect crime rates. Our research similarly indicates that while residency rules may help foster racial diversity on city police forces, this does not translate into a reduction in fatal police-civilian encounters.

To be clear, there may be other reasons why particular communities believe that residency requirements would be helpful if they increase perceptions of police force legitimacy or bolster the tax base of a city. For example, Retired Police Officer John Bennett who worked on diversity and recruitment issues for the Detroit Police department describes the negative optics of staffing the force with candidates from outside the city. “We are getting to the point where the police department will no longer reflect the community it serves...They’re bringing in candidates from northern Michigan who haven’t had contact with people of color, and you expect them to police a community that is predominantly black” (Neavling 2017). While in this paper we uncover no positive aggregate effects of blunt city residency rules on overall department performance, it is still possible that individual officers living within the communities they serve may behave in systematically different ways than those living outside city boundaries. Recent research by Ba et al. (2022) that links officers to the neighborhoods where they live offers a promising path forward in this regard, and future work might examine whether officer residency correlates with various behavioral outcomes at the individual level.

We also note that cities can enact alternative policies to encourage residency among police officers without mandating it. For example, a federal task force on police reform recommended that departments provide subsidized housing or mortgage assistance to officers willing to live within city boundaries (Hauck and Nichols 2020). Many departments, including New York, don't enforce a formal requirement but add points to test scores for local applicants who live in the city (Schulz 2021). Still other cities offer perks such as take home patrol car programs for residents (Sweeney 2019). City officials can also try to increase community trust and foster intrinsic motivation among officers through other means besides higher police residency rates, including training in procedural justice (Mazerolle et al. 2013) and casual non-enforcement contact (Peyton, Sierra-Arévalo, and Rand 2019).

Finally, we urge additional study of the potential mechanisms that lead to a reduction in fatal encounters when agencies choose to drop their residency requirements. We uncovered suggestive evidence consistent with a story whereby cities drop their residency rules in order to increase force capacity through hiring, and this change seems to be accompanied by a series of other reforms that might indicate an agency is proactively trying to improve policy-community relations. However, there are other possible reasons why civilian deaths may have decreased in cities that relaxed their residency rules internally. Surveys of police officers in cities that have changed their requirement could help to identify changes in officer attitudes or behavior that may have contributed to the reduction in civilian deaths.

A growing body of evidence-based research demonstrates that certain public safety reforms can dramatically improve police department performance. These include clear bureaucratic guidelines for monitoring civilian stops (Mummolo 2018*b*), training in procedural justice (Mazerolle et al. 2013), and the use of body cameras (Ariel, Farrar, and Sutherland 2015). However, when it comes to residency requirements, both supporters and opponents of these laws tend to make sweeping claims that simply aren't supported by evidence. Ultimately, our results are consistent with what many community reform groups have recently

argued: residency laws do not appear to improve police-community relationships and are likely not a particularly fruitful path to reform in the absence of other structural changes.

## References

- Allen, Terrence, and M. Michaux Parker. 2013. "Police Officers and Their Perceived Relationship with Urban Communities: Does Living in the Community Influence Police Decisions?" *Social Development Issues* 35(3): 82–95.
- Allport, Gordon. 1954. *The Nature of Prejudice*. Reading, MA: Addison-Wesley.
- Anderson, William. 1925. *American City Government*. New York: H. Holt.
- Ariel, Barak, William A Farrar, and Alex Sutherland. 2015. "The effect of police body-worn cameras on use of force and citizens' complaints against the police: A randomized controlled trial." *Journal of quantitative criminology* 31(3): 509–535.
- Ba, Bocar A, Dean Knox, Jonathan Mummolo, and Roman Rivera. 2021. "The role of officer race and gender in police-civilian interactions in Chicago." *Science* 371(6530): 696–702.
- Ba, Bocar, Jacob Kaplan, Dean Knox, Mayya Komisarchik, Rachel Mariman, Jonathan Mummolo, Roman Rivera, and Michelle Torres. 2022. "Who are the Police? Descriptive Representation in the Coercive Arm of Government." Working Paper, [https://policingresearch.org/wp-content/uploads/2022/03/Policing\\_and\\_Representation-1.pdf](https://policingresearch.org/wp-content/uploads/2022/03/Policing_and_Representation-1.pdf).
- Banks, Duren, Joshua Hendrix, Matthew Hickman, and Tracey Kyckelhahn. 2016. *National Sources for Law Enforcement Employment Data*. US Department of Justice, Office of Justice Programs, Bureau of Justice . . . .
- Beckett, Katherine, and Steve Herbert. 2008. "Dealing with disorder: Social control in the post-industrial city." *Theoretical Criminology* 12(1): 5–30.
- Bednar, Adam. 2020. "McCray pushes Baltimore police residency requirement bill." *The Daily Record*, January 16. <https://thedailyrecord.com/2020/01/16/mccray-pushes-baltimore-police-residency-requirement-bill/>.
- Bertrand, Marianne, and Esther Duflo. 2017. "Field experiments on discrimination." *Handbook of economic field experiments* 1: 309–393.
- Blair, Graeme, Jeremy M Weinstein, Fotini Christia, Eric Arias, Emile Badran, Robert A Blair, Ali Cheema, Ahsan Farooqui, Thiemo Fetzer, Guy Grossman et al. 2021. "Community policing does not build citizen trust in police or reduce crime in the Global South." *Science* 374(6571).
- Blumer, Herbert. 1958. "Race prejudice as a sense of group position." *Pacific sociological review* 1(1): 3–7.

- Bobo, Lawrence D. 1999. "Prejudice as group position: Microfoundations of a sociological approach to racism and race relations." *Journal of Social Issues* 55(3): 445–472.
- Bouza, A.V. 1978. *Police administration: organization and performance*. Elmsford: Pergamon Press Inc.
- Brandl, Steven G, and Meghan S Stroshine. 2013. "The role of officer attributes, job characteristics, and arrest activity in explaining police use of force." *Criminal justice policy review* 24(5): 551–572.
- CBS Minnesota. 2020. "With Only 8% of Minneapolis Police Officers Living in City, Residency Requirement Push Gaining Traction." *Motor City Muckracker*, June 17. <https://minnesota.cbslocal.com/2020/06/17/with-only-8-of-minneapolis-police-officers-living-in-city-residency-requirement-gaining-traction/>.
- Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer. 2019. "The effect of minimum wages on low-wage jobs." *The Quarterly Journal of Economics* 134(3): 1405–1454.
- Chase, A. 1979. "Residency laws: Should police be free to live where they choose?" *Police Magazine* 2: 62–65.
- City of Lima v. State of Ohio*. 2008. "Brief of Amicus Curiae: Ohio Association of Professional Fire Fighters in Support of Appellant State of Ohio." : Forthcoming.
- Cohen, Elisha, Anna Gunderson, Kaylyn Jackson, Paul Zachary, Tom S Clark, Adam N Glynn, and Michael Leo Owens. 2019. "Do officer-involved shootings reduce citizen contact with government?" *The Journal of Politics* 81(3): 1111–1123.
- Coleman, Stephen F. 1983. "The dilemmas of police residency: Views from the street." *Journal of police science and administration* 11(2): 194–199.
- Comer, Benjamin P, and Jason R Ingram. 2022. "Comparing fatal encounters, mapping police violence, and washington post fatal police shooting data from 2015–2019: a research note." *Criminal Justice Review*: Forthcoming.
- Communities United Against Police Brutality. 2021. "What Will It Take to End Police Violence? Recommendations for Reform." [cuabp.org](https://cuabp.org).
- De Chaisemartin, Clément, and Xavier d'Haultfoeuille. 2020. "Two-way fixed effects estimators with heterogeneous treatment effects." *American Economic Review* 110(9): 2964–96.
- Dorschner, J. 1989. "Dark Side of the Force." In *Critical Issues in Policing*, eds. R. Dunham, and G Alpert. Prospect Heights, IL: Waveland Press , 254–274.
- Eckhouse, Laurel. 2022. "Metrics Management and Bureaucratic Accountability: Evidence from Policing." *American Journal of Political Science* 66(2): 385–401.
- Eisinger, Peter K. 1982. "Black employment in municipal jobs: The impact of black political power." *American Political Science Review* 76(2): 380–392.

- Eisinger, Peter K. 1983. "Municipal residency requirements and the local economy." *Social Science Quarterly* 64(1): 85.
- Fatal Encounters. 2021. "Fatal Encounters Database." <https://fatalencounters.org/>.
- Federal Bureau of Investigation. 2021. "Uniform Crime Reporting Program." <https://www.fbi.gov/services/cjis/ucr>. Accessed: 2021-14-09.
- Finch, Brian Karl, Audrey Beck, D Brian Burghart, Richard Johnson, David Klinger, and Kyla Thomas. 2019. "Using crowd-sourced data to explore police-related-deaths in the United States (2000–2017): The Case of Fatal Encounters." *Open Health Data* 6(1).
- Gau, Jacinta M, and Rod K Brunson. 2010. "Procedural justice and order maintenance policing: A study of inner-city young men's perceptions of police legitimacy." *Justice quarterly* 27(2): 255–279.
- Goldstein, Rebecca, Michael W Sances, and Hye Young You. 2020. "Exploitative revenues, law enforcement, and the quality of government service." *Urban Affairs Review* 56(1): 5–31.
- Gonzalez, Yanilda. 2020. *Authoritarian Police in Democracy: Contested Security in Latin America*. New York, NY: Cambridge University Press.
- Goodman-Bacon, Andrew. 2021. "Difference-in-differences with variation in treatment timing." *Journal of Econometrics*: Forthcoming.
- Hainmueller, Jens. 2012. "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies." *Political analysis* 20(1): 25–46.
- Hainmueller, Jens, Jonathan Mummolo, and Yiqing Xu. 2019. "How much should we trust estimates from multiplicative interaction models? Simple tools to improve empirical practice." *Political Analysis* 27(2): 163–192.
- Harvey, Anna, and Taylor Mattia. 2022. "Reducing Racial Disparities in Crime Victimization: Evidence from Employment Discrimination Litigation." *Journal of Urban Economics*: Forthcoming.
- Hauck, Grace, and Mark Nichols. 2020. "Should police officers be required to live in the cities they patrol? There's no evidence it matters." *USA Today*, June 13. <https://www.usatoday.com/story/news/nation/2020/06/13/police-residency-data/5327640002/>.
- Headley, Andrea M. 2022. "Representing Personal and Professional Identities in Policing: Sources of Strength and Conflict." *Public Administration Review* 82(3): 396–409.
- Hennessey, Stephen M. 1993. "Achieving cultural competence." *Police Chief* 60: 46–46.
- Hirsch, Werner Z, and Anthony M Rufolo. 1985. "Economic effects of residence laws on municipal police." *Journal of urban economics* 17(3): 335–348.

- Imai, Kosuke, In Song Kim, and Erik Wang. 2021. "Matching Methods For Causal Inference With Time-Series Cross-Section Data." *American Journal of Political Science*: Forthcoming.
- Kaplan, J. 2021. "Jacob kaplan's concatenated files: Uniform crime reporting program data: Offenses known and clearances by arrest (return a), 1960-2020." *Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor]*. <https://doi.org/10.3886/E100707V17>: Forthcoming.
- Laniyonu, Ayobami. 2019. "The political consequences of policing: Evidence from New York City." *Political Behavior* 41(2): 527–558.
- Lerman, Amy E, and Vesla M Weaver. 2014. *Arresting citizenship*. University of Chicago Press.
- Liu, Licheng, Ye Wang, and Yiqing Xu. 2021. "A practical guide to counterfactual estimators for causal inference with time-series cross-sectional data." Available at SSRN: <http://dx.doi.org/10.2139/ssrn.3555463>.
- Livengood, Chad, and Frank Annalise. 2020. "Reexamining residency rules for Detroit police officers. 2020's tensions revive an old debate: Should police live in communities in which they work?" *Crain's Detroit Business*, July 26. <https://www.crainsdetroit.com/crains-forum/reexamining-residency-rules-detroit-police-officers>.
- Maynard, Melissa. 2013. "Should Public Workers Have to Live Where They Work?" *Pew Stateline*, March 18. <https://www.pewtrusts.org/en/research-and-analysis/blogs/stateline/2013/03/18/should-public-workers-have-to-live-where-they-work>.
- Mazerolle, Lorraine, Emma Antrobus, Sarah Bennett, and Tom R Tyler. 2013. "Shaping citizen perceptions of police legitimacy: A randomized field trial of procedural justice." *Criminology* 51(1): 33–63.
- McCrary, Justin. 2007. "The effect of court-ordered hiring quotas on the composition and quality of police." *American Economic Review* 97(1): 318–353.
- Meier, Kenneth J, Robert D Wrinkle, and Jerry L Polinard. 1999. "Representative bureaucracy and distributional equity: Addressing the hard question." *The journal of politics* 61(4): 1025–1039.
- Mosher, William Eugene, John Donald Kingsley et al. 1941. *Public personnel administration*. New York: Harper & Bros.
- Mummolo, Jonathan. 2018a. "Militarization fails to enhance police safety or reduce crime but may harm police reputation." *Proceedings of the national academy of sciences* 115(37): 9181–9186.
- Mummolo, Jonathan. 2018b. "Modern police tactics, police-citizen interactions, and the prospects for reform." *The Journal of Politics* 80(1): 1–15.

- Murphy, David W, and John L Worrall. 1999. "Residency requirements and public perceptions of the police in large municipalities." *Policing: An international journal of police strategies & management* 22(3): 327–342.
- Neavling, Steve. 2017. "Record number of Detroit police live outside the city after 1999 residency law." *Motor City Muckraker*, Jan 3. <http://motorcitymuckraker.com/2017/01/03/record-number-detroit-police-live-outside-city-1999-residency-law/>.
- Nicholson-Crotty, Sean, Jill Nicholson-Crotty, and Sergio Fernandez. 2017. "Will More Black Cops Matter? Officer Race and Police-Involved Homicides of Black Citizens." *Public Administration Review* 77(2): 206–216.
- Ogletree, Charles J, Mary Prosser, Abbe Smith, and William Talley. 1995. *Beyond the Rodney King story: An investigation of police conduct in minority communities*. Northeastern University Press Boston, MA.
- Ostrom, Elinor, and Gordon Whitaker. 1973. "Does local community control of police make a difference? Some preliminary findings." *American Journal of Political Science* 17(1): 48–76.
- Paoline, Eugene A, Jacinta Gau, and William Terrill. 2018. "Race and the Police Use of Force Encounter in the United States." *British Journal of Criminology*: Forthcoming.
- Pettigrew, Thomas F, and Linda R Tropp. 2006. "A meta-analytic test of intergroup contact theory." *Journal of personality and social psychology* 90(5): 751.
- Peyton, Kyle, Michael Sierra-Arévalo, and David G Rand. 2019. "A field experiment on community policing and police legitimacy." *Proceedings of the national Academy of sciences* 116(40): 19894–19898.
- Quillian, Lincoln. 1995. "Prejudice as a response to perceived group threat: Population composition and anti-immigrant and racial prejudice in Europe." *American Sociological Review* 60(4): 586–611.
- Riccucci, Norma M, Gregg G Van Ryzin, and Cecilia F Lavena. 2014. "Representative bureaucracy in policing: Does it increase perceived legitimacy?" *Journal of public administration research and theory* 24(3): 537–551.
- Schulz, Dorothy. 2021. "Don't Require Residency for City Cops." *City Journal*, <https://www.city-journal.org/residency-requirement-for-city-cops>.
- Selden, Sally C, Jeffrey L Brudney, and J Edward Kellough. 1998. "Bureaucracy as a representative institution: Toward a reconciliation of bureaucratic government and democratic theory." *Representative bureaucracy: Classic readings and continued controversies*: Forthcoming.
- Silver, Nate. 2014. "Most Police Don't Live In The Cities They Serve." *FiveThirtyEight*, Aug. 20. <https://fivethirtyeight.com/features/most-police-dont-live-in-the-cities-they-serve/>.

- Skogan, Wesley G, and Kathleen Frydl. 2004. *Fairness and effectiveness in policing: The evidence*. National Academies Press.
- Smith, Brad, and Malcolm Holmes. 2003. "Community Accountability, Minority Threat, and Police Brutality: An Examination of Civil Rights Criminal Complaints." *Criminology* 41(4): 1035–1064.
- Smith, Dennis C. 1980. "Police attitudes and performance: The impact of residency." *Urban Affairs Quarterly* 15(3): 317–334.
- Swank, Calvin James, and James Andrew Conser. 1983. *The police personnel system*. New York: John Wiley & Sons.
- Sweeney, Dan. 2019. "Why do cops get to take their patrol cars home, and how is it regulated?" *South Florida Sun Sentinel*, Nov. 8. <https://www.sun-sentinel.com/news/sound-off-south-florida/fl-ne-sosf-police-take-home-patrol-cars-20191108-b2s7thkvu5gtpl5ta2jy4jxy5m-story.html>.
- Theobald, Nick A, and Donald P Haider-Markel. 2009. "Race, bureaucracy, and symbolic representation: Interactions between citizens and police." *Journal of Public Administration Research and Theory* 19(2): 409–426.
- Trochmann, Maren B, and Angela Gover. 2016. "Measuring the impact of police representativeness on communities." *Policing: An International Journal of Police Strategies & Management* 39(4): 773–790.
- Wilson, Jeremy M, Erin Dalton, Charles Scheer, and Clifford A Grammich. 2010. "Police recruitment and retention for the new millennium." Santa Monica, CA: RAND Corporation.
- Wilson, O.W. 1950. *Police Administration*. New York: McGraw-Hill.
- Xu, Yiqing. 2017. "Generalized synthetic control method: Causal inference with interactive fixed effects models." *Political Analysis* 25(1): 57–76.

# Online Appendix: Residency Blues: The Unintended Consequences of Police Residency Requirements

Intended for online publication only.

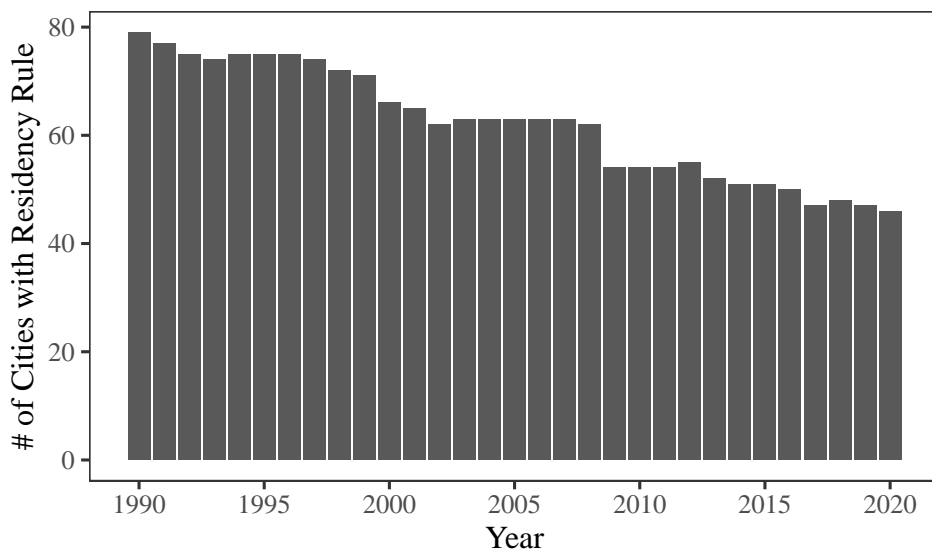
A.2	Collecting Data on Residency Requirements . . . . .	A-2
A.2.1	Descriptive Results . . . . .	A-3
A.3	Additional Statistical Results . . . . .	A-6
A.3.1	Residency Rules and Police Force Diversity . . . . .	A-6
A.3.2	Residency Rules and Fatal Encounters . . . . .	A-8
A.3.3	Residency Rules and Fatal Encounters: Mechanisms . .	A-11
A.3.4	Additional Robustness Checks . . . . .	A-16
A.4	Case Study Evidence . . . . .	A-19

## A.2 Collecting Data on Residency Requirements

To gather up-to-date information about the history of residency requirements for the police departments in our sample, we combined an original survey with additional archival research. For every city in our sample, we emailed either the local police department, the local municipal library, or city hall. If we didn't hear back from the first branch we contacted, we sent a follow-up email and then proceeded to contact another municipal branch (e.g. if the police department didn't respond, we tried the municipal librarian or city hall HR representative). The survey asked whether a city (1) currently has a police residency rule that requires officers to live within city limits as a condition of employment, (2) whether the city has had such a residency requirement at any point since 1987, and if so, (3) when and how the city changed its residency rules.

In total, 452 cities responded to our survey, although in some cases the respondent was unable to provide the information we needed. In these cases, we validated the response by relying on Google searches, local newspaper archives, collective bargaining agreements, and other sources. Using the same approach, we were able to verify the residency rules for each of the 787 cities in our sample over the course of the panel. Figure A.1 shows the number of cities with residency requirements in each year of the sample. Recall that we are defining a residency requirement as a policy that mandates that police officers live within city limits for at least five years, which is the rule in Philadelphia. County or radius residency requirements are not included in our definition. The key source of variation comes from cities dropping their residency rules over time.

**Figure A.1:** Number of Cities with Residency Requirements by Year



## A.2.1 Descriptive Results

Table A.1 shows descriptive statistics for key control variables and outcomes, and Table A.2 provides balance tests for these variables for cities with and without residency rules. Table A.3 lists each of the treated cities in our sample along with the year and method of change.

**Table A.1:** Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
City Residency Requirement	26,758	0.081	0.273	0	1
City Population	26,758	132,240	358,809	1,924	8,804,190
City Pct. White	26,758	0.709	0.192	0.025	0.989
City Pct. Black	26,758	0.153	0.176	0.000	0.935
City Median Income	26,758	43,454	21,903	8,503	225,227
Income Gini Coefficient	26,758	0.387	0.047	0.263	0.586
Number of Officers	5,623	410.246	1,657.257	11	40,435
Pct. White Officers	5,480	0.794	0.187	0.000	1.763
Violent Crimes (Per 1,000)	24,706	6.435	5.674	0.000	51.235
Violent Crimes Clearance Rate	22,779	0.490	0.164	0.002	1.000
Probability of Fatal Encounter	16,527	0.312	0.463	0	1
Community Meetings	3,577	0.829	0.377	0	1
Civilian Complaint Board	3,786	0.188	0.391	0	1
Satisfaction Survey	4,246	0.510	0.500	0	1
Total Reforms	3,336	1.555	0.805	0	3
Total Training Hours	4,546	1,068	595	0	8,704
Num. Complaints	1,641	7.124	27.325	0	451

Data for 787 cities from the years 1987 to 2020. *City Residency Requirement* was hand collected. *City Population*, *City Pct. White*, *City Pct. Black*, *City Median Income*, and *Income Gini Coefficient*, come from the 1980, 1990, and 2000 Census and 2005-2020 American Community Surveys. *Number of Officers* and *Pct. White Officers* come from the 1987, 1990, 1993, 1997, 2000, 2003, 2007, 2013, and 2016 LEMAS Survey. *Violent Crimes* and *Violent Crimes Clearance Rate* come from Federal Bureau of Investigation (2021) and Kaplan (2021) (from 1987 to 2019). *Probability of Fatal Encounter* comes from [fatalencounters.org](https://fatalencounters.org) (from 2000 - 2020). *Community Meetings*, *Civilian Complaint Board*, *Satisfaction Survey*, *Total Reforms*, and *Total Training Hours* come from the 1993, 1997, 2000, 2003, 2007, 2016, and 2020 LEMAS Survey. *Num. Complaints* comes from the 2003, 2007, and 2020 LEMAS Survey.

**Table A.2:** Summary Statistics by City Residency Rule Status

	No Req. (N=24592)		City Req. (N=2166)		$\Delta$	Std. Error
	Mean	Std. Dev.	Mean	Std. Dev.		
City Population	126616	353385	196091	410172	69475	9097
City Pct. White	0.713	0.188	0.666	0.229	-0.047	0.005
City Pct. Black	0.148	0.172	0.207	0.203	0.059	0.005
City Median Income	44017	22112	37055	18194	-6962	416
Income Gini Coefficient	0.385	0.047	0.402	0.044	0.017	0.001
Number of Officers	369.460	1613	828	2012	459	93
Pct. White Officers	0.796	0.185	0.776	0.207	-0.020	0.010
Violent Crimes (Per 1,000)	6.227	5.399	8.761	7.778	2.534	0.177
Violent Crimes Clearance Rate	0.494	0.163	0.452	0.180	-0.041	0.004
Probability of Fatal Encounter	0.312	0.463	0.319	0.466	0.007	0.014
Community Meetings	0.831	0.375	0.804	0.397	-0.026	0.025
Civilian Complaint Board	0.188	0.391	0.189	0.392	0.0009	0.023
Satisfaction Survey	0.524	0.499	0.352	0.478	-0.172	0.028
Total Reforms	1.574	0.804	1.343	0.780	-0.231	0.050
Total Training Hours	1077	596	977	576	-99.7	30.3
Num. Complaints	7.020	27.445	8.568	25.694	1.548	2.538

**Table A.3:** List of Treated Cities

City	Year	Type of Change	Method
Fort Smith, Arkansas	1991	Dropped	local law
Denver, Colorado	2001	Dropped	local law
Washington, District Of Columbia	1998	Dropped	local law
Tinley Park, Illinois	1997	Dropped	local law
Springfield, Illinois	2000	Dropped	local law
Elgin, Illinois	2002	Dropped	local law
Peoria, Illinois	2009	Dropped	local law
Michigan City, Indiana	2008	Dropped	local law
Covington, Kentucky	2002	Dropped	local law
New Orleans, Louisiana	2014	Dropped	local law
Revere, Massachusetts	2002	Adopted	local law
Fall River, Massachusetts	2012	Adopted	local law
Springfield, Massachusetts	2018	Adopted	local law
Lynn, Massachusetts	2019	Dropped	local law
Battle Creek, Michigan	2000	Dropped	state law
Detroit, Michigan	2000	Dropped	state law
Highland Park, Michigan	2000	Dropped	state law
Sterling Heights, Michigan	2000	Dropped	state law
Minneapolis, Minnesota	1999	Dropped	state law
Tupelo, Mississippi	1992	Dropped	local law
Jackson, Mississippi	2017	Dropped	local law
Lee's Summit, Missouri	1992	Dropped	local law
St Louis, Missouri	2020	Dropped	state law
Portsmouth, New Hampshire	2003	Adopted	local law
Portsmouth, New Hampshire	2016	Dropped	local law
Camden, New Jersey	2009	Dropped	local law
Poughkeepsie, New York	1990	Dropped	local law
Long Beach, New York	2004	Dropped	local law
Ramapo, New York	2018	Adopted	local law
Mansfield, Ohio	1991	Dropped	local law
Canton, Ohio	2002	Dropped	local law
Akron, Ohio	2009	Dropped	state law
Cleveland, Ohio	2009	Dropped	state law
Hamilton, Ohio	2009	Dropped	state law
Lorain, Ohio	2009	Dropped	state law
Youngstown, Ohio	2009	Dropped	state law
Wilkes Barre, Pennsylvania	1993	Dropped	local law
Upper Darby, Pennsylvania	1994	Adopted	local law
Altoona, Pennsylvania	2017	Dropped	state law
Pittsburgh, Pennsylvania	2017	Dropped	state law
York, Pennsylvania	2018	Dropped	local law
Memphis, Tennessee	2004	Adopted	local law
Memphis, Tennessee	2009	Dropped	local law
Eau Claire, Wisconsin	1998	Dropped	local law
Green Bay, Wisconsin	2002	Dropped	local law
Kenosha, Wisconsin	2013	Dropped	state law
Milwaukee, Wisconsin	2013	Dropped	state law
Waukesha, Wisconsin	2013	Dropped	state law

## A.3 Additional Statistical Results

### A.3.1 Residency Rules and Police Force Diversity

Table A.4 allows the effect of dropping the residency requirement on the percentage of white officers on the force to vary by the source of the rule change. There is no statistically distinguishable difference between the two coefficients.

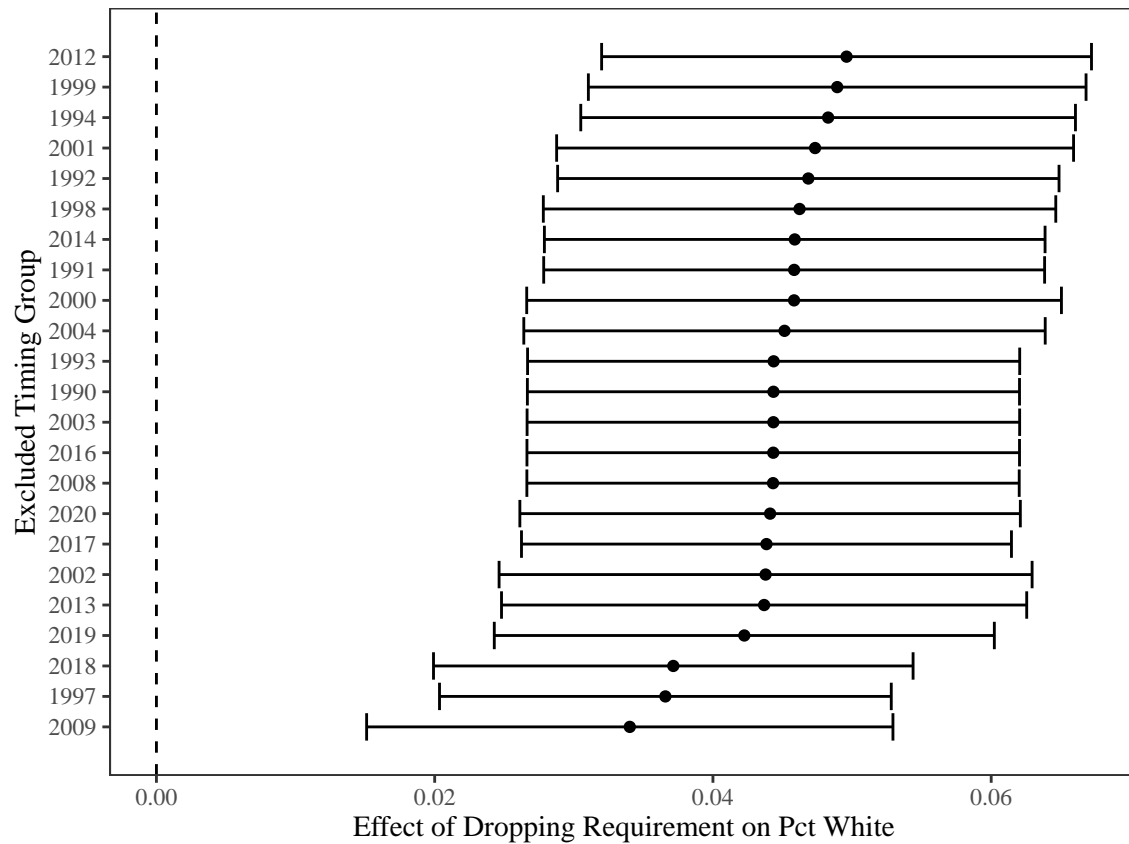
**Table A.4:** Residency Requirements and Pct. White on Police Force

	(1)	(2)
Requirement Dropped (State Law)	0.055* (0.020)	0.056* (0.019)
Requirement Dropped x Local Law	-0.003 (0.025)	-0.013 (0.024)
City and Year FEs	Yes	Yes
Controls		Yes
Mean Outcome	0.79	0.79
Num. Cities	785	785
Observations	5,480	5,480

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

Figure A.2 shows the effect of dropping a residency requirement on the percentage of white officers excluding each timing cohort one at a time. Recall that a timing group includes any treated cities in a given year plus all the “clean control” cities (AKA those that never change their residency status over the course of the panel). This analysis helps to ensure that no single city or treatment year is driving the main results.

**Figure A.2:** Residency Rules and Police Force Diversity: Dropping Each Timing Cohort



### A.3.2 Residency Rules and Fatal Encounters

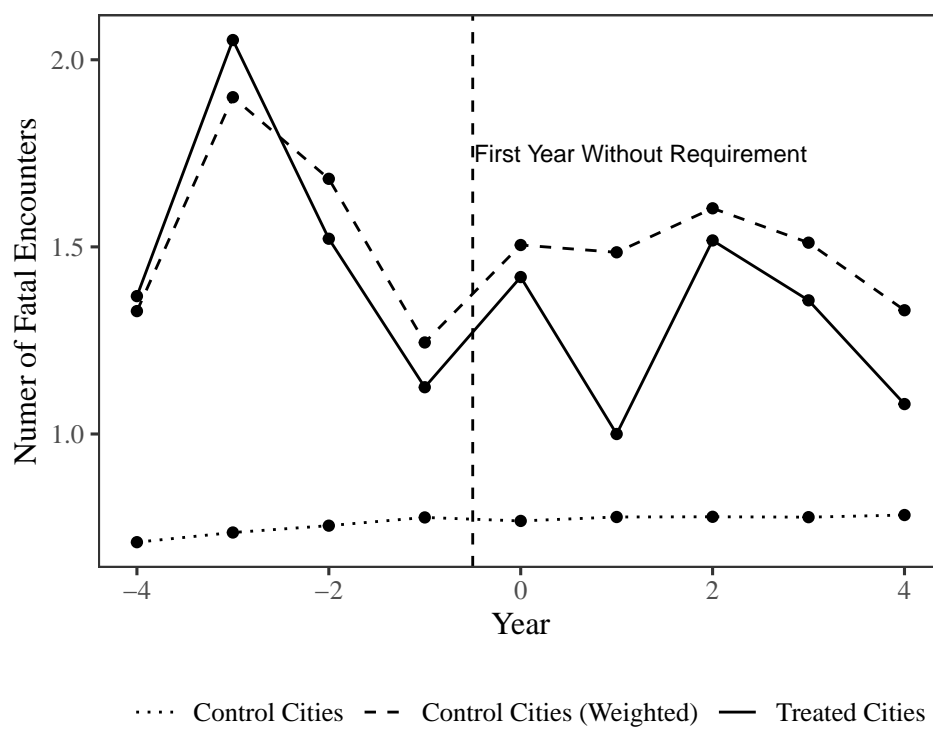
Table A.5 shows that the results remain robust if a count of total fatal encounters is used as the outcome variable. Figure A.3 shows that the balancing weights are able to achieve better parallel trends between treated and control units in the pre-period. As a result, we prefer the regression specification with balancing weights. Figure A.4 shows that the result on fatal encounters is not driven by any particular timing cohort.

**Table A.5:** Residency Requirement and Number of Fatal Encounters

	Original Data		Stacked Approach	
	(1)	(2)	(3)	(4)
Requirement Dropped	-0.342*	-0.314*	-0.333*	-0.212*
	(0.140)	(0.137)	(0.139)	(0.099)
City and Year FEs	Yes	Yes		
Controls		Yes	Yes	Yes
City and Cohort x Year FEs			Yes	Yes
Balancing Weights				Yes
Mean Outcome	0.738	0.738	0.711	0.711
Num. Agencies	787	785	774	774
Observations	16,527	16,485	233,520	171,444

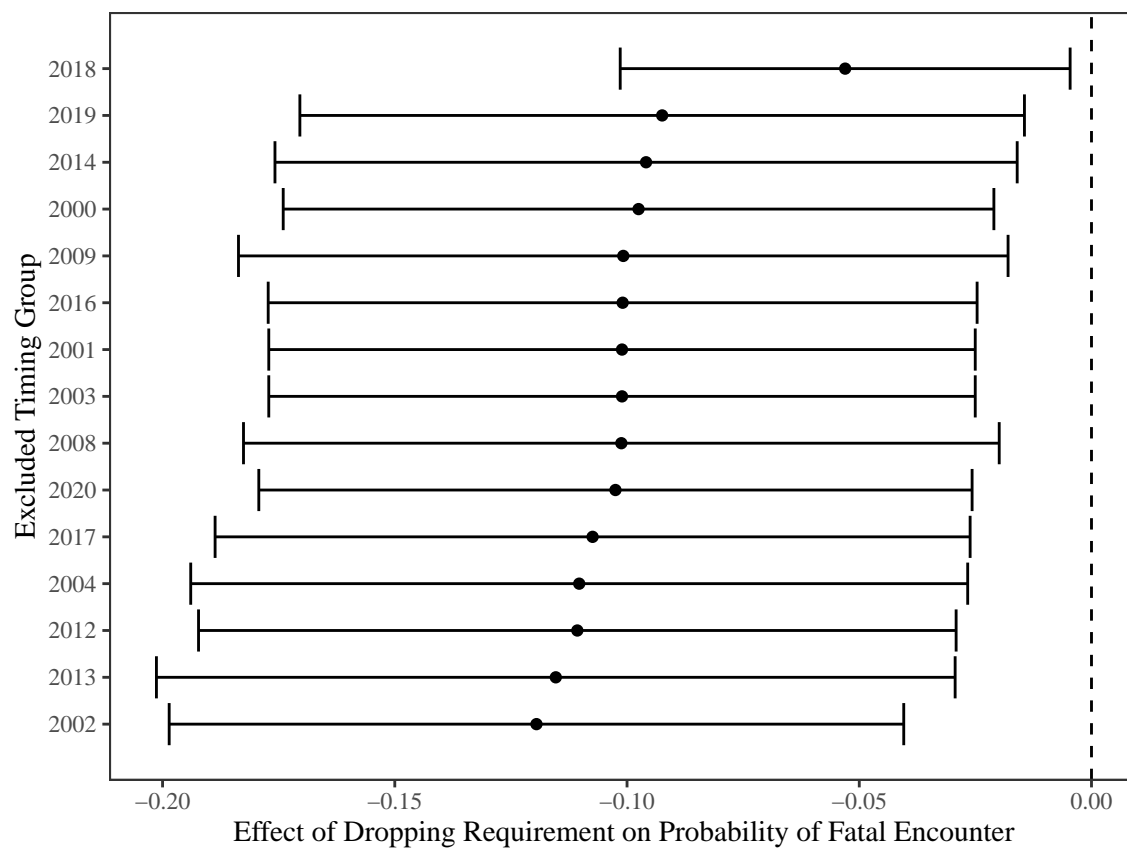
Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. \*p<0.05

**Figure A.3:** Fatal Encounters With Balancing Weights



*Note:* Figure plots raw data. Weights generated via `ebal` in R.

**Figure A.4:** Residency Rules and Fatal Encounters: Dropping Each Timing Cohort



*Note:* Figure shows coefficient estimates from specification described in Table 5 Column 4 excluding each timing cohort one at a time.

### A.3.3 Residency Rules and Fatal Encounters: Mechanisms

Table A.6 decomposes the effect of dropping the residency requirement from any potential effects of the change in the racial composition of city police forces. There is a modest and noisy increase in the probability of a fatal encounter as the percentage of white officers on the force increases, but the effect of the residency rule remains almost identical after including this variable as a control. Table A.7 adds city-specific linear trends to the fatal encounters result to test whether slow moving confounders (such as changing agency culture) might be driving the results. The reduction in fatal encounters remains robust. Table A.8 tests for differences in training requirements after cities drop their residency rule to assess if the residency requirement was substituting for formal training. If anything, there is a small but statistically insignificant decrease in the number of training hours required for new recruits after a city drops its residency requirement, suggesting that this is not a likely mechanism contributing to the reduction in fatal encounters.

**Table A.6:** Residency Requirement, Police Force Diversity, and Probability of Fatal Encounter

	(1)	(2)	(3)	(4)	(5)
Requirement Dropped	−0.097* (0.037)	−0.092* (0.036)			−0.093* (0.036)
Pct. White on Force			0.038 (0.069)	0.026 (0.069)	0.036 (0.069)
City and Year FEs	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes	Yes
Mean Outcome	0.312	0.312	0.312	0.312	0.312
Num. Agencies	787	787	785	785	785
Observations	16,527	16,527	16,485	16,485	16,485

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

**Table A.7:** Residency Requirement and Probability of Fatal Encounters: City Trends

	All Fatalities		Brutal Fatalities	
	(1)	(2)	(3)	(4)
Requirement Dropped	-0.092*	-0.080*	-0.099*	-0.088*
	(0.036)	(0.032)	(0.041)	(0.032)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
City Linear Trends		Yes		Yes
Mean Outcome	0.312	0.312	0.273	0.273
Num. Agencies	787	787	787	787
Observations	16,527	16,527	16,527	16,527

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

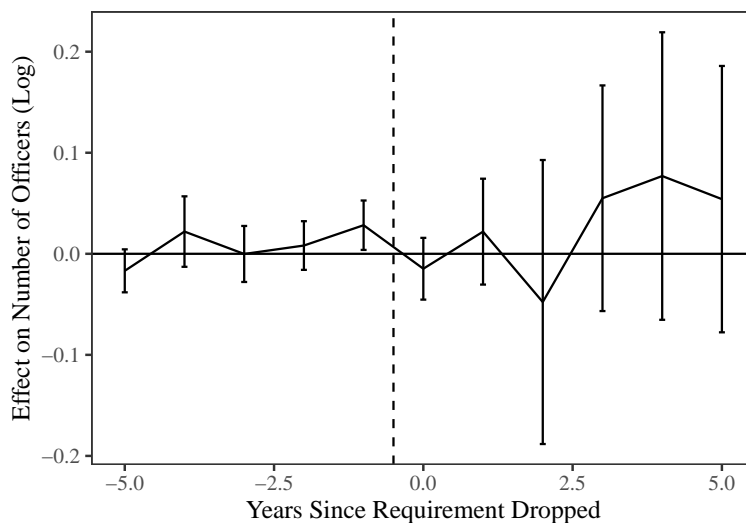
**Table A.8:** Residency Requirement and Police Training

	(1)	(2)
Requirement Dropped	-0.081	-0.080
	(0.069)	(0.071)
City and Year FEs	Yes	Yes
Controls		Yes
Mean Outcome	6.546	6.546
Num. Agencies	784	784
Observations	4,546	4,546

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city.  
\*p<0.05

Figure A.5 shows the event study predicting the number of officers before and after cities drop their residency requirement. Estimates are generated via the `fact` package in R (Xu 2017). There is a modest and very noisy uptick in the number of officers serving on a force after residency requirements are eliminated.

**Figure A.5:** Event Study: Number of Officers



**Figure A.6:** Event Studies: Number of Officers by Type of Change

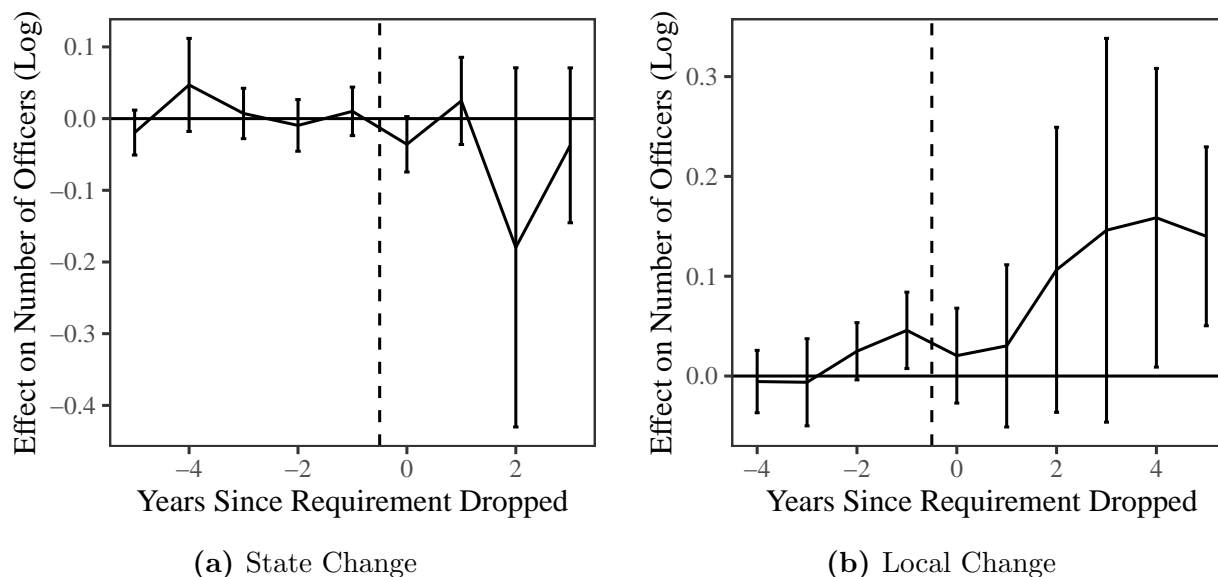


Table A.9 shows that cities that drop their residency rules internally (local change) are more likely to adopt other reforms that could indicate a shift in agency priorities. There is less evidence that this is the case with cities that are forced to drop their requirements via state mandate. However, Table A.10 shows that the reduction in fatal encounters is not driven by these other reforms. Rather, the adoption of these other reforms speaks to the type of city that chooses to drop its residency requirement.

**Table A.9:** Residency Requirements and Community Engagement

	Complaint Board	Community Meetings	Satisfaction Surveys	Total Reforms
	(1)	(2)	(3)	(4)
Requirement Dropped (State)	0.006 (0.085)	0.106 (0.055)	-0.124 (0.153)	0.094 (0.193)
Requirement Dropped x Local Change	0.231* (0.077)	0.017 (0.039)	0.191 (0.099)	0.313* (0.125)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.18	0.83	0.51	1.55
Num. Agencies	759	764	774	758
Observations	39,304	37,185	44,208	34,648

Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. \*p<0.05

**Table A.10:** Residency Requirements, Other Reforms, and Fatal Encounters

	Probability of Fatal Encounter			
	(1)	(2)	(3)	(4)
Complaint Board	−0.027 (0.032)			−0.030 (0.033)
Community Meetings		0.035 (0.023)		0.037 (0.025)
Satisfaction Surveys			0.001 (0.018)	−0.001 (0.021)
Residency Rule Dropped				−0.162 (0.088)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.34	0.34	0.34	0.34
Num. Agencies	766	772	784	766
Observations	2,878	2,972	3,641	2,863

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city.

\*p<0.05

### A.3.4 Additional Robustness Checks

**Table A.11:** Residency Requirements and Outcomes: Effect Symmetry

	Pct. White		Prob. Fatal Encounter	
	Full Sample	Drop Only	Full Sample	Drop Only
	(1)	(2)	(3)	(4)
Requirement Dropped	0.049* (0.013)	0.054* (0.013)	−0.092* (0.036)	−0.104* (0.045)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.79	0.79	0.31	0.31
Num. Cities	785	778	787	780
Observations	5,480	5,438	16,527	16,380

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

**Table A.12:** Residency Requirement and Probability of Fatal Encounter: Additional Controls

	Pct. White		Prob. Fatal Encounter	
	(1)	(2)	(3)	(4)
Requirement Dropped	0.049*	0.048*	−0.092*	−0.085*
	(0.013)	(0.012)	(0.036)	(0.036)
City and Year FEs	Yes	Yes	Yes	Yes
Regular Controls	Yes	Yes	Yes	Yes
Add Inequality Controls		Yes		Yes
Mean Outcome	0.786	0.786	0.312	0.312
Num. Agencies	785	785	787	787
Observations	5,480	5,480	16,527	16,527

Regular controls include city population (logged), median income (logged), % white residents. Inequality controls include a Gini coefficient capturing income inequality and a racial dissimilarity index to proxy for segregation. Robust standard errors clustered by city.

\*p<0.05

**Table A.13:** Residency Requirement and Main Outcomes: State Changes Only

	Pct. White on Force		Prob. Fatal Encounter	
	(1)	(2)	(3)	(4)
Requirement Dropped	0.055*	0.051*	−0.034	−0.034
	(0.020)	(0.014)	(0.046)	(0.046)
City and Year FEs	Yes		Yes	
City and Cohort x Year FEs		Yes		Yes
Balancing Weights		Yes		Yes
Mean Outcome	0.787	0.788	0.311	0.307
Num. Agencies	755	755	757	756
Observations	5,283	113,559	15,897	171,486

Shows the simple before and after effects of residency requirements in cities forced to drop their rule via state change and without control variables. Robust standard errors clustered by city. \*p<0.05

**Table A.14:** Residency Requirement and Probability of Brutal Fatal Encounter

	Full Sample	State Change	Local Change
	(1)	(2)	(3)
Requirement Dropped	−0.102* (0.041)	−0.061 (0.053)	−0.139* (0.057)
City and Year FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean Outcome	0.273	0.272	0.268
Num. Agencies	787	757	771
Observations	16,527	15,897	16,191

Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. \*p<0.05

**Table A.15:** Residency Requirements and Citizen Complaints

	Full Sample	State Change	Local Change
	(1)	(2)	(3)
Requirement Dropped	−4.979 (6.075)	−0.459 (6.614)	−9.661 (9.189)
City and Year FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean Outcome	7.12	7.07	6.89
Num. Agencies	715	687	701
Observations	1,641	1,578	1,606

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. \*p<0.05

## A.4 Case Study Evidence

Perusal of amicus curiae briefs in state cases involving the legality of residency requirements can also shed light on the rationale used to attack versus defend residency requirements. In attacking municipal residency requirements within the State of Ohio, the **Fraternal Order of Police** argued that “...choice of residence is paramount to maintaining a comfortable, healthy, and safe lifestyle. The crime rate in different areas of Ohio varies and employees have a right to decide how much risk they wish to expose themselves and their families to” (Fraternal Order of Police 2008).<sup>10</sup> Similarly, the **Ohio Association of Professional Fire Fighters** contends that residency requirements have the effect of “infringing upon the employees’ right to choose where they live, significantly limiting the employees’ residential options, and negatively impacting important matters such as family finances, family relationships, and social choices” (Ohio Association of Professional Fire Fighters 2008).<sup>11</sup> The same brief expounds on this idea further, even articulating that residency requirements would preclude officers from caring for relatives who live outside of city limits of cities with residency requirements (Ibid). In the State of Wisconsin, where a case was brought against the City of Milwaukee (which had a residency requirement), representatives of the **Milwaukee Professional Fire Fighters Association** and the **Milwaukee Police Association** argue that “state law prohibits discrimination in employment, and interference with the right to organize. Consistent with the above, the state has a legitimate interest in protecting employees against unfairly restrictive employment conditions and establishing uniform residency regulations” (Milwaukee Professional Fire Fighters Association 2014).<sup>12</sup>

For their part, advocates of municipal residency requirements have couched their defense of the policies in functional and economic terms. The **Ohio Municipal League**, for example, states that employees may not be able to adequately perform core tasks of their jobs if they live too far away from their locales of employment (Ohio Municipal League 2008).<sup>13</sup> The **City of Dayton** advances the economic argument when it argues that “As a practical matter, prohibiting Dayton from requiring residency for its employees will have a detrimental effect, both economically and socially, on the city and throughout its neighborhoods. Dayton has over 2,100 employees with 70% living in the Northeast and Southeast sections of the city. Eighty percent of the police and fire forces also live in these sections of the city. City employees who live in the neighborhoods provide a sense of unity, security, and commitment to the neighborhoods. These core essentials of maintaining a neighborhood will be greatly diminished if employees are permitted to live outside the city” (City of Dayton 2008).<sup>14</sup> The

---

<sup>10</sup>Fraternal Order of Police. 2008. “Brief of Amicus Curiae of Fraternal Order of Police of Ohio, Incorporated in Support of Appellant, State of Ohio et al.”

<sup>11</sup>Ohio Association of Professional Fire Fighters. 2008. “Brief of Amicus Curiae Ohio Association of Professional Fire Fighters in Support of Appellant State of Ohio.”

<sup>12</sup>Milwaukee Professional Fire Fighters Association Local 215. 2014. “Amended Response Brief of Intervenor-Plaintiff-Respondent Local 215 Professional Fire Fighters Association Local 215.”

<sup>13</sup>Ohio Municipal League. 2008. “Brief of Amicus Curiae Ohio Municipal League in Support of Appellee the City of Lima.”

<sup>14</sup>City of Dayton. 2008. “Brief of Amicus Curiae The City of Dayton in Support of Appellee the City of Lima.”

**City of Milwaukee** advances a similar argument and alleges that it will experience economic decline (the City conjures up a scenario where it potentially experiences a decline similar to that of Detroit) if residency requirements are lifted (Supreme Court of Wisconsin 2016).<sup>15</sup>

---

<sup>15</sup>Supreme Court of Wisconsin. 2016. “James Black, Glen Podelsnik, and Steven Van Erden versus the City of Milwaukee.”