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A Multidimensional Approach to Understanding the Value of Educational Capital in Policing

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ABSTRACT

A MULTIDIMENSIONAL APPROACH TO UNDERSTANDING THE VALUE OF EDUCATIONAL CAPITAL IN POLICING

BY

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This dissertation consists of three papers examining whether and to what degree educational capital (e.g., college-educated officers and education policy) in law enforcement influence police-citizen violence, misconduct, and crime.

Paper 1 examines the effects of officer education on police-citizen violence. Using the *Police Stress and Domestic Violence in Police Families in Baltimore, Maryland* data, we employ a doubly robust propensity score design to compare outcomes among 1,104 Baltimore city police officers. We also apply multiple imputation to address missing data and include measures capturing officer work attitudes and stress levels. We find that having a bachelor's degree is associated with a lower probability of an on-duty shooting relative to the counterfactual condition of high school education only. Results also indicate that college-educated officers are just as likely as non-college educated officers to make violent arrests and encounter physical altercations.

Paper 2 analyzes the relationship between the proportion of bachelor-degreed officers in agencies and misconduct complaints. Using a national sample of 1,023 U.S. law enforcement agencies from the *Police Use of Force: Official Reports, Citizen Complaints, and*

Legal Consequences Project data, this study employs design-based analysis and multiple imputation of missing data with a doubly robust propensity score model applied to a multivalued treatment. We observed that large city departments in the medium and high education categories generated more abuse of authority and overall misconduct complaints than departments with low education levels.

Chapter 3 examines the mediating effects of arrests on the departmental education requirements and crime association. We integrate entropy balance weighting procedures with a novel mediation approach to identify the direct and indirect effects of agency education policy on crime in 1,543 U.S. cities using multiple data sources: Law Enforcement Management and Administrative Statistics (LEMAS) survey, U.S. Census Bureau's American Community Survey (ACS), and FBI's Uniform Crime Reports (UCR). We find that police arrest productivity does not significantly mediate the impact of education policy on crime outcomes; however, significant total effects were detected. For violent crime, college requirements were associated with significant reductions in aggravated assaults. For property crime, cities adopting a college requirement for police experienced fewer burglaries.

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THADDEUS LATEEF JOHNSON

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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor in Philosophy in Criminal Justice and Criminology in the Andrew Young School of Policy Studies of Georgia State University.

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Introduction

Treat education as an investment in man… treat its consequences as a form of capital. Since education becomes a part of the person receiving it, I shall refer to it as human capital.[1]

Communities expect the police to provide safety and security without infringing upon their civil liberties. Unfortunately, this is not always the case given the legacy of questionable police practices in the US. Most salient of these are unjustifiable police shootings, misconduct, and police brutality. Periods of protest and calls for police reform often follow high-profile police-citizen encounters. Most recently, the viral images of George Floyd pleading to Minneapolis officers, "I can't breathe," along with a spate of highly publicized police shootings of unarmed Black Americans (e.g., Breonna Taylor and Rayshard Brooks), have inspired widespread demands for an overhaul of U.S. law enforcement.

While many recommendations abound about the best path toward police reform, including defunding the police and national use of force standards, one has remained constant over the past century—increase the educational requirements for police. Beginning with August Vollmer in the early 1900s, leading authorities have maintained that hiring more college-educated officers is imperative to improving the quality of police services. This assumption rests on the commonsense notion that formally educated officers are more politically and socially astute, knowledgeable of human behavior, cosmopolitan in their values, self-disciplined, and altruistic (Telep, 2011). In this way, college-educated officers would appear to be ideal candidates for democratic policing.

[1] Excerpt from *Capital Formation by Education* (Schultz, 1961, p. 1).

The nation's police forces responded to these calls for professionalization. In 1974, just five percent of jurisdictions had a college education requirement (National Institute of Law Enforcement and Criminal Justice, 1978). Today, roughly a third of departments have such a standard (Reaves, 2015). In line with broader U.S. educational attainment trends (Baro and Burlingame, 1999; U.S. Department of Education, 2018), the number of police officers who have attended college has risen three-fold since 1960 (Gardiner, 2015; National Institute of Law Enforcement and Criminal Justice, 1978).

Despite these positive trends and grand promises, higher education's practical benefit remains unclear, especially as it concerns the profession's core aspects—the use of force, ethical conduct, and crime control. The research findings on educational attainment's influence on these outcomes are decidedly mixed, owing largely to enduring methodological and theoretical shortcomings. Lamenting these limitations, the National Academies Panel on Police Policy and Performance (NAP) called attention to poor methodologies, inadequate samples, and the absence of theoretically informed variables in prior research (Skogan and Frydl, 2004).

Despite these critiques, three major limitations continue to plague this collection of scholarship. First, the research tends to be atheoretical, which leaves us without a clear framework for predicting and explaining the impacts of education on policing outcomes. Second, the evidence-base hinges primarily on correlational methods that do not adequately account for the endogeneity concerns, notably unequal selection into higher education. The third limitation concerns the lack of empirical attention to potential causal mechanisms through which education influences police performance.

The noted limitations and mixed findings of the literature serve as the impetus for this dissertation. Identifying how educational attainment affects the delivery of police services takes on added urgency in the current period of unrest and social change. Drawing on various data sources and methodological strategies, I present three papers that attempt to fill substantial gaps in the policing literature. These papers explore higher education's influence across time, place, and units of analysis (individual-, departmental-, and city-level). Specifically, I examine whether and to what degree educational capital (e.g., college-educated officers) and capital policy (e.g., educational standards) in law enforcement are associated with police-citizen violence, misconduct, excessive force, and crime control.

The first paper examines the link between officer education and the probability of being involved in a physical altercation, violent arrest, and on-duty shooting using a sample of 1,104 sworn officers from the *Police Stress and Domestic Violence in Police Families in Baltimore, Maryland* data collected between 1997 and 1999 (Gershon, 1999). The second paper investigates the association between a department's educational profile (i.e., percent of bachelor-degreed officers) and complaints of police misconduct among a nationally representative sample of police departments (N = 1,023) from the *Police Use of Force: Official Reports, Citizen Complaints, and Legal Consequences data gathered from 1991 to 1992* (Pate and Fridell, 1993). In the last paper, we integrate entropy reweighting and mediation approaches to examine the effect of agency education policy on crime rates in 1,543 U.S. cities and whether this effect operates through enhanced arrest deterrence. The data for this third paper is drawn from the Law Enforcement Management and Administrative Statistics (LEMAS) survey, the U.S. Census Bureau's American Community Survey (ACS), and the FBI's Uniform

Crime Reports (UCR). Together these three papers offer fresh insights into the complex role of higher education and its consequences for public safety.

This dissertation proceeds as follows. Before presenting the individual papers, I first provide a brief historical account of police education reform. After presenting the papers, I close with a recap of the dissertation's central goals, motivations, and implications for research and policy.

A Genealogy of Police Education Reform

For nearly 100 years, higher education requirements for officers have been widely viewed as the bedrock of police reform. August Vollmer, "the father of modern law enforcement," first called for better-educated officers during policing's political era (Brunson and Wright, 2016). This was indeed innovative, if not radical thinking, given that most officers had not graduated from high school at that time (Fogelson, 1977; Vollmer, 1931). Despite some local success in California, Vollmer's work did not garner national attention until his 1929 appointment to the Wickersham Commission (Vollmer, 1931; Walker, 1977). This early Commission held that college-educated officers would be better prepared for the ever-expanding complexities of modern policing (National Advisory Commission on Higher Education for Police Officers, 1978). Like many subsequent calls, however, this suggestion was based on conventional wisdom rather than conclusive scientific evidence.

While the appointment of the Wickersham Commission marked the dawn of a new era in policing, enhancing American law enforcement's education profile was not cast as a criminal justice priority until 30 years later (Walker, 1977). Prompted by soaring crime rates and widespread social unrest of the 1960s—largely due to the confluence of controversial policing

strategies, political turmoil, and widespread racial injustices (Fisher-Stewart, 2007; Jacobs and Magdovitz, 1977)—the government acted on this front. In response, President Lyndon B. Johnson convened the President's Commission on Law Enforcement and Administration of Justice in 1967. Tasked with authoring best-practice recommendations for the justice system, the Johnson Commission published the landmark report, *The Challenge of Crime in a Free Society*. Among many other suggestions, the report proposed that "the ultimate aim of all police departments should be that all personnel with general enforcement powers have baccalaureate degrees" (p. 109).

Despite the lack of sound evidence for this proposition, scholars, civic leaders, and government officials continued to endorse a bachelor's degree as an indispensable employment qualification for police personnel (see, e.g., American Bar Association, 1974; Kerner Commission, 1968; National Advisory Commission on Criminal Justice Standards Goals, 1973; National Commission on the Causes Prevention of Violence, 1969; National Advisory Commission on Higher Education for Police Officers, 1978). On the heels of the 1967 Commission report, the Law Enforcement Assistance Administration (LEAA) was established with the passage of the Omnibus Crime Control and Safe Streets Act of 1968. This initiative set in motion a precipitous expansion in federal spending on law enforcement with almost one billion dollars allocated under LEAA, including immense investments in police education through the Law Enforcement Education Program (LEEP) (Blomberg et al., 2013; Roberg and Bonn, 2004). As one of LEAA's signature programs, LEEP executives doled out substantial funds to grow undergraduate policing curricular at colleges and universities and incentivized college enrollment among working officers through grants and loans (Shernock, 1992).

Five years later, the National Advisory Commission on Criminal Justice Standards Goals (1973) reinforced the Johnson Commission's proposal on police education standards, noting that "twenty years ago the high school diplomas were a significant educational achievement; it is not today. To continue recruiting at this level of education is to invite mediocrity" (p. 327). That same year, the American Bar Association's Advisory Committee on urban policing proclaimed that college-educated officers are more progressive, knowledgeable of human behavior, self-disciplined, and altruistic (Telep, 2011).

In 1978, the National Advisory Commission on Higher Education for Police Officers nuanced the discourse around police education minimums. Instead of "educating the recruited," they urged departments to focus on "recruiting the educated" in their report, "The Quality of Police Education." This strategic shift was largely premised on the belief that officers who begin college after police training academy would "already have developed a set of occupational attitudes as well as a set of operational styles" (Worden, 1990, p. 568), potentially rivaling the expected benefits of higher education.

LEEP's funding eventually ended two years later in 1980 (Eskridge, 1989). Though short-lived, officers apparently took early advantage of these educational incentives. Nationally, the proportion of degreed officers climbed from three percent in 1960, to about nine percent just 14 years later (Carter and Sapp, 1990).

As law enforcement philosophies began to pivot toward community engagement in the 1980s (Paoline et al., 2000; Wilson and Kelling, 1982), officers with college credentials became a valued human resource (Carter and Sapp, 1990). As urban crime rates spiked in the early 1990s, Congress passed the Violent Crime Control and Law Enforcement Act in 1994—the most

massive anti-crime bill in U.S. history (Zhao et al., 2003). Though known mostly for its harsh sentencing provisions (e.g., three strikes mandates), this Clinton-era bill was also intended to curb violent crime by increasing the number of college-educated police officers serving our communities. Under its provisions, $200 million was authorized for college scholarships for in-service law enforcement officers and students agreeing to join the police force through Police Corps.

Insomuch as the 1994 Crime Act, along with a host of other factors (e.g., a booming economy and shrinking crack-cocaine market), has been credited with driving down violent crime rates in the 2000s (Marvell and Moody, 1996; Zhao et al., 2003; Zimring, 2017), it did little to repair the strained police-citizen relationship. Following a rash of deadly police shootings of unarmed black men, the Obama Administration assembled the President's Task Force on 21st Century Policing in 2015. Once again, the federal government positioned higher education for officers as central to the fair exercise of police authority. Representing a prominent focus of the 2015 Task Force (the fifth "pillar" of policing), they suggested that "the federal government, as well as state and local agencies, should encourage and incentivize higher education for law enforcement officers" (p. 51). Among other objectives, the Task Force sought to build public trust and improve police effectiveness by placing more college-credentialed officers on the streets.[2]

[2] No presidential commission appointments have occurred since 2015. Considering this, along with President Trump's call for more aggressive policing of urban areas, it is unclear whether increasing the number of college-educated officers in the US remains a top cabinet-level priority (Graham, 2019).

Such a commitment over the years naturally begs the question: What difference have these investments made in augmenting the education profile of American law enforcement? The financial incentives, combined with the ongoing push for a better-educated police force, clearly delivered desired returns. Even though departments have been slow to heed the advice of the various national panels, officers with a college background are now the norm. In comparison to 1960 when only three percent of police officers held a bachelor's degree, the current figure is about 11 times that amount (Reaves, 2015). Estimates also reveal that over half of all law enforcement personnel in the US have completed some college coursework (Gardiner, 2017).

At first glance, one might think that the cited government programs and policy initiatives are behind this growth. However, experts credit the rising number of college-educated officers to a parallel uptrend in educational attainment among the general population (Baro and Burlingame, 1999; Hawley, 1998; Travis, 1995). Most departments, primarily due to the lack of hard evidence, have yet to fully impose stricter education policies (i.e., less than one percent of agencies require a bachelor's degree, nearly 10% require an associate's degree, and just over 15% require some college credit; Reaves, 2015). The lack of congruence between American police officers' educational credentials and departmental education requirements indicates the need for more research in this area. In what follows, we assess the public safety value of educational capital building in policing and whether hiring college-educated officers produces more effective and ethical police forces

1 The College Shield: Examining the Role of Officer Education in Violent Police Encounters

Introduction

There is a long history of police-citizen violence in the United States. Recent and highly publicized media accounts of excessive use of police force, especially lethal force, have fueled public outrage and eroded police legitimacy (Maguire et al., 2017). As a consequence, police officers are themselves facing increasing levels of hostility and violence (Kaminski and Stucky, 2009; Mac Donald, 2016). Considering this, few public safety topics warrant more attention today than preventing police-citizen violence.

For much of the last half century, identifying ways to foster comity between law enforcement and the public by reducing violent encounters represents an enduring theme of criminal justice reform (Klinger, 2007). Among other initiatives such as officer training and civilian oversight, improving officer diversity and credentials continue to figure centrally in this discourse. Beyond recruiting more female and minority candidates, stakeholders have long encouraged hiring officers with a college degree as a way to improve police-citizen relations (e.g., see National Research Council, 2004; President's Commission on Law Enforcement and the Administration of Justice, 1967; President's Task Force on 21st Century Policing, 2015; Sherman and the National Advisory Commission on Higher Education for Police Officers, 1978).

Observers cast college-educated officers as better problem solvers, more culturally informed, and more humanistic and, therefore, less likely to be involved in violent confrontations (Carter et al., 1989). Police reformers argue that college-educated officers are more progressive and possess desirable skills, traits, and experiences that are difficult to acquire on the job or in training (Blumberg and Niederhoffer, 1985; Breci, 1994; Carter and

Sapp, 1990; Smith and Aamodt, 1997; Telep, 2011; Varricchio, 1998). Moreover, educated officers appear suited for contemporary law enforcement approaches that emphasize community building over aggressive policing (President's Task Force on 21st Century Policing, 2015).

The claim that better-educated officers are more adept at de-escalation techniques and better at defusing potentially violent situations makes sense on its face. However, the available evidence-base on this question is mixed (Chapman, 2012; Kaminski and Sorensen, 1995; Lim and Lee, 2015; Paoline and Terrill, 2007; Ridgeway, 2016; Sherman and Blumberg, 1981; Terrill and Mastrofski, 2002; van Reemst et al., 2015; Willits and Nowacki, 2014; Worrall et al., 2018). For example, whereas Ridgeway's (2016) analysis of New York City officers demonstrates that education level is not predictive of police shootings, McElvain and Kposowa's (2008) study set in Riverside, California shows that college-educated officers are less likely to use their firearms than non-college officers in similar situations. Possible explanations for these inconsistencies range from variability in research designs, measurement, and samples to omitted variable bias. Thus, we need additional research to understand whether and to what extent officer education affects the likelihood of violent police-citizen encounters.

We improve upon prior research in at least three ways. First, we control for key measures capturing work attitudes and experiences not previously considered, such as job satisfaction and burnout, that could explain differences in outcomes between college and non-college educated officers. For example, while scholars note that burnout threatens the quality of job performance (Euwema et al., 2004; Kop and Euwema, 2001), no other study on officer education and police-citizen violence accounts for this factor. Another limitation concerns the

use of correlational designs in prior research, leaving results open to selection bias and unobserved heterogeneity. Thus, we further contribute to the literature by employing a doubly robust propensity score approach to enhance the comparability of officers by educational status, and thereby mitigate some of the causal inference challenges of standard regression approaches typically used by researchers (Linden et al., 2016; Rubin, 2001). Finally, in contrast to previous studies in this area, we employ multiple imputation to prevent the loss of statistical power and biased parameter estimates due to missing data (Graham, 2009).

Before specifying our hypotheses, we first review the literature, discuss the methodological limitations of prior research examining the association between officer education and police-citizen violence, and detail how the current study addresses these shortcomings. We then describe the data and sample, measures, and analytic plan. After presenting the results, we conclude with a discussion of the implications for policy and practice.

Prior Research

Educational Attainment and Violent Police Encounters

Scholars have shed considerable light on the situational, environmental, and departmental factors influencing police-citizen violence (e.g., Shjarback and White, 2016; White and Klinger, 2012). Less well understood is the degree to which officer characteristics, namely education level, affect an officer's risk of confronting and using violence on the job. This is a critical gap, as police and municipal leaders have direct control over setting officer qualifications that may help reduce violent police encounters, including police shootings. Therefore, a deeper understanding of the link between an officer's level of education and police-citizen violence can help develop recruitment strategies and operational policies that

mitigate these risks. In this section, we review the literature exploring the influence of educational attainment on violent police encounters, including the general use of force, police shootings, and physical altercations.

Higher Education and Use of Force

Findings on the role of officer education level in police use of force encounters are mixed. Despite a few significant results, null findings have been found consistently across geographic regions, periods, and agency settings (e.g., urban/rural) (Brandl et al., 2001; Fridell and Lim, 2016; Klahm, Frank, and Brown, 2011; Morabito and Doerner, 1997; Sun and Payne, 2004; Terrill and Reisig, 2003). Small to large effect sizes, however, observed in several studies demonstrate that college-educated officers are less likely to resort to physical force than their non-college educated peers (Chapman, 2012; Paoline and Terrill, 2007; Rydberg and Terrill, 2010; Terrill and Mastrofski, 2002). For instance, Terrill and colleagues published a series of papers between 2002 and 2010 drawing on the Project on Policing Neighborhoods (POPN) data, which examined policing practices in Indianapolis, IN and St. Petersburg, FL during 1996 and 1997. Using identical samples, comparable modeling strategies, and high school education level as the reference category, the likelihood of college-educated officers threatening or inflicting physical force during citizen interactions was 4% to 50% lower across the studies. We speculate that the varying estimates reflect differences in variable selection and methodological nuances (e.g., interaction terms). Of further note, the findings may not be broadly generalizable since these studies draw from the same data.

In another study, Chapman (2012) analyzed police data from three predominately non-white cities in New Jersey in 2008, where he found that college education shares a significant

inverse relationship with both the use of force and level of force. Interestingly, the strength of this association among patrol officers drives the overall finding as education level had no statistically meaningful effects among detectives and other officers. Chapman's analysis, however, only controls for officer age, experience, and race, making it unclear whether controlling for the other relevant factors such as gender and duty assignment would render this relationship non-significant.

Higher Education and Police Shootings

With a few exceptions, the balance of the research on police use of lethal force, including police shootings, suggests higher education is statistically inconsequential across various contexts (Ridgeway, 2016; Sherman and Blumberg, 1981; Smith, 2004; Willits and Nowacki, 2014; Worrall et al., 2018). For example, drawing on NYPD Firearm Discharge Review Board data from 2004 to 2006, Ridgeway's (2016) matched case-control analysis revealed that officer education level did not significantly determine police shootings. Nonetheless, at least two studies find that college-educated officers are less inclined to use deadly force (Donner et al., 2017; McElvain and Kposowa, 2008). In a study covering the 15 years between 1990 and 2004, for instance, McElvain and Kposowa (2008) reported that college-educated officers from a Southern California agency were about 30% less likely than those without college experience to shoot a citizen. Similarly, Donner and colleagues' (2017) National Institute of Justice-sponsored study based on 2004 Philadelphia Police Department data identified a statistically significant association, wherein each completed year of post-secondary education corresponds with a 22% drop in the risk for officer-involved shootings.

Higher Education and Physical Altercations Against Police

The literature provides limited insights into how officer characteristics are associated with the likelihood of being a target of violence (Pinizzotto and Davis, 1995; Fridell and Pate, 2001; Greenan, 1987; Geller, 1982; Kavanagh, 1997), with only a handful of studies focusing specifically on education effects. One set of studies suggests that officer education has no bearing on suspect resistance and physical aggression directed toward police (Engel, 2003; Kavanagh, 1997; van Reemst et al., 2015). Other research detects a positive association between educational attainment and police assaults (Hale and Wilson, 1974). Lastly, another group of studies documents an inverse association between college education and police assaults, personal injuries, and suspect resistance (Cascio, 1977; Kaminski and Sorensen, 1995). In Cascio's (1977) study of Florida officers, for instance, college-educated officers suffered fewer injuries from altercations than their less educated coworkers. Nearly 20 years later, Kaminski and Sorensen's (1995) analysis showed that Baltimore County Police officers with a bachelor's degree were half as likely to be assaulted than less educated officers.

Limitations of Prior Research

There are some key sources of uncertainty in this literature. First, scholarship cutting across several disciplines indicates that the effects of higher education on policing are complex, with education potentially serving as a proxy for other underlying factors. For instance, aside from having relatively higher levels of moral maturity and critical reasoning (Pascarella and Terenzini, 2005), college-educated officers might be distinct from less educated officers in their attitudes and behavioral expressions. Police research shows that education is positively associated with job cynicism (Telep, 2011), ambition (Gau et al., 2013; Johnson, 2011),

perceived self-efficacy (Kakar, 1998, 2003), policy compliance (Cunningham, 2006), and careerist attitudes (Paoline et al., 2000). These attributes are strong determinants of work productivity in law enforcement settings.

Why is this important? In law enforcement, production is measured by arrests, citations, seized contraband, calls for police service, average response times, and so on. As is true of any other enterprising worker, officers must perform at high levels to compete for better pay, shifts, and career opportunities (Buckley et al., 1993). They are also fully aware that commanders use these traditional metrics to track performance, with implications for future promotion considerations. Scholars note that the attainment of a college degree often underpins an officer's desire for raises, upward mobility, or more prestigious assignments (Buckley et al., 1993; Gau et al., 2013; Hayeslip, 1989). Therefore, college-educated officers might be especially zealous in their pursuit of advancement, fostering a more energetic enforcement style that results in higher rates of stops, citations, seizures, and arrests. Importantly, these decision points precede many police confrontations involving physical force (Bonkiewicz, 2015; Brandl et al., 2001; Wilson and Zhao, 2008), with more serious altercations usually occurring during proactive officer contacts (Pinizzotto, 1997; Rydberg and Terrill, 2010; Terrill and Ingram, 2016). Despite the potential contribution of work attitudes in police-citizen interactions, previous studies rarely control for such measures, omitting potentially important factors in the relationship between educational attainment and violent police encounters.

The second major limitation is methodological in nature. Apart from omitted variable bias, previous studies examining the effects of educational attainment on police-citizen violence suffer from self-selection issues, given the combination of observational data,

correlational methods, and previously uncontrolled factors. To our knowledge, only one other study in this area attempted to overcome the threat of selection bias (Ridgway, 2016). However, educational attainment functioned as a control in Ridgeway's matched case-control analysis, and theoretically important work attitudes and experiences including job satisfaction and burnout were not considered.

The Current Study

Using officer-level data from the *Police Stress and Domestic Violence in Police Families in Baltimore, Maryland* study collected between 1997 and 1999 (Gershon, 1999), we perform a doubly robust propensity score analysis within a multiple imputation framework to investigate the effects of higher education on violent encounters between police and citizens. The present study advances prior research in several ways. First, we control for work attitudes and characteristics that may confound the association between officer education and violent encounters with the public, which few studies have previously done. In this way, we also bridge studies on college-educated officers' work attitudes and use of force behavior. Second, most findings in this area are derived from correlational studies, making them vulnerable to selection bias and other endogeneity concerns. To handle selection bias issues, we apply a doubly robust propensity score weighting design to evaluate the consequences of different levels of educational attainment for police-citizen outcomes. Our implementation captures the multivalued effects of a college education net of officer characteristics, work attitudes, and on-the-job experiences, as it incorporates a weighting scheme to balance cases on the treatment and regression adjustment on the outcome to reduce the bias in the estimated treatment

effect. Lastly, in contrast to prior studies, we employ multiple imputation to address missing data in a rigorous manner.

Despite the mixed findings on the impacts of education, we draw some key insights from the most recent research indicating that formal education lowers an officer's likelihood of both using force (see e.g., Donner et al., 2017; Lim et al., 2014; Rydberg and Terrill, 2010; Stickle, 2016) and citizen altercations (Shjarback and White, 2016). Further, researchers also note that a college education develops an officer's ability to handle complex tasks and process information, making for greater professionalism and restraint in potentially violent situations (Telep, 2011). We therefore expect that in spite of their well-documented productivity, college-educated officers will be at lower risk for involvement in police shootings, violent arrests, and physical altercations compared to those with a lesser education.

Methods

Data and Measures

Data from the *Police Stress and Domestic Violence in Police Families in Baltimore, Maryland* study were collected as part of a collaborative effort between the Baltimore Police Department, Baltimore's Fraternal Order of Police, and researchers from the Johns Hopkins' School of Public Health (Gershon, 1999). Surveys were distributed to 1,150 of the approximately 2,500 Baltimore Police Department officers attending roll call at the various precincts and headquarters. Of the selected officers, 1,104 (93.2%) who completed the questionnaire were included in our sample.[3] The original goal of the survey was to document the amount of stress among police officers and ascertain whether that stress led to adverse

[3] We were unable to locate details on why the remaining officers did not complete the survey.

behaviors, including personal and occupational violence (for complete details see Gershon, 1999).

Dependent Variables

We analyze three outcomes in this study: *police shooting*, *violent arrest*, and *citizen altercation*. We operationalized the measure of police shooting from the following survey item: "If you have ever experienced shooting someone, please indicate how much it emotionally affected you," with response options 'Not at all,' 'A little,' 'Very much,' and 'Not applicable'. To signify whether an officer had ever shot someone while on duty, respondents answering 'not applicable' were assigned a value of 0 and the remaining respondents were assigned a value of 1. Using the same operationalization scheme, the second outcome, violent arrest, is based on the survey item: "If you have ever experienced making a violent arrest, please indicate how much it emotionally affected you," with response options 'Not at all,' 'A little,' 'Very much,' and 'Not applicable'.

We dichotomize the final outcome, citizen altercation, to indicate whether an officer had experienced a physical confrontation with a citizen ("Have suspects or civilians ever gotten physical with you?"). Unfortunately, even though such acts could range from defensive resistance to a full-on assault, the question does not specify the exact nature of the physical altercation.

Independent Variables

Treatment measure

Based on responses to the survey item, "Indicate the highest level of education completed," we operationalized the focal independent variable, *educational attainment*, as a

three-level categorical variable: 0 = high school only, 1 = some college, 2 = bachelor's degree or higher.

Covariates

Our analysis includes controls found in prior work (Ivie and Garland, 2011; Manis et al., 2008; Miller, 2015; Paoline and Terrill, 2007; Rydberg and Terrill, 2010; Stichman et al., 2010), as well as theoretically relevant controls typically unobserved in previous studies. We categorized these variables into two groups. The first group of predictors measures officer and work-related characteristics. Since researchers have found that more experienced officers are less likely to rely on force during police/citizen interactions (Paoline and Terrill, 2007; Rydberg and Terrill, 2010; Johnson et al., 2005), we control for *tenure* in years. *Officer age* is also measured in years. An officer's rank or duty assignment might also affect his/her involvement in police-citizen violence (McElvain and Kposowa, 2008). Thus, for the dichotomous measure, *high rank*, we assigned lower ranked officers (i.e., officer trainee, officer, and agent) and higher ranked officers (i.e., detective, sergeant, and lieutenant or above) a value of 0 and 1, respectively. Since duty assignments requiring regular suspect contacts increase the opportunity for potentially violent encounters (Campbell et al., 2017; Chapman, 2012), we incorporated *routine citizen contact* (1 = yes) (e.g., "Do you routinely have contact with suspects?"). *Veteran status* is a dichotomous variable indicating whether an officer has ever served in the armed forces (e.g., "Did/do you serve in the military?"). *Married* is a binary variable identifying those officers who were married at the time of the survey (1 = yes). *Male* is also measured dichotomously (1 = male). We measure race/ethnicity as a simple dichotomy (i.e., white vs. nonwhite) as there are relatively few nonblack minority officers in our sample.

Lastly, we operationalized the measure, *investigated*, from the following survey item: "If you have ever experienced being the subject of an IID [internal investigation division] investigation, please indicate how much it emotionally affected you," with response options 'Not at all,' 'A little,' 'Very much,' and 'Not applicable'. Officers indicating an emotional response were coded with a "1" and those responding 'not applicable' were coded with a "0".

The second group of predictors describes officer work attitudes and outlooks. To preserve degrees of freedom, avoid issues of multicollinearity, and determine the meaningful factor structure, we conducted exploratory polychoric factor analysis (EPFA) on 36 candidate items measured using Likert-type scales (Holgado-Tello et al., 2010). As the factors are likely correlated, we applied an oblique promax rotation (Fabrigar et al., 1999; Ford et al., 1986; Gorsuch, 1997; Thompson, 2004). The EPFA identified three distinct factors with loadings of .40 or higher, conceptualized as job stress, job contentment, and perceived organizational fairness, that account for 80% of the total variance. A scree plot of eigenvalues (minimum value close to 1) verified the three-factor structure (see Figure A1.1), and the Kaiser-Meyer-Olkin value (KMO = .93) and Bartlett's Test of Sphericity (p = .000) confirmed the suitability of the data for factor analysis. Further, while there is no standardized minimum sample size for EPFA, our observed sample size reflects a 27:1 respondent to item ratio, exceeding the strictest ratio recommendation of 20:1 (Osborne, 2014). Table A1.1 presents factor analysis results, including items and factor loadings.

Police officers' routine exposure to traumatic events such as shootings, violence, and handling deceased persons increase their risk for stress-related issues (Violanti et al., 2006). (Shucard et al., 2012). The fallout from stress and trauma may include intrusion (e.g.,

nightmares), avoidance of trauma related stimuli (e.g., distressing thoughts), negative cognition and mood alterations (e.g., problems recalling key aspects of the traumatic event), and changes in arousal and reactivity (e.g., difficulty concentrating) (APA, 2013). For these reasons, the first variable, *job stress*, assesses whether officers are dealing with work-related pressures and strain. This variable encompassed 16 items measured on four-point Likert-type scales ranging from 'never' to 'always', explaining 61% of the variance: (e.g., In the past 6 months, how often did you have had spells of terror or panic, feelings of hopelessness, felt physically, emotionally, and spiritually depleted, and etc.). Higher values are associated with greater levels of stress.

The next variable, *job contentment*, measures an officer's level of workplace satisfaction. Low job satisfaction can lead to negative occupational behaviors such as poor performance, work avoidance, decreased morale, job turnover, and physical or legal liabilities (Dantzker, 1993, 1997; Dantzker and Kubin, 1998; Gau et al., 2013; Paoline et al., 2015; Terrill and Paoline, 2015). Despite the absence of an agreed upon definition of job contentment (Paoline and Gau, 2020), previous studies have generally measured this construct with a broad range of items covering the adequacy of benefits, social value and prestige, role conflict, cynicism, supervision quality, job stress, self-fulfillment, satisfaction with advancement opportunities, and peer cohesion (see Paoline and Gau, 2020 for a comprehensive review of the literature). Our construct capturing job contentment includes seven items measured on five-point Likert-type scales ranging from 'strongly agree' to 'strongly disagree' (e.g., likely to look for a job outside of the department) that accounts for 11% of the variance. Higher values signify stronger contentment with the job.

The last variable in this category is *organizational fairness*. Numerous studies in the organization/industrial psychology and management traditions report a robust association between perceptions of organizational fairness and employees' behaviors and attitudes (Colquitt et al., 2001; Cohen-Charash and Spector, 2001). Likewise, in a limited number of police studies, scholars observed that perceived fairness influences organizational commitment, job satisfaction, task performance, cooperation, and policy compliance (Donner et al., 2015; Hass et al., 2015). Scholars generally conceptualize this construct as consisting of three broad domains: distributive, procedural, and interactional fairness (Colquitt et al. 2005; Colquitt et al., 2013). Distributive fairness refers to the perceived justness of personnel outcomes, procedural fairness concerns adjudication processes, and interactional fairness refers to management's treatment of employees (Bies and Moag, 1986; Colquitt, 2001; Colquitt et al., 2013). Our measure for organizational fairness is based on four statements (e.g., promotions in this department are tied to ability and merit) with response ranging from 1 = "strongly agree" to 5 = "strongly disagree" and explains 8% of the variation. Higher scores correspond with more perceived fairness in departmental processes.

Analytic Strategy

We used a doubly robust inverse probability weighted regression adjustment (IPWRA) propensity analysis design to compare the risk for violent police-citizen outcomes between officers of varying education levels. To handle missing data in a statistically robust manner, we employed multiple imputation (Little and Rubin, 2002). In this section, we outline details of our analytical approach. All analyses were conducted with Stata 15.1.

Multiple imputation of missing data

Although levels of item missingness did not exceed 11.1% for any measure, a complete case analysis (CCA) would drop 209 cases or 18.9% of the sample. Missing data can lead to a loss of statistical power and biased treatment effects depending on the pattern of missing data. MCAR describes missingness unrelated to the treatment, covariates, or the outcome; MAR concerns missingness related to the treatment or covariates, but not the outcome; and MNAR refers to missingness potentially associated with the outcome (Rubin, 1987).

Multiple imputation (MI) is a rigorous method for addressing missing data. Under the assumption that the missingness is non-ignorable (MCAR or MAR), MI produces valid inferences. Although non-ignorability is not directly testable, we explored patterns of missingness to ascertain the plausibility that the missing data are MCAR or MAR using Little's MCAR test. The results suggest the MCAR assumption is untenable. However, the covariate-dependent missingness extension of Little's test indicates that data were consistent with a MAR pattern (Little's CDM test: $\chi 2 = 8.44$, df = 84, p = 1.00), highlighting the need to account for missing data in our analysis.

We employed fully conditional multiple imputation by chained equations (MICE) to iteratively replace the missing values (Royston and White, 2011; White et al. 2011). This process creates multiple datasets so that the standard errors of the estimates reflect the uncertainty about the missing values. Per standard practice, the MICE prediction equations included all relevant measures to preserve the relationship between the dependent and independent variables (Allison, 2002; Graham, 2009). The imputation models proceeded through 10 random draws from the posterior predictive distribution of each variable before generating a single

imputed data set, *m*. To calculate the appropriate number of *m*'s for the analysis, we applied von Hippel's (2018) two-stage quadratic rule-based approach. The first stage produced a conservative fraction of missing information (FMI) estimate and the number of imputations needed was estimated using the upper bound of the 95% confidence interval for FMI during the second stage. Results indicated that *m* = 35 was sufficient. Visual inspection of trace plots confirmed convergence, meaning that the distributions of the observed, imputed, and completed values were similar. We base our interpretations on the imputed model as sensitivity analysis not accounting for missingness produced similar results (complete case analyses not presented but available upon request).

We checked for multicollinearity between variables, and not surprisingly, tolerance values (1/VIF) for the measures age and tenure appeared to be collinear with respective 1/VIFs of .16 and .18. Although the values reached the minimum tolerance level of .10 (Tabachnick and Fidell, 2001), they fell just below the conservative threshold of .25 (Adeboye et al., 2014). Otherwise, diagnostic tests showed no indication of multicollinearity among covariates (average variance inflation factors [VIF] = 1.9). We ran alternate models with either one or both measures, and little change occurred in the strength, direction, and significance of the observed effects (results available upon request). For this reason, we decided to include both measures in the final analysis.

Propensity score model

Persons do not end up in college at random. Instead, various factors influence who is likely to attend and complete college (e.g., socioeconomic status) (see Cappelli, 2020). As officers with varying levels of education differ from one another (i.e., high school education

only, some college background, and bachelor's degree), observed differences in outcomes between these groups of officers could be correlated with certain baseline characteristics. For example, among other factors, prior scholarship indicates that increased financial difficulties, academic pressures, and the social transition to adulthood, especially for first-year college students, represent major barriers to college completion (Hartley, 2011; Park et al., 2012; Wei et al., 2005). Some scholars, therefore, suggest that beyond academic preparedness, family background, and goal orientation (Nunez and Cuccaro-Alamin, 1998), differences in emotional and educational resilience are predictive of college success (Cassidy, 2015; Hartley, 2011). Because our study draws on observational data that were collected after officers attained their educational credentials, bias from self-selectivity is likely. Thus, to accurately identify the effects of education, we must utilize methods that allow for valid comparisons between officers who completed college versus those with some or no college education.

To mitigate selection bias and imbalance among covariates, we adopted an inverse probability weighted regression adjustment (IPWRA) approach. This approach holds two advantages over traditional propensity score strategies. For one, IPWRA estimators tout a doubly robust property which returns consistent estimates, even when one of the two models (treatment or outcome) is incorrectly specified (Cattaneo, 2010)—a feature not found with standard weighting methods. Specifically, our doubly robust model combines two methods (IPW and RA) to estimate propensity score weights balancing on the treatment and weighted regression coefficients of the outcome simultaneously (Cattaneo, 2010; Imbens, 2000; Lechner, 2001; Robins et al., 1995). The IPW or treatment model calculates the weighted averages based on observed factors or the inverse of the predicted probabilities of attaining each measured

level of education for each officer. This step ensures that the treatment and comparison groups are statistically similar based on observed covariates. Using the reweighted data, the outcome or RA model estimates the net education effects by averaging the differences between the expected outcomes across officer education levels (Abadie and Imbens, 2011). Secondly, this approach accommodates a multivalued treatment, which may prove more insightful than standard propensity score designs that are only applicable to binary treatments. By accounting for a range of education levels, we can better characterize the multivalued nature of educational attainment and its impact at these various levels.

For the multivalued treatment, we estimate a multinomial logit treatment model. Let officer *i* have a violent police-citizen incident $\{Yi = Yi\ (t),\ t \in T\}$. The treatment status *t* indicates the realized value of a random treatment variable $T \in T$. $T = \{0,1,2\}$ depicts the multivalued treatment variable, where *T* represents the three education levels: 0 if high school only; 1 if some college experience; and 2 if bachelor's degree. For each officer *i*, we observed $Y_i = Y_i\ (t)$ if he/she attained education level $T_i = t$.

For the outcome model, we estimate a binary logit model for each of the three outcomes. The outcome variables (for example, police shootings) $Y_i\ (t)$ corresponding to each treatment level, t_1 and t_2 (some college experience and college completion, respectively), are compared to the comparison group's (non-college) t_0 outcomes. The treatment effect is expressed as $Y_i\ (t_0) - Y_i\ (t_2)$ (e.g., non-college officers versus bachelor-degreed officers). IPWRA analyses produce treatment effect estimates that reflect the mean outcome difference for police-citizen violence for officers with some college education and those with a bachelor's degree relative to non-college educated officers.

While all covariates were included in the outcome model, covariate selection for the treatment model requires additional consideration. While the doubly robust approach guards against misspecification, additional steps are taken to provide greater confidence in our model specification. Despite some disagreement in the literature, simulation studies suggest that incorporating all covariates potentially related to the outcome in propensity score estimation, irrespective of their relationship to treatment, reduce the bias and variance of treatment effect calculations (Brookhart et al., 2006; Pearl, 2011; Rubin and Thomas, 1996; Wyss et al., 2013). In contrast, measures that strongly predict treatment but not the outcome should be excluded from the propensity score model (Pearl, 2011). Such variables could amplify bias due to unmeasured confounding (Wyss et al., 2013, Myers et al., 2011), thereby decreasing the precision of treatment effect estimates.

Since the temporal order of most study variables is unknown, we included all measures in a series of logistic and multinomial regression models using imputed data to detect significant predictors of police-citizen violence and educational attainment, respectively. Outcomes were associated with the observed values of tenure, age, high ranked, citizen contact, veteran status, married, male, investigated, and organizational fairness. The measures, age, high ranked, and organizational fairness, were also significantly related to educational attainment, making them empirical confounders that reduce the nonsystematic bias due to a chance association. Thus, these nine covariates comprise the treatment model.

Sound inference using an IPWRA estimator relies on two strong assumptions: conditional independence assumption and the overlap assumption (DuGoff et al., 2014; Heinrich et al., 2010; Rosenbaum and Rubin, 1983). The conditional independence assumption

(CIA) or unconfoundedness implies that treatment assignment is exogenous conditional on baseline variables, meaning that once conditioned on the covariates, the treatment is "as good as random." This assumption is strong in that selection into treatment could be based on unobservables (Wooldridge, 2010). Indeed, capturing all conceptually relevant factors with observational data is no easy task, and to the extent that we do not measure relevant balancing factors, our results may be biased. Nonetheless, our study controls are good proxies of factors that might affect educational attainment among police officers.

The overlap assumption holds that each respondent has an equal probability of receiving each treatment level, or in our case, each measured level of college education. If this condition is met, we can assume that the comparison groups are statistically equivalent on observed measures (see Bang and Robins, 2005; Cattaneo, 2010; Hirano and Imbens, 2001; Linden et al., 2016; Robins et al., 1995; Scharfstein et al., 1999; van der Laan and Robins, 2003; Wooldridge; 2007 for further discussion on double robustness and multivalued treatments).

Covariate balance

For the IPWRA analysis within a MI framework, covariate balance is assessed for each dataset, *m*. We assessed covariate balance both graphically and statistically for each of the 35 imputed datasets separately. To check the overlap of propensity scores, we base our visual inspection on the estimated densities of the probability of obtaining each level of education. Figure A2 shows that the estimated densities have little mass around 0 or 1, suggesting no serious violations of the overlap assumption.

Stata software is not set up for statistical diagnostics of propensity score estimation with MI data. Following prior research (Hayes and Groner, 2008; Leyrat et al., 2019), we

performed diagnostics by averaging the standard mean difference (SMD) and variance ratio (VR) statistics across the 35 imputed models for our statistical assessment of balance. While no concrete evidence supports a particular threshold, most empiricists agree that a SMD less than 0.2 (Linden and Samuels, 2013) and VR between 0.5 and 2 (Rubin, 2001) would suggest negligible residual differences in the distribution of the baseline covariates between groups. VRs were generally within bounds in the unweighted sample but improved in the weighted. SMDs initially fell outside the threshold in 33% of covariates, all of which showed acceptable balance after weighting (see Table A1.3 in Appendix). Since correlation designs (e.g., OLS estimation) found in prior research are inconsistent and produce biased coefficients in the presence of endogeneity, standard logistic regression analysis is also performed to assess the sensitivity of estimates to imbalance among measured covariates. Per recommendations we report both the unweighted and weighted PSA results.

Findings

Table 1.1 presents imputed descriptive statistics for the study variables, including the percentage of cases with imputed missing values. Over a quarter (26%) of the sampled officers had shot a citizen/suspect, most officers reported involvement in a violent arrest (90%), and more than three-quarters (78%) experienced a physical altercation with a citizen/suspect.[4] Just

[4] Even with Baltimore's history of violence, police shootings, misconduct, and zero-tolerance enforcement, (DOJ, 2016; Wilber and Recton, 2016), the number of officers reporting involvement in a police shooting appears high. No other study analyzing the *Police Stress and Domestic Violence in Police Families in Baltimore, Maryland* data operationalized the police shooting item as a dichotomous variable, preventing us from making empirical

over 55% of the 1,104 officers completed some college coursework, and nearly 30% attained a bachelor's degree or higher. Table 1.1 also shows that 86% of the officers identified as male, 36% were non-white, 35% had prior military experience, 60% were married, and 65% endured an internal affairs investigation. Most of the respondents held the rank of patrol officer (69%), and about 73% had duties that involved regular interactions with citizens. The average officer age and years of service in the Baltimore Police Department was 36 years and 11.5 years, respectively.

For our propensity score analysis, we report the average treatment effect on the treated (ATET) for three violent police-citizen outcomes (police-involved shootings, violent arrests, and citizen altercations), which corresponds to the impact of higher education among officers that attended or graduated from college. In support of our first hypothesis, while having some college experience is not statistically relevant to police shootings, we find that on average, obtaining a bachelor's degree is significantly associated with a .10 lower probability of having been involved in an on-duty shooting, respectively, relative to the counterfactual condition of high school education only.

We estimate educational attainment's effects on officers' involvement in violent arrests and citizen altercations in the next two models (Panel B in Table 1.2). Contrary to our expectations, we find no statistically significant association between education and violent

comparisons. While we find no evidence challenging this measure's accuracy, the possibility remains that some respondents may have misreported this information.

arrests or citizen altercations.[5] In fact, the direction of effects runs opposite our expectations, although, again, these effects are not significant.

Sensitivity Analysis

Research demonstrates that propensity score methods generally return treatment effect estimates closer to the true marginal treatment effect than standard regression models (Amoah et al., 2020; Martens et al., 2008). To illustrate the advantages of our approach, we compare the doubly robust results to those from traditional covariate-adjusted regression. Similar to previous studies, a logit regression model was developed to predict college-educated officers' involvement in violent confrontations, net all measured covariates. The regression coefficients were converted to reflect the ATET by calculating the difference between college education's predicted marginal effects and those for high school education.

The findings from the main and sensitivity analyses are qualitatively similar but slightly differ in terms of statistical significance and effect sizes (see Panel A in Table 1.2). Like the main findings, we detect a statistically insignificant relationship between educational attainment and citizen altercations. Contrastingly, the sensitivity analysis indicates non-significant education effects for police shootings. The reported effect size associated with a bachelor's degree in the doubly robust model is also twice as large, suggesting that covariate imbalance slightly depressed the effects of education in the unweighted police shootings model. Also, while the effect sizes were similar in both models, unlike the primary results, we find that the association

[5] Covariate coefficients from the multinomial logit treatment model and logit outcome model are not presented but available upon request.

between a bachelor's degree violent arrest is statistically significant (Table A1.4). While these represent marginal differences, to the extent that we have measured all relevant confounders and achieved proper balance and overlap, the doubly robust estimates promise to provide less biased results than standard regression analyses.

Discussion

Since the early part of the twentieth century, improving the education profile of American law enforcement remains a centerpiece of police reform initiatives. From August Vollmer to the 1967 President's Commission, and most recently, the 2015 President's Task Force, various experts and federal commissions have called for more college-educated officers on the streets to, among other reasons, reduce police-citizen violence. The research dedicated to understanding the effects of educational attainment on violent police encounters shows mixed results, owing in part to the preponderance of correlational designs and the omission of critical measures for officer work attitudes and experiences.

To address these shortcomings, our study capitalized on the *Police Stress and Domestic Violence in Police Families in Baltimore, Maryland* data (Gershon, 1999) using a doubly robust IPWRA estimator that combines weighting and regression methods to guard against selection bias. The advantages of this approach include its production of consistent estimates even if one of the underlying methods relies on a misspecified model and the accommodation of multivalued treatments. Other methodological innovations of this study include the application of multiple imputation to address missing data and the inclusion of measures capturing officer work attitudes and perceptions.

Among a sample of 1,104 Baltimore city officers, our analysis demonstrated that bachelor-degreed officers are slightly less likely to fire a weapon at citizens, but officers of both college levels are no less likely to make violent arrests and encounter combative persons. While open to multiple interpretations, we reason our findings reflect the saliency of the outcomes. That is, for more salient events, such as shootings, education appears to reduce the risks of such involvement in small but meaningful ways. Relative to violent arrests and physical altercations that are more common in policing, shootings reflect signatory events that garner heightened media and agency attention. Thus, the effects of the unique attributes and skills possessed by bachelor-degreed officers may only emerge during the most serious liability situations.

Along this vein, we speculate that our shooting finding reflects a consequence of prolonged college exposure, wherein officers sharpen the cognitive, affective, and critical reasoning skills advantageous to such high-stakes decision-making. Educational attainment is a robust predictor of advanced critical reasoning skills—especially among college graduates (Bebeau and Thoma, 2003; King and Mayhew, 2002), and by extension, foresight and better discernment (Facione, 2009; Halpern, 1998; Helsdingen et al., 2011; Pascarella and Terenzini, 1991). In the context of policing, when officers pay more attention to surrounding dangers and take a more measured approach in their interactions, the chances of misinterpreting innocuous behaviors as imminent threats decrease and suspects are less likely to behave in ways that elicit force (Fyfe, 1986; Klinger et al., 2016).

Moving Forward

Although we believe that our study provides an important advancement over prior research, there remains ample room for improvement. First, the data are 20 years old, and policing has indeed evolved since then. Second, despite our use of propensity score methods, the data are cross-sectional and therefore appropriate caution should be used when interpreting the results of our analyses. Further, the study data come from Baltimore during the early 1990s, so the relationship between officer education and violent police encounters may not be generalizable to other times and places.

Next, the estimator used to analyze the data only accounts for observed variables. While we included both established predictors and relevant variables omitted in prior research, our design remains blind to other unobservables. Absent a randomized experimental evaluation (which is not feasible in this case), we cannot rule out the possibility that systematic differences exist in unobservable characteristics predictive of educational attainment, such as officers' motivation for advancement and ability to self-improve performance. Thus, any unmeasured factors could lead to biased estimates. For example, if unobservable characteristics result in more career-oriented officers systematically being more likely to attend and graduate from college, we could underestimate education's effects. Conversely, given the correlation between proactive enforcement and the use of force, our estimates would be biased upward if unmeasured performance incentives (e.g., merit pay) related to officer enforcement vigor (e.g., arrest) were provided.

To better control for unobserved forces, a major task for future research is to incorporate a more comprehensive set of covariates covering specific duty assignments, work

and career outlooks, demographics, personality traits, job strains, family life, and moral judgment faculty. Future research should examine how these underlying factors moderate or mediate the effects of educational attainment on police behavior. Moreover, only officer information was included in this analysis. Studies would also benefit from including measures that capture ecological (e.g., violent crime rate), situational (e.g., call type), and suspect features (e.g., demeanor). Relatedly, future studies could employ instrumental variables (IV) to address residual confounding. Based on research findings in education, economics, and medicine, future scholarship could leverage compulsory schooling laws, state school characteristics, and parental background indicators (e.g., mother's education level) as candidate instruments (see Hamad et al., 2019; Nguyen et al., 2016).

Because the focus of the data was not specifically on educational attainment, officer education level was measured with a single item. Future research should include various measures that capture related factors, such as the corresponding aptitude, cognitive ability, motivation, effort, and skills. In addition, measures indicating whether the degree was earned before or during police employment, the college major, social activities, and whether they attended online or traditional courses should be considered moving forward.

Finally, given the utilization of a cross-sectional design and potential for unmeasured confounding, our results should not be considered causal. However, our findings should be viewed as a robust estimation of the association between officer education and police-citizen violence. Such results are useful for establishing evidence of a causal argument. Thus, further examination of the potential causal effects of higher education on officer performance is warranted.

Conclusion

In the first doubly robust, quasi-experimental study of the impacts of officer education attainment on violent police-citizen encounters, we observed that the more salient and costly outcome of police shootings was diminished for bachelor-degreed officers, whereas education had no statistically significant association with two other more common policing outcomes. Whether these findings can be explained by differences that predate officers' college enrollment or were cultivated during college is beyond the scope of the data. However, we reason that police shootings are more salient, and thus have more relevance with respect to the theoretically important factors. Violent arrests and physical altercations, in contrast, are less salient and thus may not follow or support the theory. This ambiguity, nonetheless, leaves open the question of whether education capital building in law enforcement is cost beneficial due to higher salaries, education pay incentives, and tuition reimbursement. Our findings suggest, however, that agencies that hire bachelor-degreed officers may see a pay off in reduced police shootings and mitigation of associated costs.

2 Higher Education and Police Misconduct: It Takes More Than A College Degree

Introduction

Officers take an oath to wield their authority morally and ethically. When police actions deviate from this ideal, society is harmed. High-profile incidents of police misconduct such as the deaths of George Floyd and Eric Garner while in police custody may foment public outrage, but the cumulative effects of everyday police incivilities and lower-level abuses of power also have implications for police legitimacy and citizen compliance with the law (Harris and Worden, 2014; Tyler, 2004). Beyond a tarnished public image, improper police behavior, especially when concentrated in over-policed communities, engenders mistrust and undermines the authority of law enforcement (National Research Council, 2004; Skolnick and Fyfe, 1993).

Over the past 50 years, civic leaders, academics, and federal commissions have repeatedly pitched hiring more officers with a college background as one solution to mitigate police misconduct (e.g., see National Research Council, 2004; President's Commission on Law Enforcement and the Administration of Justice, 1967; President's Task Force on 21st Century Policing, 2015; Sherman and the National Advisory Commission on Higher Education for Police Officers, 1978). This claim is supported by the proposition that college-educated officers are more inclined to honor an oath to police justly and uphold the law, and in turn, help restore the public's trust in law enforcement (Carter and Sapp, 1990; Sterling, 1974).

Interdisciplinary theory and research supporting this proposition consistently demonstrate a positive relationship between educational achievement and moral development. These studies suggest that as more workers with college experience join an organization, they will promote increased morality and ethical behavior (King and Mayhew,

2002; Kohlberg, 1969, 1984; Treviño and Nelson, 2017; Victor and Cullen, 1988). Therefore, scholars and administrators expect college-educated employees to bring qualities that help cultivate a more ethical organizational culture (Bebeau and Thoma, 2003; King and Mayhew, 2002).

Existing police literature, however, provides mixed support for the educational attainment-misconduct association. Studies examining college education's effects on misconduct have been conducted at both the officer-level and agency-level. Officer-level research demonstrates that college-educated officers generate fewer complaints than those with less education (Brandl et al., 2001; Cascio, 1977; Cohen and Chaiken, 1972; Kane and White, 2009; Kappeler et al., 1992; Lersch and Kunzman, 2001; Manis et al., 2008; Sanderson, 1977). Agency-level research, in contrast, suggests a null relationship (Cao et al., 2000; Cao and Huang, 2000; Eitle et al., 2014; Terrill and Ingram, 2016).

We posit that three key limitations underlie the mixed findings on the police education-misconduct link. First, despite the complex nature of this association, the research tends to be atheoretical. Second, the methods tend to be correlational and thus likely suffer from endogeneity issues. Third, few empirical works on the educational antecedents of police misconduct evaluate a large sample of jurisdictions. Thus, unresolved research gaps remain.

We tackle these challenges to extend the literature substantively, theoretically, and methodologically. Drawing on interdisciplinary perspectives and data from the *Police Use of Force: Official Reports, Citizen Complaints, and Legal Consequences Project* (Pate and Fridell, 1993), we employ a doubly robust propensity score approach while accounting for the complex

survey design and missing data to examine the effects of higher education on misconduct complaints among 1,023 U.S. police agencies.

This paper takes the following course. We begin by detailing our theoretical rationale. We then move to a discussion of the literature examining the impacts of higher education on police misconduct. After describing the data, methods, and findings, the article closes with a discussion of implications for policy and practice.

Theoretical Rationale

Police Subculture: A Compass for Officer Behavior

The first step in controlling police misconduct concerns addressing the problem of police subculture. Police officers work in a high-pressure and polarizing profession that fosters subcultural beliefs defined as "protective, supportive, and shared attitudes, values, understandings, and views of the world" (Inciardi, 1990, p. 227). A cynical "police worldview," which describes negative, distrustful, and hostile attitudes towards the job and the public, represents a defining feature of police subculture (Skolnick, 2002; Wilson, 1968). Police misconduct, such as becoming overly forceful toward citizens, is a product of increased police cynicism (Muir, 1977; Skolnick, 2002; Worden, 1996). In departments where sworn personnel widely share this sentiment, misconduct is often learned and even endorsed (Skolnick, 2002; Wilson, 1968), creating an organizational culture conducive to the mistreatment of citizens.

Recognizing that "organizational culture eats policy for lunch," the 2015 President's Taskforce (p. 11), emphasized the imperativeness of a cultural shift in policing before the implementation of new programs or reform initiatives (see also Bradford et al., 2014; Wolfe and Piquero, 2011). Apart from other organizational factors, including managerial practices,

administrative policies, and training, agency hiring decisions are the single-most important determinant of department culture (Lee et al., 2013). Since college-educated officers are believed to hold more progressive and ethical values (Carter and Sapp, 1990), reformers contend that they can disrupt the homogeneity of conventional values and norms in policing (National Research Council, 2004). While the impact of college-educated officers on police culture remains unclear, stakeholders across most professions maintain that higher education improves employees' attitudes and work behavior (Astin, 1977; Chomal and Baruah, 2014; Pascarella and Terenzini, 2005; Roberts and Glick, 1981).

Disrupting Subculture: Higher Education, Moral Reasoning, and Ethical Climate

Theoretical work suggests a complex relationship between educational attainment and ethical behavior. In particular, Kohlberg's (1969) theory of moral development proposes that moral maturity is a byproduct of cognitive development and involves a multistage process whereby a person transitions from self-interested thinking to more enlightened reasoning that emphasizes the welfare of all citizens. That is, a morally progressive person critically evaluates dilemmas through an altruistic lens, and decisions about what is right are only made after deliberating the sides of all potentially affected parties (Juujärvi et al., 2010; Kohlberg, 1969, 1984).

Kohlberg (1984) posits that the ability to handle moral conflicts can be taught over time. In support of this view, research consistently demonstrates that educational attainment figures centrally in moral development (Bebeau and Thoma, 2003; Good and Cartwright, 1998; Hood, 1984; Pascarella and Terenzini, 1991; Rest, 1979; Rest and Thoma, 1985; Treviño et al., 2006). A robust literature indicates that moral agency increases during college years and tends to remain

in the years following graduation (King and Mayhew, 2002; McNeel, 1994; Rest, 1986; Rest, Thoma, and Bebeau, 1999). For example, using the Defining Issues Test (DIT)—an instrument developed to measure moral reasoning competencies—Bebeau and Thoma (2003) find moral reasoning increases consistently across the college career. Further, in a meta-analysis of 172 DIT studies, King and Mayhew (2002) concluded that intentional or not, educational attainment enhances moral judgment. A long record of research also shows that principled moral reasoning predicts moral conduct such as ethical workplace behavior (Arnold and Poneman, 1991) and whistleblowing (Brabeck, 1984). As such, these associations suggest that changes influenced by college education should result in morally grounded decision-making, resulting in fewer founded complaints of misconduct.

Work climate theory helps us incorporate contextual effects into this framework and provides guidance on the collective effects of worker values and characteristics. One such perspective is Victor and Cullen's (1987) ethical climate theory. Building on Kohlberg's work, they conceptualized ethical climate as "the shared perception of what is correct behavior, and how ethical situations should be handled in an organization" (1987, p. 51). Various organizational factors contribute to the ethical climate, including training standards, policy, reward structures, disciplinary processes, and the personal characteristics of employees (Kish-Gephart et al., 2010; Martin and Cullen, 2006; Newman et al., 2017; Treviño and Nelson, 2017; Victor and Cullen, 1988; Wimbush et al., 1997). Reflective of the ethical conventions and moral identity of organizations, group-based norms frame individual members' decision-making (Victor and Cullen, 1988). Misconduct occurs when the collective body of workers view aberrant behavior as trivial or matter of fact (Mayer et al., 2009).

In the context of policing, if department members widely view citizens as untrustworthy and unappreciative of police services (Skolnick, 1994), a culture permissive of misconduct can develop among the ranks (Crank, 2014; Kappeler et al., 1998; Paoline, 2003). The theory and research on educational attainment, moral development, and ethical climate (King and Mayhew, 2002; Kohlberg, 1969, 1984; Treviño and Nelson, 2017; Victor and Cullen, 1988), however, predict that as more college-educated officers join the police force, they will promote increased morality and greater recognition of civil rights. In this way, they are expected to upset the subculture and help cultivate a more ethical, service-oriented culture that encourages the fair treatment of citizens.

Prior Research

Higher Education and Police Misconduct

The research on higher education and police misconduct examines both officer and department level outcomes. Officer level studies suggest an inverse relationship between holding a bachelor's degree and misconduct (Brandl et al., 2001; Carter and Sapp, 1988; Harris and Worden, 2014; Kane and White, 2009; Kappeler et al., 1992; Lersch and Kunzman, 2001; Manis et al., 2008). For example, in one of the first studies in this area, Sanderson (1977) found that Los Angeles Police Department officers with 4-year college degrees received about a third fewer complaints than those with some college background or no college experience.

Fifteen years later, Kappeler and colleagues (1992) observed similar results among officers from a midwestern agency. Likewise, Kane and White's 2009 study of New York City police officers demonstrated that college education had a strong protective effect, as some college experience, completion of an associate's degree, and obtaining a bachelor's degree

were associated with a respective 72%, 80%, and 82% lower likelihood of committing career-ending misconduct than less educated officers. More recently, Harris and Worden (2014) reported that officers with a bachelor's degree were not only less likely to be named in complaints but when they were, the time between allegations was longer compared to officers without a degree.

In contrast, most studies employing agency-level data report no statistically discernable education effects (Cao et al., 2000; Cao and Huang, 2000; Eitle et al., 2014). The one exception is Shjarback and White's (2016) study of large city departments using the 2003 *Law Enforcement Management and Administrative Statistics* (LEMAS) data. They discovered that relative to agencies without a college requirement, the citizen complaint rate was about 10% lower for those requiring an associate's degree.

Cao and colleagues' two articles examining *Police Use of Force: Official Reports, Citizen Complaints, and Legal Consequences Project* (PUOF) data, on the other hand, indicated a statistically insignificant association between the proportion of bachelor-degreed officers and misconduct complaints. In the first analysis, Cao et al. (2000) observed that the stock of college-degreed officers exerted no measurable effects on citizen complaints of excessive, undue, and unnecessary use of physical force. Similarly, using a five-item police abuse of power index (i.e., unlawful arrest/detention, illegal search or seizure, harassment and intimidation, misuse of authority, and improper language per 1,000 officers), Cao and Huang (2000) found no meaningful education effects on police abuse of authority complaints. Like Cao and colleagues, our study also draws on PUOF data. As discussed in the next section, we apply more rigorous analytical strategies and make use of the entire sample to extend the literature.

The Current Study

Previous studies indicate both null and inverse associations between officer education and police misconduct. Several limitations underpin these mixed findings. First, few works on the educational antecedents of police misconduct examine a large sample of jurisdictions, with most inquiries drawing on individual-level data from a single department or a select sample of municipal agencies. Second, prior agency level studies focus on overall citizen complaints. Despite providing a rough barometer of citizens' perceptions of police, scholars concur that this measure confounds productivity with actual bad behavior (Brandl et al., 2001; Lersch, 2002; Terrill and McCluskey, 2002). Third, underscoring the need for more methodologically rigorous research, this collection of quantitative findings comes exclusively from observational data analyzed with correlational methods. Given the risk of bias due to selection, it remains unclear whether previous findings reflect the effects of education or pre-existing features of organizations and officers. Lastly, prior scholarship offers little theoretical justification for predictions about the officer education-misconduct link, making the expected direction of this relationship speculative.

We address these limitations to advance the literature in three ways. For our substantive contribution, we examine the collective influence of college-educated officers on substantiated agency complaint rates among a nationally representative sample of 1,023 police departments from the *Police Use of Force: Official Reports, Citizen Complaints, and Legal Consequences* data (Pate and Fridell, 1993). For our methodological contribution, this study employs design-based analysis and multiple imputation of missing data with a doubly robust propensity score model applied to a multivalued treatment. Finally, for our theoretical

contribution, we draw on perspectives from developmental psychology to explain the connection between educational attainment and police conduct. Based on theory and research (King and Mayhew, 2002; Kohlberg, 1969, 1984; Treviño and Nelson, 2017; Victor and Cullen, 1988), we predict that, all else equal, departments with a greater proportion of bachelor-degreed officers will generate fewer substantiated misconduct complaints.

Data and Methods

The data for the present study come from a national survey of U.S. police agencies conducted in 1992—the Police Use of Force: Official Reports, Citizen Complaints, and Legal Consequences Project (PUOF) (Pate and Fridell, 1993). Using a stratified sampling design to survey state, county, city, and local sheriff's departments, 1,697 agencies were initially invited to participate. Of the selected departments, 1,111 (67.2%) completed the mail survey (for full details, see Pate and Fridell, 1993). After we excluded agencies that do not answer service calls or hold arrest authority, our analytic sample stands at 1,023 agencies.

Measures

Dependent Variables

Police misconduct refers to "any inappropriate behavior on the part of any law enforcement officer that is either illegal or immoral or both" (Champion, 2001, p. 2). Examples include perjuring on reports, using excessive force, accepting bribes or gratuities, failing to report a fellow officer's wrongdoings, making unlawful arrests, and demeaning citizens. To capture the range of improper police behaviors, we consider three dependent variables representing different types of complaints filed by both citizens and department members.

The first outcome, *abuse of authority complaints*, reflects the rate of substantiated complaints per 1,000 officers about constitutional violations and incivility. Constitutional violations cover unlawful arrest/imprisonment, improper detention/interrogation, false charges, and illegal search or seizure. Acts of incivility include the use of demeaning ethnic/racial slurs, harassment, intimidation, threats, and verbal abuse. *Excessive force complaints* measures the rate of founded unnecessary use of force and brutality accusations per 1,000 officers, while *total misconduct complaints* is operationalized as the rate of all credible misconduct allegations per 1,000 officers (abuse of authority and excessive force).

Focal Variable

The focal variable, *college degree saturation*, was calculated by dividing the percentage of bachelor-degreed officers in an agency into tertiles based on the data distribution to represent low (<11.7%), medium (11.7% to 25%), and high levels (>25%) of agency educational attainment.

Covariates

We also include relevant controls, organized into four categories: 1) department composition, 2) training measures, 3) bureaucratic controls, and 4) jurisdiction controls.

First, drawing on prior research (Cao et al., 2000; Eitle et al., 2014; Schuck and Rabe-Hemp, 2016), we measure five dimensions of departmental composition, including *percent female*, *percent non-white*, *mean years of service*, *mean age*, and *department size*. Percent female is measured as the percentage of female officers in a department, and percent non-white represents the percentage of non-white officers. Mean years of service and mean age identify the average years of experience and age for all sworn officers in a police department,

respectively. The last measure in this group, department size, represents the total number of sworn officers on an agency's roster. We logged age and department size to achieve a more normal distribution.

The next set of measures controls for the effects of police training on police misconduct (Prenzler, 2009). Police training, for the most part, is a combination of pre-service training at the police academy and in-service training acquired throughout an officer's career (Morrison, 2006). The first training measure, *academy hours*, refers to the total training academy hours mandated for recruits. For the two remaining training measures, we conducted an exploratory polychoric factor analysis using promax rotation to identify the underlying factor structure among 20 candidate items on a 3-point Likert scale, with responses ranging from 1 (not provided) to 3 (mandatory). In contrast to the more common varimax rotation, promax rotation allows correlation among items and thus, improves our ability to approximate the factor structure (Fabrigar et al., 1999; Ford et al., 1986; Gorsuch, 1997; Tabachnick and Fidell, 2001; Thompson, 2004). Table A1 presents the results of the factor analysis, showing a two-factor solution with factor loadings ranging from .54 to .79 on the first factor and .66 to .92 on the second. These factors, conceptualized as *crisis intervention training* and *use of force training*, explain 92% of the total variance. Inspection of scree plot and eigenvalues verified the two-factor structure (see Figure A1). The Kaiser-Meyer-Olkin value (KMO = .94) and Bartlett's Test of Sphericity (p = .000) also met conventional standards (see Table B2.1 for items and factor loadings).

Accounting for 77% of the total variance, the first in-service training measure, *crisis intervention training*, consists of 4 items that evaluate whether a department provided in-

service training on the following topics: crisis intervention (e.g., crisis intervention and suicide prevention) and victim assistance (e.g., domestic violence and victim assistance). The second measure, *use of force training*, includes 4 items covering use of force training modules such as nonlethal force, nonlethal weapons, deadly force, and firearms requalification that explain 15% of the variance. Higher values signify more training emphasis in these areas.

Next, we account for bureaucratic controls implemented to govern police misconduct (Eitle et al., 2014; Hickman and Piquero, 2009; Terrill and Ingram, 2016). The first variable, *internal affairs*, is dummy coded to identify departments with an internal affairs division (1 = yes). *Public dispositions* distinguishes whether agencies publish summaries on misconduct investigations (1 = yes). *Citizen review* denotes whether an agency had a citizen review board (1 = yes).

The next two summative measures were modified from previous studies using PUOF data (Cao et al., 2000; Cao and Huang, 2000). *Reporting standards* was calculated by aggregating responses to 18 questions indicating the reporting policy for different use of force events (i.e., none, optional, or mandatory), including the use of deadly force (e.g., citizens killed), impact weapons (e.g., batons), and restraint techniques (e.g., handcuffing). We coded items to indicate stricter use of force reporting standards (α = .86).

For the next variable, *transparency*, we developed a standardized summative measure comprised of 45 items, of which 44 are binary and 1 ordinal. The first six items capture information on complaint avenues. Respondents were asked whether their department uses any of the following methods to inform citizens about complaint filing procedures: posters, flyers, newsletters, public service announcements, complaint hotline, or other measures. The

next six items concern the availability of the following complaint filing methods: anonymously, by mail, by telegram, by telephone (main department number), by telephone (designated number), or in person.

The next eight items were based on the question “Where can citizens file complaints of police misconduct?” (agency headquarters, precinct stations, storefronts, civil service commission, board of commissioners, civilian review agency, city hall, or other locations). The next 9-items were derived from the question “Which of the following types of assistance does your department provide when citizens file complaints?” (provides complaint form, provides bilingual complaint form, sworn officer completes form, civilian personnel complete form, assist non-English speakers, provides a copy of complaint report to citizens, informs citizen of case status, informs citizens of disciplinary actions taken, and other assistance). The corresponding question for the 9 items about places to file a complaint was “Does your department make complaint forms available for citizens at any of the following locations?” (agency headquarters, storefronts, fire stations, public libraries, public housing offices, city clerk's office, city council offices, community organizations, and other locations).

The next four items indicate whether departments required special steps for filing a complaint (sign complaint; swear to complaint; certify complaint; and notarize complaint) and whether complaints can be submitted at any time of day. The final item reflects the time limit for filing a report after an alleged incident (e.g., 0 = up to one month; 1 = one to three months; 2 = three to six months; 3 = six months to a year; 4 = more than a year; 5 = unlimited). Item responses were standardized and then summed so that higher values represent more

transparency and accountability in the complaint process (α = .74). The quadratic term for transparency is included in the model to capture any potential nonlinear effects.[6]

Our last category of variables controls for contextual effects. These measures include *large population*, *city agency*, *mean part I arrests*, and *mean part II arrests*. The binary variable, large population, denotes whether agencies served populations of at least 50,000 (1 = yes). While law enforcement agencies perform similar duties, their focus and the community's needs often vary. Thus, we include the binary variable, city agency, to account for these differences (1 = city department). Considering the link between high productivity and misconduct allegations (Hassell and Archbold, 2010; Lersch, 2002), we calculated the mean total part I (e.g., murder, rape, and burglary) and part II (e.g., simple assault, fraud, and gambling) arrests per officer.

Analytical Strategy

This study employs a doubly robust propensity score design, which estimates education effects by comparing the outcomes between departments of varying educational attainment levels. We applied sample weights to adjust for complex survey design and multiple imputation procedures to deal with missing data. Sensitivity analysis were conducted to compare treatment effect estimates from traditional regression and doubly robustly methods. We performed all analyses using Stata 15.1.

[6] Results from Poisson regression analysis revealed a significant positive linear term and negative quadratic term for the transparency measure.

Adjusting for complex survey design

A stratified sampling procedure was employed to select agencies by jurisdiction size to obtain a representative sample. PUOF investigators used a directory of law enforcement agencies from the 1990 Justice Agency List (JAL). Jurisdictions were sampled with probability proportional to size, with large jurisdictions sampled with certainty. We account for the complex sampling design in our analyses, adjusting for sample weights, primary sampling units (PSUs) (i.e., police agencies), and stratification in order to produce proper test statistics and achieve nationally representative estimates (Austin et al., 2018; Pate and Fridell, 1993).

While using sample weights with propensity score methods requires additional consideration, the little empirical guidance on survey-weighted modeling within a propensity score framework is mixed. Some works suggest incorporating sampling weights when estimating the treatment effect, but not when calculating propensity scores (Zanutto, 2006). Other scholars contend that while weighted regression is unnecessary for propensity score estimation, they recommend that analysts treat sampling weights as covariates (DuGoff et al., 2014). Ridgeway et al. (2015) demonstrate that incorporating sampling weights as survey weights (as opposed to as a covariate) throughout all stages of the analysis produces reliable results. Based on a recent evaluation of the various recommendations (Lenis et al., 2019), we follow the advice of Ridgeway and colleagues (2015) in this study.

Multiple imputation of missing data

PUOF data is missing a substantial amount of information on key variables, with missing data in our study ranging from .09% to 30.42% for specific variables (see Table 2.1). If not properly addressed, missing data can lead to biased treatment effect estimates. The pattern of

missingness informs how missing data should be addressed. According to Little and Rubin's (2002) taxonomy, there are three missing data patterns or mechanisms. Data are categorized as missing completely at random (MCAR) if the missingness is unrelated to the outcome, missing at random (MAR) if missingness is related to the observed outcome and possibly the observed covariates, or missing not at random (MNAR) if missingness is dependent on unobserved measures.

Since listwise deletion would reduce power and introduce potential bias, we implemented multiple imputation by chained equations (MICE) to preserve 839 cases (76%) without complete data. In addition to restoring the natural variability of the missing values, MI incorporates the uncertainty due to the missing data, which reduces the chance of obtaining a significant effect when the true effect is null. Achieving valid inference within an MI framework requires data to be MCAR or MAR (Rubin, 1976). However, a direct test of MAR is not possible since it requires unavailable information about the missing data. Instead, we can test the stronger missing completely at random (MCAR) assumption using Little's (1988) test. Although there is no evidence for MCAR, the sample met the MAR assumption based on the covariate-dependent missingness extension of Little's MCAR test (Little's CDM test: $\chi 2 = 23.14$, $df = 36$, $p = 0.95$).

Following standard practice, we include all measures, as well as the sample weights in the MICE prediction equations to obtain accurate point estimates and standard errors (Quartagno et al., 2019). Before producing a single imputed data set, m, the imputation models cycled through 10 repeated draws derived from the posterior predictive distribution of each measure. A two-stage quadratic rule-based equation (see von Hippel, 2018) calculated to

determine the correct number of m's indicated that our analysis required 144 imputed data sets.

Trace plots of imputed values verified convergence of the MI algorithm. We also tested the models for multicollinearity. All models had tolerance values (1/VIF) greater than .25, suggesting no multicollinearity issues (average variance inflation factors [VIF] = 1.8) (Adeboye et al., 2014).

Propensity score model

Agencies employing more bachelor-degreed officers likely differ on key characteristics from agencies on the other end of the educational spectrum. These differences might stem from hiring policies or could be connected to the demographic profile of an agency. Scholars note, for example, that minorities lack the same access to advanced educational opportunities relative to whites (Hillman, 2014, 2016). Consequently, departments requiring a bachelor's degree for new hires might have a lower percentage of minority officers. Our study draws on observational data, and given the non-random allocation into higher education, we must account for preexisting differences between departments to adjust for selection bias.

We employ a doubly robust propensity score method that combines the treatment modeling strategy of inverse-probability weighting (IPW) with the outcome modeling strategy of regression adjustment (RA) to balance covariates across weighted groups (Imbens, 2000; Funk et al., 2011; Lechner, 2001). Conditional on measured baseline covariates, IPWRA estimators use the inverse of the predicted probabilities of departments being assigned to each of the three educational attainment levels as weights to compute averages of treatment-level

predicted outcomes (Abadie and Imbens 2012). The contrasts of these averages provide the estimated treatment effects given the measured confounders.

This approach is "doubly robust" to model misspecification. The doubly robust feature allows for the consistent estimation of treatment effects even if one of the model specifications is incorrect (Wooldridge, 2010). Bias flowing from unobserved measures, however, can be reduced only to the extent that they correlate with observed characteristics (Funk et al., 2011).

Stata's implementation of IPWRA allows analysts to use a multivalued treatment. Supported by prior research and statistical theory (Hirano and Imbens, 2004; Wooldridge, 2007), this feature better captures the multivalent nature of educational attainment.[7] We estimate a multinomial logit treatment model and a Poisson outcome model, where $\{Y_i = Y_i(t), t \in T\}$ represents the substantiated misconduct complaint rate for department i and treatment status t indicates the different levels of education.

Covariate selection in propensity score (PS) analysis is key to the estimated treatment effects' efficiency and validity. Little empirical guidance exists, however, on covariate selection for PS models predicting the probability of selection into educational attainment. Both theory and simulations indicate that incorporating covariates affecting the outcomes while excluding those affecting treatment only, control for random or chance imbalances of predictors across treatment groups; thereby, improving the accuracy of treatment effect estimations (Brookhart et al., 2006; Myers et al., 2011; Wyss et al., 2013). Following these conventions, we estimated the influence of all study variables on the three outcomes and agency educational attainment

[7] We considered a continuous treatment, but data and model constraints forced us to abandon this plan.

in a series of covariate-adjusted regression models (Poisson regression for the outcomes and multinomial logistic regression for the treatment). Based on these findings, the probability of the observed treatment status (i.e., education level) is calculated from the following measures: mean years of service, mean age, academy hours, crisis intervention training, reporting standards, and city agency.[8] The multivalued treatment variable is depicted by $T = \{0, 1, 2\}$, where T represents the three educational attainment levels: 0 = low (reference category); 1 = medium; and 2 = high. For each department i, $Y_i = Y_i(t)$ if education level $T_i = t$ was attained.

Expressed as $Y_i(t_0) - Y_i(t_1)$ and $Y_i(t_0) - Yi(t_2)$, we compare the control group's (low attainment) t_0 rate of complaints with those of departments with medium and high educational attainment levels in the outcome model (t_1 and t_2, respectively). We included all measures in the outcome model. For each dependent variable, the differences between the average predicted outcomes for the three treatment levels provided average treatment effects on the treated (ATT) estimates.

The ATT is identified under two conditions jointly referred to as the ignorability assumption: conditional independence assumption and the overlap assumption (DuGoff et al., 2014; Heinrich et al., 2010; Rosenbaum and Rubin, 1983). The conditional independence assumption (CIA) or unconfoundedness states that once we control for observable pre-treatment characteristics (X_i), the selection into education level is as good as random and therefore uncorrelated with crime outcomes:

$$Y_{it} \perp T_{it} \mid X_i, \forall t \in \wedge = \{0, \ldots \phi)$$

[8] The measures, reporting standards and city agency are predictive of both the treatment and outcomes.

Here outcome Y_{it} and treatment T_{it} are independent ($\perp$), given the covariates X_i. The CIA is a strong assumption as it requires no unobservable confounders. However, we measure the most robust predictors of educational attainment (T_{it}) to enhance the accuracy of our estimates.

The overlap assumption, stated as $0 < Pr\,[T_i = tX_i = x]$, $\forall\ t \in \wedge$, holds that for every X_i there should be a positive probability that each department could be assigned to each education level. This assumption can be assessed by plotting the estimated densities of the probability of assignment into each education level (see Bang and Robins, 2005; Cattaneo, 2010; Hirano and Imbens, 2001; Linden et al., 2016; Robins et al., 1995; Scharfstein et al., 1999; Wooldridge, 2007 for further discussion on double robustness).

Covariate Balance

After weighting on the estimated propensity score, large residual differences in the distribution of the baseline covariates between the comparison groups remained. Following the suggestions of prior research (Garrido et al., 2013; Imbens and Rubin, 2015), we respecified the model using a stepwise approach and covariate adjustments (interaction terms) to achieve better covariate balance while maintaining the strength and direction of the initial ATT estimates. The final specification included indicators for the mean years of service, mean age, academy hours, crisis intervention training, reporting standards, city agency, and an interaction term between the mean years of service and mean age measures. Standardized mean differences (SMD) exceeded the threshold in 64% of covariates before weighting, but all covariates were within bounds in the weighted sample. Variance ratios met conventional criteria in both the weighted and unweighted samples (see Table B2.2).

Findings

Table 1 presents imputed descriptive statistics for the sample. The respective number of abuse of authority, excessive force, and total misconduct complaints per 1,000 officers is 23.48, 4.69, and 28.18. Overall, educational attainment was classified as low in 32% of agencies, medium in 35% of agencies, and high in 33% of agencies. Among departments, the mean percentage of female and non-white officers is 7% and 11%, respectively. About two-thirds of departments serviced urban jurisdictions (66%) and 68% served populations with more than 50,000 residents (see Table 2.1 for full descriptive statistics).

Panel B in Table 2.2 presents the ATT estimates for the effects of college degree saturation on the three outcomes. We observe no significant effects of educational attainment on misconduct rates in our main analysis. However, several findings worth noting. While the association between education level and excessive force complaint rates leaned negative as predicted, an unexpected pattern emerged for the two remaining outcomes. Albeit statistically insignificant, we detected evidence of a nonlinear relationship for abuse of authority and overall misconduct complaints.

Although misconduct transcends geography and agency type, reducing police misconduct among large urban departments has long been the focus of federal reform efforts (Chanin, 2017). As reported in Table 2.3 (Panel B), we conducted supplemental analyses on a subsample of 751 city agencies serving populations of 50,000 or more. Contrary to our expectations, all findings indicate a positive association, with abuse of authority and total misconduct significantly greater among the highest achieving departments in terms of educational attainment. In particular, for abuse of authority and overall misconduct complaints,

we find that departments in the medium and high education categories generate a respective 11 and 12 more complaints per 1,000 officers than departments with low education levels. This counterintuitive finding would indicate that educational attainment contributes to, rather than curb, the police misconduct problem in big cities.

Sensitivity Analysis

Educational attainment is not a random process and standard regression analyses that ignore nonrandom assignment in observational data risk biased and inconsistent estimates (Achen, 1986; Neyman, 1990; Rosenbaum, 2002; Rubin, 2006). Given the selection forces inherent in educational attainment, we conduct sensitivity checks against Poisson regression findings to demonstrate the consequences of not adjusting for covariate imbalance. Panel A in Table 2.2 shows three primary differences between the unweighted and propensity score weighted estimates. First, the treatment effect sizes from the doubly robust models were much larger than the regression estimates. For example, the doubly robust methodology returned treatment effect estimates for the most educated departments that were between 10 and 16 times larger than those generated by Poisson regression for the excessive force outcome. It appears that the lack of balance in covariate distributions may have suppressed the effects of education. Second, in contrast to the doubly robust findings, regression models indicated departments with medium and high educational attainment levels shared statistically significant associations with abuse of authority and total misconduct complaints. Lastly, the

nonlinear relationship detected between educational attainment and these two outcomes in the doubly robust analysis did not surface in the regression analysis.[9]

Discussion

Police reformers expect college-educated officers to promote more ethical police services that lead to lower misconduct rates (Carter and Sapp, 1990; National Research Council, 2004). However, prior research on the association between police education and misconduct is mixed. These inconsistent results arise from several limitations, including underdeveloped theory, nonrepresentative samples, and a reliance on correlational methods that fail to address endogeneity concerns. In response to this gap, we made various methodological improvements and incorporated interdisciplinary perspectives to examine the influence of departments' educational composition on misconduct complaint rates.

Specifically, we extend the literature by analyzing a nationally representative sample of 1,023 police agencies using a doubly robust propensity score weighting approach that adjusts for differences on key characteristics. Another key methodological advance includes multiple imputation of missing data. For our theoretical advancement, we apply Kohlberg's (1969) moral development perspective and Victor and Cullen's (1987) ethical work climate theory to shed light on the predicted influence of higher education on organizational outcomes.

Our primary findings showed that the proportion of bachelor-degreed officers in departments is not significantly related to the rate of abuse of authority, excessive force, and

[9] For comparability, we transformed regression coefficients into ATET estimates by calculating the difference between the reference category's predicted marginal effects and those for the two highest educational attainment levels.

overall misconduct complaints. Albeit insignificant, an interesting pattern emerged in our primary results. Excessive force findings leaned negative as anticipated. However, the results for the remaining outcomes indicated a nonlinear relationship. Though unexpected, we reason that these findings are related to police cynicism flowing from a disconnect between college-educated officers' attributes and their duties.

Scholars suggest that increased cynicism among officers—a well-known precursor to misconduct (Muir, 1977; Skolnick, 2002; Worden, 1996)—is largely indicative of departments not transforming their organizational structures to make full use of sworn personnel's characteristics and skills (Miller and Fry, 1976; Trojanowicz and Nicholson, 1976; Griffin et al., 1978). In traditional police bureaucracies, rigid regulations imposed on lower-level officers prevent those with college experience from engaging in a more autonomous and innovative style of policing. Such autocracy results in low morale and job dissatisfaction (Walker and McDonald, 2009).

Several studies indicate that college-educated officers tend to be more cynical and frustrated with the job and distrustful of citizens than their less-educated colleagues (Dantzker, 1992; Graves, 1996; Niederhoffer, 1967; Osborne, 2014; Sobol, 2010a; Paoline et al., 2000; Paoline et al., 2015). Regoli (1976) concluded that the noted misalignment causes college-educated officers to become frustrated and cynical. When department members widely hold such views, the risk for misconduct increases (Skolnick, 2002; Wilson, 1968). Thus, we speculate that agencies with medium levels of education may not yet have revamped their operations to mesh with their personnel. In contrast, those at high education levels might have begun making strides in that direction.

This logic, along with larger city departments being more prone to greater cynicism and police misconduct complaints (Klinger, 1997, 2004), might explain the positive and statistically significant associations observed in our supplemental analysis of big-city agencies. We found that the most educated departments serving large jurisdictions generated roughly 11-12 more abuse of authority and overall misconduct complaints per 1,000 officers, respectively, than agencies with low educational attainment.

Relatedly, it is possible that our findings also reflect time period effects. Changing culture in complex bureaucracies is difficult, and subcultural roots run deep in policing. Scholars assert that beyond personnel decisions, for a culture shift to occur in law enforcement, police practices must also be reoriented toward the desired change (Lum and Nagin, 2017). Along this vein, law enforcement did not widely implement community policing programs believed to take advantage of college-educated officers' skills and attributes until the passage of the 1994 Violent Crime Control and Law Enforcement Act—two years after our data was collected (Oliver, 2000). It is also worth mentioning that Section 14141 of the Crime bill authorized the Department of Justice (DOJ) to investigate and seek injunctions against police departments displaying a "pattern or practice" of police misconduct and civil rights violations (D'Souza et al., 2019). Thus, we reason that the potential offsetting effects of the pre-Crime bill era on police misconduct suggest our study should be replicated with post-Crime bill data. Such a conclusion also indicates that community policing innovations might facilitate the effects of higher education, suggesting the need for mediation analysis.

Beyond addressing potential measurement issues related to misconduct (see Fyfe, 1986; Lersch, 2002; Sherman, 1978) and the absence of structural factors in our analysis (e.g.,

unemployment rate), several other ways to improve our study deserve mention. First, situational factors and offender characteristics are essential in understanding police misconduct (Hassell and Archbold, 2010; Terrill and Mastrofski, 2002) and should be explored in future research. Second, we could only assess the influence of bachelor-degreed officers on police misconduct rates due to data constraints. Subsequent research would benefit from replicating this study with sworn personnel of varying education levels. Third, our study relies on cross-sectional data. Considering cultural change in police organizations is a gradual process (Cohen, 2017), a longitudinal analysis of trends in agency education levels and misconduct complaints could yield additional insight. Fourth, a remarkable amount of missing data plagues PUOF. This challenge may partially explain why PUOF data only appears in a few articles, even though it represents one of the most exhaustive data sources for studying police misconduct. Since a complete case analysis would likely generate inaccurate results, we implemented a multiple imputation procedure to increase the confidence in our findings. Despite the lengths taken to soundly deal with missing data, the possibility remains that our estimates are biased. Thus, these variables should be measured more reliably in future research. Lastly, unmeasured confounding is a perpetual concern in police education research. While propensity score weighting addresses observed differences in defining characteristics, considering alternative approaches such as instrumental variables can provide additional insights about potential sources of bias.

Conclusion

In this study, we were unable to find evidence of educational attainment's protective effect against police misconduct. Interestingly, the excessive force findings were insignificant in

both samples but were negative for the full sample and leaned slightly positive in the big-city sample. In addition, we observed an insignificant nonlinear association in the full analysis and statistically significant positive association between departments with high education levels and abuse of authority and overall misconduct complaints in the supplemental analysis of large city departments.

We acknowledge that the findings may be attributable to several limitations, including a large amount of missing data for the outcomes. Nonetheless, this study offers an important contribution to the literature. We demonstrate the theoretical and methodological rigor necessary for examining the police education-misconduct link by addressing selection bias issues, complex survey design, and missing data challenges, all of which have been glossed over in prior research.

We urge readers not to cast judgments about the importance of education based on our study. However, the findings suggest that initiatives focusing solely on bolstering the educational profile of U.S. police forces is not likely not eliminate misconduct. Doing so may even worsen the misconduct problem considering conventional police organizations generally constrain the discretion and creativity of college-educated officers, leading to increased police cynicism and job discontentment among the ranks. For agencies looking to recruit and hire college-educated officers, these findings might suggest the associated institutional and police subculture changes may not result in a direct benefit on misconduct rates. Still, other potential benefits remain unexplored, including how education effects may depend on how agencies utilize officers. In particular, agencies that have high educational standards and support

progressive policing strategies may lead to lower misconduct. In other words, there are unexplored heterogeneities for future research.

3 The College Sentinel: Department Education Policy and Crime Control

Introduction

From the earliest police reform efforts to present times, politicians and leading scholars have regularly proposed more stringent police education standards as one path toward increasing public safety (e.g., see President's Commission on Law Enforcement and the Administration of Justice, 1967; President's Task Force on 21st Century Policing, 2015; Vollmer, 1931). Underpinning these calls is the optimistic assumption that adopting higher education standards will help enhance the credibility, effectiveness, and overall quality of law enforcement (Carter and Sapp, 1990). It also appears that officials believe that college-educated officers possess unique qualities and attributes advantageous to crime control. Such expectations, for example, were evidenced by Congress' authorization of $200 million in college scholarships for in-service and aspiring officers under the 1994 Violent Crime Act (42 U.S.C. § 14091) to "address violent crime by increasing the number of police with advanced education and training on community patrols."

Despite prominent recommendations and federal investments, it remains an open question whether raising education standards enhances crime control, especially as it concerns the core police function of reducing serious crime. Whereas a sizeable literature exists on the crime-suppressing effects of enhancing agency resources (e.g., force size) and instituting proactive policing (e.g., broken windows policing) (see Braga et al., 2012; Levitt, 2002; Weisburd et al., 2014), no studies to date have explicitly analyzed the role of police education policy in crime prevention. This marks a critical gap in the literature, as observers have consistently maintained that police qualifications directly influence the quality of police services

(Sanders, 2003). For example, the early police reformer Owen W. Wilson (1953, p. 840) once said:

> Police service is rendered by individual policemen on a person-to-person basis. Its quality, therefore, is determined by the individual men who provide it, and obviously cannot be raised above the quality of service rendered by the individual members in the aggregate. If police service is to be of a high caliber, the members must have suitable qualifications…

The few available studies examining the impact of agency education policies on both violent and property crime control suggest null effects (Chappell et al., 2006; Doerner and Doerner, 2012; Eitle, 2005; Eitle et al., 2005; Tillyer, 2018). However, our understanding of this association remains formative because education policy indicators are not focal in prior research. Thus, this relationship has never been treated rigorously in the literature.

Considering the periphery focus on education policy's crime-reduction effects, the literature suffers from three concomitant limitations. First, this scholarship is largely atheoretical, with hypotheses often based on conventional wisdom. Second, researchers have tended to use correlational research designs, which cannot rigorously address endogeneity concerns in this literature. Lastly and most salient to this study, the underlying processes that potentially mediate the effects of education policy on agency outcomes remain unexplored. Yet, given educational attainment's positioning as a proxy for police effectiveness (Doerner and Doerner, 2012), from a deterrence perspective, education policy may not only have a direct effect on crime but may also affect crime indirectly through its impact on police arrest productivity.

The policy-relevant question then is not just whether higher education requirements have an overall effect on crime but also how these standards achieve their outcomes. Our analysis of this association enriches the literature in two ways. First, this is the only explicit examination of departmental education requirements' effects on offending. What is more, we are the first to assess the potential mechanisms by which these effects occur. The other advancement is our infusion of economic and criminological theory to formalize the relationship between police education standards and crime control.

We capitalize on interdisciplinary perspectives and data from multiple sources to assess the education policy-crime link. Methodologically, we integrate entropy balance reweighting procedures with a novel mediation approach to identify the total, direct, and indirect effects of agency education policy on crime rates in 1,543 U.S. cities using data from the Law Enforcement Management and Administrative Statistics (LEMAS) survey, the U.S. Census Bureau's American Community Survey (ACS), and the FBI's Uniform Crime Reports (UCR).

The outline for the paper is as follows. We begin by discussing the theoretical expectations for the effects of police education policy on crime control. Next, we review the literature in this area, pointing out research limitations and blind spots warranting closer examination. Following the presentation of the data, methods, and findings, the paper culminates with the study limitations, policy implications, and future directions for research.

Theoretical Context

Studies examining the effects of police education policy on crime control are frequently mute about the theoretical underpinnings of this association. In this section, we present an integrated framework that builds on insights derived from economic and criminological theory.

Education Policy and Organizational Productivity

Human Capital Theory (HCT) can be used to conceptualize the potential benefits of higher education in policing through the lens of work output or productivity (Becker, 1964; Mincer, 1958; Schultz, 1959). According to HCT, organizational investments in human capital (i.e., enhancing education of workers) will increase employee productivity as a function of increased knowledge, innovation, and commitment (Becker, 1964; Namasivayam and Denizci, 2006; Schultz, 1959). Education figures centrally in the development of human capital, which refers to the skills and intangible assets (e.g., motivation and ambition) people possess that add value to an organization (Becker, 1964). These theorists posit that advanced education improves individuals' cognitive abilities and teaches workers valuable skills that make them more productive than their less-educated colleagues (Becker, 1962; Schultz, 1963).

The human capital literature specifies three channels through which education affects productivity (Rosenzweig, 1995). First, education broadens a worker's access to various information sources. Second, education improves workers' ability to decipher and understand new information. Third, education increases a worker's ability to learn from prior experiences.

Like any other profession, law enforcement has its own, albeit crude, metrics for productivity in the form of arrests, citations, and seizures (Chalfin and McCrary, 2017; Hodgkinson et al., 2019; Sparrow, 2015; Wellford and Cronin, 1999). Of these, arrest represents law enforcement's de facto performance measure, mainly due to its two primary functions: bringing criminals to justice and reinforcing the police's ability to solve crime and apprehend offenders (Harmon, 2015). If the assumptions of HCT hold in policing, we expect departments

with college requirements to show greater prowess at preventing felony crime through increased police arrest productivity (i.e., more arrests).

Arrests as a Crime Deterrent

Four years after his publication on HCT, Becker (1968) introduced a seminal economic model of crime predicting the deterrence effects of arrest and sanctions. While economists approach this question with a focus on enforcement efficiency, similar to criminologists, they maintain that the decision to offend reflects a "gamble undertaken by a rational individual" who weighs the expected cost of committing a crime against the probability of apprehension (Chalfin and McCrary, 2017, p. 5). In other words, if the crime costs (e.g., arrests) outweigh the potential return (e.g., money, respect, and revenge), individuals will likely abstain from offending.

While severity and swiftness of punishment represent critical determinants of deterrence, the linchpin principle is the certainty of punishment. As concluded by Nagin's (2013) review of the literature, relative to the other crime control mechanisms, the certainty of apprehension exerts the greatest effect on crime. The hypothesized deterrence effects of arrests thus manifest when the perceived risk of being caught is high enough to discourage offending among both arrested persons (specific deterrence) and the broader society (general deterrence). From this perspective, cities with police departments more effective at locating and apprehending criminals should experience lower crime levels.

Despite the utility of deterrence theory for understanding the police-crime link, it is not without criticism. For one, scholars have lamented the difficulty of separating the incapacitation effects from the deterrence effects of arrests. Nonetheless, recent works provide little evidence

in favor of incapacitation effects (Chalfin and McCrary, 2017; Levitt, 1998; Owens, 2013), leading researchers to conclude that the deterrence effects of arrest are empirically more important in reducing crime (see Chalfin and McCrary, 2017). For example, Levitt (1998) addressed early criticisms of measurement error and attributed the crime-suppression effects of arrests, especially for property crimes, mainly to deterrence rather than to incapacitation. Second, some empiricists challenge the notion that intensified arrest activity averts all offending equally (e.g., crimes of passion). Countering this point, scholarship employing econometric techniques find that various property and violent crimes are highly responsive to the prospect of arrest (Bun et al., 2019). Finally, critics contend that while apprehension rates may deter arrested persons, would-be offenders in the community go undeterred. There is mixed evidence on this point. A growing literature, however, supports the general deterrence value of arrest-based strategies such as hot-spots, problem-oriented, and order-maintenance policing (Braga et al., 2012; Kubrin et al., 2010; Weisburd et al., 2014).

Framing the Police Education-Crime Nexus

Together, human capital and deterrence perspectives provide theoretical justification for studying the mediating effects of arrest productivity on the association between education policy and offending. This joint framework suggests that agencies with a college requirement should be more effective at preventing crime. Research indicates that law enforcement's ability to apprehend lawbreakers impacts citizens' perceptions of arrest risk that are integral to crime deterrence. Thus, we predict that increased police arrest productivity, informed by education policy—a proxy for agency educational composition, disincentivizes crime and therefore, reduces offending.

Prior Research

Police Education Policy and Crime

Despite numerous commentators contending college requirements for officers are fundamental to improving police effectiveness against crime, this link has received limited scholarly coverage. We located just two published studies exploring this relationship, with each returning different results. In the first study, MacDonald (2002) used 1993 and 1997 LEMAS data in a pooled cross-sectional analysis of city-level robbery and homicide rates across two periods (1993-1994 and 1997-1998). His findings showed that while agency education policy exerted minimal effects on homicide rates, an associated 10% decrease in robbery rates for departments with a college requirement was observed.

Over a decade later, Sozer and Merlo (2013) turned to 2003 LEMAS data to examine the effects of community policing on violent crime and property crime. Using a series of regression models, the authors reported negligible educational effects for both small and large agencies. Interestingly, whereas a non-significant positive leaning relationship between education policy indicators and property crime was seen across all agencies, for violent crime, this association leaned negative in smaller agencies and positive in larger ones.

These authors, as well as the others cited in this section, failed to discuss education findings thoughtfully. We reason that the periphery focus on education and the equivocal state of the literature precluded them from doing so. Nonetheless, these findings underscore the need for more detailed analysis of a national sample. Both studies also focused on the direct effects of education policy, overlooking potential mediators that may partially explain the mixed findings. Based on our theoretical rationale, we expect that agencies requiring some

college experience for new officers will generate more arrests than those without such a policy.

Police Education Policy and Arrest Rates

In the few published accounts of education's effects on city arrest rates, mostly null effects have been reported. Except for Doerner and Doerner's (2012) analysis of Florida police agencies, wherein an insignificant inverse association was found between education requirements and property crime clearances, and a positive relationship with violent crime clearances, most research in this area tends to be cross-sectional, multijurisdictional, and based on older data (Chappell et al., 2006; Eitle, 2005; Eitle et al., 2005; Tillyer, 2018). Eitle and colleagues, for instance, published two papers in 2005 using 2000 LEMAS and National Incident-Based Reporting System (NIBRS) data. Eitle et al.'s (2005) multilevel analysis of arrest probabilities for simple assault and aggravated assault across 105 cities yielded mixed results. Whereas educational minimums (i.e., 1 = no requirement to 5 = 4-year college degree) did not appreciably influence aggravated assault arrests, they observed a .17 increase in the probability of simple assault arrests per education level. Contending that more educated police forces may be relatively younger, they considered this positive association to be suggestive of greater formalism in better-educated departments that limit discretion. However, this explanation fails to square with their reported findings, as arrest decisions for simple assault are likely more discretionary than for felony assault. Moreover, using the same sample and methodology, Eitle's (2005) analysis of education policy effects on domestic violence arrests across U.S. cities found no significant effects.

Next, using 1997 LEMAS data, Chappell and colleagues' (2006) nationwide study of city agencies detected a statistically insignificant inverse association between departmental education minimums and arrest rates for both overall and violent crime. Lastly, Tillyer (2018) conducted the most recent LEMAS study in this area. In investigating community policing's impact on arrests for kidnapping, robbery, and violent index offenses across large agencies, he transformed education policy information from the 2007 LEMAS data into a series of dichotomous control variables capturing agencies' minimum education requirements (i.e., high school, some college, two-year degree, four-year degree, or no educational requirement). Again, like the other studies cited here, education policy was not predictive of arrest.

While the crime control literature indicates that departmental education requirements are not significantly associated with felony arrest rates, scholars report increased productivity among college-educated officers in the form of more stops, searches, and general arrests (Glasglow, Green, and Knowles, 1973; Rojek, Rosenfeld, and Decker, 2012; Rosenfeld et al., 2020). We are mindful of the ecological fallacy of inferring macro-level processes from individual-level relationships (Robinson, 1950); yet, the lack of comparable studies places us in mostly uncharted empirical waters. However, we draw on insights from individual-level evidence to better understand how HCT may apply in police contexts. Based on our theoretical rationale, we expect that agencies requiring some college experience for new officers will generate more arrests than those without such a policy.

The Present Study

Our review of the literature failed to locate a single focal analysis of the education policy-crime control association. Any insight into the effects of education policy on crime come

from studies in which education policy was not the primary intervention of interest. Thus, the literature suffers from three related shortcomings. First, the theory remains underdeveloped, making past predictions about this relationship speculative. Second, the lack of focus on education policy indicators has left endogeneity issues unattended. Given the reliance on observational data and correlational methods, prior research has not rigorously addressed selection bias. Third, potential mediating factors underlying this association have gone unexplored. The theoretical perspective outlined above posits that department education policy effects on crime may not only be direct but may also operate through police arrest productivity.

We address these limitations to advance the literature. The novelty of this study is the theoretical grounding and the evaluation of the mediating influence of police arrest rates or arrest productivity on the crime-suppressing effects of departmental education requirements. Drawing on a national sample of 1,543 U.S. cities, we employ a recently developed entropy weighted mediation approach that accounts for selection on education policy and the subsequent mediator to minimize endogeneity bias. Our underlying theoretical framework predicts that productivity mediates the effect of education policy on crime rates.

Methods

Data

We collated data from multiple sources. First, to account for police education policies and other agency measures, we draw on the Bureau of Justice Statistics' (BJS) 2013 Law Enforcement Management and Administrative Statistics (LEMAS) data that contain information about agency personnel, expenditures, operations, equipment, computers and information

systems, and written policies from 2013. We use 2013 data because it represents the most recent wave of available LEMAS data. While LEMAS 2013 does not provide information on the number of college-educated officers in a department, the dataset reports education policy indicators not available in other data sources.

Next, to control for city-level factors such as socioeconomic conditions and racial composition, we employ data from the U.S. Census Bureau's American Community Survey (ACS) 5-year (2009-2013) estimates for principal cities of metropolitan or micropolitan statistical areas data files. Finally, crime and arrest data come from the FBI's Uniform Crime Report (UCR) Offenses Known to Law Enforcement, Clearance, and Persons Arrested data for 2013 and 2014. UCR data provide counts of offenses and arrest by crime categories and includes information about the reporting agency.

Both LEMAS and UCR data report a unique agency identifier or Originating Agency Identifier (ORI) code. Using BJS's 2012 Law Enforcement Agency Identifiers Crosswalk file that associates each agency's ORI code with a Federal Information Processing Standards (FIPS) code indicating state, county, and jurisdiction location, we matched the ORI and FIPS code to merge LEMAS and UCR data with ACS data. Initial merging of the UCR and LEMAS data matched on 1,998 city agencies. After combining these two data sources with ACS data and removing observations with missing data, our analytic sample consisted of 1,543 municipal law enforcement agencies.

Outcome Measures

We assessed the effects of education policy on crime control using six dependent variables: homicide, personal robbery, aggravated assault, burglary, theft, and motor vehicle

theft rates per 100,000 city residents.[10] Whereas the 2013 LEMAS and UCR arrest data, respectively, provide our education policy and police arrest productivity measures (see below), we use the 2014 UCR crime data for the outcomes. This approach was taken for three reasons. For one, lagged education policy effects are plausible since police arrest productivity could take some time to influence the apprehension risk perceptions of residents (see, e.g., Corman and Mocan, 2000; Mustard, 2003). This variable sequence also accounts for the simultaneous relationship between arrest rates and crime rates (Greenberg, Kessler, and Logan, 1979; Rosenfeld and Wallman, 2019). Finally, although we do not resolve all temporal questions, this approach attempts to temporally order the education policy/arrest productivity/crime association.

Education Policy Indicators

We identify the effects of departments' educational requirement using the binary focal measure, *education minimum* (1 = some college or higher) ("What was your agency's minimum educational requirement for sworn new hires?").

Mediating Variables

Considering perceptions of police arrest productivity can appreciably impact offender decision-making (Chalfin and McCrary, 2017; Levitt, 1998; Owens, 2013), we examine the mediating effects of city-level violent crime and property crime arrest rates.[11] Violent and

[10] We omitted rape offenses because of well-documented reporting and recording issues regarding rapes.

[11] This measurement decision is somewhat arbitrary and arises out of modeling concerns. The treatment-effect/mediation analytic framework we employ cannot handle a continuous mediator since the statistical theory

property crime arrest rates are calculated as the respective arrest rate per 100,000 residents for violent offenses (homicide, aggravated assault, robbery) and property offenses (burglary, larceny, motor vehicle theft) in 2013. Both measures were dichotomized by a median split to categorize departments as having high or low arrest productivity. While offenders tend not to specialize in a crime type (e.g., only aggravated assault), some evidence indicates that they can be broadly categorized as either property or violent offenders. To better tap the deterrence effects of arrests, the violent crime and property crime arrest indicators serve as mediators in our violent and property crime models, respectively.

Contextual Measures

In addition to the education policy measures, we incorporate controls for other relevant departmental and structural factors found to be important in prior research (Braga, 2015; Chappell et al., 2006; Land et al., 1990; Levitt, 2002; Lockwood, 2014; Lum et al., 2017; Maguire, 1997; McCall et al., 2010; Pratt and Cullen, 2005; Rosenfeld and Wallman, 2019; Tillyer, 2018).

Police agency measures

Measures for *percent nonwhite officers*, as well as *percent female officers*, control for the effects of a department's racial and gender composition. *Police strength*, a proxy for police resources and response capacity (Levitt, 2002; Tillyer, 2018), is measured as the number of sworn police personnel per 100,000 residents. The measure, *administrative density*, or the

and implementation in Stata is underdeveloped. Accordingly, we dichotomize the mediator as needed for the model estimation.

number of full-time nonsworn staff per 100,000 residents, taps the level of administrative support available (Maguire, 1997). *Investigative capacity* and *patrol capacity* are operationalized, respectively, as the percentage of officers assigned to investigative duties and patrol duties. We calculated *fiscal capacity* as law enforcement expenditures per capita (total yearly police department expenditures divided by the number of residents) to measure agencies' ability to pay employees and resource capacity (Briggs et al., 2008).

We incorporate an additional agency control for the effects of community policing strategies on crime and arrest rates (Chappell et al., 2006; Litwin, 2004; Lum et al., 2017; Roberts, 2008; Tillyer, 2018). To account for these strategies, we adapt the summary construct, *community-oriented activity*, from Tillyer (2018) and Shjarback and White (2016). This measure consists of five LEMAS items with binary responses (1 = yes) indicating whether agencies: 1) actively encouraged patrol officers to engage in SARA-type problem-solving projects on their beats; 2) included collaborative problem-solving projects in the evaluation criteria of patrol officers; 3) regularly assigned the same patrol officers' primary responsibility for a particular area or beat within your agency's jurisdiction; 4) had a problem-solving partnership or written agreement with any local civic, business, or governmental organizations; and 5) conducted a community survey on fear of crime and police satisfaction. Higher values correspond with greater engagement in community policing ($\alpha = 0.62$).

Structural measures

As Wellford and Cronin (1999) point out, factors beyond police control like economic deprivation, family disruption, and racial segregation influence both crime and the police's ability to apprehend felony offenders. The next group of predictors reflect jurisdiction-level

population, economic, and demographic characteristics drawn from the U.S. Census Bureau's American Community Survey (ACS) data files. *Population*, which identifies the count of residents in a jurisdiction, is logged because of its highly skewed distribution. *Percent nonwhite residents* indicates the percent of minority residents, and *percent immigrant residents* represents the percent of foreign-born residents. The percent of renter-occupied housing represents *residential instability*, and we operationalized *educational attainment* as the percent of adults with a bachelor's degree. The *economic disadvantage* measure is derived from an exploratory factor analysis of 7 candidate items using promax rotation. With higher values reflective of greater economic deprivation in cities, this measure loaded high on city poverty rate (.919), percent of households receiving public assistance (.900) median household income (–.763), percent female headed households with young children (.727), and city unemployment rate (.686) (total explained variance = 94%). Scree plot of eigenvalues (see Figure C3.1), Kaiser-Meyer-Olkin value (KMO = .83), and Bartlett's Test of Sphericity (p = .000) verified that factor analysis was applied appropriately.

Analytical Strategy

In this section, we detail our empirical approach and the treatment of missing data. Deterministic imputation was used to replace missing data with values predicted in a regression model. We perform an entropy weighted mediation analysis to identify two primary estimates. First, the total effect quantifies the overall influence of police education policy on city crime rates. Second, mediating effect quantifies the proportion of the total effect transmitted through a police arrest productivity indicator.

Missing Data

Arrest information for homicide, robbery, and aggravated assault offenses was missing on 175 observations. Rather than discard a large number of observations due to missingness on three variables, we applied deterministic imputation to substitute missing data prior to operationalizing the arrest mediator (White and Thompson, 2005). Deterministic imputation procedures were selected because our analytical procedures do not accommodate multiply imputed datasets. We utilized all measures described below, and the imputed values replaced the missing arrest data (Graham, 2009). Similar outcomes were observed when we conducted complete case analysis as a robustness check (data not shown). We checked the models for multicollinearity and tolerance values (1/VIF) suggested no multicollinearity issues (average variance inflation factors [VIF] = 1.8). All analyses were conducted with Stata 15.1.

Entropy Balancing

Policy adoption does not happen randomly. Policy implementation is purposive, goal-oriented, and changes over time (Béland et al., 2016; Monroe, 1998). In law enforcement, economic, organizational, political, and geographic factors might influence which departments establish college education requirements. For example, some police chiefs believe that a degree requirement would drastically reduce an already shrinking applicant pool (Gardiner, 2017), making higher education requirements less appealing for understaffed or budget-strapped departments. Thus, failure to account for the endogeneity on education policy indicators may lead to biased estimates since other features could influence this association.

In this study, departments requiring a college background make up the treatment group, and those not requiring college comprise the control group. To eliminate statistical

differences in observed covariates between these groups, we used an alternative weighting method known as entropy balancing. Developed by Hainmueller (2012), the entropy-balancing algorithm provides an optimal weighting solution to balance covariate distributions in observational studies with binary treatments. This process reweights the data from the control group's observations to match the moments (e.g., means, variances, and skewness) computed from the treated observations' data.

Like propensity score methods, entropy balancing aims to achieve statistical equivalence among comparison groups. The principal advantage of this approach over propensity score strategies is that entropy balancing directly calculates weights to adjust for known sample distributions, integrating covariate balance into the weights to equalize the distribution moments for education policy predictors among comparison samples. This approach ensures that the comparison groups match each other exactly on specified moments (Hainmueller, 2012).[12]

To guard against potential selection bias and improve confidence in the ignorability assumption (see below), we condition the analysis on a rich set of empirically informed covariates. Prior research provides an indication of relevant factors for the outcomes, but we are less certain about the factors that would balance the treatment. However, our choice of variables for the balancing equation was guided by a combination of police compensation studies, human resources literature, and organizational theory (Donaldson, 2002; Eitle et al., 2005; Giblin and Galli, 2017; Zhao et al., 2010). Using Poisson and logistic regression analysis to

[12] The mean is the first moment, variance is the second moment, and skewness is the third moment.

determine the respective predictors of the outcomes and treatment, we identified a parsimonious and robust set of pretreatment measures that include: percent non-white officers, percent female officers, police strength, administrative density, investigative capacity, patrol capacity, fiscal capacity, population, percent nonwhite, percent immigrant residents, residential stability, educational attainment, and economic disadvantage. All measures are predictive of the outcomes and except for three measures (investigative capacity, patrol capacity, educational attainment, and economic disadvantage), they are also significantly related to the treatment. We balanced samples on the first moment. Table C3.1 shows that after weighting, the mean differences between the comparison samples were reduced to zero. The entropy weights were then included as weights in the subsequent mediation model estimation.

Mediation Analysis

After obtaining the weights, we conducted a formal mediation analysis to decompose the total effect or average treatment effect on the treated (ATT) of agency education policy on subsequent crime rates into two separate effects: 1) natural indirect effect representing an arrest-mediated mechanism, and 2) natural direct effect representing all other potential mechanisms. Specifically, we implement a ratio-of-mediator-probability weighting (RMPW) method of mediation analysis introduced by Hong (2010). The RMPW approach estimates the natural indirect and direct effects through mean contrasts between estimated effects of college education requirements and the counterfactual condition of no requirement.

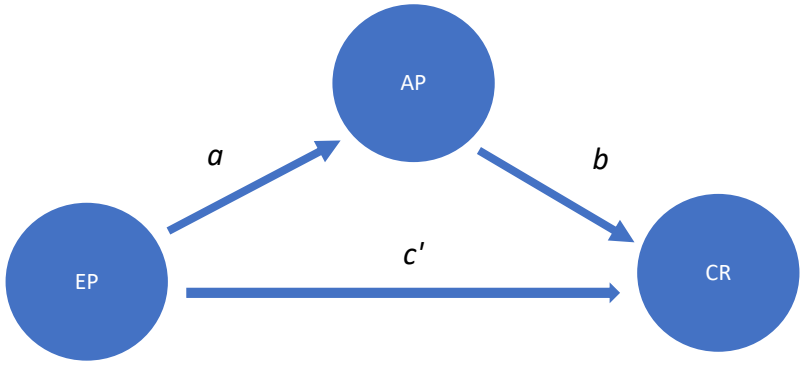
We consider these effects “natural” rather than “controlled” because the mediators can take random values, under each policy condition, that vary naturally across agencies rather

than taking fixed values controlled by analysts (Pearl, 2001). Unlike traditional mediation methods, the RMPW approach estimates the conditional probability of each mediator value under each treatment condition as a function of covariate values to explicitly define the direct and indirect effects. Thus, this estimator produces more reliable findings than conventional methods (Hong et al., 2015).

Let *X*, *T*, *M*, and *Y* denote covariates, treatment indicator, mediator, and outcome, respectively. The counterfactual direct and indirect effects are defined as the expected difference between two potential outcomes for education policy adopters and non-adopters: $E(Y_{T,M(T)} - Y_{T^*,M(T^*)})$; where $Y_{T,M(T)}$ signifies what a city's crime rates (Y) would be given two conditions: 1) the agency educational minimum (T = 1 for college requirement, 0 for no requirement); and 2) the "natural" high arrest rate jurisdictions versus those with low arrest rates under the same assigned education policy condition ($M_{(T)}$). In short, the natural direct effect estimates the impact of a college requirement on lagged city crime rates if college education policy did not influence city arrest rates [$E(Y_{1,M(0)} - Y_{0,M(0)})$]. The natural indirect effect estimates the impact of a college requirement on lagged crime rates as mediated by arrest rates [$E(Y_{1,M(1)} - Y_{1,M(0)})$].

The mediation model is depicted graphically in Figure 3.1. Education policy influences arrest productivity (a) which in turn influences city crime rates (b); education policy may also

Figure 3.1. The relationship between departmental education policy (EP), arrest productivity (AP), and crime rate (CR)



affect subsequent crime either directly or through unobserved alternative mechanisms (*c'*). The statistical model representing the graph in Figure 3.1 has a linear equation for the crime outcome and a logistic equation for the arrest mediator, which we dichotomized into low and high rates to accommodate modeling constraints.

The key identifying assumption is the sequential ignorability assumption which expresses that, for $t, t' = 0, 1; m \in M$,

$$\{Y_i(t, m), Y_i(t', m), M_i(t), M_i(t')\} \perp\!\!\!\perp T_i \mid X_i$$

$$Y_i(t, m), \perp\!\!\!\perp \{M_i(t), M_i(t')\} \mid T_i = t, X_i.$$

This assumption holds that within levels of the observed covariates, treatment status and the mediator value under each treatment condition is as "good as random" (Hong et al., 2015).

Findings

Table 3.1 shows summary statistics for the study variables. The sample of 1,543 cities averaged 3.9 homicides, 82.4 robberies, 218.6 aggravated assaults, 555.3 burglaries, 2,074.1

thefts, and 179.9 motor vehicle thefts per 100,000 residents in 2014. Regarding officer demographics, a minority of agencies required some college education (16.9%), female officers made up a small fraction of sworn personnel (8.6%), and the mean representation of non-white officers was modest (15.5%). Other contextual factors indicate nearly a quarter of all city residents were non-white (24%) and roughly 5%, on average, were college graduates.

Table 3.2 presents the results of the mediation analysis performed to isolate the direct and indirect effects of departmental education policy on violent crime. Several conditions are necessary to establish a mediation model (Baron and Kenny, 1986). First, the association between the predictor and the outcome variable must be statistically significant. Second, the mediator must share a significant relationship with both the predictor and the outcome variable. Finally, the mediating variable must significantly reduce the association between the predictor and the outcome variable.

For violent crime, Table 3.2 shows a significant total effect of education policy on aggravated assaults only. The overall effect or ATT indicates that all else equal, cities requiring a college background for officers experienced 22 fewer aggravated assaults per 100,000 residents. We then decomposed the ATT of education policy into direct and indirect effects. The findings indicate that while violent crime arrest indicators do not significantly mediate the impact of education policy on the three violent crime outcomes, including aggravated assaults, the direct effect on aggravated assault rates are negative and sizeable. Since a college requirement is directly associated with a reduction of 19 aggravated assaults per 100,000, the ATT is primarily driven by the direct effect. Albeit insignificant, indirect effects were also negative, resulting in an overall effect in the same direction.

Table 3.3 shows the property crime models. Except for burglary offenses, the ATT of education policy did not reach conventional levels of statistical significance. For burglaries, the total effect indicates that if all agencies adopted a college requirement, cities could expect an associated 46 fewer burglaries per 100,000 residents. When examining the mediating influence of police arrest productivity, non-significant direct and indirect effects were observed across all offenses. Albeit insignificant, while direct effects are larger than the indirect effects, both effects work in the same direction to produce an overall significant negative effect.

Discussion

Since the early 1900s, various national commissions and academics have maintained that increased education standards for law enforcement could improve public safety. Yet, the research on police education requirements and crime control indicates otherwise. However, what we know about this association comes from studies in which education policy indicators are not focal. Thus, prior studies did not address theoretical and methodological challenges specific to the study of agency education minimums and crime. Beyond the underdevelopment of theory, the inherent endogeneity of education policy has gone unaddressed, raising questions about the accuracy of past results. Moreover, potential mediators that transmit the effects of education policy have gone unexplored, despite the theoretical indication that such policies might, directly and indirectly, influence crime through its impact on police productivity.

Using national data on 1,543 city police agencies, we implement a novel entropy balanced ratio-of-mediator probability mediation weighting analysis to decompose the direct and indirect effects of departmental education requirements on city crime rates. We did not find statistically significant mediating effects of arrest for either violent or property crimes. Yet,

regardless of the statistical significance, ATT estimates mostly aligned with our expectations (except for robbery and theft which leaned positive).

For violent crime, we found that a college requirement negligibly influenced homicide and robbery rates, but significantly impacted aggravated assaults. Cities with a college policy for police experienced 22 fewer aggravated assaults per 100,000 residents relative to those without this requirement. For property crime, we detected significant negative total and direct effects of education policy on burglaries. Overall, establishing a college requirement for new officers is associated with a decrease of nearly 46 burglaries per 100,000 city residents. These effects appear promising, but only one previous study allows for empirical comparisons withour findings. MacDonald (2002) performed regression analyses that returned much smaller treatment effect sizes for robbery offenses, while the magnitude of education policy's effect on homicides in both studies was close in magnitude. Like the current study, MacDonald reported an insignificant association with robbery offenses that leaned positive for robbery and negative for homicide offenses.

Deciphering how agency education requirements contribute to lower crime rates without increasing arrest rates is both perplexing and intriguing. Adding to this challenge, no similar evaluations exist to compare our findings against. Up to this point, we, like many other stakeholders, naturally associated police effectiveness with increased arrests. However, our findings suggest the need to revisit standing definitions of police effectiveness, as we found that departments adopting a college policy can unsurprisingly affect crime through means other than arrest.

Recent studies have supported the use of alternative crime interventions. Though not directly examined here, several works show that "guardian actions" or less punitive and informal police activity strongly contributes to crime prevention. Nagin and colleagues (2015) described such an approach as the sentinel role of police, which has found support in recent research. For instance, in an experimental study, Ariel and colleagues (2016) placed uniformed civilian police staff in crime hot spots. "Soft patrol," as termed by the authors, was associated with 39% less crime and 20% fewer service calls in target areas than control areas. This finding would suggest that crime reduction is not contingent on punitive enforcement strategies. Further, a meta-analysis of Broken Windows interventions demonstrated that community and problem-solving interventions exhibited the largest program effect sizes (Braga et al., 2015).

Most recently, Piza (2018) analyzed data from the Operation Impact foot patrol experiment data in Newark, New Jersey. His findings revealed an associated 51% decreased likelihood of violent crime during the operational period (6:00 p.m. to 2:00 a.m.) and 52% decrease in the daily probability of robbery occurring for every 1-unit increase in guardian actions (e.g., citizen contacts, business checks, bus checks, and taxi inspections). Such research findings suggest that informal community engagement and less invasive police contacts may matter as much as traditional law enforcement activities.

Thus, considering the characteristics attributed to college-educated officers (e.g., better communication, de-escalation, and problem-solving skills), departments mandating more education may operate differently. It appears that these agencies might emphasize enforcement strategies that effectively prevent crime without subjecting communities to over-enforcement. This type of approach would maximize their attributes and is particularly salient

for minority communities disparately affected by aggressive enforcement activities (Kochel and Weisburd, 2017; U.S. Department of Justice, 2017). While we did not examine differences in enforcement practices between departments with and without a college requirement, descriptive statistics show that the mean value for our community-oriented activity measure was 25% higher for agencies with a college requirement. Police studies find that community engagement influences offending levels through the development of effective partnerships with residents that lead to increased crime reporting and citizen cooperation with police investigations (Litwin, 2004; Roberts, 2008). This increased trust and willingness of citizens to come forward about crime could possibly explain the positive effects of education policy on robberies and thefts. Future research should consider the interaction effects of police education policy and various crime control strategies such as community policing.

Despite employing a robust entropy weighting mediation approach, study limitations prevent a full endorsement of college requirements for police. Most prominently, our approach does not account for unobserved factors. For example, violent offending and arrest rates have steadily declined since 2000 (Sabol et al., 2019). The study data were also collected during a period of intense criminal justice reform (Sabol and Bauman, 2020). Thus, readers should interpret our findings with caution, as we recognize that our results could partially reflect these secular factors. While we are confident our findings accurately characterize the association between education policy and crime prevention, the possibility remains that unobserved forces inform this relationship. Therefore, despite the difficulty of finding a credible instrumental variable (IV), to generate more robust knowledge in this area, future studies should endeavor to examine this relationship across time using an IV approach.

Our study could also be improved in other ways. First, the outcome measures in our analyses represent the total rate for crime types, which include crimes regardless of the circumstances. It is unclear whether we should expect education policy to impact gang killings, for example, the same as the rate of homicides resulting from domestic violence. Future research should examine the effects of departmental education policy on violent offenses broken out by the circumstances surrounding the crime.

Our inability to detect mediating effects could flow from measurement or data constraints. We used two binary indicators rather than continuous measures as mediating variables in our two sets of analyses. By dichotomizing our mediators, we may have sacrificed statistical variation in outcomes between groups (Altman and Royston, 2006). However, this is the first mediation study is this subfield. Considering the lack of empirical precedence, we relied on theoretical insight and the available data. That said, it is possible that our data or measurement approach simply could not surface this association. In particularly, given that the theft and motor vehicle theft models, while insignificant, show sizable and offsetting indirect effects, future studies examining various data sources should replicate this study using continuous mediators.

Another limitation concerns the unit of analysis used in the study. Cities are composed of neighborhoods with varying crime rates, arrest rates, and socioeconomic conditions (Peterson and Krivo, 2010; Krivo et al., 2009). Therefore, a city-level analysis might mask the more nuanced effects of police education policy. Data sparseness would most likely complexify things, yet future research based on neighborhood and other sub-city areas could help detect more localized associations.

Finally, we use educational minimums as a proxy for the educational composition of police departments. Ideally, we would directly examine the impact of the total number of college-educated officers in agencies on crime control. Unfortunately, such measures for nationally representative sample are not currently available, pointing to the need for more detailed data on officer education, including information on college major and whether college credentials were earned before or after joining the police force.

Conclusion

Based on our estimates, departments with college requirements apparently can affect reported crime in a manner that relies on strategies other than arrest. We reason that agencies with a college requirement may have transitioned from "enforcer" philosophies that emphasize punitiveness to the "sentinel" philosophies that "promote crime reduction while building public trust for police officers" (Task Force on 21st Century Policing, 2015, p. 1). Despite some evidence in favor of the crime control effects of education policy, nuances remain that require consideration. Specifically, because our research design only accounts for measured factors, unobserved forces may influence this association. This restriction makes it difficult to parse out whether education policy or other secular forces such as local reform efforts or civic leadership changes are the cause. Either way, our findings indicate there is something both promising and different about law enforcement operations in cities with a college requirement for police personnel.

4 Discussion

More education for America's police forces represents a perennial theme in police reform efforts. However, our understanding of higher education's influence on police performance is not fully developed for three primary reasons. For one, previous studies have left endogeneity issues unattended. Along with omitting important factors like officer work attitudes and ecological measures, prior studies potentially suffer from selectivity issues that might bias estimates. Another reason is the literature's atheoretical nature, as conventional wisdom has dominated explanations of how a college education influences police behavior. Lastly, empirical accounts on educational attainment have focused solely on direct effects, giving little attention to the indirect impacts and the mechanisms through which education effects are expressed.

Filling these research gaps is of both public and government interest. In terms of public interest, high levels of crime, misconduct, and use of force incidents threaten both police legitimacy and public confidence in the police (Roberts and Lyons, 2011; Tankebe, 2013). For city leaders, if highly educated officers contribute only marginally more to department performance than less educated officers do, then the higher costs of staffing college-educated officers (e.g., increased salaries, education incentive pay, and tuition reimbursement) may not be fiscally responsible.

This dissertation addressed the noted challenges to examine the effects of educational capital on three of the most salient police outcomes: police-citizen violence, police misconduct, and crime control. Rather than assessing a single unit of analysis, this study considers the impacts of higher education at the officer-, agency-, and city-levels across time and place to

extend the public safety literature in three ways. First, we employed robust methods new to this subfield to guard against endogeneity problems. Second, we applied interdisciplinary perspectives on moral development, organizational climate, human capital, and deterrence to bolster the theory in this area. Third, unlike prior research, this study examines both the overall influence of education and the mechanisms that might transmit its effects.

Although we find partial support for our hypotheses, three main takeaways from this dissertation set the stage for research and policy development. First, the effects of education on police-citizen violence are more likely to emerge during signatory events like police shootings than more frequent occurrences such as suspect resistance and physical altercations during arrests. This finding could, in part reflect critical reasoning skills found among college graduates that sharpen college-educated officers' foresight and discernment (Bebeau and Thoma, 2003; Facione, 2009; Halpern, 1998; Helsdingen et al., 2011; King and Mayhew, 2002; Pascarella and Terenzini, 1991). In policing, these attributes bode well for officers, affording them keener awareness of the surrounding environment and specific cue recognition skills. While some violent police encounters are beyond the control of officers, those possessing these attributes tend to avert situations where potential deadly force usage is likely through more measured interactions with the public (Fyfe, 1986; Klinger et al., 2016).

The second major takeaway is that college-educated officers are not a panacea for police misconduct. Following the recent spate of high-profile killings of unarmed black citizens by white officers, hiring more college-educated officers has once again been positioned as foundational to ethical police behavior (see, e.g., Bouffard and Armstrong, 2020). Our findings indicate that more research is required on this front. For instance, our police misconduct study

shows that policies focused solely on officer education will not reduce misconduct. In fact, doing so might even make matters worse, at least in larger jurisdictions. Our findings indicate that the most educated big-city departments generate significantly higher misconduct and abuse of authority complaint rates than the least educated departments. We believe this indication reflects the higher levels of police cynicism and job discontentment among college-educated officers flowing from a misalignment of qualifications and work duties. Scholars find that officers tend to become cynical of the job and the public when departments are resistant to changing their operations to match the attributes of their sworn personnel (Miller and Fry, 1976; Trojanowicz and Nicholson, 1976; Griffin et al., 1978). Such a mismatch might cause the most educated officers to become cynical and, in turn, increase their risk for engaging in misconduct (Muir, 1977; Skolnick, 2002; Worden, 1996).

The final takeaway concerns the association between agency educational requirements and crime control. A longstanding question in law enforcement is how police can improve public safety without infringing upon the rights of citizens. We found that departments with a college requirement are able to significantly impact felony crime rates without relying on arrest to control offending. Though not directly examined in this dissertation, we conclude that these agencies rely on "guardian actions" or less punitive and informal police activity to prevent crime.

Despite this dissertation's contributions, there remains room for improvement. First, we drew on cross-sectional data owing mostly to the absence of comprehensive police data options. Although the LEMAS data used in Chapter 3 are available across multiple waves spanning 1987 to 2013, several education policy indicators and key covariates were not

consistently measured throughout this period. Further, only about 200 large city departments provided data for all waves, limiting our ability to examine the full spectrum of U.S. law enforcement agencies.

While our cross-sectional analysis provided nuanced insights into the effects of education in policing such data do not indicate the sequence of events. Thus, causal inference is precluded because we cannot determine whether educational capital improvements occurred before, after, or during the outcome's onset. Moving to a longitudinal setting does not guarantee unbiased estimates as endogeneity problems, including simultaneity, remain a threat to the findings' accuracy (Wooldridge, 2002). Instrumental variables estimation represents a potential solution to this problem. A valid IV is related to the treatment, only influences the outcome through its effect on the treatment, and shares no common cause with the outcome (Baiocchi et al., 2014). Thus, in principle, IV techniques can mitigate potential bias due to the inherent endogeneity of educational achievement.

To extend this research, we present two candidate instruments worth consideration. Since it is conceivable that intense public scrutiny or external regulation spurred by a pattern or practice of police misconduct could result in departments adopting a college education requirement, indicators signifying whether agencies experienced either circumstance warrants IV testing. Another candidate instrument is the educational attainment level of the jurisdiction served as we expect departments establish educational requirements congruent with the educational composition of their communities.

Another approach for estimating the potential bidirectional relationship between police education policy and outcomes (e.g., crime rates) is dynamic simultaneous-equation modeling.

This method adjusts for bias caused by the dynamic correlation between outcomes and predictors by transforming data in first differences to account for the link between the lagged endogenous regressor and the error term (Wintoki et al., 2012).

Higher education and the Future of Policing

The police killings of George Floyd and Breonna Taylor were the proverbial "straw that broke our nation's back," leading to mass protests and public demands for systemic reform in law enforcement. Among many other recommendations, including defunding and abolishing the police, part of the new vision for law enforcement includes college-educated police forces. The optimistic assumption is that police departments composed of officers who have “certain qualities (motivation, self-discipline, general intelligence) … and certain characteristics (civility, urbanity, self-control)” (Wilson, 1969, p. 73) will be able to effectively tackle crime in a procedurally just manner. Other commentators have added that augmenting an agency’s educational commitment has the power to transform its culture and practices. For instance, Murphy (1989) notes that:

> In general, a police department that has had a four-year college degree as an entry requirement for ten years or more can be quite a different organization from one requiring only a high school diploma. More responsibility can be placed on the officers, and a more collegial style of management can be utilized. The college-educated force sets higher professional standards and goals, which in turn, command public respect and help shape public opinion. Finally, a college-educated police force has the potential to proactively, rather than just reactively, address the crime and drug problems that plague society today. (p. iv)

We caution, however, that hiring more college-educated officers is not enough. As evidenced by our findings on police-citizen violence and misconduct, education alone will not solve the problems in policing. Raising the educational standards for police is only a first step. A college education may indeed make officers more receptive to new ideas. However, under the traditional regime of policing that emphasizes punitiveness, college-educated officers' ambitious disposition, high productivity, and policy-compliant nature could lead to more misconduct complaints and situations where the use of force is necessary.

Americans must reconceptualize the role of law enforcement and make financial and policy investments to turn the idea into reality. To transform the police mission from that of warrior to guardian, law enforcement must move beyond traditional metrics like arrests, citations, and seizures. Instead, departments should incentivize and reward officers in a way that promotes a new style of policing. Since college-educated officers tend to favor community policing over conventional practices (Chappell and Gibson, 2009; Gau and Paul, 2019), we believe that these officers would thrive in a system that encourages innovation, as well as more community- and problem-centered police services. Not necessarily because they are inherently more community-oriented than their less-educated colleagues but because they "adhere more closely to departmental policies and procedures in the hope of furthering their careers" (Rosenfeld et al., 2020, p. 229). If these officers adhere to their new role and duties as diligently as they have traditional operations, as our findings suggest, they will be able to provide residents security and protection without subjecting them to invasive police practices.

Appendix A

Table 1.1. Descriptive Statistics of Study Variables (*N* = 1,104)

Variable	M/Percent	SD	Range	Percent imputed
Dependent Variables				
Police Shooting	.26	-	0-1	1.00
Violent Arrest	.90	-	0-1	1.36
Citizen Altercation	.78	-	0-1	1.99
Treatment				
Educational Attainment			1-3	0.91
High School	.15	-	-	-
Some College	.55	-	-	-
Bachelor's Degree	.30	-	-	-
Covariates				
Tenure	11.52	9.32	0-44	3.99
Age	36.06	9.09	20-66	2.08
High Ranked	.31	-	0-1	0.36
Citizen Contact	.73	-	0-1	2.72
Veteran Status	.35	-	0-1	0.27
Married	.60	-	0-1	0.45
Male	.86	-	0-1	0.36
Non-White	.36	-	0-1	1.09
Investigated	.65	-	0-1	1.09
Job Stress	1.97	.54	1.14-5.16	11.05
Job Contentment	4.78	.99	1.18-7.22	11.05
Organizational Fairness	1.67	.82	-.16-4.05	11.05

Table 1.2. Effects of College Education on Police Shootings, Violent Arrests, and Citizen Altercations[a]

Panel A (unweighted)						
	Police Shootings		Violent Arrest		Citizen Altercations	
	(*N* = 1,104)		(*N* = 1,104)		(*N* = 1,104)	
	b	95% CI	*b*	95% CI	*b*	95% CI
Education level						
Some College	-0.05 (0.02)	0.02, -0.08	0.01 (0.01)	0.00, 0.03	0.01 (0.03)	-0.02, 0.04
College Degree	-0.06 (0.02)	0.03, -0.08	0.02 (0.01)*	0.00, 0.04	0.05 (0.03)	-0.02, 0.03
Panel B (weighted)						
	Police Shootings		Violent Arrest		Citizen Altercations	
	(*N* = 1,104)		(*N* = 1,104)		(*N* = 1,104)	
	b	95% CI	*b*	95% CI	*b*	95% CI
Education level						
Some College	-0.07 (0.04)	-0.14, -0.01	0.02 (0.02)	-0.02, 0.06	0.01 (0.04)	-0.06, 0.08
College Degree	-0.10 (0.04)*	-0.18, -0.01	0.03 (0.03)	-0.02, 0.08	0.05 (0.04)	-0.02, 0.13

*Notes: *p < .05. **p < .01 ***p < .001*
[a]*Standard errors in parentheses*

Figure A1.1. Scree plot showing eigen values by factors

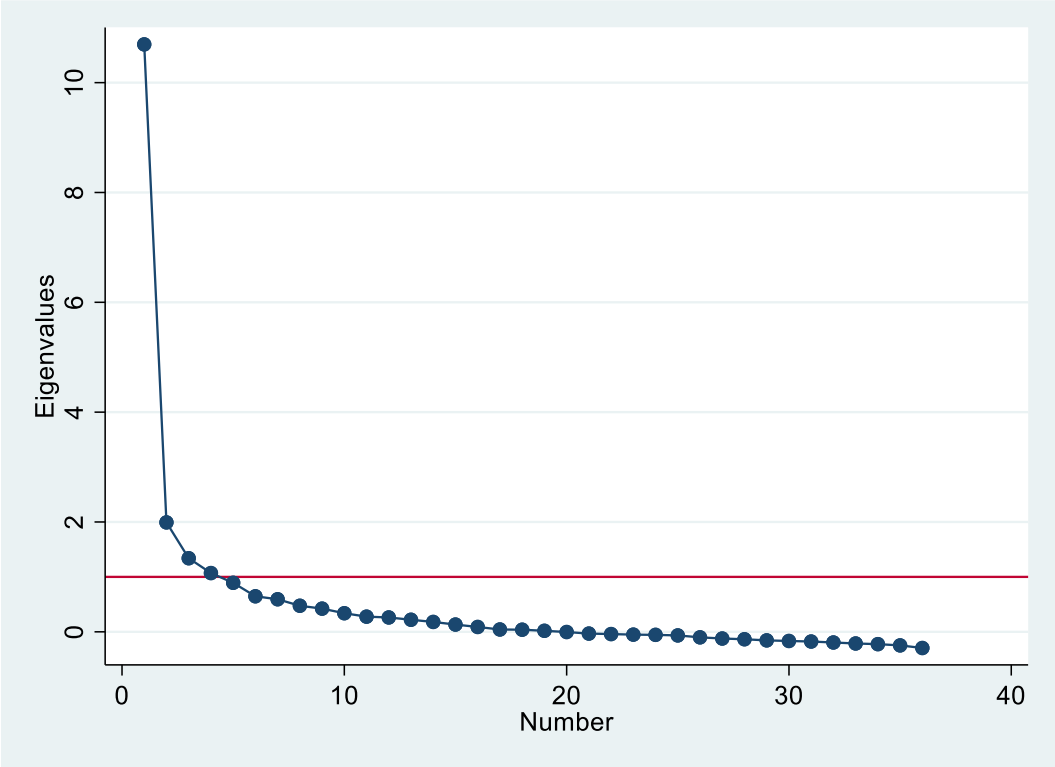


Figure A1.2. Density Plots for Control and Treated Samples

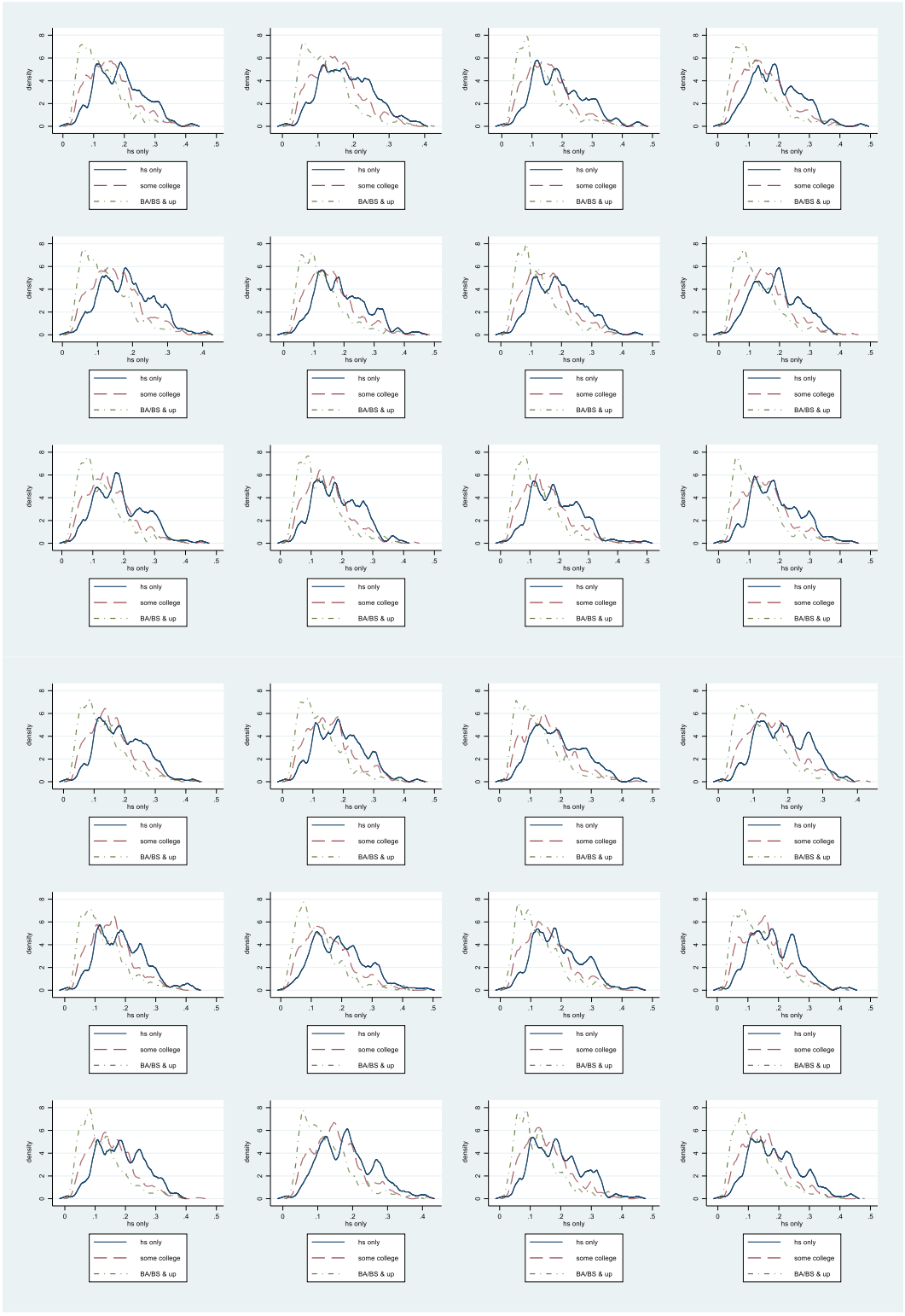


Figure A1.2. Density Plots for Control and Treated Samples (cont.)

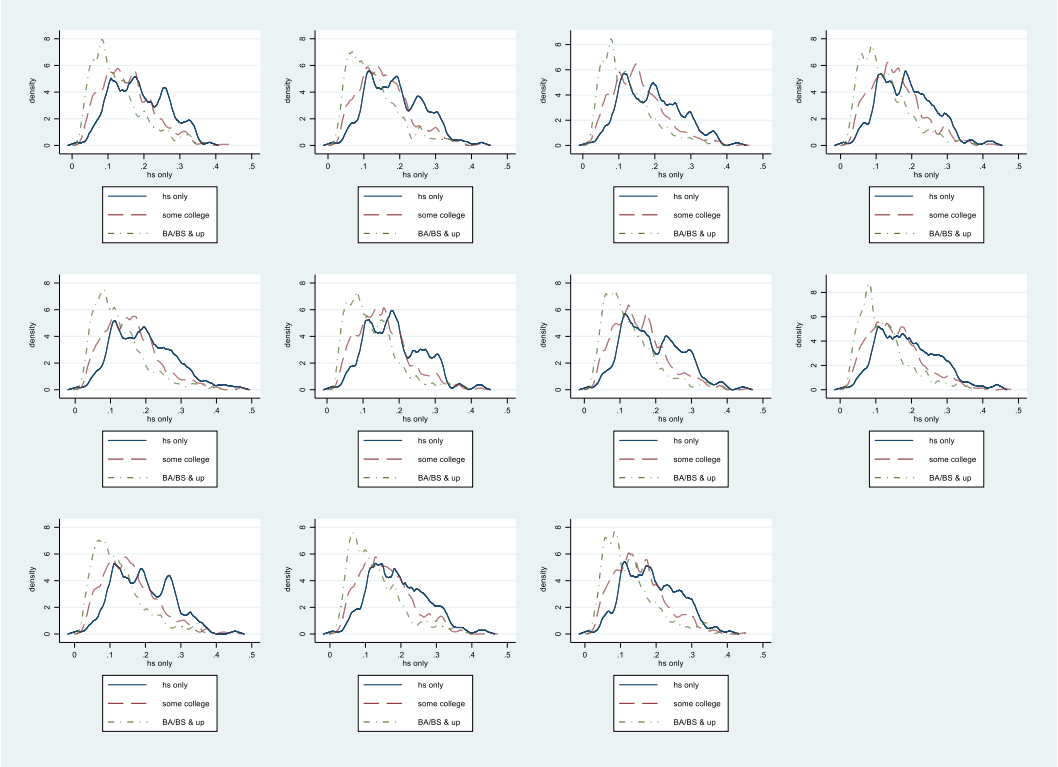


Table A1.1. Polychoric factor analysis with promax rotation (three factors)

Item	Factor Structure		
	Job stress	Job contentment	Organizational fairness
1. Feeling hopeless about the future	0.7781		
2. Spells of terror or panic	0.7573		
3. Feeling so restless you couldn't sit still	0.7479		
4. Crying easily	0.7298		
5. Feeling that something bad was going to happen to you at work	0.6718		
6. I feel tired at work even with adequate sleep	0.6375		
7. I am moody, irritable, or impatient over small problems	0.7371		
8. I want to withdraw from the constant demands on my time and energy from work	0.7528		
9. I feel negative, futile or depressed about work	0.7032		
10. I think that I am not as efficient at work as I should be	0.6234		
11. I feel physically, emotionally and spiritually depleted.	0.7952		
12. My resistance to illness is lowered because of my work	0.6677		
13. My interest in doing fun activities is lowered because of my work	0.6417		
14. I feel uncaring about the problems and needs of the public	0.4598		
15. I have difficulty concentrating on my job	0.7353		
16. When I ask myself why I get up and go to work, the only answer that occurs to me is "I have to"	0.5032		
17. I view my work as just a job - it is not a career		0.5408	
18. It is likely I will look for another full-time job outside this department within the next year		0.5301	

Table A1.1. Polychoric factor analysis with Promax rotation (three factors) (cont.)

Item	Factor Structure		
	Job stress	Job contentment	Organizational fairness
19. Compared to my peers (same rank), I find that I am likely to be more criticized for my mistakes		0.5745	
20. I feel that I am less likely to get chosen for certain assignments because of "who I am" (e.g., race, gender, sexual orientation, physical characteristics)		0.5123	
21. Within the department, gender related jokes are often made in my presence			
22. When I am assertive or question the way things are done, I am considered militant		0.5148	
23. Promotions in this department are tied to ability and merit			0.5562
24. The administration supports officers who are in trouble			0.5260
25. The department tends to be more lenient in enforcing rules and regulations for female officers			-.5089
26. Female officers are held to a higher standard than male officers			.4452
27. I feel burned out from my job		.5996	
28. I feel like I am at the end of my rope		.5297	
29. I feel I treat the public as if they were impersonal objects		.4663	
Eigenvalue	10.70	1.99	1.34
Percentage of variance (%)	61%	11%	8%

Note. *N* = 982, Factor loadings < 0.4 are not shown.

Table A1.2. Differences in the treatment levels before and after weighting on the propensity score

	Before weighting				After weighting			
	Standard Mean Difference		Variance Ratio		Standard Mean Difference		Variance Ratio	
Covariates	Some College	Bachelor's Degree	Some College	Bachelor's Degree	Some College	Bachelor's Degree	Some College	Bachelor's Degree
Tenure	0.12 (33)	0.13 (33)	0.93 (0)	0.95 (0)	0.00 (2)	-0.05 (2)	0.94 (0)	1.00 (0)
Age	**0.26 (33)**	-0.02 (33)	0.90 (0)	0.98 (0)	0.00 (2)	-0.01 (2)	0.77 (0)	0.98 (0)
High Ranked	**0.22 (33)**	0.18 (33)	1.37 (0)	1.09 (0)	0.01 (2)	0.01 (2)	1.01 (0)	0.97 (0)
Citizen Contact	**0.27 (33)**	**0.43 (33)**	1.01 (0)	0.93 (0)	0.01 (2)	-0.02 (2)	0.84 (0)	0.93 (0)
Veteran Status	-0.02 (0)	-0.03 (0)	0.98 (0)	1.02 (0)	-0.05 (2)	0.02 (2)	0.98 (0)	0.97 (0)
Married	-0.05 (0)	**0.72 (33)**	1.11 (0)	1.78 (0)	-0.03 (2)	0.02 (2)	1.05 (0)	1.03 (0)
Male	0.17 (0)	0.13 (0)	0.91 (0)	0.94 (0)	-0.01 (2)	-0.01 (2)	1.01(0)	1.01 (0)
investigated	0.13 (0)	-0.02 (0)	0.95 (0)	0.98 (0)	0.00 (2)	0.02 (2)	1.00 (0)	1.25 (0)
Job Contentment	**0.21 (19)**	-0.05 (6)	1.02 (0)	1.11 (0)	0.01 (2)	-0.03 (2)	0.96 (0)	1.05 (0)
Observations	1,104	1,104	1,104	1,104	1,104	1,104	1,104	1,104
Mean values	0.16	0.22	1.00	1.11	0.02	-0.02	0.95	1.00

Note: The number of imputed data sets out of 35 with SMD and VR statistics exceeding the recommended thresholds in parentheses

Appendix B

Table 2.1. Imputed descriptive statistics for study variables (*N* = 1,023)

Variable	M/Percent	SD	Range	Percent imputed
Dependent Variables				
Abuse of authority complaints	23.48	104.71	0-2,941.18	0.72
Excessive force complaints	4.69	14.18	0-333.33	26.37
Total misconduct complaints	28.18	106.21	0-2,941.18	29.88
Focal Variable				
College degree saturation				26.01
Low	0.32	0.02	-	-
Medium	0.35	0.02	-	-
High	0.33	0.02	-	-
Department Composition				
Percent female	0.07	.60	0-0.46	17.10
Percent non-white	0.11	0.14	0-0.99	18.09
Mean years of service	10.32	3.65	1-37	27.63
Mean age (logged)	3.58	0.12	3.18-4.09	27.54
Department size (logged)	4.48	1.48	0-10.19	0.00
Police Training				
Academy hours	548.84	240.90	0-3,200	19.26
Crisis intervention training	-0.00	0.95	-2.13-1.98	26.46
Use of force training	-0.01	0.93	-3.40-1.04	26.46
Bureaucratic Controls				
Internal affairs unit	0.46	0.02	0-1	1.44
Public dispositions	0.21	0.01	0-1	0.81
Citizen review	0.08	0.01	0-1	0.45

Table 2.1. Imputed descriptive statistics for study variables (cont.)

Reporting standards	1.33	0.40	0-2	1.08
Transparency	-0.00	0.28	-0.88-1.48	0.09
Jurisdiction Controls				
Large population	0.68	0.01	0-1	0.00
City agency	0.66	0.01	0-1	0.00
Mean Part I arrests	8.21	8.95	0-96.57	29.34
Mean Part II arrests	23.08	24.22	0-270.80	30.42

Table 2.2. Effects of Department Education Level on Police Misconduct Complaints[ab]

Panel A (unweighted)						
	Abuse of Authority		Excessive Force		Total Misconduct	
	(*N* = 1,023)		(*N* = 1,023)		(*N* = 1,023)	
	b	95% CI	*b*	95% CI	*b*	95% CI
Education level						
Medium level	9.88 (0.10)**	9.74, 10.02	0.54 (0.11)	0.43, 0.64	9.58 (0.08)**	9.33, 9.82
High level	7.16 (0.10)**	6.52, 7.80	-0.39 (0.12)	-0.22, -0.57	6.50 (0.09)*	6.33, 6.67
Panel B (weighted)						
	Abuse of Authority		Excessive Force		Total Misconduct	
	(*N* = 1,023)		(*N* = 1,023)		(*N* = 1,023)	
	b	95% CI	*b*	95% CI	*b*	95% CI
Education level						
Medium level	25.48 (35.68)	-44.45, 95.41	-5.13 (13.30)	-31.21, 20.94	22.00 (35.39)	-47.36, 91.37
High level	-6.53 (19.99)	-45.72, 32.66	-6.13 (13.24)	-32.09, 19.83	-11.21 (20.25)	-50.91, 28.49

Notes: *p < .05. **p < .01 ***p < .001

[a]*Robust standard errors in parentheses.*

[b]reference category = Low education level

Table 2.3. Effects of Department Education Level on Police Misconduct Complaints (over 50,000 residents)[ab]

Panel A (unweighted)						
	Abuse of Authority		Excessive Force		Total Misconduct	
	(*N* = 751)		(*N* = 751)		(*N* = 751)	
	b	95% CI	*b*	95% CI	*b*	95% CI
Education level						
Medium level	7.14 (0.09)***	7.03, 7.25	0.39 (0.14)	0.07, 0.71	7.80 (0.08)***	7.55, 8.04
High level	9.58 (0.09)***	9.49, 9.66	0.29 (0.16)	0.01, 0.58	10.14 (0.08)***	9.93, 10.35
Panel B (weighted)						
	Abuse of Authority		Excessive Force		Total Misconduct	
	(*N* = 751)		(*N* = 751)		(*N* = 751)	
	b	95% CI	*b*	95% CI	*b*	95% CI
Education level						
Medium level	7.54 (4.29)	-0.87, 15.95	0.51 (1.31)	-2.07, 3.09	8.19 (4.65)	-0.94, 17.31
High level	11.72 (7.33)	-2.65, 26.09	0.71 (1.76)	-2.74, 4.16	12.27 (6.40)*	-0.29, 24.82

Notes: *p < .05. **p < .01 ***p < .001
[a]Robust standard errors in parentheses.
[b]reference category = Low education levels

Figure B2.1. Scree plot showing eigen values by factors

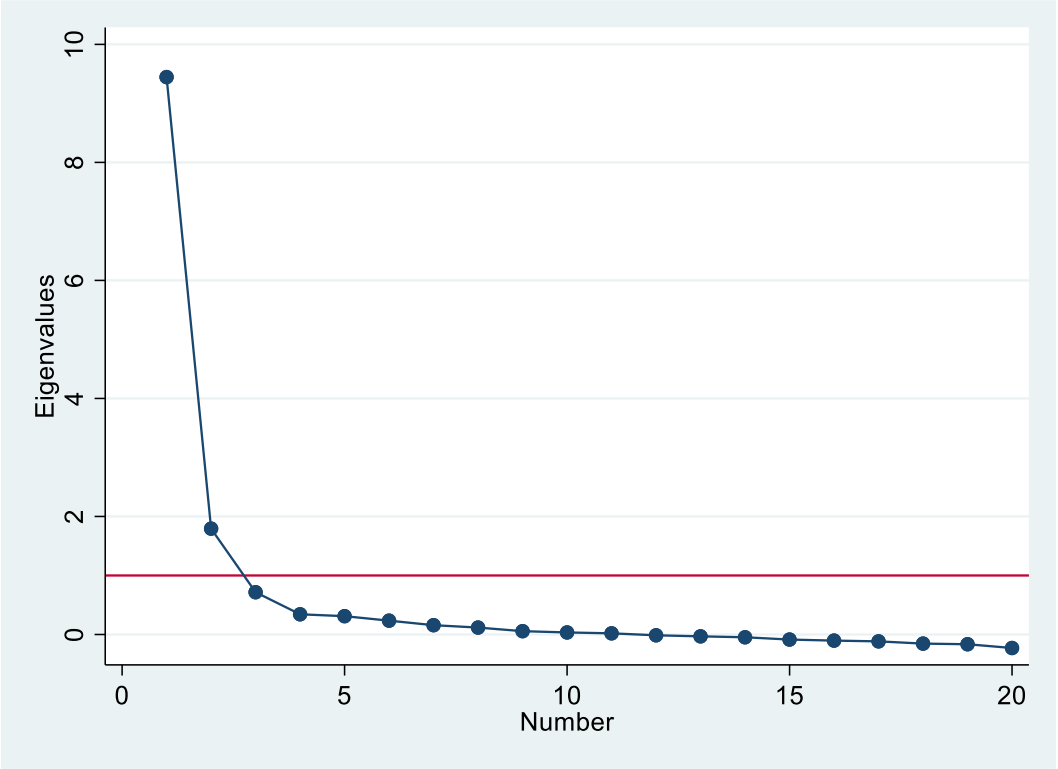


Table B2.1. Principal axis factor analysis with promax rotation (two factors)

Item	Factor Structure	
	Crisis intervention training	Use of force training
30. Non-lethal use of force in-service training		.8910
31. Use of deadly force in-service training		.9227
32. Use of non-lethal weapons in-service training		.7515
33. Firearm qualification in-service training		.6529
34. Crisis intervention skills in-service training	.5386	
35. Suicide prevention in-service training	.5694	
36. Victim assistance in-service training	.7521	
37. Domestic violence in-service training	.7993	
Eigenvalue	9.43	1.79
Percentage of variance (%)	77%	15%

Table B2.2. Differences in the treatment levels before and after weighting on the propensity score[a]

	Before weighting				After weighting			
	Standard Mean Difference		Variance Ratio		Standard Mean Difference		Variance Ratio	
Covariates	Medium Education	High Education	Medium Education	High Education	Medium Education	High Education	Medium Education	High Education
Mean years of service	**0.25 (56)**	**0.31 (56)**	0.76 (0)	0.76 (0)	0.13 (0)	-0.01 (2)	0.86 (0)	0.84 (0)
Mean age	0.05 (46)	0.16 (66)	0.64 (0)	0.68 (0)	0.23 (14)	0.06 (7)	0.86 (0)	0.85 (0)
Mean years of service x mean age	0.20 (14)	**0.44 (55)**	0.75 (0)	0.97 (0)	0.18 (40)	-0.04 (9)	0.98 (0)	0.86 (0)
Academy hours	0.16 (0)	0.21 (56)	0.66 (0)	0.61 (0)	-0.01 (0)	-0.01 (12)	0.77 (0)	0.73 (0)
Crisis intervention training	**0.25 (14)**	**0.38 (35)**	0.87 (0)	0.86 (0)	-0.02 (2)	-0.02 (0)	0.88 (0)	0.83 (0)
Reporting standards	**0.52 (46)**	**0.88 (56)**	1.22 (0)	1.08 (0)	-0.11 (0)	0.06 (6)	0.76 (0)	0.67 (0)
City agency	**0.37 (31)**	**0.39 (106)**	0.82 (0)	0.73 (0)	0.18 (23)	0.05 (0)	0.88 (0)	0.93 (0)
Observations	1,023	1,023	1,023	1,023	1,023	1,023	1,023	1,023
Mean values	0.26	0.40	0.82	0.81	0.13	0.05	0.86	0.82

Note: The number of imputed data sets out of 144 with SMD and VR statistics exceeding the recommended thresholds in parentheses.
[a]reference category = Low education level

Appendix C

Table 3.1. Imputed descriptive statistics for study variables (*N* = 1,543)

Variable	M/Percent	SD	Range
Dependent Variables			
Homicide rate	3.96	10.06	0-202.12
Robbery rate	82.42	116.16	0-988.50
Aggravated assault rate	218.67	250.37	0-2,599.94
Burglary rate	555.31	516.79	0-6,036.04
Theft rate	2,074.16	1,596.10	0-15,688.95
Motor vehicle theft rate	179.91	211.46	0-2,083.33
Focal Variable			
Education minimum	.17		0-1
Mediators			
High violent crime arrest rate	.54		0-1
High property crime arrest rate	.52		0-1
Covariates			
Percent nonwhite officers	0.15	0.19	0-1
Percent female officers	.09	.07	0-0.50
Police strength	225.19	138.84	12.96-2,551.02
Administrative density	50.57	50.38	0-569.80
Investigative capacity	.15	1.00	0-100
Patrol capacity	.71	.16	0.100

Table 3.1. Imputed descriptive statistics for study variables (continued)

Fiscal capacity	266.17	190.16	15.16-4,057.27
Community-oriented activity	.45	.31	0-1
Population (logged)	9.84	1.62	5.28-15.17
Percent nonwhite	.24	.20	0.10-99.83
Percent immigrant residents	.11	.11	0-0.73
Residential stability	.38	.14	0.01-0.83
Educational attainment	.05	.04	0-0.52
Economic disadvantage	-.03	.95	-2.43-3.76

Table 3.2. Direct, indirect, and total effects of agency education requirements on violent crime [a]

	Homicide		Robbery		Aggravated Assault	
	(*N* = 1,543)		(*N* = 1,543)		(*N* = 1,543)	
	b	95% CI	*b*	95% CI	*b*	95% CI
ATT	-0.30 (0.29)	-0.85, 0.24	2.81 (4.11)	-5.24, 10.85	-21.83 (8.39)**	-38.27, -5.39
DE	-0.17 (0.30)	-0.75, 0.42	3.69 (4.00)	-4.14, 11.52	-18.54 (8.45)*	-35.11, -1.97
IDE	-0.14 (0.13)	-0.38, 0.11	-0.89 (1.58)	-3.97, 2.20	-3.29 (3.74)	-10.63, 4.05

Notes: *p < .05. **p < .01 ***p < .001; ATT = Average treatment effect on the treated; DE = direct effect; IDE = indirect effect
[a]Robust standard errors in parentheses.

Table 3.3. Direct, indirect, and total effects of agency education requirements on property crime [a]

	Burglary		Theft		Motor Vehicle Theft	
	(*N* = 1,543)		(*N* = 1,543)		(*N* = 1,543)	
	b	95% CI	*b*	95% CI	*b*	95% CI
ATT	-46.39 (17.95)**	-81.57, -11.20	80.58 (70.74)	-58.07, 219.23	-11.70 (10.78)	-32.83, 9.44
DE	-33.37 (23.71)	-79.85, 13.10	94.91 (73.79)	-49.71, 239.52	-20.66 (14.06)	-48.22, 6.90
IDE	-13.01 (16.90)	-46.13, 20.11	-14.33 (38.69)	-90.17, 61.51	8.96 (9.20)	-9.07, 26.99

Notes: *p < .05. **p < .01 ***p < .001; ATT = Average treatment effect on the treated; DE = direct effect; IDE = indirect effect
[a]Robust standard errors in parentheses.

Figure C3.1. Scree plot showing eigen values by factors

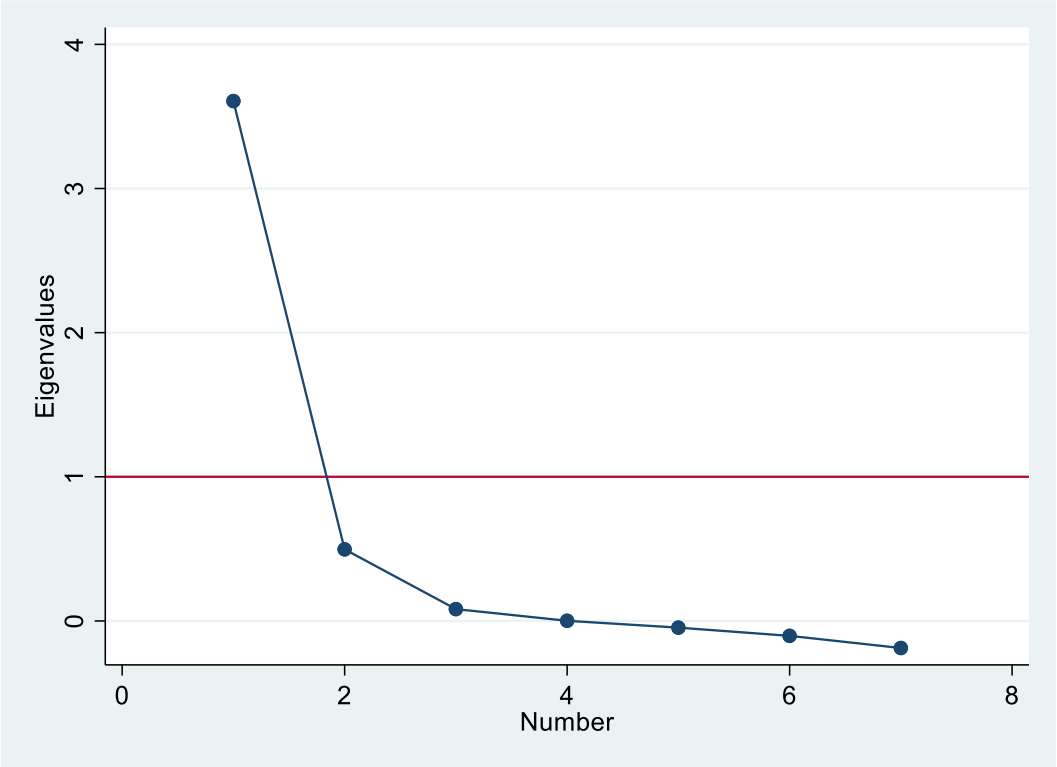


Table C3.1. Mean differences between comparison groups pre and post entropy balancing

Covariates	Before weighting	After weighting
Percent non-white officers	-2.73	0
Percent female officers	2.31	0
Police strength	-43.70	0
Fiscal capacity	17.20	0
Community-oriented activity	0.11	0
Population (logged)	0.86	0
Educational attainment	-0.41	0

(N = 1,543)

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VITA

Thaddeus Lateef Johnson was born in Memphis, Tennessee to parents, Galvin and Regina Johnson. The youngest of two sons, Thaddeus graduated high school and attended the University of Tennessee at Chattanooga. Although his first attempt at a college degree was unsuccessful, he returned home and began a career in law enforcement. Several years later, Thaddeus met his wife, Natasha Johnson, and they left the United States to reside in the Caribbean nation of Jamaica, West Indies. During his time in Jamaica, Thaddeus completed his Bachelor of Science degree in Criminal Justice through a distance learning program at the University of Tennessee at Chattanooga in May 2014. In August 2014, the couple returned to the U.S. to attend graduate school at Thaddeus' alma mater. His wife graduated with her Ed.S degree in Instructional Leadership in August 2015 and Thaddeus graduated with a Masters of Science in Criminal Justice in May 2016. They both went on to complete doctorate degree programs in their respective fields at Georgia State University, with Thaddeus being awarded his Ph.D. in Criminal Justice and Criminology in August 2020.

Thaddeus will join the faculty at his home department at Georgia State University as an Assistant Professor in August 2020. He continues to engage in research and service centered on police reform and criminal justice disparities. Thaddeus may be contacted at tjohnson230@gsu.edu.