Residency Blues: The Unintended Consequences of Police Residency Requirements *

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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 600 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 difference-in-difference 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 don't appear to offer a particularly fruitful avenue for reform.

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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 change in state law (Neavling 2017). The Minnesota State Legislature similarly overturned a local 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 600 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-community relations. Using a within-city design, we find that residency rules are associated with less white police forces, but we also find that civilian fatalities 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 improve police-civilian contact.

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*; Blair et al. 2021; Eckhouse 2022). However, while our study provides some of the most systematic and credible evidence to date on the effects of residency requirements, we also emphasize that one of the issues with current debates about these requirements is that scholars face considerable obstacles to conducting robust and generalizable policy analysis given the available data.

2 Why Residency Rules Might Improve Police Performance

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 policy-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 residency rules affect the racial composition of municipal police force and the prevalence of fatal encounters between police and civilians.

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). In 2014, 49 percent of black police officers in the 75 largest departments lived within the boundaries of the cities they served, while only 35 percent of white officers did (Silver 2014).

There are a variety of reasons why racially diverse police forces might in turn lead to better relationships between police and communities. Scholars have demonstrated that more descriptively representative police departments are 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 elemen-

tary 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 recently 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.[1] 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 change the available labor pool and limit the ability of white suburban officers to work in central cities, *we should expect these laws to be associated with greater racial diversity among police officers.* 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.*

3 The Downsides of Residency Requirements

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

[1] For a review, see Pettigrew and Tropp (2006).

to unintended consequences. For example, cities and police departments can engage in recruitment efforts to hire officers from diverse 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 talent pool—an argument frequently made by public safety unions (Bouza 1978; Dorschner 1989). In an amicus curaie 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."[2]

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) demonstrates this pattern even more systematically 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. Even if residency rules do lead to more frequent interactions between cops and community members, recent 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 (Blumer 1958; Quillian 1995; Bobo 1999).

[2] *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.

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 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). Unions complain that no other profession is subject to similar stipulations. "What other kind of business puts those kind of restrictions on a person?" asked Donald Taylor, president of the Retired Detroit Police and Fire Fighters Association (Livengood and Annalise 2020). If residency requirements cause cops to feel resentful and fear retaliation, they may be less well-equipped to perform their jobs. In another amicus curiae brief filed in the Ohio case, the Fraternal Order of Police asserted, "After all, an employee who is comfortable is a better employee. Returning to the comforts of home is vital to the maintenance of a healthy mental state."[3]

To sum up, if the arguments described in this section are correct, we *may or may not observe a difference in racial diversity* among police officers when cities adopt residency rules, and we would expect the *rate of fatal encounters to be no better (and perhaps even worse)* when these requirements are in place.

4 Existing Empirical Research

Despite the lively theoretical debates that continue to surround the idea of residency requirements, empirical academic research in this area has lagged behind. 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

[3] *City of Lima v. State of Ohio* (2008) Brief of Amicus Curiae: Memorandum in Support of Amicus Curiae of Fraternal Order of Police. In Appendix A.3 in the on-line supporting information, we provide additional quotes and qualitative evidence outlining the arguments for and against police residency requirements drawn from the text of recent state supreme court cases in Ohio and Wisconsin.

(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 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) find 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.

Over the past few years, several notable reform organizations like Communities United Against Police Brutality (CUAPB) have published recent 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). Given that the existing empirical literature is both conflicting and sparse and as major cities in the U.S. deliberate these rules with renewed vigor, we are overdue for a systematic examination of the effects of residency requirements.

5 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 that appeared in at least one Law Enforcement and Administrative Statistics (LEMAS) survey between 1987 and 1997—nearly 600 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. 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 types of cities that have historically experimented with residency requirements are large urban cities nestled among surrounding suburbs or exurbs where police officers might choose to live while commuting into the central city for work (Eisinger 1983). 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 applies are explicitly geared toward large and mid-sized cities with sizeable police forces.[4]

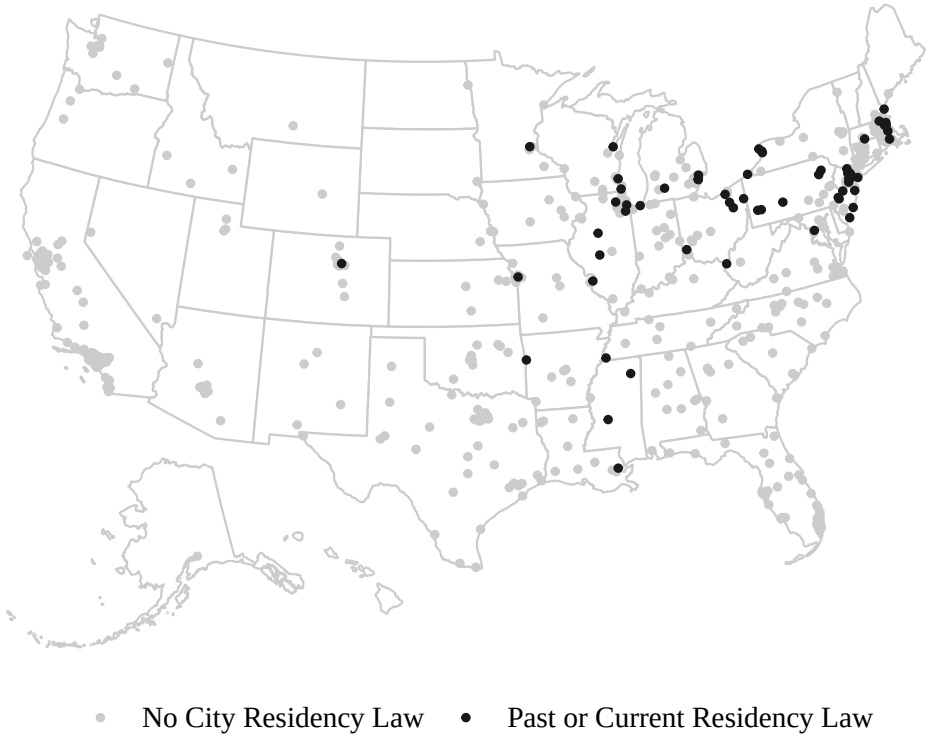
Although the LEMAS survey originally included a question about residency requirements, it stopped doing so in 1997. Because many of the high profile instances of state governments banning local residency requirements occurred during the 1990s and early 2000s (Wilson et al. 2010), we opted to contact agencies directly to gather up-to-date information about their residency rules. In total, 380 cities responded to our survey, and we were able to obtain information on the history of residency requirements for the remaining 204 cities by relying on local newspapers, city council codes, collective bargaining agreements, and other publicly available sources. The final panel consists of 584 municipal law enforcement agencies and runs from 1987 to 2020.[5] For additional details about the data collection process, see the on-line appendix.

Although many cities and states are currently contemplating changes to their residency laws, the data reveal that municipal residency requirements for police are relatively rare. In fact, only 42 cities in our sample (7%) 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

[4]Later, we show that our results are in fact even stronger if we restrict our sample to cities with more than 100,000 residents, suggesting that our threshold is effectively capturing the relevant universe of cities.

[5]Of the 595 total agencies we identified by the LEMAS survey, 11 were either disbanded or otherwise restructured so as to no longer meet the sampling criterion.

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.



a particular 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 residency law at the heart of most theoretical and policy debates.

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.

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 outcomes at the heart of theoretical debates about these laws. First, we examine whether police residency requirements increase the racial diversity of police forces by focusing on the *Percentage of 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 the ability of white officers to live in the suburbs while working within the 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 in 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 and doesn't extend past 2016. However, after filing Public Records requests with dozens of cities in our data, it became clear that LEMAS was the only feasible source for documenting officer race over time.[6]

Next, we examine the number of *Fatal Police 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

[6]For example, some cities only maintain databases on the race of their officers going back a few years, and many cities currently have covid-era exemptions in place that allow them to deny certain public records requests due to staffing shortages.

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 or misconduct complaints, 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 and Communities United Against Police Brutality.

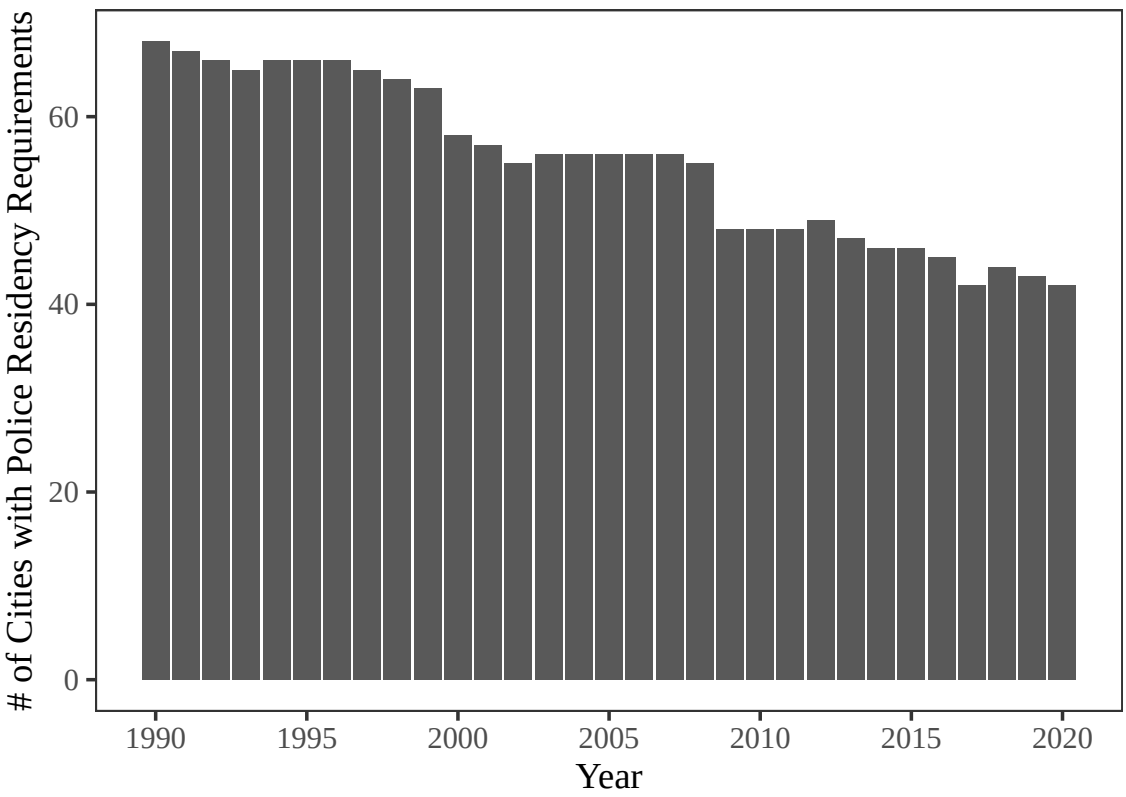
Finally, to compare our results to existing cross-sectional work, in the appendix we also study the relationship between residency rules and violent crime and crime clearance rates, which are taken from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) data (Federal Bureau of Investigation 2021; Kaplan 2021). Unfortunately, the UCR data have been demonstrated to be unreliable. Both crime and crime clearance rates are self-reported, and police agencies have the ability to manipulate these statistics (e.g. Cook and Fortunato 2022). We perform this exercise simply to demonstrate that a cross-sectional approach would uncover a spurious relationship between residency requirements and crime rates that is attenuated by the two-way fixed effects design. While crime would be a theoretically meaningful outcome that should be studied in future work, higher quality panel data on crime rates (such as the Bureau of Justice Statistics's National Crime Victimization Survey) currently don't exist for a large enough set of municipalities in our sample beyond the largest metropolitan areas.

6 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 or generalized difference-in-differences approach where we examine how outcomes evolve in cities that change their requirements relative to cities that don't. In the early 1990s, 68 cities enforced residency laws. Between 1990 and 2020, 40 cities changed their rules, with 33 cities dropping their requirements and 7 cities adding new requirements. Figure 2 shows this variation over time, and Table A.3 in the appendix 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.

Figure 2: Number of Cities with Residency Requirements by Year



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.[7] 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

$$y_{it} = \beta_1 Dropped\ Requirement_{it} + \beta_2 X_{it} + \gamma_i + \delta_t + \varepsilon_{it}. \tag{6.1}$$

The coefficient of interest, β_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 γ_i are city fixed effects that account for persistent characteristics like agency culture, and the δ_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

[7] To be clear, we include all 40 cities with rule changes in our analyses, and the estimator essentially captures average differences between periods with and without residency requirements. Later, we show that the results are not sensitive to dropping the 7 cities that added requirements as opposed to dropping them.

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:

$$y_{igt} = \beta_1 Dropped\ Requirement_{igt} + \beta_2 X_{it} + \gamma_{ig} + \delta_{tg} + \varepsilon_{igt} \tag{6.2}$$

.

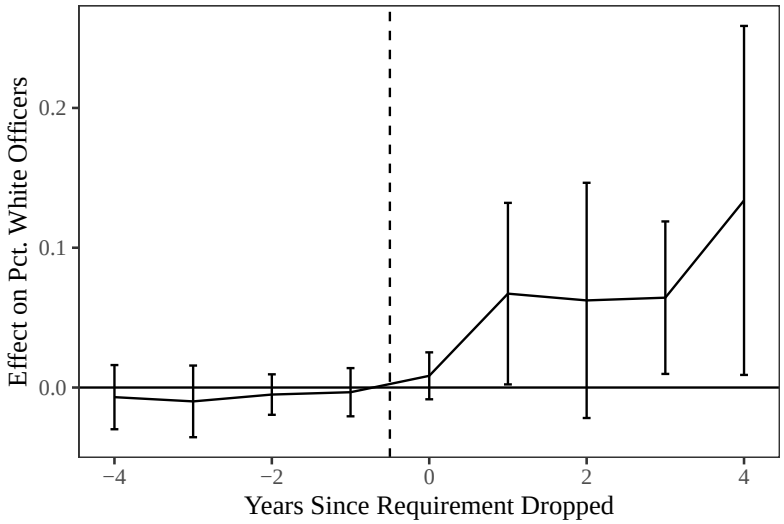
Now, g identifies the timing group, and δ_{tg} represents year-by-group fixed effects. We can interpret β_1 as the effect of dropping a residency requirement on the outcome of interest under the assumption that treated and never treated cities would have been on the same average trajectory had neither 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.

7 Residency Rules Improve Officer Diversity

We begin by addressing the question of whether residency requirements help to increase the racial diversity on municipal police forces. First, we show an event study that plots the percentage of 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 `fect` 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 3: Event Study: Police Force Diversity



Note: Figure generated via `fect` in R. See Table 1 Column 2 for regression results.

Figure 3 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 year leading up the change in residency rules. However, we show 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 1: Residency Requirements and Pct. White on Police Force

	Original Data		Stacked Approach	
	(1)	(2)	(3)	(4)
Requirement Dropped	0.044*	0.038*	0.038*	0.041*
	(0.015)	(0.016)	(0.016)	(0.011)
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	584	584	577	577
Observations	4,030	4,030	68,157	68,157

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

Table 1 displays the results generated via equations 6.1 and 6.2 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 approch" 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 percentage points relative to non-treated cities. Note that this estimate includes both any increases in the proportion white among cities that drop their rules as well as decreases in the proportion white among cities that don't change their rules.[8]

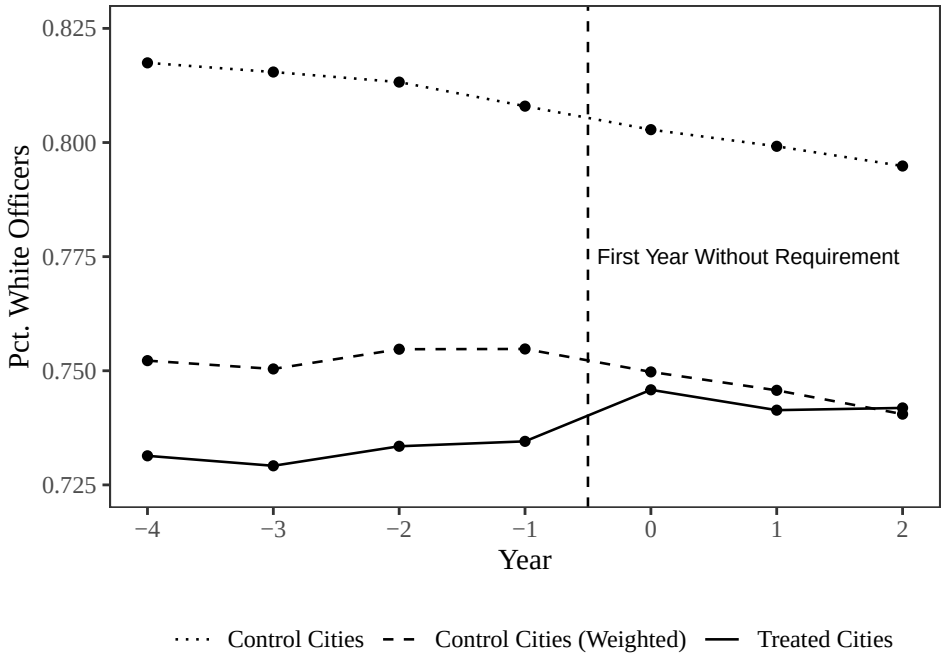
[8]While a 4 percentage point increase would be quite massive (e.g. a median sized force of 170 officers and 65% white would have to hire 20 new white officers to increase the proportion white to just under 69%), the raw data reveal that this effect is driven by both a small increase in the number of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

In the appendix, we also demonstrate that the results are robust to dropping each timing cohort from the analysis (Figure A.1). 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.1 shows that the results are not sensitive to excluding any specific timing cohort.

Finally, Column 4 in Table 1 adds balancing weights to ensure that treated and control cities are following the same trajectory prior to the change in policy. To provide intuition about what this balancing achieves, Figure 4 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 4, 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 4, 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.

Figure 4: Residency Rules and Police Force Diversity: Trajectory Balancing



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

7.1 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 2. 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 estimates become slightly noisier with the reduction in the number of treated cities, but the pattern is very similar in both cases. This analysis suggests that it really is something about the residency restriction that is changing the racial composition of the pool of officers. For example, if the effect were driven by cities that 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.

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

	State Change	Local Change	Pop > 100,000	City < 70% White
	(1)	(2)	(3)	(4)
Requirement Dropped	0.038*	0.039	0.047*	0.072*
	(0.016)	(0.022)	(0.014)	(0.024)
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	584	570	260	326
Observations	4,030	3,931	1,777	1,744

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

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 white (the sample average). While we don't have strong theoretical predictions about the types of cities where the effects of residency rules will be strongest, the cities currently debating these laws tend to be both large and racially diverse. Strikingly, the effect is almost twice as big for agencies in racially diverse cities (Column 4) compared to the estimates for the full sample introduced in Table 1. Among cities where fewer than 70 percent of residents are white, eliminating residency rules leads to a 7.2 percentage point increase in the percentage of white officers relative to other cities.[9]

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

[9]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 3: Residency Requirements and Pct. White on Police Force by Agency Growth

	Shrinking Agencies	Growing Agencies
	(1)	(2)
Requirement Dropped	−0.008	0.051*
	(0.036)	(0.018)
City and Year FEs	Yes	Yes
Controls	Yes	Yes
Mean Outcome	0.8	0.8
Num. Cities	165	419
Observations	1,000	3,030

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

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 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 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 3 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.

8 Fewer Fatal Encounters After Cities Drop Residency Rules

We now turn to the question of whether residency requirements improve interactions between police and communities. To account for any changes in outcomes that might be due to a shift in the racial composition of the force, we now adjust for a time-varying measure of the percentage of white officers in each of the following analyses (although the results are not sensitive to this choice). The fact that police departments become whiter after dropping their residency laws—especially in diverse cities—might mean that police-civilian relations subsequently deteriorate. On the other hand, if relaxing these laws bolsters the quality of the talent pool or enhances officer morale, it's possible that outcomes might instead improve. To proxy for the quality of the relationship between city residents and their police forces, we rely on the Fatal Encounters data and study how eliminating residency requirements impacts the number of civilian deaths that occur during police interactions. After presenting the main results, we perform several robustness checks to address some of the known issues associated with these 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 6.1 predict the probability of a fatal encounter conditional on treatment.[10] In Table 4, we observe a dramatic decrease in the probability that a civilian dies during an interaction with the police after cities drop their

[10] 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.4

Table 4: Residency Requirement and Probability of Fatal Encounter

	Original Data		Stacked Approach	
	(1)	(2)	(3)	(4)
Requirement Dropped	−0.090*	−0.085*	−0.087*	−0.070*
	(0.039)	(0.038)	(0.039)	(0.026)
City and Year FEs	Yes	Yes		
Controls		Yes	Yes	Yes
City and Cohort x Year FEs			Yes	Yes
Balancing Weights				Yes
Mean Outcome	0.352	0.352	0.346	0.346
Num. Agencies	584	584	577	577
Observations	12,264	12,264	172,641	126,777

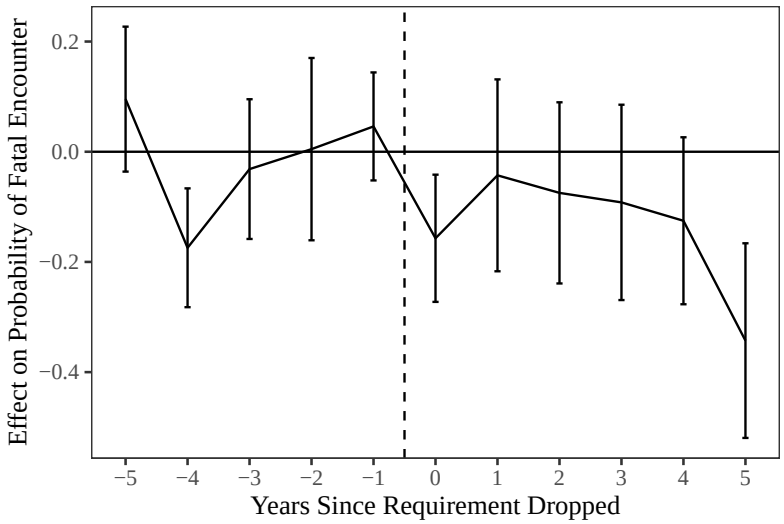
Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. *p<0.05

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. On average, the probability of a fatal encounter in any given year is around 35%. A 7-9 percentage point decrease in this probability is thus substantively quite large.

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 5 are a bit imprecise, which makes sense given that civilian deaths are relatively rare events. Although the effects jump around a bit, there is no clear trend in civilian deaths during police encounters before cities drop their residency requirements. After these laws are abandoned, however, cities experience a substantially lower likelihood of a fatal encounter, especially after a few years have passed.

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 noisy the data are and how few cities actually dropped

Figure 5: Event Study: Fatal Encounters



Note: Figure generated via `fect` in R. See Table 4 Column 2 for regression results.

their requirements over the course of the panel. At the same time, police killings can also serve as inflections points that can 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 after residency rules were loosened. Moreover, in Figure A.3, 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.

8.1 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 5, 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). Interestingly, the method of the change matters a great deal in terms of predicting the probability of

a fatal encounter. 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 in a police encounter when states overturn city residency requirements, this effect is much smaller and not statistically distinguishable from zero.

Table 5: Residency Requirement and Probability of Fatal Encounter

	State Change	Local Change	Pop > 100,000	City < 70% White
	(1)	(2)	(3)	(4)
Requirement Dropped	−0.027	−0.127*	−0.099*	−0.127*
	(0.058)	(0.046)	(0.048)	(0.047)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.352	0.346	0.59	0.435
Num. Agencies	560	570	265	357
Observations	11,760	11,970	5,024	6,194

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

This finding begins to hint at mechanisms that might be driving the average effects uncovered in Table 4. For example, it seems unlikely that the decrease in fatal encounters is caused by changes in officer morale. Whether the 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. Instead, the fact that the results are driven by cities that drop their residency laws internally suggests that there may be other conditions changing within either the city or the agency that might be responsible for the decrease in fatal encounters. For example, perhaps police departments relax their requirements because they perceive police-community relations to be improving, or because the composition of the city council changes in a particular ideological direction.

Qualitative evidence suggests that cities drop their residency rules for a variety of idiosyncratic reasons. Police unions are sometimes able to loosen restrictions under new collective bargaining agreements or consent decrees, and city councils periodically review their residency rules. To understand the political dynamics accompanying the choice to drop such

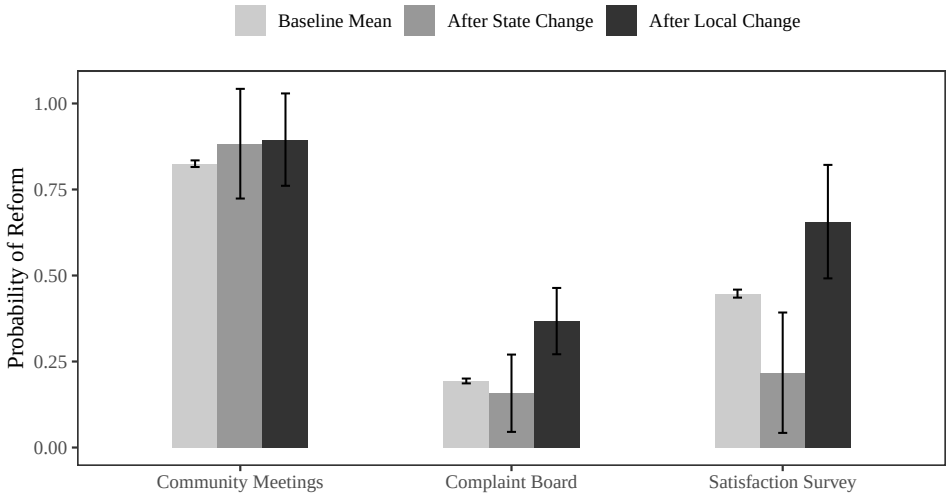
requirements, we examined the elections leading up to the policy change for the cities in our sample for which we could find this information. We found no correlation between election timing or city council turnover and the likelihood that a city changed its residency rules.

Of course, if a city changes its police residency policy, there is a good chance that it might also be enacting 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 a shift in police culture. 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 6 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 ranges from 0 to 3. All of these results are shown formally in Table A.5 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.

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, it becomes difficult to

Figure 6: Other Reforms After Dropping Residency Requirement



Note: See Table A.5 for regression results.

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 community outcomes. 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 simply no evidence that these policies in and of themselves improve the risk of fatal police-civilian encounters.

9 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. First, a handful of cities in our sample including Nashville and Indianapolis have metropolitan police departments that serve not only the central city but the surrounding county. In Table

A.6 in the appendix, we confirm that dropping these cities from our analyses does not change the main results. Throughout the paper, the key source of the variation comes from 33 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 nearly identical to the overall effects (Table A.7).

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.9 and Figure A.4. To address the fact that the Fatal Encounters dataset contains higher rates of missingess 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.10).

To build more directly off existing research, we performed an auxiliary analysis to examine whether residency rates appear to influence violent crime and crime clearance rates. We choose to focus specifically on the dynamics of violent crimes to address the fact that changes to petty crime arrests may reflect over-policing, which does not necessarily indicate that public safety conditions are improving—especially in predominantly poor and minority neighborhoods (Greene 1999). As we discussed early, scholars have typically relied on the FBI's Uniform Crime Reporting program for data on crime rates, but recent research suggests that these measures are simply too unreliable to draw strong conclusions (Cook and Fortunato 2022). Using our two-way fixed effects approach, we find no effect of residency rules on either of these crime-based outcomes (Tables A.11 and A.12).

As a methodological point, we note that a pooled cross sectional approach uncovers a spurious and significant relationship between residency laws and violent crime rates that attenuates after including city fixed effects. Interestingly, we also uncover no pre-treatment

trends for either of the crime related outcomes prior to the change in residency rules (Figure A.5). We caution against interpreting these results at face value given data quality issues. However, even if we assume that police agencies are strategically manipulating their crime and crime clearance rates, we can rule out that cities dramatically change their reporting behavior after dropping their residency requirements. We also demonstrate that all of the main results remain consistent after adjusting for time varying reported crime rates in Table A.8.

10 Discussion

Our survey of the largest municipal police departments provides a much needed update to the literature on police residency requirements. Theoretically, there are a variety of arguments both for and against residency rules for local bureaucrats. Despite the fact that many state and local officials are actively debating the merits of these laws, it turns out that relatively few large cities currently enforce residency requirements for their police officers. 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 on the force but are associated with higher rates of fatal civilian encounters.

Future work might expand on this research in several ways. Our outcome measures of police performance are fairly blunt and do not necessarily capture the complex ways in which police and communities interact. While these indicators are commonly used in academic research due to the fact that they are available for a large number of agencies over time, a deep dive into a few large cities might allow researchers to collect more granular and meaningful data on police-community outcomes at the city level. 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.

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. 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). However, the residency laws that we study in this paper are at the heart of current policy debates and much of the theoretical scholarship.

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

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

Intended for online publication only.

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, 305 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 584 cities in our sample over the course of the panel.

A.2.1 Descriptive Results

Table A.1 shows descriptive statistics for key control variables and outcomes. Table A.2 provides balance tests for these variables for cities with and without residency requirements in our 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. 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

	N	Mean	SD	Min	Max
City Residency Requirement	19,856	0.096	0.295	0	1
City Population	19,856	158,566	411,865	2,148	8,419,316
City Pct. White	19,856	0.711	0.192	0.034	0.994
City Pct. Black	19,856	0.161	0.177	0.000	0.937
City Median Income	19,856	42,072	20,805	8,503	225,227
Number of Officers	4,029	474	1,848	2	40435
Pct. White Officers	4,028	0.786	0.180	0.000	1.000
Violent Crimes (Per 1,000 Residents)	185,44	6.900	5.699	0.002	69.539
Violent Crimes Clearance Rate	17,658	0.479	0.161	0.002	1.000
Probability of Fatal Encounter	12,264	0.352	0.478	0.000	1.000
Community Meetings	2383	0.827	0.378	0.000	1.000
Civilian Complaint Board	2,599	0.197	0.398	0.000	1.000
Satisfaction Survey	2,832	0.450	0.498	0.000	1.000
Total Reforms	2,259	1.485	0.807	0.000	3.000

Data for 584 cities from the years 1987 to 2020. *City Residency Requirement* was hand collected. *City Population*, *City Pct. White*, *City Pct. Black*, and *City Median Income* 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` (from 2000 - 2020). *Community Meetings*, *Civilian Complaint Board*, *Satisfaction Survey*, and *Total Reforms* come from the 1993, 1997, 2000, 2003, 2007, and 2016 LEMAS Survey).

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

	No Req. (N=17,943)		City Req. (N=1,913)			
	Mean	Std. Dev.	Mean	Std. Dev.	Δ	Std. Error
City Population	152,759	409,182	213,030	432,568	60,271	10,351
City Pct. White	0.715	0.188	0.672	0.228	-0.042	0.005
City Pct. Black	0.156	0.173	0.215	0.202	0.059	0.005
City Median Income	42,685	21,054	36,332	17,285	-6,352	425
Number of Officers	426	1,810	911	2113	485	109
Pct. White Officers	0.789	0.177	0.765	0.201	-0.023	0.010
Violent Crimes (Per 1,000)	6.647	5.298	9.203	8.172	2.556	0.195
Violent Crimes Clearance Rate	0.483	0.159	0.447	0.178	-0.036	0.004
Probability of Fatal Encounter	0.354	0.478	0.330	0.471	-0.024	0.015
Community Meetings	0.827	0.379	0.831	0.376	0.004	0.027
Civilian Complaint Board	0.197	0.397	0.202	0.402	0.005	0.027
Satisfaction Survey	0.463	0.499	0.316	0.466	-0.147	0.031
Total Reforms	1.500	0.809	1.327	0.770	-0.174	0.057

Table A.3: List of Treated Cities

City	Year	Type of Change	Method
Fort Smith, AR	1991	Dropped	local law
Denver, CO	2001	Dropped	local law
Washington, DC	1998	Dropped	local law
Elgin, IL	2002	Dropped	local law
Peoria, IL	2009	Dropped	local law
Springfield, IL	2000	Dropped	local law
Tinley Park, IL	1997	Dropped	local law
Michigan City, IN	2008	Dropped	local law
New Orleans, LA	2014	Dropped	local law
Fall River, MA	2012	Adopted	local law
Lynn, MA	2019	Dropped	local law
Revere, MA	2002	Adopted	local law
Springfield, MA	2018	Adopted	local law
Battle Creek, MI	2000	Dropped	state law
Detroit, MI	2000	Dropped	state law
Highland Park, MI	2000	Dropped	state law
Sterling Heights, MI	2000	Dropped	state law
Minneapolis, MN	1999	Dropped	state law
St. Louis, MO	2020	Dropped	state law
Jackson, MS	2017	Dropped	local law
Tupelo, MS	1992	Dropped	local law
Portsmouth, NH	2003	Adopted	local law
Portsmouth, NH	2016	Dropped	local law
Camden, NJ	2009	Dropped	local law
Long Beach, NY	2004	Dropped	local law
Suffern, NY	2018	Adopted	local law
Akron, OH	2009	Dropped	state law
Canton, OH	2002	Dropped	local law
Cleveland, OH	2009	Dropped	state law
Hamilton, OH	2009	Dropped	state law
Youngstown, OH	2009	Dropped	state law
Altoona, PA	2017	Dropped	state law
Pittsburgh, PA	2017	Dropped	state law
Upper Darby, PA	1994	Adopted	local law
Wilkes-Barre, PA	1993	Dropped	local law
Memphis, TN	2004	Adopted	local law
Memphis, TN	2009	Dropped	local law
Green Bay, WI	2002	Dropped	local law
Kenosha, WI	2013	Dropped	state law
Milwaukee, WI	2013	Dropped	state law

A.3 Additional Statistical Results

A.3.1 Residency Rules and Police Force Diversity

Figure A.1 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.1: Residency Rules and Police Force Diversity: Dropping Each Timing Cohort

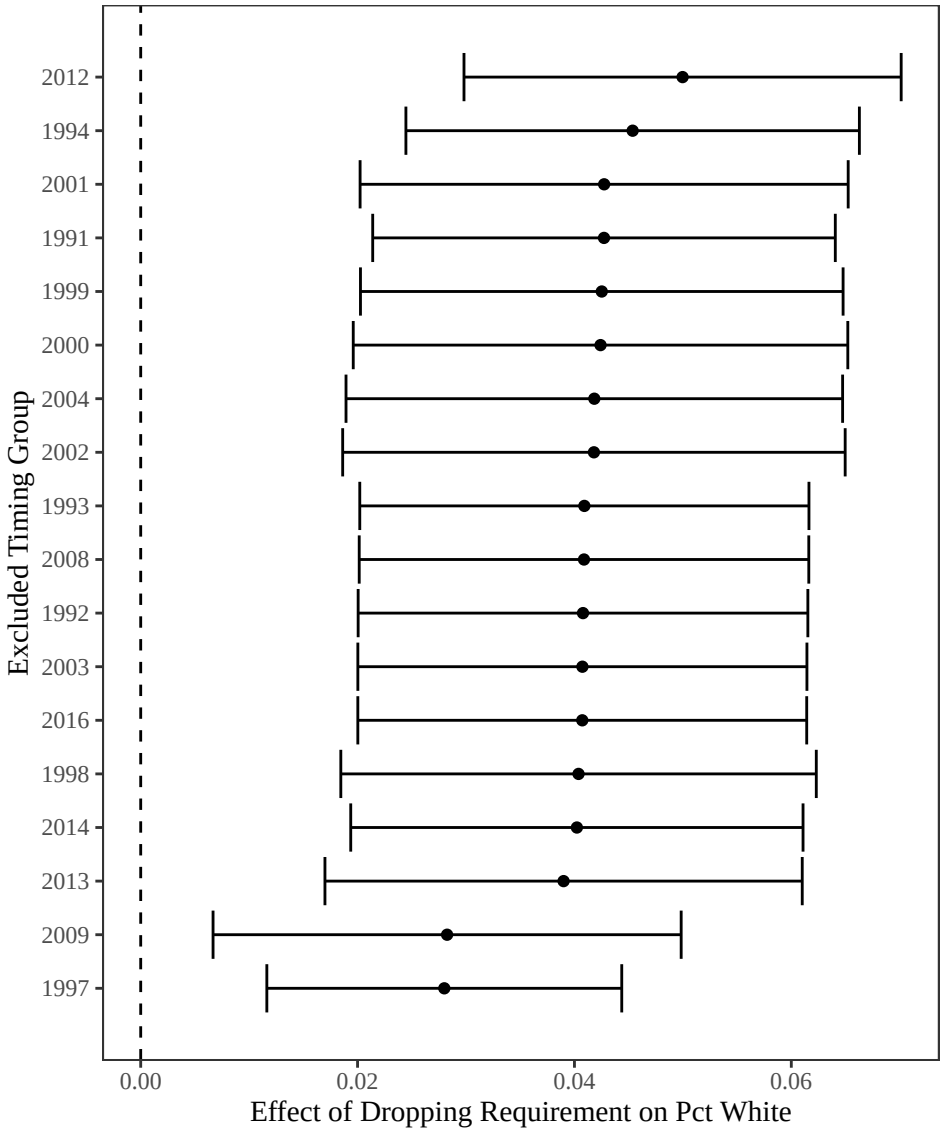


Figure A.2 shows the event study predicting the number of officers before and after cities drop their residency requirement. Estimates are generated via the `fect` package in R (Xu 2017). There is a modest and very noisy uptick in the number of officers serving on a force after residency requirements are eliminated.

Figure A.2: Event Study: Number of Officers

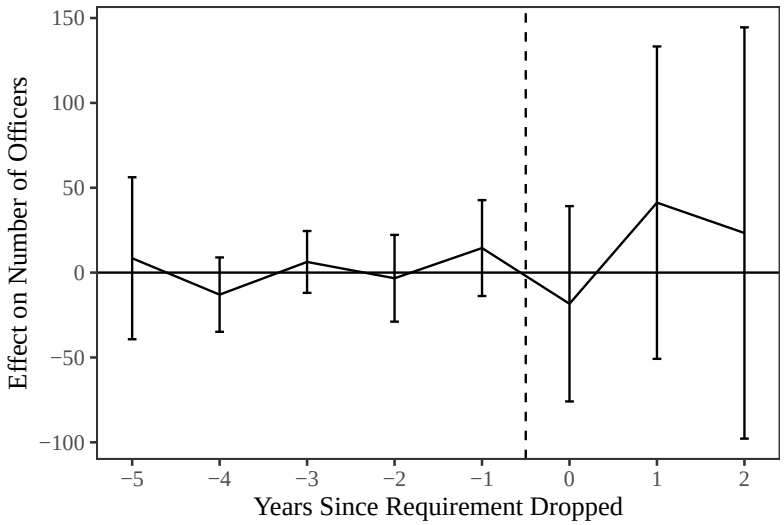


Table A.4: Residency Requirement and Number of Fatal Encounters

	Original Data		Stacked Approach	
	(1)	(2)	(3)	(4)
Requirement Dropped	−0.370*	−0.308*	−0.325*	−0.231
	(0.161)	(0.153)	(0.155)	(0.126)
City and Year FEs	Yes	Yes		
Controls		Yes	Yes	Yes
City and Cohort x Year FEs			Yes	Yes
Balancing Weights				Yes
Mean Outcome	0.907	0.907	0.874	0.875
Num. Agencies	584	584	577	577
Observations	12,264	12,264	172,641	126,777

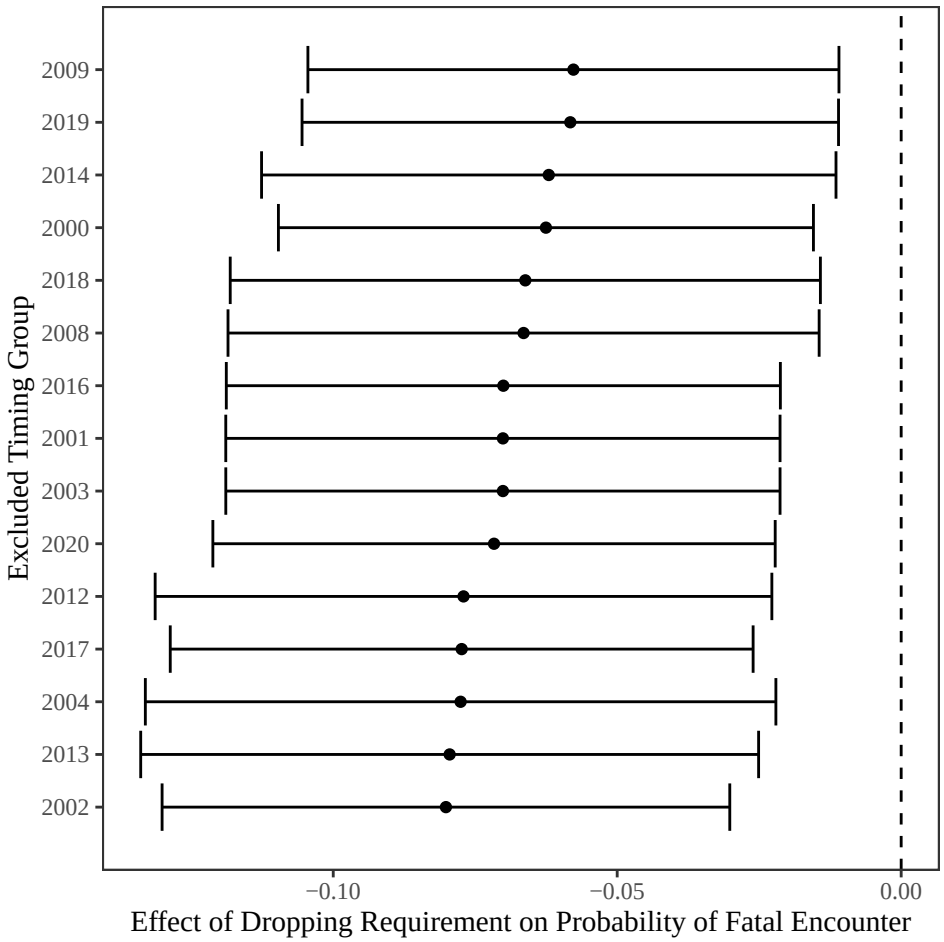
Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. *p<0.05

Table A.5: Residency Requirements and Community Engagement

	Complaint Board	Community Meetings	Satisfaction Surveys	Total Reforms
	(1)	(2)	(3)	(4)
Requirement Dropped (State)	−0.036	0.058	−0.230*	−0.234
	(0.050)	(0.089)	(0.100)	(0.202)
Requirement Dropped ×	0.210	0.012	0.439*	0.784*
Local Change	(0.148)	(0.107)	(0.169)	(0.352)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.2	0.8	0.4	1.5
Num. Agencies	564	569	577	563
Observations	2,599	2,383	2,832	2,259

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: Residency Rules and Fatal Encounters: Dropping Each Timing Cohort



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

A.3.2 Additional Robustness Checks

Table A.6: Residency Requirements and Outcomes: Excluding Metropolitan Departments

	Pct. White		Prob. Fatal Encounter	
	Full Sample	No Metro	Full Sample	No Metro
	(1)	(2)	(3)	(4)
Requirement Dropped	0.038*	0.040*	−0.084*	−0.085*
	(0.016)	(0.017)	(0.038)	(0.038)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.79	0.79	0.35	0.35
Num. Cities	584	578	584	578
Observations	4,030	3,980	12,264	12,138

No Metro sample drops metropolitan police departments. Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. *p<0.05

Table A.7: 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	0.038*	0.047*	−0.084*	−0.095*
	(0.016)	(0.016)	(0.038)	(0.048)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.79	0.79	0.35	0.35
Num. Cities	584	577	584	577
Observations	4,030	3,996	12,264	12,117

Drop Only sample omits departments that added residency rules (vs. dropping them). Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. *p<0.05

Table A.8: Residency Requirements and Outcomes: Controlling for Crime

	Pct. White		Prob. Fatal Encounter	
	(1)	(2)	(3)	(4)
Requirement Dropped	0.038*	0.030*	−0.084*	−0.081*
	(0.016)	(0.013)	(0.038)	(0.041)
City and Year FEs	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes
Adjust for Crime Rate		Yes		Yes
Mean Outcome	0.79	0.79	0.35	0.35
Num. Cities	584	583	584	583
Observations	4,030	3,906	12,264	11,382

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

Table A.9: Residency Requirement and Probability of Brutal Fatal Encounter

	Full Sample	State Change	Local Change
	(1)	(2)	(3)
Requirement Dropped	−0.111*	−0.085	−0.133*
	(0.045)	(0.064)	(0.060)
City and Year FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Mean Outcome	0.309	0.308	0.303
Num. Agencies	584	560	570
Observations	12,264	11,760	11,970

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.4: Residency Rules and Brutal Fatal Encounters: Event Study

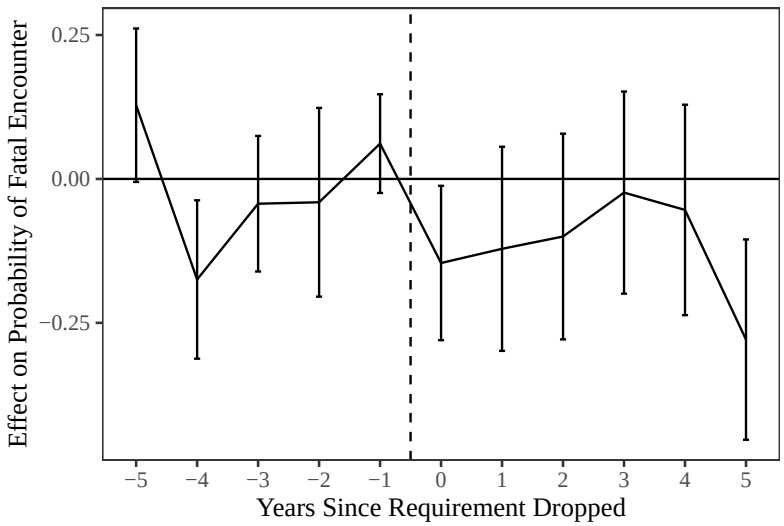


Table A.10: Residency Requirement and Probability of Fatal Encounters: City Trends

	All Fatalities		Brutal Fatalities	
	(1)	(2)	(3)	(4)
Requirement Dropped	−0.084*	−0.064*	−0.102*	−0.085*
	(0.038)	(0.030)	(0.045)	(0.034)
City and Year FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
City Linear Trends		Yes		Yes
Mean Outcome	0.352	0.352	0.309	0.309
Num. Agencies	584	584	584	584
Observations	12,264	12,264	12,264	12,264

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

A.3.3 Residency Rules and Crime and Crime Clearance Rates

Tables A.11 and A.12 show the effects of residency requirements on both population-adjusted violent crime rates and violent crime clearance rates. In both cases, we begin by showing pooled cross-sectional models in Column 1 so that we can compare our estimates to existing research that suggesting a relationship between residency rules, crime rates, and crime clearance rates Smith (1980). We uncover these correlations, but in both cases the inclusion of city and year fixed effects in Columns 2 and 3 substantially attenuate the results. Residency rules appear to do little to improve either violent crime or crime clearance rates, and event studies confirm these results (Figure A.5). We uncover nearly identical results when looking at overall crime (instead of violent crime)—these results are in the on-line replication files.

Table A.11: Residency Requirements and Violent Crime Rates (Per 1,000 Residents)

	Pooled	Within City	State	Local
	(1)	(2)	(3)	(4)
Requirement Dropped	−1.095*	−0.697	−1.818	−0.027
	(0.469)	(0.739)	(1.680)	(0.568)
State and Year FEs	Yes			
City and Year FEs		Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	7.047	7.047	6.968	6.924
Num. Agencies	583	583	559	569
Observations	18,544	18,544	17,805	18,094

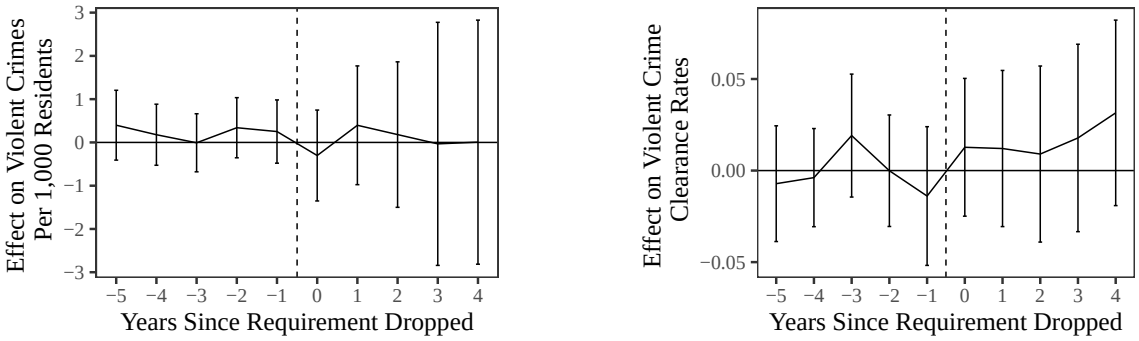
Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. *p<0.05

Table A.12: Residency Requirements and Crime Clearance Rates

	Pooled	Within City	State	Local
	(1)	(2)	(3)	(4)
Requirement Dropped	0.017	0.021	0.018	0.023
	(0.014)	(0.022)	(0.034)	(0.029)
State and Year FEs	Yes			
City and Year FEs		Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Mean Outcome	0.481	0.481	0.481	0.483
Num. Agencies	583	583	559	569
Observations	17,611	17,611	16,963	17,175

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.5: Event Studies: Crime-Related Outcomes



(a) Crime Rates

(b) Proportion of Crimes Cleared

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).[11] 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).[12] 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).[13]

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).[14] 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*

[11] 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."

[12] Ohio Association of Professional Fire Fighers. 2008. "Brief of Amicus Curiae Ohio Association of Professional Fire Fighters in Support of Appellant State of Ohio."

[13] Milwaukee Professional Fire Fighters Association Local 215. 2014. "Amended Response Brief of Intervenor-Plaintiff-Respondent Local 215 Professional Fire Fighters Association Local 215."

[14] Ohio Municipal League. 2008. "Brief of Amicus Curiae Ohio Municipal League in Support of Appellee the City of Lima."

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).[15] The **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).[16]

[15]City of Dayton. 2008. "Brief of Amicus Curiae The City of Dayton in Support of Appellee the City of Lima."

[16]Supreme Court of Wisconsin. 2016. "James Black, Glen Podelsnik, and Steven Van Erden versus the City of Milwaukee."