'Who You Gonna Call?' 911 Call Takers and Police Discretion

Austin V. Smith*

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Abstract

Police make high-stakes decisions under multiple sources of uncertainty, often resulting in adverse outcomes that are disproportionately experienced by minority citizens. What source of information do police use to make their enforcement decisions and does it generate accurate decisions? I examine a common information source for police officers throughout the United States: risk assessments conducted by 911 call takers. Exploiting random variation in the automated call distribution system used by a large metropolitan 911 center, I show that police are 37.7% more likely to make arrests at calls that the call taker deems risky. I demonstrate that officer perceptions are a primary channel through which the effects operate. This results in lower quality police decisions: misdemeanor arrests made at calls that are on the margin of being classified as risky are 14.9 percentage points less likely to result in a conviction. In cases where the race of the involved civilians is the same as the responding officer or the responding officer has more job experience, call taker information has a negligible effect on arrests. The results suggest that the strength of an officer's priors determines the extent to which officers rely on information transmitted by call takers.

1 Introduction

In 2014, 12-year old Tamir Rice was killed by police officers who believed Rice was reaching for a gun, an object which was later revealed to be a toy replica of a firearm. Dispatch and 911 call audio from the event revealed that the dispatched officers were told they were responding to a male pointing a gun at people. They were not informed that Rice was a child and that the gun was likely fake, both pieces of information which were communicated by the civilian who called 911 (WKYC, 2015). In 2009, Harvard Professor Henry Louis Gates Jr. was arrested after police were sent to his house suspecting a potential break-in. A neighbor had called 911 after seeing Gates attempting to force open his front door which had been jammed shut. Analysis of the audio transcripts from the 911 call revealed that the caller suspected that Gates lived in the house, but the call taker swayed the caller into agreeing with a breaking-and-entering classification (Gillooly, 2020).

Though differing in the severity of their outcomes, both of these instances highlight the importance of 911 call information in shaping how police respond to requests for law enforcement services. Police rely on the ability of 911 call takers to interpret both the needs of the caller and the potential risk of any law enforcement interaction. The information supplied by call takers is often the only information that police have about a given call before they arrive at a potentially dangerous scene. As a result, this information may play an outsize role in officer behavior once they arrive.

* *University of Arizona*, email: austinvsmith@arizona.edu

In this paper, I present the first empirical evidence that police enforcement decisions are meaningfully swayed by variation in the risk assessment preferences of call takers by estimating the effect of call taker priority classifications on arrests. In my setting, call takers can choose to give an incident priority due to heightened risks to civilians or potential danger to the responding officers. Priority classifications increase the speed of police response and act as one of the few pieces of information that officers have before entering a potentially risky scenario. Because a call's priority status is correlated with unobserved suspect behaviors that influence the chances of an arrest, I employ an examiner design identification strategy that leverages quasi-random assignment of call takers to 911 calls in order to obtain causal estimates of priority upgrading on arrests (Doyle, 2007; Dobbie et al., 2018; Arnold et al., 2018). I instrument for priority using Call Taker Score, a measure of the assigned call taker's innate propensity to upgrade a call to priority status.

I document substantial variation in the preferences of call takers. Replacing a call taker at the bottom of the Call Taker Score distribution with one at the top leads to a 43 percentage point increase in the likelihood of a call receiving priority status, an increase of over 100% relative to the mean priority rate. Call takers who upgrade calls more frequently tend to be relatively inexperienced, however I show that there is a concentrated right tail of experienced call takers who consistently upgrade calls at a higher rate than most of their colleagues.

The IV allows me to estimate differences in police behavior that are caused by changes in priority status driven only by underlying variation in the preferences of the assigned call taker. Using a call sample that comprises mostly of low-level non-index offenses, I find that priority signals cause officers to make more arrests at calls which are otherwise similar to non-priority incidents: a priority signal creates 1 additional arrest per 100 calls, a 37.7% increase on the mean arrest rate. I show that these results are robust to the inclusion of rich officer and call covariates as well as sample selection and call transfer propensities which address threats to quasi-random variation in call taker assignment. I estimate that these results are in part driven by a 40% increase in misdemeanor arrests. While the results on felony arrests are inconclusive, I interpret these findings in the context of my analysis sample as evidence that call takers significantly influence enforcement outcomes for low-level crimes.

I decompose my estimate into two potential mechanisms: an information effect and a response time effect. Priority acts both as a tool to quickly assign officers to a call (response time effect) and a signal of the suspect's behavior given to the responding officers (information effect). I provide evidence that both the information and response time channels play a role. Response times decrease by over 37 minutes as a result of priority classification. While I do not find evidence that this reduces the likelihood that witnesses vacate the premises, I do find suggestive evidence indicating that the suspect is less likely to flee the scene. Priority increases the chances that an arrest is made within 15 minutes, a proxy for an on-sight arrest in the spirit of Vidal and Kirchmaier (2018), by 36%. However, I also find that arrests occurring after longer lag times, from 45 to 60 minutes, increase at an even larger rate, suggesting that the results are not driven by response times alone.

As supporting evidence for the role of information effects, I show that misdemeanor arrests are 14.9 percentage points less likely to result in conviction when taking place at a priority call, indicating that officers incorporate these signals into their assessments of suspect guilt and are induced into making lower quality arrests. To further understand how police use this information, I consider two situations where police priors over the civilians they encounter are likely to vary: officers dealing with suspects who are the same race as themselves and officers gaining experience. I implement a difference-in-difference specification that exploits within-officer variation in the location and timing of call dispatches in order to estimate how the gap between priority and non-priority arrests changes as officers age and work in neighborhoods that share a greater resemblance with themselves.

I find that arrests increase in response to priority signals *less* when the officer is in a same-race neighborhood. A 10 percentage point increase in the proportion of a neighborhood that is the same race as the officer reduces the priority effect by .0015 percentage points, or 5% on the mean. These effects are driven by Black and white officers, whose priority arrest gaps shrink in Black (white) neighborhoods more relative to their non-Black (non-white) counterparts. I also show that the priority effect on arrest falls as officers gain more experience. I estimate that by the time an officer has been in the force for 22 years, the effect of changing a call's priority on their arrest decision is negligible. These findings demonstrate that officers are more willing to incorporate noisy outside information when they have weaker priors.

Overall, my findings highlight the importance of call takers in shaping the police response process and the role of police information processing in enforcement decisions. The results suggest two potential directions for policy to improve the quality of police decision-making. First, given the large effects that I find on arrests that are generated by priority signals and the substantial variation in call takers' willingness to make these decisions, this paper points toward the need for increased training for call takers that standardizes the intake process so as to reduce the outsize effect of their classification discretion. Second, my findings that officers incorporate this information differently depending on their prior information point toward the need for policies that attract officers with stronger priors toward the communities that they police. In particular, given the discrepancies in the use of outside information by neighborhood and officer race, and prior studies showing that minority civilians are more likely to experience adverse policing outcomes in excess of their offending likelihood (e.g. Goncalves and Mello, 2021; Hoekstra and Sloan, 2022), I provide evidence that attracting more minority officers can help reduce disparate policing outcomes by improving the likelihood that responding officers have familiarity with the people that they encounter.

In studying how police use information to make enforcement decisions, this paper relates to a broad literature on police discretion (Mastrofski, 2004; Nickels, 2007; Weisburst, 2022; Goncalves and Mello, 2023). Prior papers have uncovered several channels that can explain variation in police behavior over time, within departments, or throughout an officer's career: union membership (Mas, 2006; Cunningham et al., 2021), peers (Holz et al., *Forthcoming*; Cho et al., 2021), training (Adger et al., 2022), and innate preferences for punishing guilty civilians (Chalfin and Goncalves, 2021; Goncalves and Mello, 2023). My findings are complementary to recent work by Dube et al. (2023), who highlight that police are forced to make high-pressure decisions in limited time, which may lead them to rely on easily available yet potentially misleading sources of information to make enforcement decisions that can impose excessive costs on the public. By showing that police decisions anchor on call taker priority signals, I provide evidence on cognitive distortions driven by a particular information source. This highlights a situation in which the authors' behaviorally-informed training program may be effective.

A notable strand of the discretion literature deals with examining how discretion contributes to racial disparities in policing (e.g. Knowles et al., 2001; Anwar and Fang, 2006; Horrace and Rohlin, 2016; Grogger and Ridgeway, 2006; Fryer, 2019; Goncalves and Mello, 2021). By showing that police are more likely to rely on outside information in neighborhoods that are a different race than their own, I highlight information driven by familiarity as a key mechanism through which racial disparities in policing may arise, similar to Domenech-Arumi (2023). In using 911 call assignment to show that the effects of information vary according to the race of the neighborhood to which an officer is dispatched, my findings are similar to Hoekstra and Sloan (2022), who leverage the as-if random assignment of officers to neighborhoods via the 911 process to show that white officers scale up their use of force more than minority officers when dispatched to minority neighborhoods. The evidence suggests that hiring minority officers can produce benefits for minority civilians, which complements previous literature showing the importance for officer race in determining police interactions (Ba et al., 2021b) and police personnel decisions (Rim et al., Forthcoming).

I also contribute to work within criminology and sociology on the role of call takers in the law enforcement process. Much of this work emphasizes the role of call takers as gatekeepers to the criminal justice system, as they determine who comes into contact with police officers and thus are crucial to efforts to divert police resources away from low-level incidents (Lum et al, 2020; Goodier and Lum, 2022). Recently, Gillooly (2020) emphasized the additional role that call takers have as risk appraisers. In addition to deciding who gets a police response, call takers also must evaluate the severity of incidents and set expectations for officers. Gillooly emphasizes that these expectations may influence how officers perceive civilian actions, a claim which is bolstered by Taylor (2020), who finds using an experimental design that law enforcement officers are more likely to unjustifiably use deadly force in a simulation environment when given erroneous information by the call taker. I further develop this literature by examining how call taker risk appraisal behavior impacts the discretion of police officers. In my case I consider discretionary actions, arrests, that are far more common than deadly force.

In a similar vain to this paper, Gillooly (2022) uses plausibly exogenous variation in call taker assignment to evaluate how risk appraisal affects police perceptions. She finds that exogenous shifts in priority signals for mental health and domestic violence calls cause officers to believe the call is high priority. An advantage of her setting is that she has a direct measure of police beliefs. An advantage of my setting is that I am able to measure police actions along a variety of dimensions. Both of our papers highlight the importance of call takers in the 911 response process. My work contributes by using 911 call takers to demonstrate how police use information to make enforcement decisions and the situations in which this information is most important.

The rest of the paper proceeds as follows. Section 2 discusses the institutional details of the Dallas Police Department that motivate the empirical strategy; Section 3 introduces the data; Section 4 proposes the empirical model and discusses its assumptions; Section 5 presents the results; Section 6 discusses mechanisms; and Section 7 concludes.

2 Institutional Background

The setting for my analysis is the Dallas Police Department. When a 911 call is made in Dallas, it is assigned to an available call taker using an automated assignment system. The system finds a call taker who is not actively responding to a caller, and notifies them of a call with a beep on their headset. There is no pre-screening process to calls, so call takers cannot selectively answer based on call characteristics. Additionally, call takers are not assigned by the machine according to any features of the call, such as its location. Any call taker can be assigned to any call made during their shift if they are available. While many cities in the US have several location-specific 911 call centers for the same police department, one advantage of my setting is that Dallas PD operates a centralized telecommunications building that receives all 911 calls made in the city. Thus, all callers who work the same shift are exposed to the exact same set of calls. Call taker shifts are determined once or twice a year by a bidding process which prioritizes seniority, a process which is also commonly used by police officers to assign shifts and patrol beats (Ba et al., 2021a). Call takers work 5 days a week, and can be assigned to one of three shifts, which roughly corresponds to morning/mid-afternoon, afternoon/evening, and night.

On rare occasion, calls may be transferred within the call center. The primary reason this happens is because translation is needed when a caller prefers to speak in a language other than English. Since Hispanics make up nearly 40% of the Dallas population, most of the time this language is Spanish. If a bilingual call taker is not available, the call taker can access a 3rd party translation service. When I spoke with call takers who work for DPD, they told me that transfers based off of the content of a call are possible, but infrequent and generally discouraged by the department. These transfers only happen if a call taker loses control of an incident and a supervisor needs to step in. One call taker I spoke to suggested that transfers of these type happen once or twice a week, which, given their 911 call volume, would equate to roughly 1 in every 10,000 calls.

Once the call taker answers, their goal is to assess the caller's situation and determine which of 85 incident classifiers to assign to the call. Each classifier covers a range of closely related incident types, such as burglaries or shootings, and is attached to a priority number, which I will refer to as the incident's queue order so as to avoid confusion. The queue order ranges from 1 to 4 and is used by dispatchers to assign officers to calls in an order consistent with the severity of the call. Call takers can make comments, known internally as a "signal justification", in order to give a description of the incident which is consistent with their assigned classifier. Call takers have substantial discretion in deciding how to classify an incident.

Most of the incident types that call takers can choose from are hard-coded to a queue order. However, there are 7 classifiers that contain two possible queue orders that call takers can choose from: residential, business, and vehicle burglaries; thefts; criminal mischief; missing persons; and "Other," a catch-all category for incidents that do not fit into the other classifiers but which usually amount to trespassing, non-violent public disturbances such as drunkenness, and turning people in for outstanding warrants. Within these incidents, call takers use their discretion to upgrade a call's place in the dispatch queue based on features of the call, including the potential for violence, presence of armed suspects at the scene, and the number of victims. For the purpose of the empirical analysis, I say a call is given priority status if the call taker chooses to upgrade its queue order. These priority upgrades increase the speed with which officers will be sent to the call and also send a signal to officers that call is potentially severe and/or dangerous. Thus, two near-identical offenses may receive different police responses because the call taker deemed one a priority incident and the other a non-priority incident. For incidents that do not fall into one of the 7 listed classifiers, call takers either have limited discretion to control its queue order or would choose another incident classification in order to upgrade the call. The empirical strategy of this paper leverages call taker discretion in sending priority signals within the 7 known incident classifications.

Once call takers assign the call signal, the call sheet is sent to the dispatcher, who uses the location of the call, the call signal, and the call priority to assign police units to the scene using the Computer Aided Dispatch (CAD) system. Dispatchers assign calls according to their queue order. An example of call entries in CAD is given by Figure 1. In the picture, the call taker's incident classification is given by the column titled "Nature of Call" and the queue order attached to the incident is given by the "Priority" column. An example of priority upgrading by the call taker is given in rows 4 and 6. In row 4, the call is assigned the code "40 - Other", which indicates a non-priority "Other" call; in row 6, the call is assigned the code "40/01 - Other", indicating a call that is classified as "Other" with priority. The enhanced priority of the call in row 6 is reflected by the higher queue ordering in the "Priority" column, a 2 versus a 3.

Civilian 911 calls are a significant source of police activity. The average patrol officer in my sample responds to 3 911 calls per day, each of which takes 47.6 minutes to complete on average. Thus the average patrol officer working an 8 hour shift spends nearly a third of their day responding to civilian-initiated 911 calls. Among all arrests made by the Dallas Police Department, 41% derive from a 911 response. Police have limited discretion in choosing which calls they respond to, as dispatcher assignments override any officer choice (Weisburst, 2022). Officers can view the call details for all active 911 calls on their in-car computer terminals, however officers do not observe which call taker was assigned to each call. Accordingly, police officers may not be fully aware of underlying variation in call taker behavior that affects call codes, and they lack the information necessary to adjust for it. Call takers do not have direct contact with officers in the field and officers are not generally familiar with the call takers.

Call taker discretion may, in part, be formed by the call taker training process. Call taker training in Dallas is structured similarly to police officer training, with extended coursework followed by supervised work in the field. Call takers train in the classroom for 6 weeks, which involves, among other things, learning all of the dispatch codes, how to manage callers who are in-crisis, and how their job impacts police in the field. Police officers also receive call taker training during their time at the academy, which allows them to work in the call center on days where the department is short-staffed. At the end of the coursework, call takers must pass a Texas Commission On Law Enforcement (TCOLE) exam to receive their public safety telecommunications license. This process mirrors the police academy, which concludes with officers taking the TCOLE Peace Officer's license exam. After receiving their license, call takers begin field training in the Dallas call center. They are assigned a mentor who they observe responding to calls. Once deemed ready by the mentor, call takers are allowed to respond to calls under supervision. The mentor is expected to step in if the trainee loses control of their calls in this period. Because of these potentially frequent transfers, I do not consider trainees in the empirical analysis. After graduating from trainee to full-time, a call taker may respond to any call that comes into the call center.

3 Data

This paper uses administrative data obtained from the Dallas Police Department and Dallas District Attorney's office through public records requests. I combine the universe of civilian calls-for-service made to the Dallas Police Department from 2015 to 2019 to arrest reports, criminal charges, use of force reports, police personnel files, and all legal cases disposed in Dallas County courts.

Calls-for-service data are pulled from CAD and include all incidents which are responded to by Dallas PD officers that derive from 911 calls, as well as other incidents which are self-initiated or derive from 311 calls. I use data cleaning methods described by Weisburst (2022) to narrow the sample down to just those incidents which are most likely to derive from 911 calls. The 911 call sample includes information about the time and location of the call, the incident classifiers and queue orders, the badge numbers of all officers who respond to the incident, and the name of any call taker who handles the call, including the one who answers and the one to whom the call is transferred. The 911 data also include call disposition codes, which indicate the status of incident report documentation that is required to be written by officers before they end their shift. Call dispositions may take on a variety of values, though the majority of calls receive a classification of "Report"(indicating that an offense report was written), "No Police Action" (when the call did not require police action), or "No Complainant" (when persons making the criminal complaint are not present when the officer arrived).

I use a call identifier to link a call to arrest and use of force reports. I consider an arrest (use of force) to have occurred at a call if any arrest (use of force) report is linked to the call. For each arrest, I use data on charges to determine whether an arrest was made for a felony, misdemeanor, or n-class[1] offense. I denote a call as resulting in a felony (misdemeanor) arrest if *any* of the arrests made at the call resulted in felony (misdemeanor) charges. The arrest data contain the first and last name of the arrestee, which allows me to link an arrest to a court case using a fuzzy match on the defendant's full name and the date of the offense.[2] I consider a defendant guilty if they are found guilty by judge or

[1]N-class offenses are generally for pre-existing warrants. These warrants may be issued by Dallas PD or an outside agency.

[2]In particular, I merge arrestees to court cases using the first two letters of the first name and the first 3 letters of the last name. I then keep matches where the first and last names have a Levenshtein distance of 2 or less and the offense date is within 10 days of the arrest date. I measure Levenshtein distances using the stringdist package in R.

jury or plead guilty, including the use of a plea bargain. At the level of the 911 call, I say that an arrest at the call resulted in a guilty finding if any of the arrestees from the call are found guilty in court.

In addition to adminstrative data sources, I augment the 911 data with block group demographics using the 2010 Census. The data are merged using the latitude/longitude coordinates that are automatically coded to each 911 call when it is made. I use these coordinates to assign a call the demographic characteristics of the block group in which the call originated. The raw 911 call sample contains 2,411,148 911 calls answered by 1,444 unique call takers.

3.1 Sample Selection

I employ a few restrictions to obtain the sample used for empirical analysis from the raw data. First, I isolate the sample to include only patrol officers and calls derived from full-time call takers.[3] These sample restrictions leave 1,907,279 911 calls, answered by 164 call takers.

I further restrict the call sample to the 7 incident classifications that contain variation in priority levels discussed in Section 2. Because there are a large number of incident types that call takers can assign to a call, it is generally difficult to pin down the choice set for a call taker when they answer any call. For example, a call taker may handle calls that end up higher in the call queue, on average, because they are more likely to classify a loud argument that sounds like somebody banging against the wall as a Major Disturbance (queue order 2) than a noise complaint (queue order 4), while another may be more likely to classify the same call as an injured person (queue order 3) than a noise complaint. Instrumenting for propensity to select each of the distinct incident classifiers would risk introducing many weak instruments (Bhuller et al., 2020). Moreover, the parameters in such a model would be difficult to interpret in an average treatment effects framework, since I do not have a measure of each call taker's next-best classification option for each call (Kirkeboen et al., 2016).

Restricting the sample to these 7 call types leaves 598,077 911 calls for the baseline analysis sample. This sample contains all 164 of the full-time call takers. In terms of sample size, the analysis sample is roughly equivalent to a full day's worth of 911 calls throughout the United States (NENA, 2023).

3.2 Summary Statistics

Table 1 presents summary statistics for each call in the analysis sample broken out by call type. These calls make up roughly 1/3 of all 911 calls. The "Other" category is the second most frequent call code within the full 911 data, accounting for 20% of all 911 calls. It is also by far the most frequent incident type in the analysis sample, accounting for over half of calls. Calls result in arrest 2.9% of the time and 38% are upgraded to priority status. Nearly half of "Other" calls are given priority status and 4.1% of these calls result in arrest - both higher than the other call types, which have priority for between 10% and 37% of calls and lead to arrest in less than 1% of instances.

Nearly half of arrests carry misdemeanor charges and 12% (not listed) are charged with felonies. Less than a fourth of arrests - 21.6% can be linked to a conviction in Dallas courts, which I use as a proxy for the quality of an arrest. Table A1 shows that nearly a third of all in-sample arrests are for prior warrants, which are unlikely to be linked to court data since the offense date and arrest date may not match and the warrant may be for an outside agency. As A1 demonstrates, a substantial proportion of the arrests are made for non-index crimes such as disorderly conduct, drug offenses, trespassing, and

[3]In practice, this means I eliminate calls that receive responses from officers who do not have patrol assignments in order to narrow the sample to only patrol officers. In order to identify full-time call takers, I use records obtained from Dallas PD that record the number of full-time call takers at the end of each year. Within each year, I rank call takers by the number of calls they answered. I use the recorded number of full time call takers as a ranking cutoff and only keep calls from call takers who are above that cutoff.

public intoxication, which make up 46% of all arrests. As such, my analysis provides insight into the mechanics that affect arrest decisions for which there may be limited public safety benefits (e.g. Cho et al., 2023).

Tables A3 and A4 provide summary statistics for other sample variables and the 164 full-time call takers. Call takers are less white than patrol officers; only 21% compared to nearly half. The average call taker answers over 3,500 in-sample 911 calls. Call takers also vary in how often they assign priority signals, with the lowest priority rate at 21% of calls and the highest at 66%. Part of this variation is explained by turnover, as not all call takers are in the sample for the full time-period, with some leaving or being newly hired. The average call taker is in the sample for 2.1 years.[4] On average, only 3% of calls are transferred in my sample, which is consistent with the relatively low transfer rate that was described by the call taker that I spoke to. There are no significant outliers, as the maximum transfer rate is only 5%. For my analysis, I consider the assigned call taker to be the one who originally answered the call.

Arrest rates vary substantially between priority and non-priority calls, as evidenced by Figure 2. Only 1.28% of non-priority calls lead to arrest compared to 5.42% of priority calls. This is true across all of the call types in the sample. This reflects differences in both the underlying features of the alleged crime and the resulting police response. The quality of arrests at priority calls is a mixed bag. On average, convictions are 9 percentage points *less* likely to happen for arrests made at priority calls. Figure A1 demonstrates that this negative gap is primarily driven by Other and Missing Persons calls, which are the only two incident types for which conviction rate drops under priority arrests.

Ultimately, the descriptive evidence suggests that the outcomes of priority calls differ from those that don't receive priority status. However, it is not possible to disentangle the causal effects of priority signals from selection of disparate criminal incidents without plausibly exogenous assignment in an incident's priority.

4 Empirical Design

My empirical strategy exploits the quasi-random assignment of call takers in order to estimate the causal effect of priority designations on enforcement decisions. I employ the following baseline linear specification:

$$Arrest_c = \beta_0 + \beta_1 Priority_c + \beta_2 \mathbf{X_c} + \epsilon_c. \tag{1}$$

Officers decide to arrest at a call c based on the priority signal, $Priority_c \in \{0, 1\}$, and observed features of the call, $\mathbf{X_c}$, which includes the police division, and proportion of the call block group that is minority (i.e. Black or Hispanic) as well as an indicator for whether the call taker is Hispanic and month by year and day of week by hour fixed effects. I also include fixed effects for each incident type in $\mathbf{X_c}$, which controls for variation in average arrest rates across crimes and controls for the differences in the queue order of priority calls that arise between call types. [5] The error term, ϵ_c, contains determinants of arrest which are observed by the responding officers but not by researcher, such as the behavior of the suspect. The priority of a call will be positively correlated with ϵ_c, since the call taker uses suspect behavior in order to make priority classifications. OLS estimates of β_1 will thus overstate the causal effect of priority on an officer's arrest decision.

[4]While I am unable to observe total employment duration for all call takers, I can, with high confidence, distinguish the call takers who are newly full-time within the sample period, which accounts for 45% of the call takers observed in the data. This is consistent with internal DPD communications that indicated net losses in call taker positions over this time period.

[5]For example, Other calls with priority are given queue order 2 versus queue order 3 for their non-priority version. Thefts with priority are given queue order 3 instead of 4.

I implement an examiner design strategy that uses call takers' innate propensities to upgrade a call, which I call the Call Taker Score, as an instrument for an individual call's priority. I calculate Call Taker Score using a standard leave-out mean procedure in order to avoid the mechanical correlation that arises from using observation c to calculate its own instrument value. For a call c, I calculate the residualized priority rate P_c^* using an OLS estimate of α in the equation below:

$$P_c^* = Priority_c - \alpha \mathbf{X_c^p} \tag{2}$$

$$= Z_{cj} + \epsilon_c \tag{3}$$

where $\mathbf{X_c^p}$ is a vector of day of week-by-hour, month-by-year, and Hispanic call taker fixed effects. I include these controls in order to separate out variation in priority setting that arises from nonrandom features of call taker assignment. Because call takers work different shifts and are active during different calendar periods in the sample, differences in priority setting may arise between call takers who work during periods with different crime rates. For example, a call taker could choose a schedule that excludes weekends or they could have chosen to retire once serious crime rates in the city escalated, each of which would reduce the call taker's observed priority rate in ways that are correlated with unobserved call characteristics. I also include an indicator for whether the call taker is Hispanic in order to control for the likelihood that a call taker needs to access the third party translation service, since this is unobserved in the data and could bias the Call Taker Score if calls involving Spanish-speaking callers have different unobservables than other calls.

After calculating P_c^*, I construct Call Taker Score, Z_{cj}, using a leave-out mean:

$$Z_{cj} = \left(\frac{1}{n_j - 1}\right)\left(\sum_{k=0}^{n_j} P_{kj}^* - P_{cj}^*\right), \tag{4}$$

where n_j is the number of calls answered by call taker j. The distribution of Call Taker Score is plotted in Figure 3. Even after residualizing for nonrandom features, there is still substantial variation in call taker behavior. Going from the least to the most alarmist call taker entails a 43 percentage point increase in the likelihood of a priority signal. Figures A2 and A3 show variation in Call Taker Score broken down by race and experience. Call takers that are more likely to upgrade calls tend to be less white and less experienced, however the experience distribution has a thick right tail of experienced call takers who upgrade calls at a higher rate than most others.

Under three assumptions, IV estimation of equation 1 using Call Taker Score as an instrument identifies a Local Average Treatment Effect (LATE). In particular, the LATE identified is a variance-weighted average of the effects of being given priority status just because the call received a call taker with a slightly higher propensity to give a priority designation (Imbens and Angrist, 1994).

The first assumption is that Call Taker Score shifts the likelihood of a priority signal. I estimate the first-stage relationship using the following specification:

$$Priority_c = \gamma_1 Z_{cj} + \gamma_2 \mathbf{X_c} + u_i. \tag{5}$$

Figure 3 plots the first stage relationship using a nonparametric local linear estimator. The OLS estimates of the first stage are presented in column 1 of Table A6. The OLS estimate of γ_1 is 0.9602. The first stage F statistic is 4,808.5, which is well past the rule of thumb threshold. This suggests that the relevance assumption holds.

The second assumption is that Call Taker Score is correlated with a call's arrest outcome only through the priority level assigned to it, conditional on the shift and call taker ethnicity controls. Because call takers are assigned to calls via an automated process and have limited opportunity for transferring calls between one another, it is unlikely that the characteristics of the call taker are systematically correlated with the unobserved details of the call. As a partial test of this exogeneity assumption, I regress Call Taker Score on call observables. In order to include officer characteristics, I use a regression of the following form:

$$CallTakerScore_{ic} = \psi' \mathbf{X_{ic}} + \epsilon_{ic}, \tag{6}$$

where $\mathbf{X_{ic}}$ adds officer characteristics such as gender, race, and years of experience to $\mathbf{X_c}$. I weight the regression by the inverse number of responding officers so that the estimates are interpreted at the call level.

I report the estimates for equation 6, along with results from a benchmark regression that uses the endogenous variable as the outcome, in Table 2. The F-statistic of joint significance of the covariates is 2.0422 for Call Taker Score compared to 311.97 for Priority. While the F-test in column 2 rejects the null hypothesis that all coefficients in the Call Taker Score specification are jointly zero, the estimated coefficients are relatively small, especially when compared to the benchmark regression as is shown in Figure A4. Only 3 of the coefficients exceed 1% of a standard deviation, with the coefficient on Criminal Mischief being equal to 6.7% of a standard deviation. Additionally, these variables explain little of the variation in the instrument; the incremental R^2 for the relevant controls is 0.0008, ten times smaller than in the benchmark regression. I interpret these findings as evidence in support of the exclusion restriction.

It is still possible for the exclusion restriction to be violated if call takers have a direct effect on arrests. Since officers do not observe the call taker who answered their call and call takers do not directly communicate with the officer, this is unlikely to be the case. However, one may be concerned that the call taker notes that justify their choice of call signal could have a direct effect on officer behavior. To address these concerns, column 2 of Table A6 provides the baseline results using the reduced form regression of Call Taker Score directly on arrests. This regression estimates the effect of receiving a call taker who is more likely to upgrade a call - including the effects they have through other channels that are not the priority signal - on arrest behavior. The estimates in this case are statistically indistinguishable from the IV estimates.

One may be worried about the possibility for exclusion restriction violations created by transfers between call takers. While the phenomenon is rare, using calls that the call taker received via transfers to estimate the Call Taker Score would mean that the sample of calls used to construct the instrument for each call taker would be selected based on unobserved call features. Because I assign each call to the call taker who initially answered the phone, this problem will not arise, since Call Taker Score is calculated only using the calls to which a call taker is assigned in a plausibly random manner. The Call Taker Score thus reflects the net effect of a call taker's preferences to upgrade calls and the propensity for their calls to be upgraded when they are transferred to someone else. In order to demonstrate the limited impact of transfers on my estimates, I provide robustness results in Section 5 that control explicitly for transfers and show that the estimates for β_1 are nearly identical to the baseline.

The third assumption is that the effect of Call Taker Score on priority setting is monotonic across calls. That is, there can be no calls for which a call taker with a low score would assign priority but a high score call taker would not. I perform a partial test of the monotonicity assumption by calculating the first stage relationship between Call Taker Score and priority within subgroups of the analysis sample. In Table A7, I show that these first stage relationships are positive and statistically significant across all subsamples of the data.

The causal effect that I estimate provides insight into the importance of information in determining police enforcement actions. Given that the sample contains an overwhelming number of non-index offenses, the causal effect answers a question with meaningful policy implications: how much do low-level arrest decisions depend on the risk-assessment capabilities of upstream actors? How important is information that is external to an officer's investigation process in their enforcement choices?

5 Results

Table 3 presents the baseline IV and OLS regression estimation results for Equation (1). The estimated coefficient on Priority using two-stage least squares with the Call Taker Score instrument is 1.08 additional arrests per 100 calls, a 37.7% increase on the mean arrest rate. I am able to rule out any positive effect smaller than 9% on the mean. Using OLS, the estimate for β_1 is 0.0333, over 3 times larger than the IV estimate. The size of the OLS estimate relative to that obtained using IV is consistent with positive correlation between the error and Priority. I interpret the fact that the IV is significantly smaller than the OLS but still provides a positive estimate as indirect support for the validity of the instrument. Despite the reduction of the effect size when using IV, the estimate of the effect of Priority is economically large. The effect implies that officers are more likely to make an arrest at a call solely due to the priority designation assigned to it. Marginal decisions by call takers have large downstream effects on enforcement outcomes.

I implement several checks on the robustness of these estimates, which are summarized in Table A13. First, I consider whether the estimates are driven by endogenous selection of call takers into the incident types used in the sample. If some call takers are more likely to select in-sample incidents types, such as if call takers who were less skilled at defining call types overwhelmingly classified calls as Other, then this would induce a violation of the exclusion restriction. Replicating the balance regression from column 2 of Table 2 using the full sample, I find evidence that call takers with a high score are less likely to select in-sample call types. In column 1 of Table A13, I add the call taker's in-sample propensity as a control variable, which I construct using the full call data as the leave-out mean in-sample incident rate, Z^s_{cj}, to the baseline regression (Herbst, 2023).

In column 2 of Table A13, I include the call taker's transfer propensity as a control, again constructed as the leave-out mean transfer rate. In column 3 of A13, I estimate the regression using officer-call level data and incorporate officer covariates, as in equation 6.[6] In column 4, I estimate a within-officer version of column 3 that controls for officer fixed effects in order to control for the effect of individual officer arrest preferences on arrest decisions (Weisburst, 2022). Column 5 includes officer fixed effects, non-fixed officer covariates, and the in-sample and transfer propensities. All specifications without officer fixed effects produce estimates which are arbitrarily close to the baseline. The estimates that include officer fixed effects are slightly smaller, at 0.009 and 0.0087, respectively, but neither can be distinguished from the baseline estimates even under a liberal 50% confidence threshold.

Subsetting the data by call type, I show in Table A21 that these results are entirely driven by the Other call category, with the remaining categories producing noisy negative point estimates and significantly weaker first stage correlations. Since Other makes up the majority of my sample and has a similar distribution of arrested offenses to the full sample (see Table A2), this does not significantly change how I interpret the estimates. Because the non-Other call types typically carry less discretion

[6] I present results from a secondary test of the robustness of my results to call transfers in Table A9. I reconstruct the instrument using an indicator for whether the call was transferred as an additional control variable in $\mathbf{X_c^P}$. The estimate of β_1 is similar to the baseline.

over whether an arrest should occur, it is sensible that the effects would be concentrated in situations where the criminality of an individual's actions is more ambiguous.

I also consider how priority signals change different types of arrest by constructing dummy variables for misdemeanor and felony arrests and using those as outcomes in my baseline specification. I report the results from these regressions in Table A11. I estimate a positive, statistically significant effect on misdemeanor arrests of over 40% on the mean and a positive but imprecise effect on felony arrests of 45% on the mean. Since felony arrests occur in only .3% of the sample, I interpret these results as suggestive evidence that priority signals increase both low-level and more serious arrests, but with effects concentrated in misdemeanors.

I also consider a secondary outcome of interest: use of force. In Table A12, I compare IV and OLS estimates of the baseline specification using an indicator for whether force occurred at the call as the outcome. While the OLS estimate is positive and economically large, the IV estimate is negative and suggests a 41% reduction on the force rate at priority calls relative to the mean. Priority may have a negative effect on use of force by preventing the escalation of incidents via quicker response times (Deangelo et al., 2023). However, the IV is imprecise and a 95% confidence interval cannot rule out positive effects on use of force of less than 172% on the mean, so I do not interpret these results as evidence of force effects in either direction.

6 Mechanisms

The results thus far demonstrate that enforcement decisions at low-level crimes are sensitive to idiosyncratic choices made by 911 operators. To what extent can this be attributed to behavioral responses from police officers? To answer this question, I differentiate between two functions that a priority signal serves in the law enforcement process. It is, first and foremost, a direct means of increasing the speed of the law enforcement response. While criminologists have argued that response speeds have limited impact on solving crimes (e.g. Sherman, 2013), recent quasi-experimental evidence suggests that quicker arrival times enhance the incapacitation effects of police (Vidal and Kirchmaier, 2018; Mastrobuoni, 2019). At the same time, priority signals also serve as information supplied to officers who enter interactions that are laden with uncertainty. Thus, I consider the estimated effects to be a composite of response time effects and information effects.

In cases where an officer's job is to assess whether a crime has taken place, such as when an intoxicated person is causing a public disruption, the information effect of a priority signal shapes their judgment. I consider the information channel as the main mechanism that generates a behavioral response. On the other hand, the response time effect is produced by changes in officer capabilities to make arrests rather than their assessments of suspect guilt.

6.1 Information Effects

To test for evidence of information effects, I first examine the effect of priority on the quality of arrests, as proxied by court convictions. I do this by restricting the sample to arrests which were given either felony or misdemeanor charges and using the baseline empirical specification to analyze the impact of priority on whether the arrestee was found guilty in court. I present the results from this regression, separately for felony and misdemeanor arrests, in Table A14. The smaller sample size leads to a significantly weaker instrument (Kleibergen-Paap F stats of 118 and 57 for misdemeanor and felony arrests, respectively), though the first stage correlation between Call Taker Score and Priority is similar (see A15). For both felony and misdemeanor arrests, I find negative point estimates which suggest that arrests made at priority calls are less likely to result in convictions, however the estimate is only statistically significant for misdemeanor arrests. The estimated effect on convictions for misdemeanors is -14.9 percentage points, a reduction of nearly 50% on the mean conviction rate.

I interpret these results as evidence that behavioral responses can in part explain my baseline estimates. The information effect of call priority changes the likelihood that an officer places on a civilian having committed a crime. In a Bayesian framework, one can think of priority as a signal that increases officers' posterior probability of guilt. For a marginally upgraded call, this means that officers are more likely to view the suspect as guilty despite them exhibiting similar behavior to a suspect whose call was not upgraded because they received a different call taker, which would reduce the quality of these arrests.

The sensitivity of officer assessments to priority signals depends on the strength of their priors over suspect guilt. In situations where we expect officers to have stronger priors, priority would have less of an information effect on their arrest decisions. I consider two prominent cases in which we might expect there to be variation in officer priors: officers dealing with suspects who are the same race as themselves and officers gaining experience. In the former situation, we expect officers to have tighter priors when dealing with involved parties who are the same race as the officer, since they share a socio-cultural background that allows officers to more easily interpret whether suspect behaviors are aggressive or better communicate with witnesses. When officers gain tenure in the force, they have more experience to draw from to form their prior, thus limiting outsize influence of any single piece of information.

I first consider calls where officers are dispatched to neighborhoods that are the same race as themselves, using data at the officer-call level as in equation 6. In the spirit of Hoekstra and Sloan (2021), I estimate the following difference-in-difference style regression:

$$\begin{aligned} Arrest_{ic} = \alpha_0 &+ \alpha_1(ProportionSameRace_{ic}) + \alpha_2 Priority_c \\ &+\alpha_3(Priority * ProportionSameRace)_{ic} \\ &+Officer_i + \alpha_4 X_c + u_{ic}, \end{aligned} \tag{7}$$

where $ProportionSameRace_{ic}$ is the proportion of civilians who are the same race as officer i in the Census Block Group of the call. By including officer fixed effects, $Officer_i$, I net out the average arrest rate for each officer in the data and exploit within-officer variation in the racial composition of the neighborhoods to which they are dispatched. I instrument for $Priority_c$ and $(Priority * ProportionSameRace)_{ic}$ using $CallTakerScore_c$ and $(CallTakerScore * ProportionSameRace)_{ic}$, respectively. The parameter of interest is α_3, which captures the effect of an officer seeing a priority indicator in a same-race neighborhood on their decision to make an arrest. Intuitively, α_3 measures how the difference between priority and non-priority arrest rate changes as a neighborhood begins to look more like an officer.

Figure 4 captures this intuition by plotting officer arrest rates - demeaned by the officer-level average - against $ProportionSameRace$ separately for the top and bottom quartile of Call Taker Score. For neighborhoods that contain the smallest proportion of residents that are the same race as the officer, the gap between arrest rates for likely priority and likely non-priority calls is the largest. When the officer is dispatched to a neighborhood that has more residents who share the officer's race, this gap shrinks.

In column 1 of Table 4, I present results for IV estimation of equation 7. I estimate an α_3 of -0.0149 which is statistically significant at the 5% level. The estimate implies that a 10 percentage point increase in the proportion of a neighborhood that is the same race as an officer reduces the impact of a priority signal on arrests by .0015 percentage points. Given the estimated effect of priority in a neighborhood that has no citizens that are the same race as the officer, provided by the estimate of α_2, my estimate suggests that priority information has no positive effect on an officer's arrest decision when dispatched to a neighborhood that consists of at least 95% citizens who share the officer's race.

I further examine which officers are driving this phenomenon using a triple-differences specification. For an officer of generic "Race", these regressions take the following form:

$$\begin{aligned} Arrest_{ic} = {} & a_0 + a_1(Proportion\text{“}Race\text{”}_{ic}) + a_2Priority_c + Officer_i \\ & + a_3OtherInteractions_{ic} + a_4(Priority * \text{“}Race\text{”}Officer * Proportion\text{“}Race\text{”})_{ic} \\ & + a_5X_c + e_{ic}, \end{aligned} \tag{8}$$

where $OtherInteractions_{ic}$ includes the lower order interaction terms $Priority_c * \text{“}Race\text{”}Officer_i$, $Priority_c * Proportion\text{“}Race\text{”}_c$, $\text{“}Race\text{”}Officer_i * Proportion\text{“}Race\text{”}_c$. The coefficient a_4 describes how the priority arrest gap changes as a neighborhood becomes more "Race" for "Race" officers relative to non-"Race" officers. I consider how this effect differs for white, Black, and Hispanic officers (i.e. "Race" = white/Black/Hispanic). Appendix Figure A5 provides descriptive evidence of the heterogeneity in officer responses to priority information in same race neighborhoods by officer race. The figures suggests that white and Black officers both become relatively less responsive to priority signals when in same-race neighborhoods, whereas there appears to be very little change in the behavior of Hispanic officers.

The estimates of the triple-differences specification in columns 2-4 of Table 4 confirm these observations. In column 2, the point estimate of -0.0400 implies that sending a white officer to a neighborhood that is 10 percentage points more white results in a 0.4 percentage point reduction in the priority arrest gap relative to non-white officers. This represents a 13% reduction on the mean arrest rate of 0.02861. For Black officers, the estimated effect is even larger at -0.0512. As such, increasing the black share of a neighborhood by 10 percentage points is associated with a 0.5 percentage point reduction in the priority arrest gap for Black relative to non-Black officers, a 17% reduction relative to the mean. These estimates are significant at the 5% and 10% level, respectively. For Hispanic officers, the estimated coefficient for the triple interaction term is 0.0162, implying that Hispanic officers increase their arrests *more* in response to priority signals when dispatched to Hispanic areas, however the estimate is imprecise and I am unable to rule out negative effects of less than 5% on the mean for a 10 percentage point increase in the Hispanic share of a neighborhood. These results provide evidence that priority signals are less useful information for officers when working in neighborhoods in which we would expect them to have greater cultural familiarity.

Next, I consider the effect of priority by officer experience. I do so by replacing $ProportionSameRace_{ic}$ in equation 7 with years of experience. In Table 5, I present results from this estimation. The estimate for the interaction term coefficient is -0.0007 and is statistically significant at the 5% level. Thus, each year of experience reduces the effect of a priority signal by .07 percentage points. Given the estimated average effect of priority on arrests of .0154 percentage points in this specification, these estimates imply that an officer with 22 years of tenure is not swayed by priority signals. This is despite the fact that experience seems to have no independent effect on arrests in this setting, which is highly selected compared to other settings where authors have estimated the effect of experience on enforcement behavior (e.g. Ba et al., 2023).

Taken together, these results provide evidence of the information effect of priority as well as evidence of the importance of information in police arrest decisions. I show that officer enforcement is sensitive to their prior information. When officers have weaker priors, information external to their own assessment of the civilian's behavior can play an outsize role in determining whether they use their arrest powers.

6.2 Response Time Effects

Marginally upgraded calls receive much faster police responses. In Table 6, I report results from IV regressions that substitute arrest in the baseline specification for response time, time to dispatch, and time taken to drive to the incident.[7] Calls with priority signals receive officers 37 minutes quicker than similar calls without these signals. This represents a 66% reduction on the average response time of 56 minutes. This happens entirely because dispatchers take 37 minutes less to select officers for a call; there is no effect on the amount of time it takes officers to travel to the call.

To the extent that response times increase the likelihood that police are able to apprehend a suspect, the significant reductions that result from a call being upgraded to priority status likely increase arrests. Even though the calls in my sample are predominantly low-level incidents where arrests may not be necessary, long waits may lead to natural dissipation of the incident that leads suspects or witnesses to leave the scene. Relatedly, Vidal and Kirchmaier (2018) provide evidence on a few of the mechanisms through which quicker response times increase the likelihood that police are able to solve a crime. These include increasing the chances that a cooperative witness is present to name a suspect and enhancing the possibility that police are able to arrest the suspect at the scene before they have fled. I examine each of these mechanisms in order to assess the importance of the response time effect.

First, I estimate the baseline specification using indicators for call dispositions of "No Complainant" and "No Police Action" as outcome variables in the baseline IV specification. The "No Complainant" disposition serves as a proxy for the presence of witnesses. Officers would only use this disposition in the case that they arrive to a call and they cannot locate the person who wants to make a criminal complaint. The "No Police Action" disposition is more broad and applies to cases when the officer identifies the complainant but determines that police action is not required in the situation. This may happen because the officer determines that the complainant's issue is not a crime, but also because the suspect has left the scene and the complainant no longer wishes to make a crime report.

Table A20 reports the result of these regressions. There is insufficient evidence to suggest that marginal priority calls are less likely to be marked as having no complainant present, as the coefficient on priority is imprecisely estimated as a 4% reduction on the mean. However, I estimate a statistically significant reduction of 9.6 percentage points (24% on the mean) in the likelihood that the responding officers report that the call did not require police action. I cannot determine how much this can be attributed to suspects being at the scene when officers arrive versus officers' assessments being different, so this test provides inconclusive results.

As a secondary test of the role of response times, I use the timestamp within the arrest report to estimate the effect of priority on the speed of arrest. Specifically, I use arrests made within 15 minutes as a proxy for arrests made "on-sight," as in Vidal and Kirchmaier (2018), since these arrests are likely to be driven by presence of the suspect at the scene. In Table A17, I report regression results where I use an indicator for whether arrests occurred within a certain amount of time after officer arrival as the outcome. I find that there is a relatively large increase in arrests within 15 minutes of arrival. I estimate that priority signals increase the likelihood of these immediate arrests by .4 percentage points, a 36% increase on the average. While I estimate imprecise positive effects on arrests within 15 to 30 minutes and 30 to 45 minutes, I estimate a precise increase in arrests made 45 to 60 minutes after arrival by .2 percentage points. This magnitude is especially stark given that the probability that arrests are made in this time frame is 10 times smaller than the probability that arrests are made within 15 minutes. I

[7] Due to inconsistencies in the reporting of officer arrival times, I perform these regressions on a truncated sample which includes only calls for which arrival times are recorded. In Table A18, I demonstrate that this sample produces equivalent results for the baseline regression specification. The likelihood of arrest in the truncated sample is .02537, similar to that in the analysis sample.

interpret these results as suggestive evidence that priority signals both improve officers' ability to catch some suspects before they flee and also sway officers' decisions at calls in which making an arrest is a more time-intensive process.

7 Conclusion

This paper provides novel quasi-experimental evidence on the role of call takers in shaping police discretion and the importance of information processing for police decision making. I show that 911 information external to the police officer's experience at an incident shapes how police determine the appropriate law enforcement action. Despite the importance of this information, it is not provided uniformly or objectively; it can differ substantially depending on the risk preferences of the call taker who supplies it. Given the potential economic and social harms of arrests - even those for which charges are eventually dropped - and the lack of conclusive evidence supporting low-level arrests as an effective public safety tool, my findings highlight an inefficiency in the provision of public safety.

One simple and potentially cost-effective tool for reducing the negative effects of call taker discretion is to provide police officers with the identity of the call taker who made the assignment choices. In many large police agencies, call takers and dispatchers are separate roles (Transform911, 2022). While I am unable to say whether other agencies provide call taker identities to officers via CAD, it seems unlikely given that most direct communication with the call center happens with dispatchers. Providing officers with call taker identities would provide them information that they could use to adjust for the quality of call taker classifications once they are exposed to enough calls from each handler.

My findings also highlight the importance of police information processing capabilities in determining their choices. I show that police are less likely to rely on outside information when responding to calls in which they have stronger priors over suspect behavior. These results shed additional light on the mechanisms through which minority officers differ in their treatment of minority civilians. Since officers can better distinguish useful information in situations where they have a similar socio-cultural background to the people they encounter, they are less likely to rely on easily available information which may be generated in inaccurate or biased ways. This also highlights the importance of training programs that can improve police officers' abilities to process information without cognitive biases.

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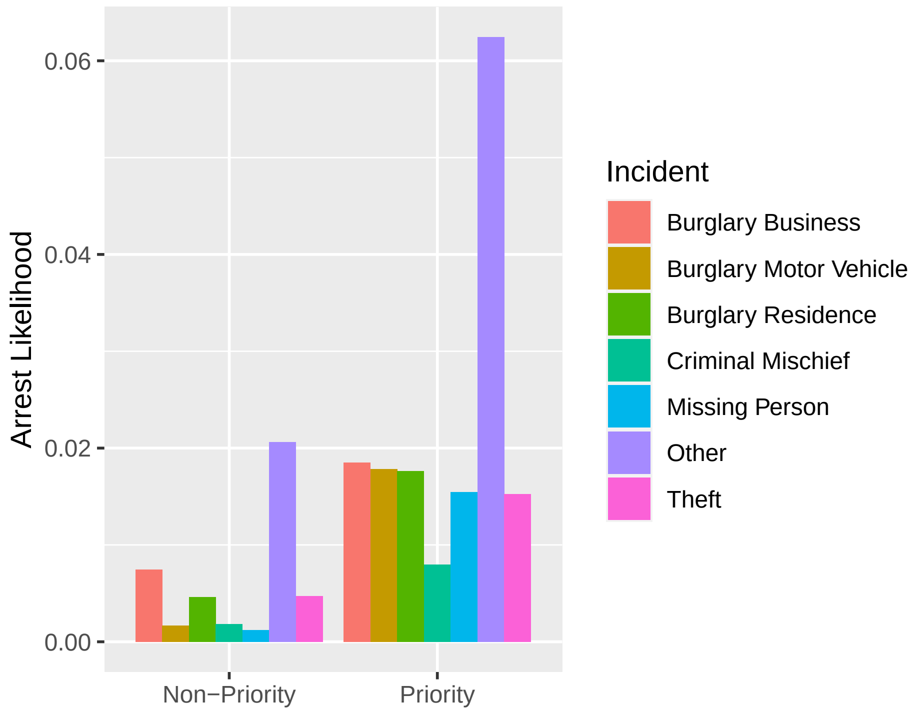
8 Tables and Figures

Figure 1: Example Call Data

Incident …	Division	Nature of Call	Priority	Date_…	Date	T… ↑	Unit Num	Block	Location	Beat
24-0807559	North Central	11R - Burg of Res	3		05/07/2024	16:08:20	EXP04	2500	Players Ct	611
24-0803076	South Central	16 - Injured Person	3		05/07/2024	16:19:23	F745	2900	MORGAN DR	755
24-0808706	North Central	6X - Major Dist (Violence)	2		05/07/2024	16:21:53	C692	14200	Dallas Pkwy	631
24-0808487	North Central	40 - Other	3		05/07/2024	16:22:02	C693	5400	Preston Oaks Rd	631
24-0808659	Central	DAEF-Dist Armed Encounter	1		05/07/2024	16:22:46	L342	900	Pacific Ave	133
24-0808643	Northeast	40/01 - Other	2		05/07/2024	16:26:51	C232		N Central Serv Nb / Wa	211
24-0807597	Southwest	40/01 - Other	2		05/07/2024	16:28:11	C437	10200	Ironwood Ln	436
24-0808693	Southeast	7X - Major Accident	2		05/07/2024	16:28:47	C326	2200	S BUCKNER BLVD	324
24-0808666	South Central	6X - Major Dist (Violence)	2		05/07/2024	16:29:49	CIT7	1400	Five Mile Dr	727
24-0808705	Southeast	7X - Major Accident	2		05/07/2024	16:31:39	C314		DOLPHIN RD / CULVER	314

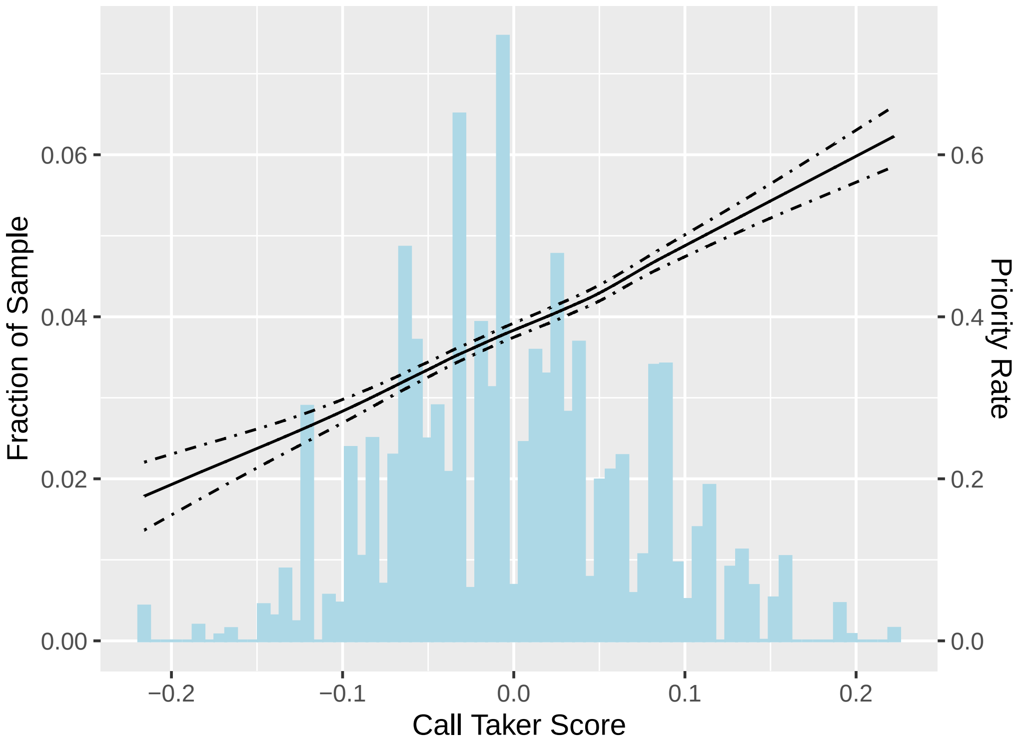
Notes: This figure depicts CAD data as it would be seen by dispatchers and officers. The data were pulled from the Dallas Police Department's "Active Incidents" on May 7, 2024.

Figure 2: Arrest Rate by Priority



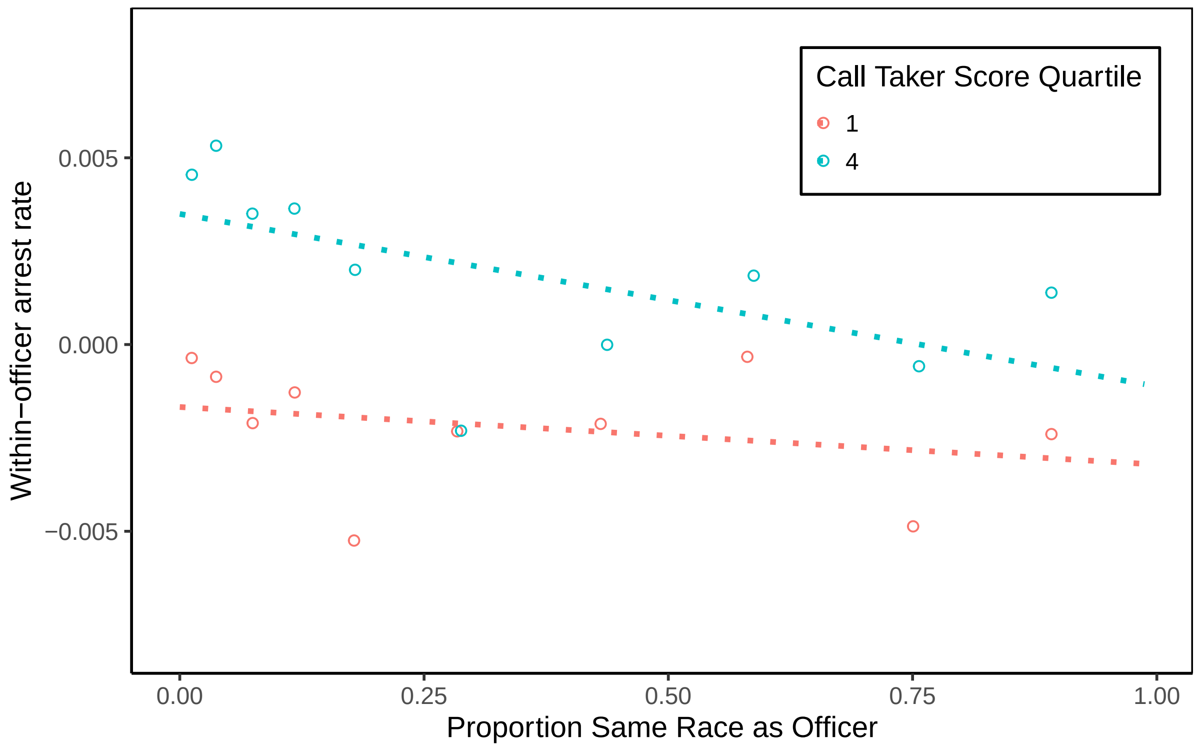
Notes: This figure depicts arrest rate for the 7 incident types included in the analysis sample, separated by Priority classification.

Figure 3: Call Taker Score Variation



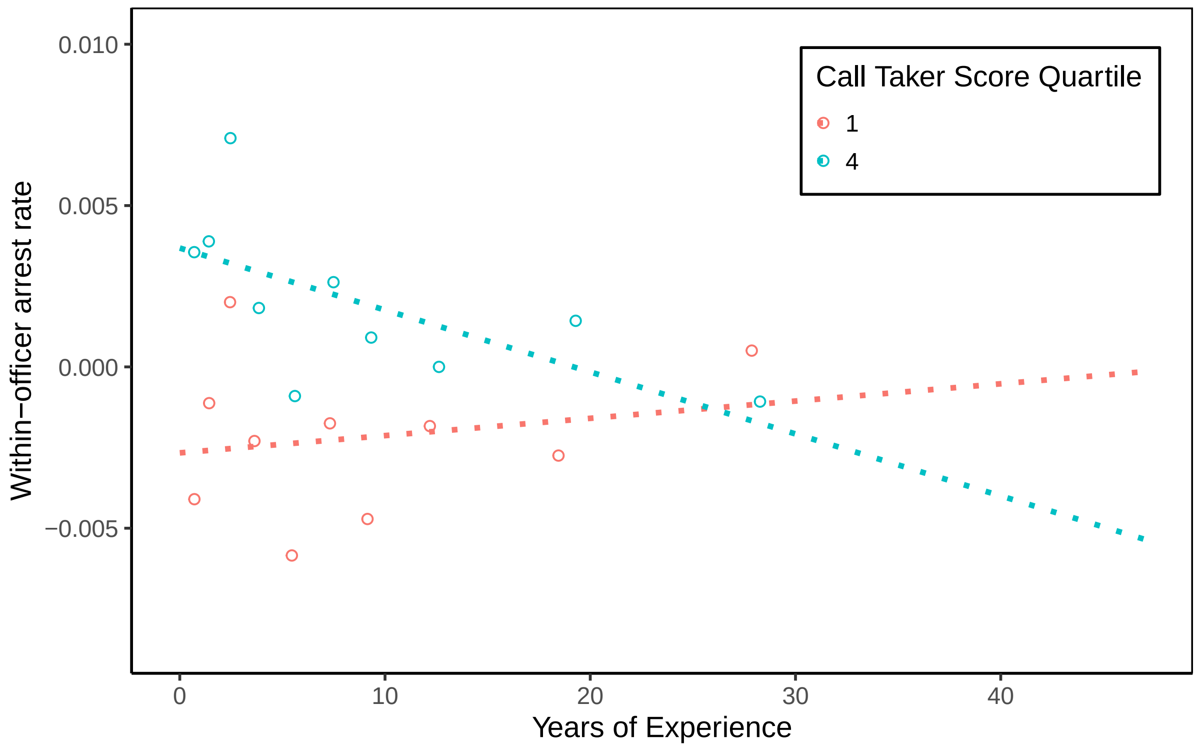
Notes: This figure reports first-stage effects and the distribution of Call Taker Score, which is the leave-out mean Priority rate calculated using data from other calls answered by the call taker following the procedure described in Section 4. The solid black line, plotted against the right axis, indicates predicted means from a local linear regression of Priority signal on Call Taker Score. The histogram, plotted against the left axis, provides the distribution of Call Taker Score measures across all 911 calls in the analysis sample.

Figure 4: Arrest Rates by Neighborhood Racial Composition



Notes: The y-axis measures the likelihood of an arrest relative to an officer's average arrest rate in the sample. The x-axis is the proportion of the Census Block Group that is the same race as the officer. Observations are grouped so that each point includes an equal number of calls. The fitted lines are linear fits across each of the plotted call taker score quartiles.

Figure 5: Arrest Rates by Experience



Notes: The y-axis measures the likelihood of an arrest relative to an officer's average arrest rate in the sample. The x-axis is the officer's years of experience in the Dallas Police Department. Observations are grouped so that each point includes an equal number of calls. The fitted lines are linear fits across each of the plotted call taker score quartiles.

Table 1: Call Type Summary Statistics

Incident Type	Total Calls	Prop. of All Calls	Priority Rate	Pr(Arrest)	Misdemeanor Charges|Arrest	Conviction Rate
Burglary Business	12570	0.007	0.107	0.009	0.193	0.496
Burglary Motor Vehicle	56403	0.030	0.105	0.003	0.544	0.526
Burglary Residence	38412	0.020	0.170	0.007	0.271	0.456
Criminal Mischief	35143	0.018	0.185	0.003	0.390	0.264
Missing Person	21247	0.011	0.368	0.006	0.044	0.043
Other	392006	0.206	0.482	0.041	0.476	0.205
Theft	42296	0.022	0.276	0.008	0.413	0.371
Analysis Sample	598077	0.314	0.383	0.029	0.467	0.216

Notes: This table presents summary statistics for the analysis sample, separated by call type. The column Prop. of All Calls measures the proportion of all 911 calls dispatched in Dallas that comprise of the listed call type.

Table 2: Covariate Balance

Dependent Variables:	Priority	Call Taker Score
Model:	(1)	(2)
Variables		
Years of Experience	-0.0006***	0.0000*
	(0.0001)	(0.0000)
Burglary Motor Vehicle	-0.0092	-0.0002
	(0.0060)	(0.0011)
Burglary Residence	0.0567***	0.0005
	(0.0064)	(0.0011)
Criminal Mischief	0.0722***	-0.0055***
	(0.0150)	(0.0017)
Missing Person	0.2417***	0.0005
	(0.0165)	(0.0018)
Other	0.3561***	0.0022
	(0.0097)	(0.0015)
Theft	0.1617***	-0.0003
	(0.0098)	(0.0016)
Male Officer	0.0000	0.0001
	(0.0012)	(0.0002)
Black Officer	-0.0457***	-0.0008**
	(0.0029)	(0.0004)
Hispanic Officer	-0.0086***	-0.0005
	(0.0027)	(0.0005)
White Officer	0.0059**	0.0001
	(0.0025)	(0.0004)
Division - North Central	-0.0128***	0.0010*
	(0.0033)	(0.0006)
Division - Northeast	0.0013	0.0004
	(0.0028)	(0.0004)
Division - Northwest	-0.0060**	0.0000
	(0.0028)	(0.0005)
Division - South Central	0.0530***	0.0008
	(0.0034)	(0.0006)
Division - Southeast	0.0301***	0.0011**
	(0.0031)	(0.0005)
Division - Southwest	0.0321***	0.0012**
	(0.0029)	(0.0005)
Minority Percentage	-0.0739***	-0.0009
	(0.0038)	(0.0010)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Fit statistics		
Observations	1,170,045	1,170,045
Incremental R^2	0.08511	0.00082
F Stat	311.97	2.0422

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table reports results from a test for covariate balance. The sample used is at the call by officer level. Regressions are weighted by the inverse of the number of responding officers. Column 1 uses the endogeneous Priority variable as the outcome and Column 2 uses the Call Taker Score as the outcome. Incremental R^2 reports the R^2 added to the regression for just the variables with reported estimates.

Table 3: Baseline Arrest Results

Dependent Variable:	Arrest	
Model:	IV	OLS
Variables		
Priority	0.0108**	0.0333***
	(0.0042)	(0.0009)
Minority Percentage	-0.0003	0.0013
	(0.0010)	(0.0009)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	598,077	598,077
R^2	0.01882	0.02269
Dependent variable mean	0.02861	0.02861
Kleibergen-Paap F-Stat, Priority	4,808.5	

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table contains results for the main arrest specification at the call level. In Column 1, Call Taker Score, constructed as described in Section 4, is used as an instrument for Priority.

Table 4: IV Results by Race

Dependent Variable:	Arrest			
Model:	Diff-in-Diff	Triple-Diff	Triple-Diff	Triple-Diff
"Race" =		White	Black	Hispanic
	(1)	(2)	(3)	(4)
Variables				
Priority	0.0140***	0.0055	0.0056	0.0081
	(0.0048)	(0.0061)	(0.0059)	(0.0059)
Proportion Same Race	0.0051*			
	(0.0029)			
Priority * Proportion Same Race	-0.0149**			
	(0.0073)			
Priority * Proportion "Race" * "Race" Officer		-0.0400***	-0.0512***	0.0162
		(0.0153)	(0.0165)	(0.0155)
Priority * Proportion"Race"		0.0201	0.0117	0.0003
		(0.0138)	(0.0142)	(0.0108)
Priority * "Race" Officer		0.0079	0.0193**	-0.0035
		(0.0057)	(0.0078)	(0.0077)
Proportion "Race" * "Race" Officer		0.0138**	0.0169**	-0.0056
		(0.0061)	(0.0065)	(0.0059)
Proportion "Race"		-0.0059	-0.0054	0.0001
		(0.0051)	(0.0059)	(0.0042)
Fixed-effects				
Hispanic Call-Taker	Yes	Yes	Yes	Yes
Incident	Yes	Yes	Yes	Yes
Division	Yes	Yes	Yes	Yes
Day of Week-by-Hour	Yes	Yes	Yes	Yes
Month-by-Year	Yes	Yes	Yes	Yes
Officer	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,170,045	1,170,045	1,170,045	1,170,045
R^2	0.02671	0.02662	0.02654	0.02687
Dependent variable mean	0.02861	0.02861	0.02861	0.02861

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: The table reports results for regressions using the specifications in equations 7 and 8. Column 1 reports the difference-in-difference specification from equation 7. Proportion Same Race is given by the proportion of civilians who are the same race as the focal officer in the block group in which the call was made. In columns 2-4, I report results from estimations of equation 8. The race of the officer and block group measured by "Race" is denoted in the row immediately above the column numberings. All interaction terms that use Priority are instrumented using the interaction of Call Taker Score and the other terms in the interaction.

Table 5: IV Results by Experience

Dependent Variable:	Arrest
Model:	(1)
Variables	
Priority	0.0154***
	(0.0055)
Years of Experience	0.0002
	(0.0005)
Priority * Years of Experience	-0.0007**
	(0.0003)
Minority Percentage	-0.0008
	(0.0010)
Fixed-effects	
Hispanic Call-Taker	Yes
Incident	Yes
Division	Yes
Day of Week-by-Hour	Yes
Month-by-Year	Yes
Officer	Yes
Fit statistics	
Observations	1,170,045
R^2	0.02702
Dependent variable mean	0.02861

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table reports results for a specification similar to equation 7 that uses Years of Experience as the interaction term instead of Proportion Same Race. Priority * Years of Experience is instrumented using Call Taker Score * Priority.

Table 6: Response Time

Dependent Variables:	Response Time	Dispatch Time	Driving Time
Model:	(1)	(2)	(3)
Variables			
Priority	-37.14***	-37.20***	0.0607
	(2.398)	(2.382)	(0.1936)
Minority Percentage	4.451***	5.589***	-1.138***
	(0.5197)	(0.5246)	(0.0633)
Fixed-effects			
Hispanic Call-Taker	Yes	Yes	Yes
Incident	Yes	Yes	Yes
Day of Week-by-Hour	Yes	Yes	Yes
Month-by-Year	Yes	Yes	Yes
Division	Yes	Yes	Yes
Fit statistics			
Observations	522,588	522,588	522,588
R^2	0.21339	0.21285	0.03025
Dependent variable mean	56.221	46.808	9.4131
Kleibergen-Paap F-Stat, Priority	4,502.7	4,502.7	4,502.7

Clustered (Call Taker) standard-errors in parentheses

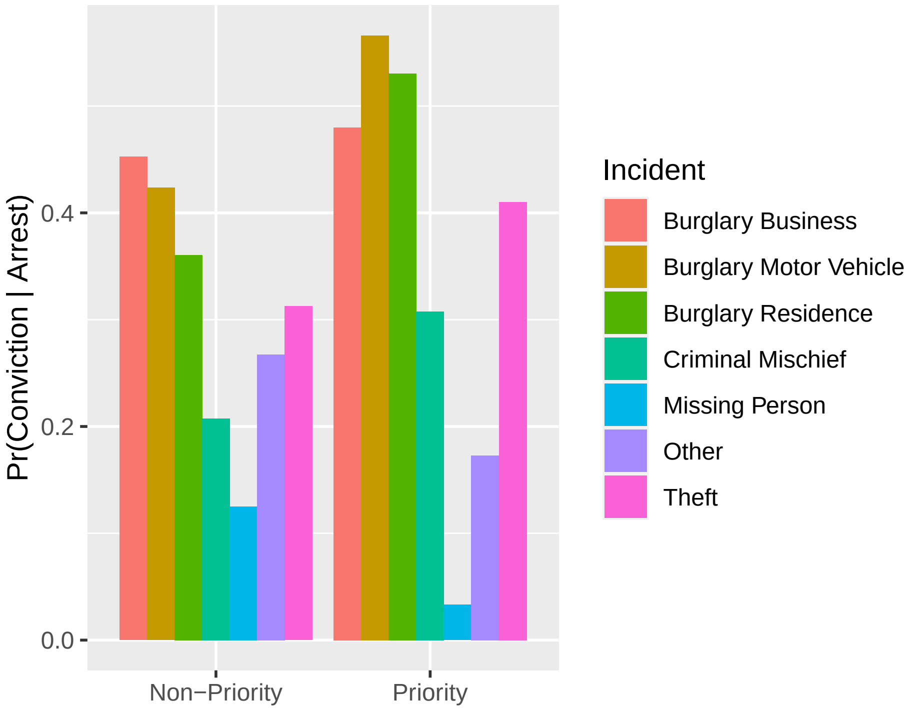
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table contains results for regressions of the same form as in Table 3, but replacing the dependent variable of arrest with Response Time in column 1, Dispatch Time in column 2, and Drive Time in column 3. The coefficients on Priority are estimated using the Call Taker Score IV. All times are measured in minutes. Response Time measures the difference between the time of the call and the time of the arrival of the first officer. Dispatch Time measures the difference between the time of the call and the time of the first officer being assigned. Drive Time measures the difference between the time of the first officer being assigned and the time they arrive at the call.

A Appendix

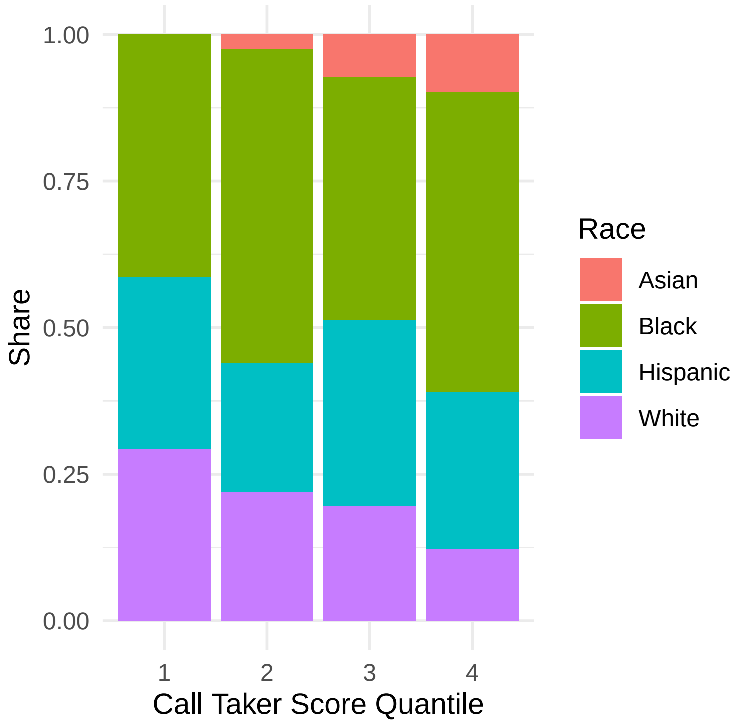
A.1 Supplemntary Tables and Figures

Figure A1: Arrest Conviction Rates by Priority



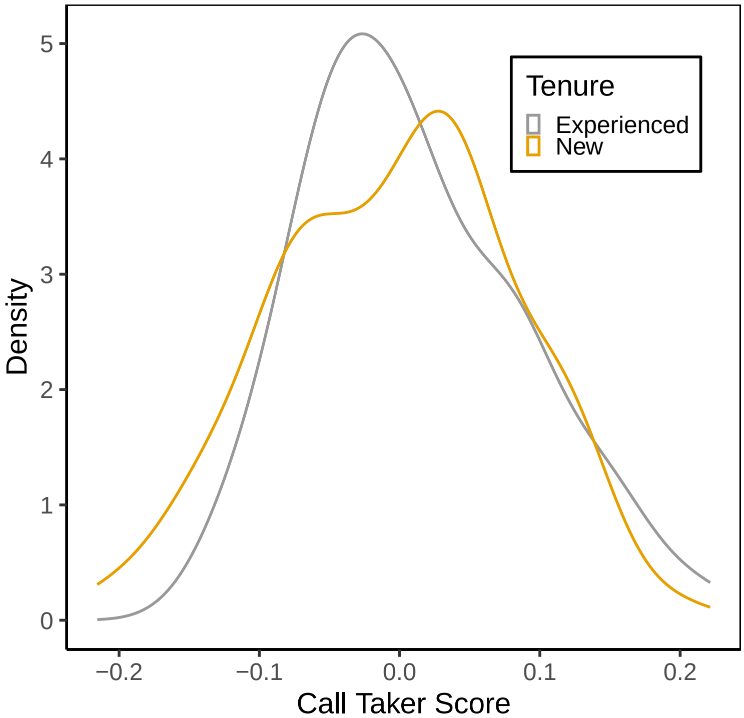
Notes: This conviction rate for arrests made at calls that are classified under each of the 7 incident types included in the analysis sample, separated by Priority classification of the call.

Figure A2: Call Taker Demographics



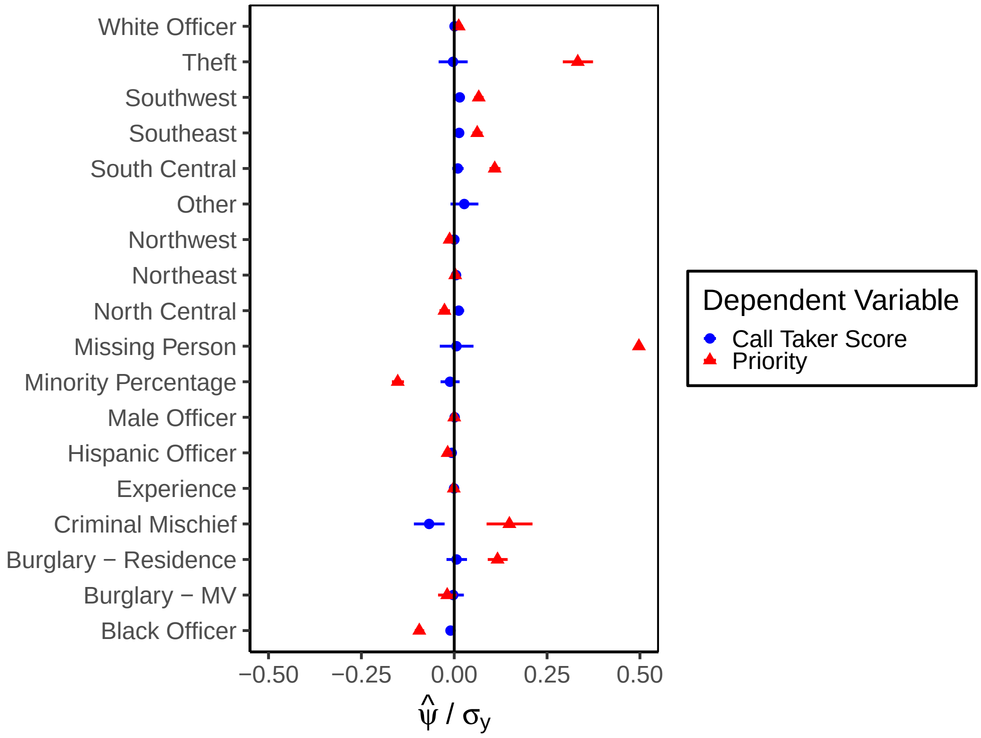
Notes: This figure depicts the racial/ethnicity demographics of call takers by quintile Call Taker Score. Each colored section within a bar represents the percent of call takers within that quintile of Call Taker Score who are of the race/ethnicity identified by the color of the section. Race/ethnicites are identified using the Rethnicity package in R, which uses the full name of the call taker to predict their race/ethnicity, using a machine learning algorithm that is trained with Florida voter registration data.

Figure A3: Call Taker Score by Experience



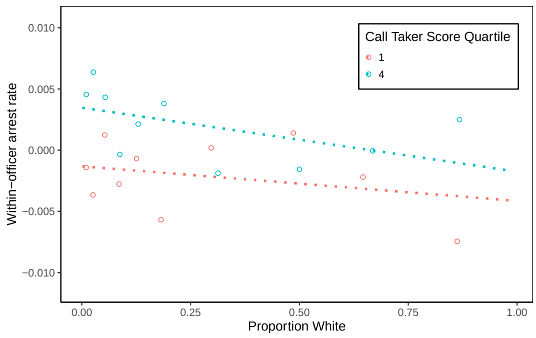
Notes: A call-taker is classified as Experienced if their first in-sample call is observed in the first month of the sample and new otherwise; 45% of call takers are considered Experienced under this classification. The average Call Taker Score is .0104 for Experienced and -.0033 for New. The median is -.0046 for Experienced and .00219 for New.

Figure A4: Balance Table Coefficients

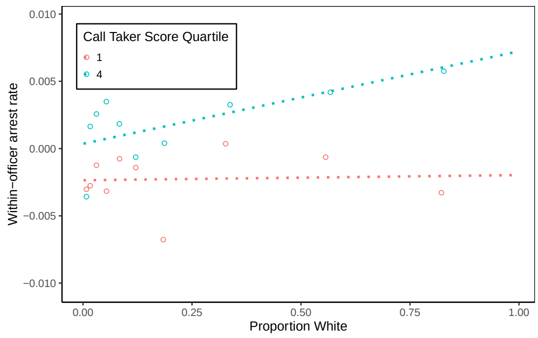


Notes: This figure depicts coefficient estimates and confidence intervals for equation (6), relative to the standard deviation of the dependent variable. The blue points plot estimates using Call Taker Score as the dependent variable. The red triangles plot estimates using Priority as the dependent variable.

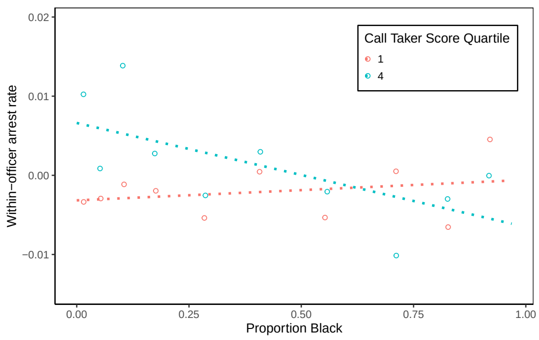
Figure A5: Triple Differences Intuition



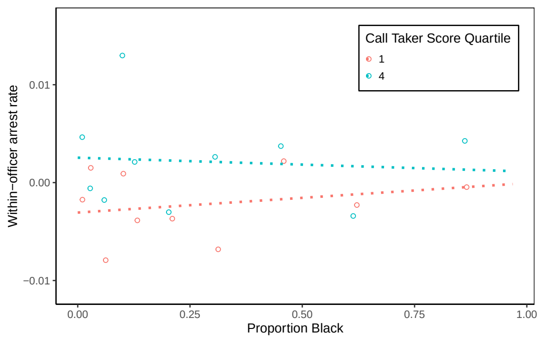
(a) White Officers



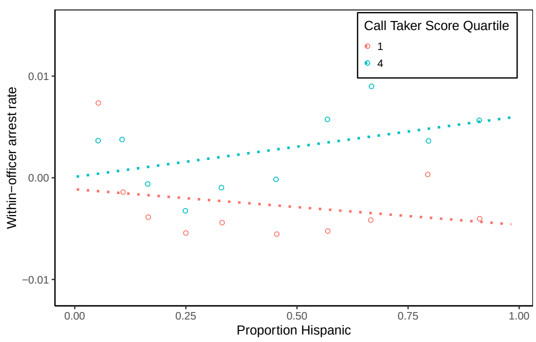
(b) Non-White Officers



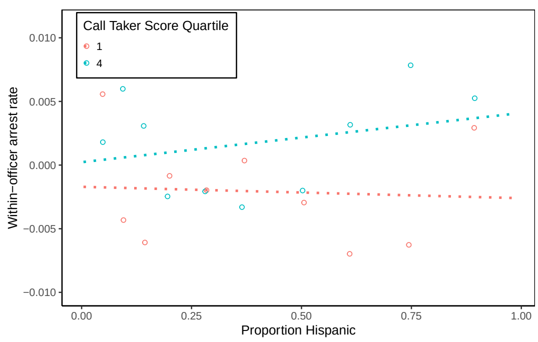
(c) Black Officers



(d) Non-Black Officers



(e) Hispanic Officers



(f) Non-Hispanic Officers

Notes: In every panel, the y-axis measures the likelihood of an arrest relative to an officer's average arrest rate in the sample. In panels a and b, the x-axis is the proportion of the call Census Block Group that is non-Hispanic white. In panels c and d, the x-axis is the proportion of the call Census Block Group that is non-Hispanic Black. In panels e and f, the x-axis is the proportion of the call Census Block Group that is Hispanic. The panel titles reflect the subset of officers for which the data is used to build the figures. Observations are grouped so that each point includes an equal number of calls. The fitted lines are linear fits across each of the plotted call taker score quartiles.

Table A1: Top In-Sample Arrest Offenses

	Offense	Proportion of All Arrests	Percent Priority	Percent Non-Priority
1	WARRANT	0.30	0.62	0.38
2	DISORDERLY CONDUCT	0.21	0.83	0.17
3	NARCOTICS & DRUGS	0.11	0.67	0.33
4	TRESPASS	0.07	0.25	0.75
5	PUBLIC INTOXCIATION	0.07	0.85	0.15
6	ASSAULT	0.04	0.82	0.18
7	DWI	0.03	0.96	0.04
8	OTHER-MISDEMEANOR	0.02	0.55	0.45
9	THEFT-OTHER	0.02	0.64	0.36
10	FAIL TO ID	0.02	0.60	0.40
11	FORGE/COUNTERFEIT	0.01	0.92	0.08
12	WEAPONS	0.01	0.75	0.25
13	FRAUD	0.01	0.79	0.21
14	RESIST ARREST	0.01	0.65	0.35
15	BURGLARY	0.01	0.62	0.38
16	BURGLARY-VEHICLE	0.01	0.78	0.22
17	EVADING	0.01	0.55	0.45
18	TRAFFIC	0.01	0.81	0.19
19	THEFT-RETAIL	0.01	0.65	0.35
20	AGG ASSAULT	0.01	0.86	0.14

Notes: This table lists the 20 most frequent arrest charges among the 7 types of incidents used for the analysis sample, as described in Section 3.1, in order of their frequency. The 2nd named column reports the proportion of all arrests which are accounted for by that charge. The 3rd and 4th named columns report the proportion of those arrests that are Priority and Non-Priority, respectively.

Table A2: Top Arrest Offenses for 'Other'-Coded Incidents

	Offense	Proportion of 'Other' Arrests	Percent Priority	Percent Non-Priority
1	WARRANT	0.28	0.65	0.35
2	DISORDERLY CONDUCT	0.22	0.84	0.16
3	NARCOTICS & DRUGS	0.11	0.68	0.32
4	TRESPASS	0.08	0.24	0.76
5	PUBLIC INTOXCIATION	0.07	0.85	0.15
6	ASSAULT	0.04	0.83	0.17
7	DWI	0.03	0.97	0.03
8	OTHER-MISDEMEANOR	0.02	0.55	0.45
9	FAIL TO ID	0.02	0.59	0.41
10	FORGE/COUNTERFEIT	0.02	0.93	0.07
11	WEAPONS	0.01	0.79	0.21
12	FRAUD	0.01	0.83	0.17
13	RESIST ARREST	0.01	0.68	0.32
14	THEFT-OTHER	0.01	0.76	0.24
15	AGG ASSAULT	0.01	0.88	0.12
16	EVADING	0.01	0.56	0.44
17	TRAFFIC	0.01	0.82	0.18
18	OTHER	0.01	0.81	0.19
19	UUMV	0.00	0.77	0.23
20	THEFT-RETAIL	0.00	0.67	0.33

Notes: This table replicates Table A1 using only the subsample of calls classified as Other.

Table A3: Call Taker Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Pctl. 25	Pctl. 75	Max
Calls Answered	164	3647	3265	456	1227	4883	14489
Priority Rate	164	0.4	0.095	0.21	0.33	0.47	0.66
Pr(Arrest)	164	0.031	0.0089	0.015	0.025	0.035	0.056
Conviction Rate	164	0.2	0.055	0.061	0.17	0.23	0.38
Days in Sample	164	769	524	152	361	1098	1666
Transfer Rate	164	0.035	0.0057	0.016	0.032	0.039	0.051
Race	164						
... Asian	8	5%					
... Black	77	47%					
... Hispanic	45	27%					
... White	34	21%					

Notes: This table presents summary statistics at the level of the call taker.

Table A4: Other Call Summary Statistics

Variable	Mean	St. Dev.	Min	Max
Proportion White	0.258	0.278	0.001	0.987
Proportion Black	0.299	0.284	0.000	0.969
Proportion Hispanic	0.401	0.280	0.004	0.982
White Officer	0.470	0.417	0	1
Black Officer	0.266	0.387	0	1
Hispanic Officer	0.222	0.337	0	1
Years of Experience	10.011	7.770	0	47

This table depicts summary statistics at the 911 call level, using variables not included in Table 1

Table A5: Call Taker Score Variation by Call Taker Demographics

Group	Average Call Taker Score
White	-0.0260
	(0.0156)
Black	0.00902
	(0.00956)
Hispanic	0.00597
	(0.0104)
Asian	0.0506
	(0.0166)
Experienced	0.0104
	(0.00901)
New	-0.00330
	(0.00897)

Notes: Race is determined using full names and the rethnicity package in R. A call-taker is classified as Experienced if their first in-sample call is observed in the first month of the sample and new otherwise; 45% of call takers are considered Experienced under this classification. Standard errors are provided in parentheses.

Table A6: First Stage and Reduced Form

Model	First Stage	Reduced Form
Dependent Variables:	Priority	Arrest
	(1)	(2)
Variables		
Call Taker Score	0.9602***	0.0104**
	(0.0138)	(0.0040)
Minority Percentage	-0.0740***	-0.0011
	(0.0038)	(0.0009)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	598,077	598,077
R^2	0.12282	0.01424
Dependent variable mean	0.38258	0.02861
Kleibergen-Paap F-Stat, Priority	4,808.5	

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table contains first stage and reduced form estimates for the baseline specification.

Table A7: First Stage by Subsamples

Subsample	First Stage Estimate	Standard Error
Incident - Other	1.23	0.03
Incident - Burglary Residence	0.43	0.09
Incident - Burglary Motor Vehicle	0.32	0.06
Incident - Theft	0.53	0.10
Incident - Criminal Mischief	0.54	0.15
Incident - Missing Person	0.30	0.10
Incident - Burglary Business	0.36	0.07
Division - Central	0.97	0.03
Division - North Central	0.90	0.04
Division - South Central	0.98	0.03
Division - Northwest	0.93	0.02
Division - Northeast	0.97	0.02
Division - Southeast	1.00	0.03
Division - Southwest	0.95	0.03
>50% Minority	0.97	0.01
<50% Minority	0.92	0.02
Overnight Shift	0.94	0.03
Day Shift	0.91	0.03
Evening Shift	0.90	0.03

Clustered (Call Taker) standard-errors in Column 3

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A8: *Notes*: This table presents estimates and standard errors of the first stage coefficient of the baseline regression estimated within different subsamples of the data.

Table A9: Using Transfer IV

Dependent Variable:	Arrest
Model:	(1)
Variables	
Priority	0.0108**
	(0.0042)
Minority Percentage	-0.0003
	(0.0010)
Call Transfer	0.0062***
	(0.0014)
Fixed-effects	
Hispanic Call-Taker	Yes
Incident	Yes
Day of Week-by-Hour	Yes
Month-by-Year	Yes
Division	Yes
Fit statistics	
Observations	598,077
R^2	0.01887
Dependent variable mean	0.02861
Kleibergen-Paap F-Stat, Priority	4,854.5

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table presents the baseline results using a reconstructed version of the IV that controls for whether a call transfer occurred on that call.

Table A10: Balance Test: Full Call Sample

Dependent Variable:	Call Taker Score
Model:	(1)
Variables	
Years of Experience	0.0000*
	(0.0000)
Burglary Motor Vehicle	0.0001
	(0.0012)
Burglary Residence	0.0011
	(0.0011)
Criminal Mischief	-0.0050***
	(0.0016)
Missing Person	0.0012
	(0.0018)
Other	0.0027*
	(0.0015)
Out of Sample	0.0019*
	(0.0011)
Theft	0.0002
	(0.0016)
Male Officer	0.0001
	(0.0001)
Black Officer	-0.0002
	(0.0003)
Hispanic Officer	-0.0001
	(0.0004)
White Officer	0.0004
	(0.0003)
Division - North Central	-0.0002
	(0.0003)
Division - Northeast	-0.0002
	(0.0003)
Division - Northwest	0.0000
	(0.0004)
Division - South Central	-0.0001
	(0.0004)
Division - Southeast	0.0000
	(0.0003)
Division - Southwest	0.0000
	(0.0003)
Minority Percentage	-0.0001
	(0.0009)
Fixed-effects	
Hispanic Call-Taker	Yes
Day of Week-by-Hour	Yes
Month-by-Year	Yes
Fit statistics	
Observations	4,191,851
R^2	0.01335
F Stat	2.2052

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table reports results from a balance test that uses the full sample of 911 calls, including call types not in the analysis sample. Call Taker Score is assigned to non-sample calls as average Call Taker Score across all of the call taker's calls. The variable Out of Sample is an indicator for whether the call is not an in-sample incident type.

Table A11: Arrest Type

Dependent Variables:	Misdemeanor Arrest	Felony Arrest
Model:	(1)	(2)
Variables		
Priority	0.0055*	0.0016
	(0.0029)	(0.0010)
Minority Percentage	-0.0011	0.0011***
	(0.0007)	(0.0003)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	598,077	598,077
R^2	0.01243	0.00214
Dependent variable mean	0.01336	0.00353
Kleibergen-Paap F-Stat, Priority	4,808.5	4,808.5

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table presents results for the baseline specification using misdemeanor and felony arrests as outcomes.

Table A12: Use of Force

Dependent Variable:	Use of Force	
Model:	(1)	(2)
Variables		
Priority	-0.0004	0.0009***
	(0.0006)	(8.88×10^{-5})
Minority Percentage	-0.0001	-2.12×10^{-5}
	(0.0002)	(0.0002)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	598,077	598,077
R^2	0.00060	0.00100
Dependent variable mean	0.00087	0.00087
Kleibergen-Paap F-Stat, Priority	4,808.5	

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table presents results from the baseline specification, using a dummy for use of force as the outcome variable. Use of force equals one if any use of force report was generated that can be linked to the call. Call Taker Score is used as the IV for Priority in Column 1.

Table A13: IV Robustness

Dependent Variable:			Arrest		
Model:	Sample Selection	Transfer Propensity	Officer Covariates	Within-Officer	Full
Variables					
Priority	0.0108**	0.0106**	0.0104**	0.0090**	0.0087**
	(0.0042)	(0.0042)	(0.0042)	(0.0042)	(0.0043)
Minority Percentage	-0.0003	-0.0004	-0.0005	-0.0008	-0.0008
	(0.0010)	(0.0010)	(0.0010)	(0.0010)	(0.0010)
In-Sample Propensity	-0.0020				-0.0018
	(0.0127)				(0.0125)
Transfer Propensity		-0.0587			-0.0734
		(0.0817)			(0.0796)
Years of Experience			-0.0004***	-2.08×10^{-5}	-2.01×10^{-5}
			(1.96×10^{-5})	(0.0005)	(0.0005)
Fixed-effects					
Hispanic Call-Taker	Yes	Yes	Yes	Yes	Yes
Incident	Yes	Yes	Yes	Yes	Yes
Day of Week-by-Hour	Yes	Yes	Yes	Yes	Yes
Month-by-Year	Yes	Yes	Yes	Yes	Yes
Division	Yes	Yes	Yes	Yes	Yes
Officer Gender			Yes		
Officer Race			Yes		
Officer				Yes	Yes
Fit statistics					
Observations	598,077	598,077	1,170,045	1,170,045	1,170,045
R^2	0.01882	0.01874	0.01940	0.02686	0.02675
Dependent variable mean	0.02861	0.02861	0.02861	0.02861	0.02861

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table contains estimates for β in Equation (1) using Call Taker Score as an IV for Priority. Column 1 includes the propensity for call-takers to select in-sample call types, calculated as a leave-out mean, as a control. Column 2 includes the propensity for call takers to transfer calls, calculated as a leave-out mean within the analysis sample, as a control. Column 3 uses runs the baseline regression on the call-by-officer dataset, including officer controls and weighting each observation by the inverse number of responding officers, so that the estimates can be interpreted at the call-level. Column 4 adds officer fixed effects to Column 3. Column 5 adds the sample selection and transfer propensity controls to Column 4.

Table A14: Conviction Results

Dependent Variable:	Conviction	
Model:	Misdemeanor Arrest	Felony Arrest
Variables		
Priority	-0.1490**	-0.1638
	(0.0727)	(0.1376)
Minority Percentage	0.0866***	0.0911*
	(0.0233)	(0.0512)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	7,938	2,107
R^2	0.11596	0.14236
Dependent variable mean	0.30665	0.69722
Kleibergen-Paap F-Stat, Priority	118.18	57.120

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

This table presents results for an IV regression that uses the baseline specification with conviction as the outcome. In column 1, I only use the sample of misdemeanor arrests and in column 2 I only use the sample of felony arrests.

Table A15: Arrest Subsample First Stage

Dependent Variable:	Priority
Model:	(1)
Variables	
Call Taker Score	0.8163***
	(0.0650)
Minority Percentage	-0.0831***
	(0.0197)
Fixed-effects	
Hispanic Call-Taker	Yes
Incident	Yes
Day of Week-by-Hour	Yes
Month-by-Year	Yes
Division	Yes
Fit statistics	
Observations	9,309
R^2	0.10349
Dependent variable mean	0.71995

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table conducts the baseline first stage estimation using the subsample of calls which result in either a misdemeanor or felony arrest.

Table A16: Immediate versus Delayed Arrests

Dependent Variables:	Immediate Arrest	Delayed Arrest
Model:	(1)	(2)
Variables		
Priority	0.0041^{*}	0.0048^{*}
	(0.0023)	(0.0028)
Minority Percentage	-0.0005	-6.31×10^{-5}
	(0.0007)	(0.0007)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	522,588	522,588
R^2	0.00975	0.00830
Dependent variable mean	0.01160	0.01364
Kleibergen-Paap F-Stat, Priority	4,502.7	4,502.7

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: Immediate arrest refers to an arrest made within 15 minutes of the first officer's arrival. Delayed arrest refers to an arrest made after 15 minutes following the first officer's arrival.

Table A17: Effect of Priority on Arrest Timing

	Arrest				
Time to Arrest:	15 Minutes or Less	15 to 30 Minutes	30 to 45 Minutes	45 to 60 Minutes	Over an Hour
Variables					
Priority	0.0041*	0.0023	0.0009	0.0024***	-0.0008
	(0.0023)	(0.0016)	(0.0011)	(0.0008)	(0.0011)
Minority Percentage	-0.0005	0.0007	-9.11×10^{-5}	-9.73×10^{-6}	-0.0006*
	(0.0007)	(0.0005)	(0.0003)	(0.0002)	(0.0003)
Fixed-effects					
Hispanic Call-Taker	Yes	Yes	Yes	Yes	Yes
Incident	Yes	Yes	Yes	Yes	Yes
Day of Week-by-Hour	Yes	Yes	Yes	Yes	Yes
Month-by-Year	Yes	Yes	Yes	Yes	Yes
Division	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	522,588	522,588	522,588	522,588	522,588
R^2	0.00975	0.00520	0.00246	0.00170	0.00052
Dependent variable mean	0.01160	0.00617	0.00293	0.00146	0.00309
Kleibergen-Paap F-Stat, Priority	4,502.7	4,502.7	4,502.7	4,502.7	4,502.7

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table depicts results for regressions with the same right-hand side as those used to estimate Table 3, but replacing the dependent variable with an indicator for whether an arrest was made in the time frame listed at the top of each column. The Call Taker Score measure is used as an instrument for priority.

Table A18: IV Results: Timestamp sample

Dependent Variable:	Arrest
Model:	(1)
Variables	
Priority	0.0091**
	(0.0040)
Minority Percentage	-0.0005
	(0.0010)
Fixed-effects	
Hispanic Call-Taker	Yes
Incident	Yes
Day of Week-by-Hour	Yes
Month-by-Year	Yes
Division	Yes
Fit statistics	
Observations	522,588
R^2	0.01749
Dependent variable mean	0.02537
Kleibergen-Paap F-Stat, Priority	4,502.7

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table contains IV results for the baseline estimation of Equation (1) on the subample of the data for which assignment and arrival timestamps are available. The table demonstrates that the sample is similar to the analysis sample.

Table A19: Number of Officers Dispatched

Dependent Variable:	Dispatched Officers
Model:	(1)
Variables	
Priority	0.4845***
	(0.0310)
Minority Percentage	0.0671***
	(0.0056)
Fixed-effects	
Hispanic Call-Taker	Yes
Incident	Yes
Day of Week-by-Hour	Yes
Month-by-Year	Yes
Division	Yes
Fit statistics	
Observations	597,889
R^2	0.14773
Dependent variable mean	1.9613
Kleibergen-Paap F-Stat, Priority	4,821.4

Clustered (Call Taker) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table contains IV results for the baseline estimation of Equation (1) using the number of dispatched officers as a dependent variable. Call Taker Score is used as an instrument for Priority.

Table A20: Effects on Unit Dispositions

Dependent Variables:	No Complainant	No Police Action Required
Model:	(1)	(2)
Variables		
Priority	-0.0048	-0.0961***
	(0.0107)	(0.0130)
Minority Percentage	0.0084***	-0.0107***
	(0.0020)	(0.0027)
Fixed-effects		
Hispanic Call-Taker	Yes	Yes
Incident	Yes	Yes
Day of Week-by-Hour	Yes	Yes
Month-by-Year	Yes	Yes
Division	Yes	Yes
Fit statistics		
Observations	597,888	597,888
R^2	0.01369	0.14809
Dependent variable mean	0.10586	0.40339
Kleibergen-Paap F-Stat, Priority	4,821.7	4,821.7

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table reports IV regression results equivalent to those in Table 3 but with disposition outcomes as the dependent variables. In Column 1, the outcome is a dummy for whether the reporting unit indicated that the complainant was not present upon arrival. In Column 2, the outcome is a dummy for whether the reporting unit determined that no police action was required.

Table A21: Effects by crime type

Dependent Variable:	Arrest			
Call Type:	Property Crime	Missing Person	Criminal Mischief	Other
Variables				
Priority	-0.0109	-0.0041	-0.0026	0.0149***
	(0.0083)	(0.0310)	(0.0094)	(0.0047)
Fit statistics				
Observations	137,111	21,247	35,143	392,006
Dependent variable mean	0.00565	0.00645	0.00299	0.04078
Kleibergen-Paap F-Stat, Priority	42.887	9.1601	12.120	1,554.1

Clustered (Call Taker) standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Notes: This table displays results for the baseline specification, estimated by call type. Property crimes include burglaries and thefts. Property crime regressions include controls for the specific call code, such as business burglary or theft.