Residency Blues: The Minimal Impact of Police Residency Requirements *

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Abstract

Do residency requirements improve police-community relations? While residency rules were popular in the 1970s, many cities and states abolished these policies in the 1990s and early 2000s under pressure from police unions. 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 combine this information with panel data on the racial composition of city police forces, crime rates, and fatal police encounters. Using a difference-in-difference design, we find that residency requirements modestly improve police diversity but have no impact on crime or crime clearance rates. Notably, 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 improve police performance and don’t appear to offer a particularly fruitful avenue for reform.

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1 Introduction

Following the 2020 killing of George Floyd by a Minneapolis police officer, protests against police brutality swept the country. Community-led movements called for reforms ranging from defunding the police to ending chokeholds and investing in new social programs, and dozens of cities and law enforcement departments pledged to change their policies. Amid these debates, one proposal that emerged was the idea of residency requirements, or mandating that police reside in the communities where they work. In the 1970s, this was a fairly common practice, and more than half of America’s biggest cities required public safety officers to live within the boundaries of the cities they served as a condition of employment (Hirsch and Rufolo 1985). But in response to political pressures, many cities and states across the country have rolled back their residency rules over the past few decades. In Detroit, non-residency rates among police jumped from 20% to 75% following a change in state law (Neavling 2017). The Minnesota State Legislatures 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).

Can residency requirements improve police-civilian interactions? Supporters of residency 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 (Eisinger 1983). 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. The existing empirical research on the effects of these laws is mixed but discouraging on balance. At the same time, most 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. Recent headlines have read: “Reexamining residency rules for Detroit police officers” (2020), “With Only 8% of Minneapolis Police Officers Living in City, Residency Requirement Push Gaining Traction” (2020), and “Bill targeting Memphis police residency requirement advances in state House” (2022). 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 debate by conducting an original survey of nearly 600 cities to learn about their history of residency requirements over the past three decades. We employ a within-city design to provide estimates of the effects of reform on three types of outcomes: the racial composition of the police force, crime and crime clearance rates, and fatal encounters with civilians. We find that residency rules are associated with more diverse police forces but have no impact on crime or crime clearance rates. 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-community relationships.

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 current policy debates that are playing out in cities and states across the U.S. Finally, we add to a growing body of empirical literature both in the U.S. and in the comparative context showing that many popular community-based
reforms fail to meaningfully improve police performance (e.g. Blair et al. 2021; Eckhouse 2021). While our study provides the most systematic and credible evidence to date on the effects of residency requirements, we also caution that one of the issues with the current debates about these requirements is that scholars largely lack the ability to conduct robust and generalizable policy analysis given the available data.

2 Historical Background and Theoretical Perspectives

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, however, 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 criminal justice 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).

2.1 Arguments In Favor of Residency Requirements

Proponents of residency requirements have pointed to several theoretical mechanisms that might link these laws to better outcomes in terms of police-civilian interactions. First, residency rules are posited to 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).

There are a variety of reasons why racially diverse police forces might lead to better relationships between police and communities. Scholars have demonstrated that more descriptively representative police departments are associated with an increased sense of community legitimacy, 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 2019) 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 2021). 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).

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 inter-
pretation 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 jump-start trust-building between police and the residents they serve (Ogletree et al. 1995).

2.2 Arguments Against Residency Rules

However, scholars and policy practitioners who oppose residency requirements have pointed out that these rules may not actually facilitate the goals outlined above and may even lead 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 curiae brief filed in support of an Ohio law banning municipal residency requirements, the Association of Professional Fire Fighters claimed that the bill “actually 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. Before residency requirements were banned in Detroit, clusters of police officers lived in “copper canyons”—middle-class neighborhoods predominantly home to cops and

¹For a review, see Pettigrew and Tropp (2006)
firefighters on the farthest east and west sides of the city (Livengood and Annalise 2020). 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).

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 restrictions. “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.”

2.3 Existing Empirical Research

Despite the lively theoretical debates that regularly 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

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Footnote:

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 level 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.

Finally, 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 sparse, conflicting, and largely cross-sectional, we are overdue for a systematic examination of the effects of residency requirements.

3 New Data on Residency Rules

For our analysis, we hand collected data on the residency requirements of nearly 600 of the largest municipal police departments over the past 30 years. We began by conducting an original survey of the universe of municipal police agencies with at least 100 residents that appeared in at least one Law Enforcement and Administrative Statistics (LEMAS)
survey between 1987 and 1997. The LEMAS survey has been administered by the Bureau of Justice Statistics roughly every three years since 1987. The goal of the survey is to obtain information about the responsibilities, operations, and agency characteristics of local law enforcement agencies across the U.S.

While 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 2020.4

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 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,

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4Of the 595 total agencies identified by the LEMAS survey, 11 were either disbanded or otherwise restructured so as to no longer meet the sampling criterion. For additional details about the data collection process, see the Appendix.
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.

In Table A.1 in the Appendix, we show descriptive statistics at the city level for various demographic variables for cities with and without residency rules. Cities with municipal residency requirements are larger, more urban, and less white than other cities. 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 three main outcomes. 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.\textsuperscript{5} Next, we study the relationship between residency rules and \textit{Crime Rates} and \textit{Crime Clearance Rates}, which are taken from the Federal Bureau of Investigation’s Uniform Crime Reporting (UCR) data (Federal Bureau of Investigation 2021). Crime clearance rates report the proportion of crimes that result in an arrest and are widely used to proxy for police effectiveness (e.g. McCrary 2007; Doerner and Doerner 2012; Ornaghi 2019).\textsuperscript{6} This measure ranges from 0 to 1, with higher values indicating higher clearance rates. Finally, we examine the number of \textit{Fatal Police Encounters}, which captures the annual number of civilian fatalities that occur in 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).

It’s important to note that each of these outcome measures suffer from various limitations. The LEMAS data on police force diversity are collected only periodically and don’t extend past 2016. The UCR data on crimes and crime clearance rates are self-reported, and police agencies have the ability to manipulate these statistics. The Fatal Encounters data rely on crowd-sourced public records requests and media reports and cover only the period after 2000. Nevertheless, these indicators are some of the few measures of police quality for which longitudinal data exist for a large sample of municipalities over many years. They are also theoretically meaningful and allow us to build directly on existing literature that study these outcomes cross-sectionally.

4 Empirical Strategy

The choice to adopt residency rules is not random, and the small number of cities that employ such laws differ in ways both observable and unobservable from other cities. To account for

\textsuperscript{5}Note that the LEMAS survey is conducted only every 3 years and consists of a sample of local law enforcement agencies, which means we lose a considerable number of observations for this analysis. On average, however, the agencies in our sample appear in the LEMAS data 7 times over the course of the panel.

\textsuperscript{6}For a review of the strengths and limitations of using crime clearance rates as a measure of police quality, see Baradaran Baughman (2020)
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, 33 cities dropped their requirements, and 7 cities added new requirements. Figure 2 shows this variation over time.

**Figure 2:** Number of Cities with Residency Requirements by Year

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 due to 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. 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

$$y_{it} = \beta_1 Dropped \text{ Requirement}_{it} + \beta_2 X_{it} + \gamma_i + \delta_t + \epsilon_{it}. \quad (4.1)$$

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, and the $\delta_t$ are year fixed effects. Standard errors are always clustered at the city level.

Recent econometrics literature shows that standard difference-in-differences regressions can be biased 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:

\[ y_{igt} = \beta_1 \text{Dropped Requirement}_{igt} + \beta_2 X_{it} + \gamma_{ig} + \delta_{tg} + \varepsilon_{igt} \]  

(4.2)

Now, \( g \) identifies the timing group, and \( \delta_{tg} \) represents year-by-group fixed effects. We can interpret \( \beta \) 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 \texttt{ebal} package in R (Hainmueller 2012), which we discuss in more detail in the next section.

5 Residency Rules Modestly 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 \texttt{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 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 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 displays the results generated via equations 4.1 and 4.2 described in the previous section. 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. 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
Table 1: Residency Requirement and Pct. White on Police Force

<table>
<thead>
<tr>
<th></th>
<th>Original Data</th>
<th>Stacked Approach</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Requirement Dropped</td>
<td>0.044*</td>
<td>0.038*</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.016)</td>
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<tr>
<td>City and Year FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Controls</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td>City and Cohort x Year FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Balancing Weights</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Mean Outcome</td>
<td>0.786</td>
<td>0.786</td>
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<tr>
<td>Num. Agencies</td>
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<td>584</td>
</tr>
<tr>
<td>Observations</td>
<td>4,030</td>
<td>4,030</td>
</tr>
</tbody>
</table>

Controls include city population (logged), median income (logged), % white residents. Robust standard errors clustered by city. *p<0.5

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 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 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 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.
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 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**

<table>
<thead>
<tr>
<th>Requirement Dropped</th>
<th>State Change (1)</th>
<th>Local Change (2)</th>
<th>Pop &gt; 100,000 (3)</th>
<th>City &lt; 70% White (4)</th>
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<tr>
<td></td>
<td>0.036</td>
<td>0.039</td>
<td>0.047*</td>
<td>0.072*</td>
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<td></td>
<td>(0.021)</td>
<td>(0.022)</td>
<td>(0.014)</td>
<td>(0.024)</td>
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<td>City and Year FEs</td>
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<td>Yes</td>
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<td>Yes</td>
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<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Mean Outcome</td>
<td>0.8</td>
<td>0.8</td>
<td>0.7</td>
<td>0.7</td>
</tr>
<tr>
<td>Num. Cities</td>
<td>560</td>
<td>570</td>
<td>260</td>
<td>326</td>
</tr>
<tr>
<td>Observations</td>
<td>3,874</td>
<td>3,931</td>
<td>1,777</td>
<td>1,744</td>
</tr>
</tbody>
</table>

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. *p<0.5

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 de-
departments serving cities that are less than 70 percent white (the sample average). Strikingly, the effect is almost twice as large for agencies in racially diverse cities 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.  

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 influx of white hirers following the change in requirements. Interestingly, the data reveal that agencies don’t always grow after the elimination of residency rules. Figure A.2 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.

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 split 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 one

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7The 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

<table>
<thead>
<tr>
<th>Requirement Dropped</th>
<th>Shrinking Agencies (1)</th>
<th>Growing Agencies (2)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>−0.008 (0.036)</td>
<td>0.051* (0.018)</td>
</tr>
<tr>
<td>City and Year FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Mean Outcome</td>
<td>0.8</td>
<td>0.8</td>
</tr>
<tr>
<td>Num. Cities</td>
<td>165</td>
<td>419</td>
</tr>
<tr>
<td>Observations</td>
<td>1,000</td>
<td>3,030</td>
</tr>
</tbody>
</table>

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. *p<0.5

The reason why residency requirements might promote racial diversity is because they limit the number of white officers who are able to work for an agency while living outside city limits.

### 6 Crime Rates, Fatal Encounters, and Residency Requirements

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 adjust for a time-varying measure of the percentage of white officers in each of the following analyses. 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. We focus on two proxies for the quality of the relationship between city residents and their police forces. First, we examine how residency requirements affect crime and crime clearance rates. Next, we next turn to the Fatal Encounters data and study...
how eliminating residency requirements impacts the number of civilian deaths that occur during police interactions.

We find no effect of residency rules on either of the crime-based outcomes. Neither violent nor overall crime rates change before or after cities drop their requirements, and neither does the probability that a crime is cleared by arrest. To conserve space, we show these results in Tables A.2 and A.3 in the Appendix. We note here that one of the advantages of our within-city design is that we can avoid uncovering a spurious relationship between residency rules and crime rates that could instead be due to the fact that cities with different levels of crime might be more likely to adopt these policies in the first place. While pooled cross-sectional models uncover results similar to Smith (1980) and suggest that cities with residency laws have lower crime rates and higher crime clearance rates, these correlations attenuate substantially with the inclusion of city and year fixed effects.

However, we discover a very different picture when we examine the relationship between residency requirements and fatal encounters between police and civilians. Such encounters 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.\(^8\) In Table 4, we observe a dramatic decrease in the probability that a civilian dies during interactions 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. 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) \texttt{fect} package in R) displays the probability of a fatal encounter in the years before and after treatment. The

\(^8\)Note that the results are also robust but slightly noisier when using the total number of fatal encounters as the outcome.
Table 4: Residency Requirement and Probability of Fatal Encounter

<table>
<thead>
<tr>
<th></th>
<th>Original Data</th>
<th>Stacked Approach</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Requirement Dropped</td>
<td>−0.090*</td>
<td>−0.085*</td>
</tr>
<tr>
<td></td>
<td>(0.039)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>City and Year FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>City and Cohort x Year FEs</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Balancing Weights</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>Mean Outcome</td>
<td>0.352</td>
<td>0.352</td>
</tr>
<tr>
<td>Num. Agencies</td>
<td>584</td>
<td>584</td>
</tr>
<tr>
<td>Observations</td>
<td>12,264</td>
<td>12,264</td>
</tr>
</tbody>
</table>

Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. *p<0.5

estimates 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. We urge caution in interpreting these results given how noisy the data are and how few cities actually dropped 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.
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 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.

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 officers 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 maybe the choice to drop the requirement is accompanied by other reforms. Unfortunately, we largely lack panel data that would allow us to definitively test different mechanisms that might explain this result.

However, the LEMAS survey does provide a few indicators of community involvement in policing that provide some very suggestive evidence that when cities drop their residency rule, they go on to adopt other practices that might indicate a shift in police culture. The three outcomes available 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 are indicators, 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 focus specifically on the cities that experienced a local rule change for this analysis.

Table 6 shows the results for each of the three indicators. The LEMAS survey asked about these outcomes only periodically over the course of the panel, and the dramatic reduction in
observations means that the estimates are quite imprecise. But while the results are noisy, they are also consistent. After a city internally drops its residency requirement, it is more likely to institutionalize a civilian complaint board, more likely to hold meetings with community groups, and more likely to administer resident satisfaction surveys. While extremely tentative, this analysis is consistent with the idea that perhaps residency requirements are simply blunt policies that are substituting for more meaningful reform.

**Table 6: Residency Requirements and Community Engagement**

<table>
<thead>
<tr>
<th></th>
<th>Complaint Board</th>
<th>Community Meetings</th>
<th>Satisfaction Surveys</th>
</tr>
</thead>
<tbody>
<tr>
<td>Requirement Dropped</td>
<td>0.175 (0.139)</td>
<td>0.071 (0.062)</td>
<td>0.210 (0.141)</td>
</tr>
<tr>
<td>City and Year FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Mean Outcome</td>
<td>0.2</td>
<td>0.8</td>
<td>0.5</td>
</tr>
<tr>
<td>Num. Agencies</td>
<td>551</td>
<td>556</td>
<td>564</td>
</tr>
<tr>
<td>Observations</td>
<td>2,535</td>
<td>2,324</td>
<td>2,762</td>
</tr>
</tbody>
</table>

Controls include city population (logged), median income (logged), and % white residents. Robust standard errors clustered by city. *p<0.5

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 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 these policies to reduce incidents of police violence, there is simply no evidence that residency requirements improve the risk of fatal police-civilian encounters.
7 Discussion

Our survey of the largest municipal police departments provides a much needed update to the literature on police residency requirements. 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 help to promote racial diversity on the force but do little to improve crime-related outcomes and are actually associated with higher rates of fatal civilian encounters.

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. 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. But we emphasize that both supporters and opponents of these laws often tend to make sweeping claims that simply aren’t supported by evidence. In fact, our research reveals just how difficult effective policy evaluation is due to data quality issues and a lack of credible research designs.

We plan to expand on this initial 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 given the fact that they are available for a large number of agencies over time, we are looking into the possibility of trying to collect more granular and meaningful data on police-community outcomes at the city level. We have already filed public records requests with 5 large cities in our sample that changed their requirements (Denver, Minneapolis, Milwaukee, Pittsburgh, and Cleveland) to learn more about how officer recruitment, hiring, and number of civilian complaints changed before and after the policy was dropped.
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. We welcome feedback on all aspects of our data collection, research design, and interpretation of our preliminary results as we continue with this research.
References


Online Appendix: Residency Blues: The Minimal Impact of Police Residency Requirements

Intended for online publication only.

A.1 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.1.1 Descriptive Results

Table A.1 shows descriptive information about key control variables and outcomes for cities with and without residency requirements in our sample. Recall that we are defining as 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>
<thead>
<tr>
<th>Variable</th>
<th>Requirement</th>
<th>N</th>
<th>Mean</th>
<th>S.D.</th>
</tr>
</thead>
<tbody>
<tr>
<td>City Population</td>
<td>No City Requirement</td>
<td>511</td>
<td>153128</td>
<td>412658</td>
</tr>
<tr>
<td>City Population</td>
<td>Residency Requirement</td>
<td>76</td>
<td>204619</td>
<td>389058</td>
</tr>
<tr>
<td>Num. Officers</td>
<td>No City Requirement</td>
<td>511</td>
<td>353</td>
<td>1595</td>
</tr>
<tr>
<td>Num. Officers</td>
<td>Residency Requirement</td>
<td>76</td>
<td>715</td>
<td>1741</td>
</tr>
<tr>
<td>City Income</td>
<td>No City Requirement</td>
<td>468</td>
<td>40863</td>
<td>13410</td>
</tr>
<tr>
<td>City Income</td>
<td>Residency Requirement</td>
<td>65</td>
<td>34621</td>
<td>9755</td>
</tr>
<tr>
<td>City % White</td>
<td>No City Requirement</td>
<td>468</td>
<td>0.70</td>
<td>0.17</td>
</tr>
<tr>
<td>City % White</td>
<td>Residency Requirement</td>
<td>65</td>
<td>0.67</td>
<td>0.22</td>
</tr>
<tr>
<td>City % Black</td>
<td>No City Requirement</td>
<td>468</td>
<td>0.16</td>
<td>0.17</td>
</tr>
<tr>
<td>City % Black</td>
<td>Residency Requirement</td>
<td>65</td>
<td>0.23</td>
<td>0.21</td>
</tr>
<tr>
<td>City % Latino</td>
<td>No City Requirement</td>
<td>468</td>
<td>0.16</td>
<td>0.17</td>
</tr>
<tr>
<td>City % Latino</td>
<td>Residency Requirement</td>
<td>65</td>
<td>0.14</td>
<td>0.18</td>
</tr>
<tr>
<td>% White Officers</td>
<td>No City Requirement</td>
<td>510</td>
<td>0.81</td>
<td>0.17</td>
</tr>
<tr>
<td>% White Officers</td>
<td>Residency Requirement</td>
<td>76</td>
<td>0.79</td>
<td>0.18</td>
</tr>
<tr>
<td>Crime Rate (Per 1,000 Residents)</td>
<td>No City Requirement</td>
<td>510</td>
<td>55</td>
<td>25</td>
</tr>
<tr>
<td>Crime Rate (Per 1,000 Residents)</td>
<td>Residency Requirement</td>
<td>76</td>
<td>59</td>
<td>32</td>
</tr>
<tr>
<td>Crime Clearance Rate %</td>
<td>No City Requirement</td>
<td>510</td>
<td>0.21</td>
<td>0.06</td>
</tr>
<tr>
<td>Crime Clearance Rate %</td>
<td>Residency Requirement</td>
<td>76</td>
<td>0.20</td>
<td>0.06</td>
</tr>
<tr>
<td>Probability of Fatal Encounter</td>
<td>No City Requirement</td>
<td>511</td>
<td>0.35</td>
<td>0.28</td>
</tr>
<tr>
<td>Probability of Fatal Encounter</td>
<td>Residency Requirement</td>
<td>76</td>
<td>0.35</td>
<td>0.34</td>
</tr>
<tr>
<td>Meetings With Community</td>
<td>No City Requirement</td>
<td>498</td>
<td>0.82</td>
<td>0.23</td>
</tr>
<tr>
<td>Meetings With Community</td>
<td>Residency Requirement</td>
<td>72</td>
<td>0.81</td>
<td>0.25</td>
</tr>
<tr>
<td>Civilian Complaint Review Board</td>
<td>No City Requirement</td>
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<td>0.17</td>
<td>0.31</td>
</tr>
<tr>
<td>Civilian Complaint Review Board</td>
<td>Residency Requirement</td>
<td>70</td>
<td>0.22</td>
<td>0.35</td>
</tr>
</tbody>
</table>
A.2 Additional Statistical Results

A.2.1 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**

![Event Study: Number of Officers](image)
A.2.2 Residency Rules and Crime and Crime Clearance Rates

Tables A.2 and A.3 show the effects of residency requirements on both population-adjusted crime rates and 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 suggests a positive relationship between residency rules, lower crime rates, and higher 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 crime or crime clearance rates, and event studies confirm these results (Figure A.3).

Table A.2: Residency Requirements and Crime Rates (Per 1,000 Residents)

<table>
<thead>
<tr>
<th>Requirement Dropped</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Requirement Dropped</td>
<td>$-13.691^*$</td>
<td>$-7.248$</td>
<td>$-4.069$</td>
</tr>
<tr>
<td></td>
<td>(4.206)</td>
<td>(4.199)</td>
<td>(3.505)</td>
</tr>
</tbody>
</table>

State and Year FEs Yes  
City and Year FEs Yes Yes  
Controls Yes  
Mean Outcome 56.5 56.5 56.5  
Num. Agencies 583 583 583  
Observations 18,595 18,595 18,595

Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city. *p<0.5
Table A.3: Residency Requirements and Crime Clearance Rates

<table>
<thead>
<tr>
<th>Requirement Dropped</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.022*</td>
<td>-0.007</td>
<td>-0.008</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.011)</td>
<td>(0.011)</td>
</tr>
</tbody>
</table>

State and Year FEs: Yes  
City and Year FEs: Yes, Yes  
Controls: Yes  
Mean Outcome: 0.2, 0.2, 0.2  
Num. Agencies: 583, 583, 583  
Observations: 17,789, 17,789, 17,789  

Controls include city population (logged), median income (logged), % white residents, and % white on police force. Robust standard errors clustered by city.  
*p<0.5

Figure A.3: Event Studies: Crime-Related Outcomes

(a) Proportion of Crimes Cleared  
(b) Crime Rates
Figure A.4: Residency Rules and Fatal Encounters: Dropping Each Timing Cohort

Excluded Timing Group

Effect of Dropping Requirement on Probability of Fatal Encounter