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Inequality in the Provision of Police Services: Evidence from Residential Burglary Investigations

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Abstract

When crime victims call the police for help, what type of response do they receive? While scholars have extensively documented racial inequalities in the police's punitive functions, this paper considers the police as service providers. It leverages uniquely granular data on over 2,500 residential burglary investigations in Tucson, Arizona, to consider the predictors of investigative thoroughness. Contrary to conventional wisdom about police behavior, the demographics of victims or officers do not consistently predict investigative thoroughness. Instead, the most important predictor of investigative thoroughness is whether the burglary involved a forced entry into the residence, since forced-entry cases feature more evidence and thus provide greater likelihood of case clearance. However, the probability of forced entry differs significantly by neighborhood, which means that the seemingly neutral decision to maximize clearance rates has unequal consequences.

1. Introduction

When victims of a crime call the police for help, what type of response do they receive? Does the quality of police service provision vary depending on the race, gender, income, age, or neighborhood of the victim, and do officers of certain races provide better or worse services to victims of certain races? Scholars have extensively documented racial inequalities in the police's punitive functions—

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pursuing, searching, and arresting criminal suspects (Knowles, Persico, and Todd 2001; Beckett, Nyrop, and Pfingst 2006; Golub, Johnson, and Dunlap 2007; Gelman, Fagan, and Kiss 2007; Persico and Todd 2008; Antonovics and Knight 2009; Mitchell and Caudy 2017)—and traffic ticketing (Baumgartner et al. 2017; West 2018; Goncalves and Mello 2021). Much less scholarly attention has been devoted to inequalities in police services rendered to victims of crimes or disturbances, despite the fact that there are many more crime victims than there are arrests every year (Truman and Morgan 2016; Department of Justice 2016). Just as scholars have aimed to detect the presence of racial bias in police stops and arrests (Knowles, Persico, and Todd 2001; Grogger and Ridgeway 2006; Persico and Todd 2006; Mitchell and Caudy 2017; Goncalves and Mello 2021), this paper aims to detect any such parallel bias in police service provision.

This paper is the first to use incident-level data to consider whether there are inequalities—based on race, gender, income, age, or neighborhood—in the quality of policing services rendered to victims of crimes. It finds that demographic characteristics do not dictate what sorts of burglary investigations victims receive. The main determinant of investigative thoroughness is instead whether the burglary involved a forced entry into the residence. Forced-entry burglaries, however, are disproportionately concentrated in better-off neighborhoods of the city, and so even though residents of poorer neighborhoods are not directly discriminated against conditional on their neighborhood, unconditional social inequalities in service provision remain.

Scholarship analyzing inequality in police service provision is rare in part because incident-level data are usually difficult to access. In this paper, I leverage a novel and uniquely granular data set from the Tucson Police Department (TPD). By partnering with the TPD, I obtained data on every residential burglary in Tucson in calendar year 2016 (over 2,500 burglaries), including information about the nature of the incident, the races and genders of each civilian and police officer present at each burglary scene, and the activities officers undertook at the scene. I also use the address of each incident to link incidents to American Community Survey data on neighborhood demographics. I then compare three measures of service quality for residential burglary investigations—the amount of time spent at the scene, whether officers dusted for fingerprints, and whether a burglary detective was assigned to the case—with the incident- and neighborhood-level characteristics.

Contrary to conventional wisdom, burglary victims of different races and genders do not receive different levels of thoroughness in their investigations, and officers of different races and genders do not provide different levels of investigative thoroughness; there is also no significant interaction between the races of victims and the races of officers. Instead, I find that, conditional on a host of contextual variables, the main determinant of investigative thoroughness is whether the burglary involved a forced entry. Officers spend 14 more minutes (19 percent more time) at the scene of forced-entry burglaries, they are 67 percent more likely to dust for fingerprints, and they are over 45 percent more likely to assign a detective to the case, conditional on relevant situational and demographic variables.

Making a causal claim about the relationship between forced entry and investigative thoroughness is challenging because forced entries are not random. To overcome the potential for selection bias, I estimate regressions that employ three sets of fixed effects: date level (to account for the seasonality of crime, differences in police resources on weekends and holidays, and any other unobserved time-varying differences), hour level (to account for differences between daytime and nighttime incidents, differences between officers' work shifts, and any other unobserved patterns related to time of day), and census-block-group level (to account for cross-sectional confounding at the level of the census block group, such as neighborhood socioeconomic characteristics). The regressions also include a full battery of controls for incident-level confounders, including level of urgency at the time of the emergency (911) call (indexed by a dispatcher-assigned priority level), the number and demographics of the victims present, and the number and demographics of the officers who responded.

These results run contrary to much research about racial discrimination by police officers in their punitive roles (Eberhardt et al. 2004; Carr, Napolitano, and Keating 2007; Epp, Maynard-Moody, and Haider-Markel 2014; Braun, Rosenthal, and Therrian 2018) and to some research arguing that police officers do not serve racial minorities as well as they serve whites who are victims of crimes (Natapoff 2006; Roberts and Lyons 2011; Fagan and Geller 2018). However, they are consistent with the classic Besley and Coate (1991) model, which predicts that inequalities in public services will be greatest where it is possible for wealthy citizens to purchase higher-quality private goods to substitute for lower-quality public goods (Trounstine 2015).

Although economic analysis of crime has traditionally focused on the incentives of would-be offenders (Becker 1968; Freeman 1999; Pinotti 2017), my results are also consistent with research showing that police officers, too, respond to incentives (in their case, the incentives used to evaluate their performance) (Mas 2006; Carpenter 2010). Lipsky (1980) famously argues that street-level bureaucrats, including police officers, strive to be seen as high performing. The most basic measure of police performance is the clearance rate: the number of cases that are closed, or cleared, divided by the total number of crimes (Rayman 2013). Thus, police should focus on cases with the highest probability of clearance. With respect to burglaries, criminology research distinguishes between forced-entry burglaries (such as those involving a picked lock or broken window) and unforced-entry burglaries (such as those involving entry through a door or window left open). Forced-entry burglaries provide more analyzable evidence and are thus perceived as more solvable, compared with unforced-entry burglaries (Coupe 2016; Shannon and Coonan 2016; Killmier, Mueller-Johnson, and Coupe 2019), which means that the police's incentive structure encourages them to focus scarce investigatory resources on forced-entry burglaries.

This paper makes several contributions to the research literature. First, the finding that police do not behave in a racially discriminatory manner in their service provision role considerably complicates our understanding of race and policing in light of the large literatures on racial bias in traffic stops, pedestrian stops, and arrests (Golub, Johnson, and Dunlap 2007; Goel, Rao, and Shroff 2016; Ritter 2017; Braun, Rosenthal, and Therrian 2018). Second, the fact that burglary investigation services provided to low-income, heavily minority neighborhoods are not conditionally of worse quality provides empirical support for the classic Besley and Coate (1991) model, which predicts that inequalities in public service provision will emerge only for substitutable public goods. Third, the unconditional inequality in investigative thoroughness in poorer and wealthier neighborhoods illustrates how inequalities can emerge from a neutral bureaucratic incentive structure. Finally, that police officers exercise the most effort on cases they are most likely to solve provides evidence that police officers respond to incentives when choosing where to focus their efforts.

The remainder of this paper proceeds in three parts. First, I describe the policing context in Tucson and burglary investigations in general. Second, I present results from the analysis of over 2,500 residential burglaries. Finally, I draw conclusions and suggest directions for future research.

2. Data: Policing in Tucson

The primary data for this project are detailed records for every residential burglary in Tucson, Arizona, in 2016. Burglary is defined by Arizona statute as "entering or remaining unlawfully in or on a residential structure with the intent to commit any theft or any felony therein" (Ariz. Rev. Stat., sec. 13-1507). The data include the street address of the residence; the division (one of four geographic areas) in which the burglary took place; the priority level that the police dispatcher placed on the emergency call;[1] the time the call was placed; the time that officers arrived; the time that officers left; the number of people and the ages, races, and genders of everyone at the scene; and detailed information about the nature of the incident and the activities undertaken by the officers at the scene. Officers also record how many officers were present at the scene and their badge identification numbers, how many victims were present, and victims' races and genders. The TPD provided employee records, which allowed me to merge information about the ages, races, and genders of the officers at each scene. The TPD system later adds information about whether a detective was eventually assigned to the case, which is a decision made by the burglary sergeant in the division in which the burglary took place. (One burglary sergeant is assigned to each division and works in one division at a time.)

For purposes of comparability of incidents, I exclude any residential burglary initially reported as a larceny (a theft that does not involve an unlawful entrance) and later coded as a burglary on the basis of further investigation, since the initial phase of a larceny investigation is much less thorough than that of a burglary

[1] The priority levels range from 1, which means an emergency call for which dispatched officers use lights and sirens in arriving, to 4, which is the lowest priority.

investigation. I also exclude incidents reported using the TPD online interface rather than a phone call or an alarm system, since they reflect a much lower level of urgency on the part of the individual in need of police service. These exclusions leave 2,763 burglaries.[2] The locations of the burglaries are shown in Figure 1.[3]

My data, with 2,763 burglaries needing to be at least partially addressed by Tucson's 870 sworn officers, reflect a very high level of capacity constraint. If only 10 person-hours are spent investigating each residential burglary I examine, this would require each officer to spend about 32 hours on a burglary when they also need to be investigating dozens of murders, hundreds of rapes, and other assaults and responding to over 190,000 911 calls for service. Police department policy makers and officers, then, need to be extremely judicious about how they allocate time to officers' activities, but particularly activities that are unlikely to result in a cleared case.

The criteria I use to define a thorough burglary investigation are the amount of time police spent at the scene, whether the police dusted for fingerprints, and whether a detective was eventually assigned to the case. Conversations with TPD researchers, patrol officers, burglary detectives, and burglary sergeants revealed that the standard practice at burglary scenes is to conduct a thorough interview with the victim or victims and to dust for fingerprints where they are most likely to have been left by the perpetrator (which depends on the victim's description of missing items and the perpetrator's likely point of entry). Other potential measures of thoroughness, such as whether DNA was collected, whether full lists of stolen items were recorded, or whether the police recorded stolen items' serial numbers, are not widely applicable to all cases: DNA is collected only if the perpetrator appears to have left a sample of bodily fluid, serial numbers are recorded only for items that have them (such as high-value electronics and firearms), and full lists of stolen items are sometimes not possible to record at a scene because victims are often too distressed to know exactly what has been taken. Sergeants assign detectives primarily on the basis of their perception of the solvability of the case. In practice, sergeants view cases as solvable when there is potential physical evidence that prosecutors can later use to definitively link a suspect to a burglary.

In classifying burglaries as forced or unforced entries, I follow both the TPD and the Federal Bureau of Investigation's Uniform Crime Reporting program, which distinguish between the two types (Department of Justice 2014). The TPD classifies as forced entry any burglary in which the perpetrator entered the residence through a locked door or window, whether by breaking the window or door, using any tool to force or pry open the window or door, or using a tool to disable the lock on a window or door. In many cases, a forced entry is obvious,

[2] The Federal Bureau of Investigation's Uniform Crime Reporting data for Tucson in 2016 record 4,138 burglaries. I have records for 3,994 of these: 3,224 are residential (rather than commercial), 127 were reported using the Tucson Police Department's online interface, and 334 were initially reported as larcenies. Online Appendix Table OB2 includes the reports made online and incidents initially classified as larcenies, and the results are very similar to those for the full sample.

[3] In Figures 1 and 2, the shading indicates persons per square mile in the census block group. Areas with crosshatching are the independent municipality of South Tucson, which has its own police department (smaller area on the left), and Davis-Monthan Air Force Base (larger area on the right).

Figure 1. Burglaries by population density in Tucson, 2016

Table 1
Summary Statistics

	N	Mean	SD	Min	Max
Forced entry	2,763	.645	.479	0	1
Poverty share (census block group)	2,763	.298	.173	0	.749
Hispanic share (census block group)	2,763	.423	.259	.023	.960
White share (census block group)	2,763	.458	.247	.012	.964
Black share (census block group)	2,763	.042	.050	0	.339
Victim age (mean)	2,639	43.287	17.732	0	99
All white officers	2,763	.443	.497	0	1
Share white officers	2,763	.574	.435	0	1
Share Black officers	2,763	.039	.171	0	1
Share Hispanic officers	2,763	.316	.406	0	1
Share male officers	2,763	.779	.372	0	1
All white victims	2,763	.482	.500	0	1
Share white victims	2,763	.511	.486	0	1
Share Black victims	2,763	.051	.214	0	1
Share Hispanic victims	2,763	.271	.434	0	1
Share male victims	2,763	.496	.462	0	1
Victims	2,763	1.355	.802	1	15
Officers	2,763	2.164	2.136	1	24
Apartment	2,763	.311	.463	0	1
Priority level (1–4)	2,763	3.647	.805	1	5
East	2,763	.211	.408	0	1
West	2,763	.275	.447	0	1
Midtown	2,763	.255	.436	0	1
South	2,763	.202	.402	0	1
Incident on weekend or holiday	2,763	.248	.432	0	1
Incident hour	2,757	14.631	6.313	1	24
Time spent (minutes)	2,620	87.098	79.058	1	910
Prints collected	2,763	.399	.490	0	1
Detective assigned	2,763	.345	.475	0	1
Case cleared	2,763	.102	.303	0	1

as it caused damage to the window or door and/or the surrounding area of the residence. In some cases if there are no signs of forced entry, patrol officers need to decide whether the victim is being truthful that he or she locked all doors and windows, and the entry is classified as forced if officers believe the victim's account and unforced if they do not.

In addition to collecting the police-provided information about each burglary, I link the addresses of the burglaries to American Community Survey data (2011–15 estimates) on census-block-group demographics. Census block groups are the smallest level of aggregation at which the Census Bureau reports demographic data; in dense urban areas, they typically correspond to individual square city blocks. The average population of a census block group in Tucson is 1,540. Summary statistics for the 2016 residential burglaries and the census block groups in which they took place are presented in Table 1.

Tucson is an especially interesting setting for this study because the very high level of racial diversity (47 percent white non-Hispanic, 42 percent Hispanic, 5 percent Black, 3 percent Native American, and 3 percent Asian, as of the 2010 census) and socioeconomic diversity (25 percent of persons in Tucson lived in poverty as of 2015, and the median household income was below the national median at $37,149). These demographics indicate that there is a great deal of variation in types of neighborhoods and individuals the police serve.

As Figures 1 and 2 show, burglaries are more prevalent in denser and poorer areas of the city. The retirement communities of the west part of Tucson and the wealthy neighborhoods of the most northern part of Tucson experienced few residential burglaries in 2016. Burglaries were most concentrated in the poor neighborhoods immediately east of Interstate 10, which divides the city's eastern and western parts.

In Table 1, where the number of observations is less than 2,763, the relevant data are missing for some burglaries. Often data are missing because officers did not record the information in their narrative reports. Missing data on time spent at the scene result from patrol officers editing their narrative reports after submitting them, which causes the time to default to 0, and failing to manually input the time stamp.

3. Results and Discussion

3.1. Race and Investigative Thoroughness

Table 2 presents the results of ordinary least squares regressions predicting the three indicators of investigative thoroughness. All regressions include the total numbers of victims and officers as controls. Regressions with fixed effects for the TPD's five (geographic) police divisions control for differences in capacity between different divisions, and fixed effects for month control for the basic seasonality of crime patterns. Regressions with this minimum set of controls, especially in regressions without fixed effects, illustrate that the comparatively large standard errors on the coefficients for All White Officers, All White Victims, and the interaction term are not an artifact of collinearity in the control variables (Chatterjee and Simonoff 2013). Incident- and victim-level variables, such as 911 call priority level, the indicator that the residence is an apartment, the genders of victims and officers, and the age of the victims, are omitted here because they are plausibly posttreatment effects with respect to the race of the victims.

Table 2 shows that the relationship between investigative thoroughness and the races of victims and investigating officers is substantively small and not statistically significant at conventional levels, even when only a few control variables are included in the regression. Of particular concern to scholars of race and policing is that white officers may treat white victims differently from nonwhite victims (Forman 2017; Taylor, Holleran, and Topalli 2009), a hypothesis that is tested here via the inclusion of the interaction term. If white officers conducted more thorough investigations for white victims than for nonwhite victims, the coefficient on the interaction term would be positive and significant. Instead, the coefficients are either close to 0 or negative at the $p < .1$ level, which suggests that,

Figure 2. Burglaries by income in Tucson, 2016

Table 2

Race, Investigative Thoroughness, and Clearance of Burglary Cases

	Log Time Spent		Prints Collected		Detective Assigned		Case Cleared	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
All White Officers	−.052	−.055	.017	.023	.039	.025	.008	.0004
	(.044)	(.045)	(.027)	(.027)	(.025)	(.026)	(.016)	(.016)
All White Victims	−.064	−.055	.014	.021	.023	.015	.010	.003
	(.041)	(.043)	(.025)	(.026)	(.024)	(.025)	(.015)	(.016)
Victims	−.021	−.013	.052**	.057**	.029*	.033**	−.016*	−.015+
	(.021)	(.021)	(.013)	(.013)	(.012)	(.012)	(.007)	(.008)
Officers	.130**	.128**	.007	.005	.044**	.042**	.044**	.044**
	(.008)	(.008)	(.005)	(.005)	(.005)	(.005)	(.003)	(.003)
All White Victims × All White Officers	.041	.025	−.043	−.050	−.068+	−.059	−.011	−.007
	(.061)	(.062)	(.037)	(.037)	(.036)	(.036)	(.022)	(.023)
Constant	3.963**	3.992**	.310**	.286**	.199**	.289**	.023	.044
	(.040)	(.075)	(.025)	(.046)	(.023)	(.044)	(.015)	(.027)
Month fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Division fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
N	2,620	2,620	2,763	2,763	2,763	2,763	2,763	2,763
R^2	.113	.121	.011	.040	.049	.067	.088	.094

Note. Robust standard errors, clustered at the census-block-group level, are in parentheses.

$^{+}$ $p < .1$.

* $p < .05$.

** $p < .01$.

if anything, white officers are slightly less likely to assign a detective to a case in which all victims are white than to a case in which not all victims are white. White officers do not adjust the thoroughness of their investigation depending on whether victims are white versus nonwhite. Similarly, nonwhite officers do not adjust the thoroughness of their investigation depending on whether the victims are white or nonwhite.

Interestingly, this result is inconsistent with existing research showing that police—a majority of whom are white in most cities—are less likely to clear a homicide case when the victim is Black or Hispanic than when the victim is white (Roberts and Lyons 2011; Fagan and Geller 2018). However, while some scholars and journalists believe that this disparity is due to officers' deliberate lack of thoroughness in investigating homicide cases with Black and Hispanic victims (Leovy 2015), others argue that it is more due to a lack of cooperation with homicide investigators in communities where relations between the police and the community are strained (Roberts 2015; Mancik, Parker, and Williams 2018). This debate highlights the distinction between investigative thoroughness and clearance: if factors beyond investigators' control largely determine clearance rates, clearance is not an accurate reflection of thoroughness. One advantage of the present study is that I measure investigative thoroughness directly using administrative records, without the complications inherent in using clearance as a proxy. Unlike in studies that measure thoroughness by whether a case was cleared, this study shows directly that white and nonwhite crime victims do not receive differently thorough investigations depending on the race of the responding officers.[4]

Given the standard errors on the regression estimates of the association between all-white groups of victims and investigative thoroughness presented in Table 2, at the upper bound all-white victim groups could—all else equal—expect the police to spend about 3 percent more time with them than with non-all-white groups of victims ($-.055 + 1.96 \times .043$), for the police to be about 7 percentage points more likely to collect prints from them than from non-all-white groups ($.021 + 1.96 \times .026$), for the police to be about 6 percentage points more likely to assign a detective to their cases ($.015 + 1.96 \times .025$), and for the police to be about 3 percentage points more likely to clear their cases ($.003 + 1.96 \times .016$). These estimates are upper bounds of 95 percent confidence intervals, all of which include 0.

This result is also surprising in the context of existing research on racial bias in policing. The bulk of quantitative nonexperimental research on this topic uses data on pedestrian or traffic stops or on police-involved fatalities to assess the extent of racial bias. Most studies of pedestrian stops find that racial minorities are more likely than whites to be stopped (Gelman, Fagan, and Kiss 2007) and that officers appear to use a lower threshold of suspicion when deciding to stop nonwhites compared with whites (Goel, Rao, and Shroff 2016), although some find limited evidence of bias in arrests conditional on stops (Coviello and Persico

[4] These findings on race and investigative thoroughness—despite being null in the frequentist sense—are quite informative in the Bayesian sense. I discuss this further in Online Appendix OD.

2015). Studies of traffic stops also mostly find significant bias against minority drivers (Baumgartner et al. 2017). Most studies of police-involved fatalities also find that Black individuals are more likely than whites to be killed by the police (Ross 2015; Knox, Lowe, and Mummolo 2020), although agreement on this score is not universal (Fryer 2019). But this literature is focused solely on officers' roles in stopping suspicious pedestrians and vehicles or arresting criminal suspects rather than on their roles as service providers. Inequalities in one context might not translate to another.

In particular, a key feature of pedestrian and vehicle stops is that those contexts require police to make nearly instant judgments about an individual's likelihood of possessing drugs or weapons. A wealth of psychological evidence shows that unintentional biases are magnified under snap-judgment-decision conditions (Payne 2006; Freeman and Johnson 2016). Anxiety can also magnify the reliance on heuristics such as racial stereotypes. When police officers stop pedestrians and vehicles in search of contraband, they tend to be alert to potentially dangerous situational developments (Woods 2019). Psychological studies show that anxiety inhibits typical information processing and can increase the use of stereotypes in decision-making (Wilder 1993; Hilton and von Hippel 1996; Hamilton and Sherman 2014). The relatively slow-moving pace of a residential burglary investigation, when the community members with whom police are interacting are victims rather than suspects, distinguishes police investigations of residential burglaries from the more heavily studied cases of police pedestrian and vehicle stops.

Nevertheless, there at least two reasons to expect that inferior services might be provided to victims from racial minority groups. First, other urban public services, such as education and housing, reflect extreme racial and socioeconomic inequalities (Krivo and Kaufman 2004; Duncan and Murnane 2011), and these patterns may well extend to burglary investigation services. Second, and more importantly, a large, multidisciplinary research literature finds that law enforcement's poorer treatment of racial minorities includes underenforcement of law, underprotection of vulnerable people, neglect of investigations, and general indifference to crime and violence when racial minorities are the victims. Natapoff (2006, p. 1717), for example, writes that "[u]nderenforcement [of laws] can also be a form of deprivation, tracking familiar categories of race, gender, class, and political powerlessness." Concretely, Rios (2011, p. xi) writes that, after asking an Oakland police officer whether the police would try to catch the man who murdered his friend, the officer replied, "What for? We want you to kill each other off." Many scholars of law, sociology, and criminology persuasively argue that failure to take crimes seriously when racial minorities are the victims is a defining feature of the racially discriminatory criminal justice system in the United States (Kennedy 1988; Sklansky 2005; Currie 2020). Finding that the police do not discriminate against racial minorities when they are residential burglary victims, then, represents an informative null result.

Two possible threats to validity are addressed in Online Appendix OA: that nonwhites call the police for systematically more serious reasons than whites and that nonwhite officers are systematically assigned to cases with nonwhite victims (as Ba et al. [2021] find with patrol assignments in Chicago). That analysis indicates that neither pattern is present in these data.

3.2. Forced Entry and Investigative Thoroughness

Table 3 presents the results of fixed-effects regressions predicting the three indicators of investigative thoroughness in burglaries with forced entry. Comparing the regressions with no controls and those with controls for all available victim, officer, and incident characteristics and fixed effects reveals that while some of the variation is explained by the controls and the fixed effects, forced entry nevertheless has a statistically significant and substantively large effect on investigative thoroughness. The estimates for the fully specified regressions reflect that officers spend 19 percent more time at the scene, are 18 percentage points more likely to dust for fingerprints, and are 12 percentage points more likely to assign a detective to the case when the burglary involved a forced entry than when it did not, conditional on all situational and demographic variables and fixed effects. The means of the dependent variables in unforced-entry cases are 74 minutes at the scene, 27 percent likelihood of collecting fingerprints, and 25 percent likelihood of assigning a detective, and so these coefficients represent officers spending 14 more minutes at the scene, a 67 percent increase in the likelihood of collecting fingerprints, and a 49 percent increase in the likelihood of assigning a detective.

Do these results vary on the basis of officers' or victims' race? Given the importance of forced entry to investigative thoroughness, the values of the three-way interaction of Forced Entry, All White Officers, and All White Victims and of the concordant two-way interactions may be relevant. In Online Appendix Table OB3, I examine these interactions and find that almost none are significant. If anything, all-white officer groups and all-white victim groups appear less likely to have a detective assigned ($.01 < p < .05$), and none of the three-way interactions are significant at conventional levels. Relatedly, I examine the role of race in the subset of forced-entry cases only in Table OA3 to see whether officers' race and victims' race are significant predictors of thoroughness in these more serious incidents; mostly they are not, and if anything somewhat less time is spent at scenes with all white victims ($.01 < p < .05$). Another way of thinking about the role of race in the subset of forced-entry cases is to conduct a partial F-test comparing models that include and exclude the variables for officers' and victims' race. The results of these tests are in Table OA3; none are statistically significant at conventional levels, which indicates that models including those variables are not more predictive of thoroughness than models excluding them, even in the subset of forced-entry cases. Table OA4 further confirms that, if anything, burglary scenes that feature all-white officer groups and all-white victim groups are slightly worse served than scenes that feature mixed-race groups of officers, victims, or both,

Table 3
Forced-Entry Burglaries and Investigative Thoroughness

	Log Time Spent		Prints Collected		Detective Assigned	
	(1)	(2)	(3)	(4)	(5)	(6)
Forced Entry	.262**	.193**	.203**	.181**	.142**	.123**
	(.033)	(.045)	(.019)	(.026)	(.019)	(.026)
All White Officers		−.052		−.0001		.053
		(.065)		(.036)		(.038)
All White Victims		−.108+		.002		.037
		(.055)		(.034)		(.033)
Apartment		−.060		−.043		−.097**
		(.051)		(.028)		(.027)
Victim age (mean)		.00001		−.001		−.001+
		(.001)		(.001)		(.001)
All male officers		−.004		−.001		.033
		(.048)		(.028)		(.026)
All male victims		−.030		−.033		−.008
		(.043)		(.026)		(.024)
Victims		−.011		.022		.028
		(.028)		(.017)		(.019)
Officers		.116**		.012		.035**
		(.013)		(.008)		(.007)
All White Victims × All White Officers		.069		−.045		−.093+
		(.085)		(.047)		(.050)
Constant	4.003**	4.181**	.268**	−.691	.253**	−.183
	(.027)	(.554)	(.015)	(.236)	(.015)	(.245)
Fixed effects	No	Yes	No	Yes	No	Yes
N	2,620	2,502	2,763	2,633	2,763	2,633
R^2	.023	.433	.039	.358	.021	.359

Note. Robust standard errors, clustered at the census-block-group level, are in parentheses. Fixed effects include division, census block group, date, priority, and hour.
$^{+}p < .1$.
$^{**}p < .01$.

but there is no statistically significant evidence for less investigative thoroughness if the racial makeup of the officer and victim groups differs.

The prediction intervals in Figure 3[5] show the large magnitude of predicted differences in investigative thoroughness between forced-entry and unforced-entry cases. With continuous covariates held at their means and categorical covariates held at their modes, the linear models in Table 3 predict 48 minutes (58 minutes) spent at the scene, an 18 percent (36 percent) probability of fingerprint collection, and a 10 percent (22 percent) probability of detective assignment for an unforced-entry (forced-entry) burglary.

These results are consistent with the large body of literature on bureaucratic decision-making that argues that bureaucrats are focused on maximizing their performance on observable, quantifiable measures that enhance their reputations and as a consequence enhance their future professional prospects (Carpenter 2010). The results conform to a model of police officers as bureaucrats maximizing their main performance measure—clearance rates—just as other types of bureaucrats seek to maximize their performance measures (Brodkin 2008; Balla and Gormley 2018). An officer, in other words, reasons that a forced entry is more likely to result in a cleared case than is an unforced entry, regardless of the demographics of the victim. Interestingly, I am not able to confirm that forced-entry cases are more likely to be cleared than unforced-entry cases because investigative thoroughness—particularly the assignment of detectives—strongly predicts clearance, and forced entry and investigative thoroughness are confounded. I discuss this issue further in Online Appendix OC.

The key identifying assumption for a causal interpretation of the relationship between forced entry and investigative thoroughness is that in a census block group, date, and hour of the day, forced and unforced entry are randomly assigned to residences. In other words, although forced-entry and unforced-entry residential burglary incidents take place in systematically different types of homes, I argue that the assumption that both types of incidents that take place in the same neighborhood, on the same day, and at the same hour do not take place at systematically different types of homes is quite plausible, and thus the three sets of fixed effects capture all relevant variation.

First, census block groups effectively capture neighborhood-level variation because they are small: in Tucson, on average, they are composed of just 1,540 individuals, or 616 households. It is thus reasonable to assume that residences' level of physical security; the size of burglary victims' houses, yards, and garages; or any other physical characteristics of homes do not vary systematically between forced-entry and unforced-entry incidents in these small neighborhoods. This is especially likely because of the high level of socioeconomic and racial housing segregation in Tucson (Brocious 2019).

Second, fixed effects for the calendar date effectively capture the time variation at the level of the day or longer. Crime and police capacity are time vary-

[5] In Figure 3, the points are predicted values, and gray bars represent 95 percent confidence intervals.

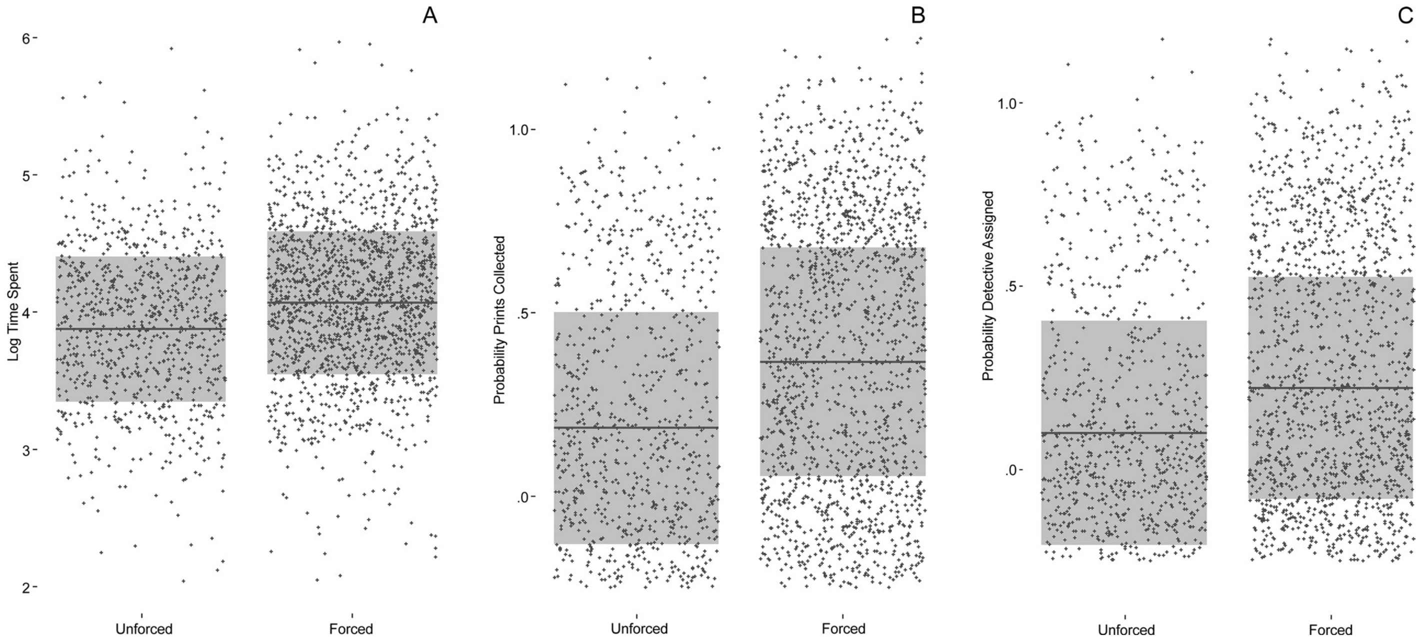

Figure 3. Predictions from regressions

ing in many ways: according to the seasons; according to whether it is a weekday, a weekend day, or a holiday; and even, some have argued, according to the timing of public benefits distribution (Foley 2011). Even though the prevalence of forced-entry and unforced-entry incidents differs systematically according to the month, day of the week, and likely other observable and unobservable day-, week-, and month-level characteristics, this variation is accounted for by date fixed effects.

Finally, the prevalence of forced-entry and unforced-entry incidents also differs systematically by time of the day for many reasons: because of light and darkness, because of when individuals are likely to be at home versus at work, because of changing police shifts, and probably because of other observable and unobservable hour-level characteristics. Fixed effects for hour account for this observed and unobserved variation.

Because an officer's experience is a potentially important omitted variable, I requested and received this information from the TPD. Some officers' identification information is missing from the burglary investigation data, and so I am not able to include officers' experience for all incidents. Officers' experience is not a statistically significant predictor of investigative thoroughness, and its inclusion shrinks the sample; it is analyzed in Table OB1. The insignificance of officers' experience is particularly notable in light of the model in DeAngelo and McCannon (2016), which predicts that more- and less-experienced officers differentially respond to reputational threats stemming from citizens' challenges to their behavior. If this model translates to burglary investigations (their model focuses on arrests), then it would predict that more-experienced officers would make a stronger effort than less-experienced officers to treat white and nonwhite victims equally. Table OB1 shows that interactions between officers' experience, officers' race, and victims' race are not statistically significant at conventional levels.

Even with the inclusion of these strict fixed effects, there may still be a concern about omitted-variable bias. To check if this might be the case, I carried out the calculations suggested by Altonji, Elder, and Taber (2005) and Oster (2019). Both approaches offer a way to calculate the "ratio of selection on unobservables to selection on observables that would be required if one is to attribute the entire effect" of the treatment variable to selection bias (Altonji, Elder, and Taber 2005, pp. 151–52). These approaches rely on different assumptions, but both broadly show in this case that the "relative degree of selection on observed and unobserved variables" (Oster 2019, p. 188) would have to be extremely large to explain away the entire effect of forced entry on investigative thoroughness. The six ratios (calculated for the three outcome variables with both econometric methods) are presented in Table 4.

For ratios calculated using the Altonji, Elder, and Taber (2005) method, the coefficient of interest is divided by the product of two numbers: first, the ratio of the variance in the outcome variable to the variance in the residuals of the regression that predicts the treatment variable with relevant covariates and, second, the difference in mean residuals of the regressions of interest for forced-entry and

Table 4

Implied Ratios of Selection on Unobservables to Selection of Observables Required to Explain the Effect of Forced Entry on Thoroughness

	Altonji, Elder, and Taber (2005)	Oster (2019)
Log time spent	4.6	2.62
Prints collected	−10.76	3.55
Detective assigned	−3.07	3.21

Table 5

Implied Ratios of Selection on Unobservables to Selection of Observables Required to Explain the Effect of Having All White Victims on Thoroughness

	Altonji, Elder, and Taber (2005)	Oster (2019)
Log time spent	−1.81	1.52
Prints collected	−.08	−.07
Detective assigned	−3.29	−1.01

unforced-entry incidents. Ratios calculated using the Oster (2019) method use the Stata package psacalc and rely on the assumption that the inclusion of omitted variables will increase the R^2-value by no more than 30 percent above the R^2-value calculated when a full set of controls is used to predict the outcome variable.

Since there may also be concerns about omitted-variable bias in the determination of no relationship between officers' race, victims' race, and investigative thoroughness, I repeat the calculations for the coefficient on All White Victims. The results are presented in Table 5. Although the Altonji, Elder, and Taber (2005) and Oster (2019) methods are generally used to show that a nonzero coefficient is robust, it is encouraging that five of the calculations in Table 5 reveal that the ratio of selection on observables to selection on unobservables required to explain the (small) measured effects of victims' race on thoroughness is also quite small. This indicates that selection explains roughly the entire association and that there is no causal relationship between victims' race and investigative thoroughness.

To further confirm that the coefficients on forced entry are not sensitive to model selection, I follow Card, Fenizia, and Silver (2019) and Bleemer (2022) and perform a Monte Carlo simulation in which I include a set of between 0 and 13 randomly selected covariates (the 13 variables are the nine main effects reported in Table 3, along with fixed effects for priority of the call, census block group, date, and hour of the incident). Figure 4 shows that the coefficients on forced entry are remarkably stable across the possible regression specifications, and these

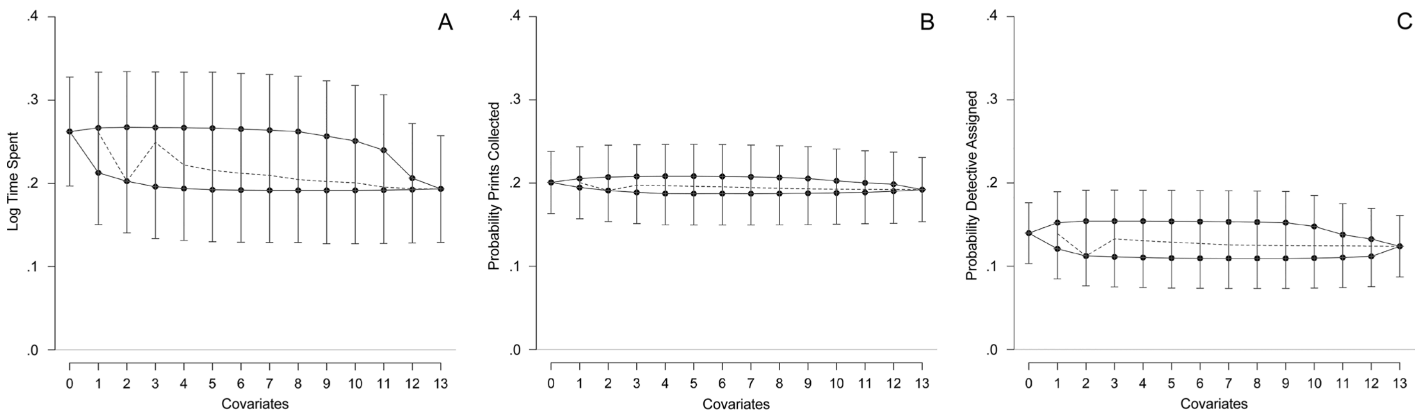

Figure 4. Alternative covariate specifications: effect of forced entry

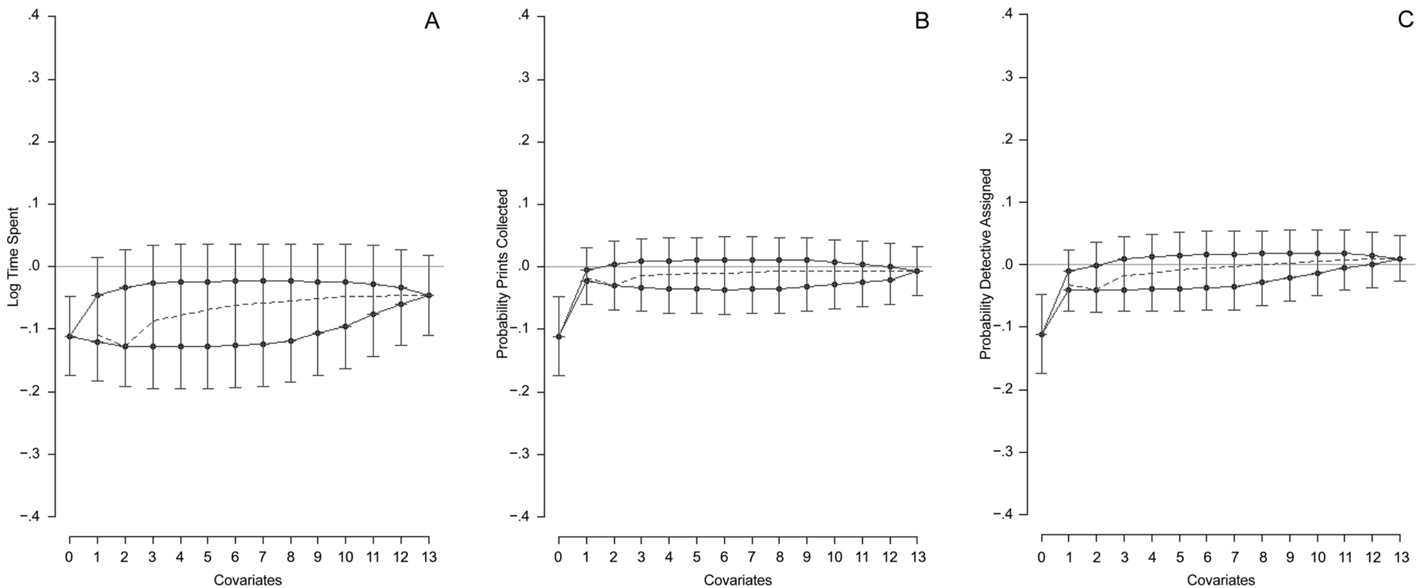

Figure 5. Alternative covariate specifications: effect of all white victims

Table 6
Economic Disadvantage in Burglaries

	Overall Mean	Forced-Entry Share	Unforced-Entry Share	p-Value
Apartment	.31	.28	.37	<.01
High-poverty neighborhood	.26	.24	.29	<.01
High-renter-share neighborhood	.39	.36	.44	<.01

results would not be changed by using a different regression specification.[6] The results also strongly suggest that additional covariates would not move the coefficients toward 0. I repeat this exercise with the coefficients on All White Victims in Figure 5; in this case, the plots show that in nearly all possible regression specifications, the presence of an all-white victims group does not significantly predict investigative thoroughness at the 95 percent confidence level—nearly all 95 percent confidence intervals on these coefficients include 0.

3.3. Unconditional Inequality in Service Provision

Although Tucson residents experience similar-quality burglary investigations conditional on whether the burglary involved a forced or an unforced entry, they do not experience similar-quality burglary investigations overall. Service provision is unconditionally unequal.

Because the regression models in Table 3 account for many types of variation that could affect investigative thoroughness, the results mask neighborhood- and residence-level differences in the incidence of forced and unforced entry. In particular, there are marked unconditional differences in the rates of forced and unforced entry in different types of residences and different types of neighborhoods. Table 6 shows that apartments, high-poverty neighborhoods, and neighborhoods with a high renter share (more than two-thirds of residences are rented) are overrepresented among unforced-entry cases and underrepresented among forced-entry cases (forced-entry cases are 65 percent of cases). Table 6 shows that unforced entry is more likely in apartments and high-poverty areas: 39 percent of Tucson's burglaries in 2016 took place in census block groups with a high share of renters, and 44 percent of unforced-entry cases took place in these block groups. Similarly, 26 percent of burglaries took place in high-poverty neighborhoods, but 29 percent of the unforced-entry burglaries did. Finally, 31 percent of burglaries took place in apartments, but only 28 percent of the forced-entry cases took place in apartments. Unforced-entry cases, then, unconditionally take place more often in less-advantaged residences and neighborhoods.

[6] In Figures 4 and 5, the points represent the minimum and maximum estimated coefficients for each regression specification, and bars represent the union of 95 percent confidence intervals for the minima and maxima. The dashed lines indicate the median estimated coefficients for all randomly selected sets of $k = 0, \ldots, 13$ coefficients.

Although it is impossible to tell with certainty why this is the case, the clearest explanation is that landlords in the private rental market have less incentive to invest in security measures for their properties than homeowners do (Hamilton-Smith and Kent 2005). In addition, Tucson Police Department burglary officials speculated that in multifamily residences it is easier to case many residences quickly for unlocked doors or windows, and there is an economy of scale for a burglar in learning a way into a housing complex with a large number of units. This mechanism would be consistent with criminological literature that finds that event dependence for repeat and near-repeat burglaries (that is, burglaries that occur more than once in a year at the same residence or in the same small cluster of residences) is greater where the distance between homes is smaller (Short et al. 2009). Taken together, this evidence demonstrates that officers' seemingly neutral decisions to attempt to maximize clearance rates has distributional consequences.

4. Conclusion: Bureaucratic Incentives, Resource Allocation, and Inequality

Policing, like other public services, involves the provision of services to a diverse set of residents with differing interests and needs. Local police departments provide a public service to residents by responding to calls for assistance. Scholars of policing have focused extensively on racial inequalities in the police's punitive functions, especially pursuing and arresting criminal suspects (Persico and Todd 2006; Grogger and Ridgeway 2006; Beckett, Nyrop, and Pfingst 2006; Golub, Johnson, and Dunlap 2007; Gelman, Fagan, and Kiss 2007; Antonovics and Knight 2009; Mitchell and Caudy 2015; Legewie 2016; Horrace and Rohlin 2016; Baumgartner et al. 2017). And research that leverages incident-level data to measure inequalities focuses on either traffic-ticketing patterns (DeAngelo and Owens 2017; West 2018, 2019) or racial patterns in criminal victimization (DeAngelo, Gittings, and Pena 2018). These methods have not been previously used to explore inequalities in police services rendered to victims and witnesses of crimes or disturbances, despite the crucial role of service provision to victims in the policing function (Goldstein 1977; Friedman 2021).

In this paper, I show that police officers in Tucson primarily use a simple rule—whether a burglary features a forced entry—in deciding how thoroughly to investigate a residential burglary. Demographic characteristics of victims and officers, including the interaction of officers' and victims' race, are not significantly conditionally associated with additional investigative thoroughness, but officers do devote greater investigative resources to forced-entry burglaries. Officers spend 14 more minutes at the scenes of forced-entry burglaries, they are 18 percentage points more likely to dust for fingerprints, and they are 12 percentage points more likely to eventually assign a detective to the case when the burglary features a forced entry, conditional on the available situational and demographic variables and fixed effects for census block group, date, and hour.

Are these results likely generalizable to cities other than Tucson? While it is impossible to say for certain, residential burglaries take place in every city, and investigating them is a core component of police training across the United States (Weisel 2002; Antrobus and Pilotto 2016). Clearance rates, too, are a key metric of police performance in every department (Mas 2006; Sonnichsen 2007; Pare 2014). Thus, it is reasonable to expect that the police's incentive to clear residential burglary cases shapes police behavior in many cities. Further research is necessary to determine how the clearance incentive interacts with other incentives—including, perhaps, the incentive to provide more racially or socioeconomically advantaged citizens with higher-quality public services (Keefer and Khemani 2005).

The findings in this study beg the question of why police might be less likely to discriminate against racial minorities in residential burglary investigations than they are in vehicle stops, pedestrian stops, and use of force. More generally, why might street-level bureaucrats exhibit different levels of bias in different contexts? Three areas of literature provide plausible answers.

A first hypothesis requiring further research, for which this paper provides preliminary support, is that police behave differently when providing substitutable and nonsubstitutable public goods (Besley and Coate 1991). Not all police-provided services are nonsubstitutable: private patrol is available from private firms (Trounstine 2015), and the wealthy routinely call on civil lawyers or therapists for mediation services for which poorer citizens routinely call the police (Friedman 2021). A second hypothesis is that if street-level bureaucrats tend to prioritize the most straightforward cases (Lipsky 1980), discrimination by street-level bureaucrats may be most prevalent in administratively simple cases, and these may be cases in which clients are racially or socioeconomically advantaged. A final hypothesis is that different contexts implicate different psychological responses. Burglary investigations can be done slowly and methodically, and victims of residential burglaries are not perceived as threats to officers' safety. In pedestrian and vehicle stops, however, officers are trained to be alert for armed or dangerous civilians (Woods 2019). Officers might therefore rely less on racial stereotypes when carrying out a residential burglary investigation than when stopping pedestrians or vehicles.

Each hypothesis opens the door for further research on the determinants of discrimination—or the lack thereof—by police in different contexts. A focus on the nature of the goods that police provide, the incentives they face, and the psychological pressures they encounter can provide a more nuanced view of police discrimination.

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