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

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Abstract

Objectives: The latest spate of deadly police encounters across the US sparked renewed calls for agencies to hire more college-educated police officers. But educational attainment’s impact on police-citizen altercations remains unclear. Using secondary data, this study examines the association between officer education level and three outcomes: police shootings, violent arrests, and physical altercations.

Method: 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 police officers. Results: We find that, on average, officers with some college experience or a completed bachelor's degree are 8% to 10% less likely, respectively, to be involved in a shooting compared to officers with a high school education only. Conversely, results show null effects of college-education on violent arrests and physical altercations. Conclusions: Prior research suggests that college-educated officers are more effective at deescalating potentially volatile situations. Our research suggests this may be the case only during the most dangerous and fraught encounters, such as those that often lead to shots fired.

Keywords: police shootings, use of force, officer education, police altercations
The College Shield: Examining the Role of Officer Education in Violent Police Encounters

Recent highly publicized 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, America’s police forces are themselves facing increasing levels of hostility and violence (Engel et al., 2020; Hutchinson, 2020; Mac Donald, 2016). Ways to reduce these tragic altercations and their aftermath represent one of the most pressing police reform issues in the US. Among many other proposals (e.g., improved training), the latest spate of fatal police encounters sparked renewed calls for agencies to hire more college-educated police officers (see Bouffard & Armstrong, 2020; Dowd, 2020; President’s Task Force on 21st Century Policing, 2015; Yancey-Bragg, 2020).

Leading authorities have long cast college-educated officers as better problem solvers, more socially and culturally informed, self-disciplined, altruistic, more humanistic, and less likely to be involved in violent altercations (Aamodt, 2004; Carter et al., 1989; Feldman & Newcomb, 1969; Hawley, 1998; Telep, 2011; Vollmer, 1936). Despite the longstanding claims that better-educated officers are more adept at de-escalation and defusing potentially violent situations, the available evidence base on this proposition is mixed. Besides the variability in study design and measurement, the inconsistent research findings might be largely owed to self-selection and omitted variable bias. This study addresses the noted limitations through improved research design and model specification to clarify the effects of educational attainment on police shootings, violent arrests, and physical altercations.

The paper proceeds as follows. Before specifying our hypotheses, we first comprehensively review the literature and discuss key methodological limitations of prior research in this area. We then describe the sample, measures, and analytic plan. After presenting the results, we conclude with a discussion of the implications for research and practice.
Higher Education and the Use of Non-Lethal Police Force

Research examining the influence of officer education on police use of non-lethal force tends to find null effects across different geographic regions, time periods, and agency settings (e.g., urban/rural) (Brandl et al., 2001; Fridell & Lim, 2016; Klahm et al., 2011; Sun & Payne, 2004; Terrill & Reisig, 2003). Still, a handful of studies found that college-educated officers are significantly less likely to resort to physical force than their non-college educated peers (Chapman, 2012; Paoline & Terrill, 2007; Rydberg & Terrill, 2010; Terrill & 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. Across these studies, the likelihood of college-educated officers threatening or inflicting physical force during citizen interactions was 4% to 50% lower, depending upon control variables and modeling strategies employed (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 New Jersey cities predominated by residents of color in 2008. He found that college education is significantly inversely associated 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, raising concerns of omitted variable bias.

Higher Education and the Use of Lethal Police Force

On balance, research across various contexts also suggests that higher education is not significantly associated with the use of deadly police force, including police shootings (Ridgeway, 2016; Sherman & Blumberg, 1981; Smith, 2004; Willits & 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 predict police shootings. Nonetheless, at least two studies find that college-educated officers are less inclined to use deadly force (Donner et al., 2017; McElvain & 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 found that each completed year of post-secondary education was associated with a 22% reduction in 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 (Fridell & Pate, 2001; Geller, 1982; Greenan, 1987; Kavanagh, 1997; Pinizzotto & Davis, 1995), 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 & 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 & 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, interdisciplinary scholarship indicates that educational attainment potentially serves as a proxy for underlying personal and work-related factors not considered in earlier works. For instance, aside from having relatively higher levels of moral maturity and critical reasoning (Pascarella & Terenzini, 2005), college-educated officers might be distinct from less educated officers in their attitudes and behavioral expressions. Police research also 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).

Omitting such potentially important factors could bias findings regarding the link between educational attainment and violent police-citizen encounters. For example, 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). Thus, the weight of traditional enforcement activities (e.g., stops, citations, searches, and arrests) in officer evaluations could compel college-educated officers to be especially zealous in enforcing the law. Importantly, these activities represent the most common opportunities for confrontations that lead officers to use force, including lethal force (Willits & Nowacki, 2014). Moreover, serious altercations more often occur during police-initiated officer contacts (Pinizzotto et al., 1997; Rydberg & Terrill, 2010; Terrill & Ingram, 2016).

From this perspective, college-educated officers might find themselves in enforcement situations justifying the use of force more often than their less educated coworkers.

The second major limitation is that prior research examining the educational attainment and police-citizen violence association suffers from selection bias. Selection bias occurs when participants in the treatment group are systematically different from those in the control group. Such concerns arise with educational attainment, as people self-select or decide to apply to college, owed largely to certain abilities or access to resources. Colleges in turn accept students based on attributes predictive of
postsecondary success (see Cappelli, 2020). Thus, it is highly probable that observed differences in outcomes for officers of varying education levels (i.e., high school education only, some college background, and bachelor’s degree) could be correlated with certain baseline characteristics. To our knowledge, only one study in this area attempted to overcome the threat of self-selection (Ridgway, 2016). However, educational attainment functioned as a control in Ridgway’s matched case-control analysis, and theoretically important work attitudes and experiences including job satisfaction and burnout were not examined.

The Current Study

Using officer-level data from a large urban police department, the present study advances the officer education and police-citizen violence research in two main ways. First, we control for theoretically important factors not commonly found in previous studies to mitigate potential omitted variable bias. Most previous studies control for select personal and occupational factors associated with officers’ likelihood of being involved in violent encounters, including age and job experience (Paoline & Terrill, 2007; Rydberg & Terrill, 2010), rank and duty assignment (Campbell et al., 2018; Chapman, 2012), and race and gender (McElvain & Kposowa, 2008). Determining the unique role of educational attainment, however, requires additionally controlling for commonly excluded factors such as job stress (Shucard et al., 2012; Violanti et al., 2006), burnout (Euwema et al., 2004; Kop & Euwema, 2001), job satisfaction (Dantzker, 1993; Gau et al., 2013; Paoline et al., 2015; Terrill & Paoline, 2015), and perceived agency fairness (Donner et al., 2015; Hass et al., 2015) that may confound this relationship.

Although their effects on police use of force behaviors remains underexplored, several studies illustrate the importance of officer work attitudes and experiences in understanding violent police encounters. For example, Paoline et al. (2015) revealed that college-educated officers tend to exhibit greater job frustration and cynicism toward supervision, both of which carry potentially negative consequences for police treatment of citizens (Manzoni & Eisner, 2006). Scholars including Gershon and
colleagues (2002) assert that job-related stress in policing correlates with hyper-aggressiveness and the use of force. In another study, Kop and Euwema (2001) found significant positive associations between officer burnout and favorable attitudes toward police force usage, self-reported use of force, and observed use of force among a sample of Dutch police officers. Thus, we extend the evidence-base on the officer education and violent altercations association by considering such factors in our statistical models.

Second, previous findings in this area are vulnerable to selection bias and other endogeneity concerns. To guard against selection bias, 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 incorporates a weighting scheme to balance cases on the treatment (education level) and regression adjustment on the outcome to reduce the bias in the estimated treatment effect.

Despite the noted limitations and mixed findings, we draw some key insights from the most recent research indicating that formal education lowers an officer’s likelihood of both using force and citizen altercations (Donner et al., 2017; Lim et al., 2014; Rydberg & Terrill, 2010; Shjarback & White, 2016; Stickle, 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 hypothesize that in spite of college-educated officers’ well-documented productivity, educational attainment will be negatively associated with police shootings (Hypothesis 1), violent arrests (Hypothesis 2), and physical altercations (Hypothesis 3).

Methods

Participants

We conducted secondary analyses of data from the Police Stress and Domestic Violence in Police Families in Baltimore, Maryland, 1997-1999 collected as part of a collaborative effort between the Baltimore Police Department, Baltimore’s Fraternal Order of Police, and the Johns Hopkins’ School of
Public Health. The original goal of the survey was to document the amount of stress police officers experience and ascertain whether that stress leads to adverse behaviors, including personal and occupational violence (for complete details, see Gershon, 1999). Of the 1,104 sampled officers, most identified as male (85.5%) and either White (63.7%) or Black (32.5%). Overall, 35% had prior military experience, 60% were married, and 65% endured an internal affairs investigation. The majority of respondents held the rank of patrol officer (69%), and about three-quarters (73%) had duties that involved regular interactions with citizens. The average officer age and years of service in the department were 36 years ($SD = 9.09$, range = 20-66) and 11.5 years ($SD = 9.32$, range = 0-44), respectively.

**Procedure**

At each of the nine Baltimore City Police Department precincts, project staff attended roll calls for one to two shifts to obtain a convenience sample of volunteers. Research team members distributed anonymous questionnaires to all sworn precinct employees. Of the 1,150 officers attending roll call, 1,104 (96%) completed the self-report survey. On average, participating officers completed the five-page questionnaire in 15 to 20 minutes (for more information concerning sampling procedures and survey items, see Gershon, 1999).

**Measures**

**Dependent Variables**

We analyzed three outcomes in this study: *police shooting*, *violent arrest*, and *citizen altercation*. We operationalize 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’. Per survey instructions, ‘Not applicable’ responses indicate respondents did not experience a given encounter. We created a binary measure to
signify whether an officer had ever shot someone while on duty (1 = yes). 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 dichotomized the final outcome, citizen altercation (1 = yes), 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 altercation.

**Independent Variables**

**Focal 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. While the study data does not specify whether successful GED completion qualifies for the high school only category, the Bureau of Justice Statistics’ Law Enforcement Management and Administrative Statistics data (1997, 2000) indicate that the Baltimore Police Department only hired officers who graduated from grade 12 or equivalent during the study period.

**Covariates.** Our analysis includes controls found in prior work, as well as theoretically relevant controls typically unobserved in previous studies. We categorized these variables into two groups. The first group of covariates measures officer and work-related characteristics. We controlled for tenure and officer age in years. 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. 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 (1 = yes) (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 measured race/ethnicity as a simple dichotomy (i.e., Caucasian vs. person of color) as there are relatively few non-Black/African American minority officers in our sample. Lastly, we operationalized the dichotomous measure, investigated (1 = yes), 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 covariates 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 (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. The eight candidate items with loadings below .40 were excluded from the factor analysis. A scree plot of eigenvalues (minimum value close to 1) verified the three-factor structure (see Figure S1 in Supplemental Materials), and the Kaiser-Meyer-Olkin value (KMO = .91) and Bartlett’s Test of Sphericity (p < .001) 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) (see Table S1 in the Supplemental Materials for full factor analysis results, including items and factor loadings).
The first measure, *job stress*, assessed whether officers were dealing with work-related pressures and strain. This variable encompassed 16 items measured on four-point Likert-type scales ranging from 1 = “never” to 4 = “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*, measured an officer’s level of workplace satisfaction. Our construct capturing job contentment includes eight items measured on five-point Likert-type scales ranging from 1 = “strongly agree” to 5 = “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*. 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.

**Data Analysis Plan**

We used a doubly robust inverse probability weighted regression adjustment (IPWRA) propensity analysis design to compare the likelihood of involvement in violent police-citizen violence between officers of varying education levels. Using G*Power version 3.1.9.7 (Faul et al., 2009), we estimated expected power of .98 to detect a 3% percent difference in violent police-citizen outcomes (ATE = 0.03) for $\alpha = 0.05$, $N = 1104$, and number of predictors $k = 12$, indicating that our analyses are sufficiently powered to detect a small but meaningful effect size. To handle missing data statistically, we employed multiple imputation. We also performed sensitivity analyses with traditional regression procedures to assess the stability of the results. In this section, we outline details of our analytical
approach. All analyses were conducted with Stata 16.1 (see Supplemental Materials for more information on model diagnostics and the multiple imputation process).

**Propensity Score Model**

Because our study draws on observational data that were collected after officers attained their educational credentials, selection bias is a threat to validity. Thus, we utilize methods that allow for valid comparisons between officers who completed college versus those with some or no college education to accurately identify the effects of education. To achieve covariate balance across education levels, we adopt 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). 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). 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 & Imbens, 2016). Secondly, this approach accommodates a multivalued treatment, which may prove more insightful than standard propensity score designs that only accommodate binary treatments (see details on the methods, covariate selection, and balancing diagnostics in the Supplemental Materials).

**Results**
Descriptive and Correlational Statistics

Table 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. Just over 55% of the 1,104 officers completed some college coursework, and nearly 30% attained a bachelor’s degree or higher.

Bivariate correlations emphasizing previously omitted measures of officer work attitudes and experiences are shown in Table S2. Educational attainment was positively correlated with job contentment ($r = .10$), whereas job contentment was negatively associated with violent police encounters (ranging from $r = -.12$ to -.18). In contrast, job stress (ranging from $r = .08$ to .23) and organizational fairness (ranging from $r = .12$ to .26) were positively related to all outcomes. Estimated correlations were stronger for the two non-shooting outcomes (see Supplemental materials for full bivariate results).

Hypothesis 1: Officer Education Level and Police Shootings

For our propensity score analysis, we report the average treatment effect (ATE) 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, the main results in Table 2 (Panel A) shows that, on average, officers with some college experience or a completed bachelor's degree are 8% ($B = -.08, p < .05, 95\% CI [-.15, -.01]) to 10% ($B = -.10, p < .05, 95\% CI [-.18, -.02]) less likely, respectively, to be involved in a shooting compared to officers with a high school education only.

Hypotheses 2 and 3: Officer Education Level, Violent Arrests, and Physical Altercations
We estimate educational attainment’s effects on officers’ involvement in violent arrests and citizen altercations in the next two models of Panel A. Not supporting our second and third hypotheses, we find no statistically significant association between education and violent arrests or citizen altercations. In fact, the direction of effects runs opposite our expectations, albeit not statistically 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, holding constant all covariates. The regression coefficients were converted to reflect the ATE by calculating the difference between college education’s predicted marginal effects and those for high school education. Our sensitivity analysis suggests that our results accurately characterize the police education and police-citizen violence association (see full sensitivity analysis results in the Supplemental Materials).

**Discussion**

Since the early part of the twentieth century, improving the educational profile of American law enforcement has been 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. However, the research dedicated to understanding the effects of educational attainment on violent police encounters shows mixed results, owing in part to enduring self-selection issues 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. We also included measures capturing officer work attitudes and perceptions not considered in previous studies.

We found mixed support for our hypotheses that college-educated officers are less likely to be involved in violent altercations among a sample of 1,104 Baltimore police officers. Consistent with Hypothesis 1, our analysis shows that college-educated officers are significantly less likely to fire a weapon at citizens. However, contrary to our other hypotheses, we find they are no less likely to make violent arrests (Hypothesis 2) and face altercations (Hypothesis 3). While open to multiple interpretations, we reason our findings reflect the stakes of the outcomes (i.e., potential loss of life). That is, for more high stakes and potentially life-threatening behaviors, such as shootings, education appears to reduce the likelihood of involvement. Relative to violent arrests and physical altercations that are more common in policing, we surmise that the effects of the distinctive attributes and skills possessed by college-educated officers become evident during the most dangerous and fraught encounters.

We speculate that our shooting finding reflects a consequence of 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 & Thoma, 2003; King & Mayhew, 2002), and by extension, foresight and better discernment (Facione, 1990; Halpern, 1998; Helsdingen et al., 2011; Pascarella & 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 suspected offenders are less likely to behave in ways that elicit more serious police force (Fyfe, 1986; Klinger et al., 2016).
Limitations

Our study has numerous strengths, but several limitations warrant acknowledgment. First, the data are 20 years old, and policing has indeed evolved since then. For example, police forces have become more racial/ethnically diverse in many settings since the study data were collected (Council on Criminal Justice, 2020). Thus, the generalizability of results to more contemporary and racially diverse contexts is limited. Second, given the utilization of a cross-sectional design and potential for unmeasured confounding, our results should not be considered causal. However, the findings should be viewed as a robust estimation of the association between officer education and police-citizen violence.

Third, although prior research employs similar measurement conventions, our dependent and focal measures consist of only one self-reported item, subjecting them to reliability and validity concerns. Moreover, our outcome measures do not differentiate between reasonable or justifiable force and excessive force, masking critical aspects of violent police-citizen encounters. This limitation might also explain the insignificant findings for the non-shooting outcomes. With about 90% of sampled officers involved in a violent arrest and 78% in a physical altercation, our two dichotomous non-shooting outcomes may have simply lacked sufficient nuance to capture this relationship (e.g., number of occurrences or indicators tapping severity of incidents). Similarly, our single-item focal measure fails to tap educational attainment’s complex, heterogeneous, and multi-dimensional nature.

Fourth, while we included both established predictors and relevant variables omitted in prior scholarship, our research design only accounts for observed variables. 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 that could bias estimates, such as officers’ motivation for advancement and ability to self-improve.
performance. Lastly, we cannot guarantee that this study is without residual confounding or endogeneity that requires more sophisticated methods.

**Future Research Directions**

Based on the noted limitations, our study could be improved in four ways. First, to improve the generalizability of results, we encourage future research to replicate this study with more recent data from multiple agencies. Second, given the remaining endogeneity threat, future studies should build on our results by considering experimental and longitudinal designs with instrumental variables to establish causation. Future studies 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). Third, along with more information on police altercations and citizen resistance, future research should include various education-related measures capturing factors such as the corresponding aptitude, cognitive ability, motivation, effort, and skills. In addition, measures indicating whether officers earned college credentials before or during police employment, college major, social activities, and mode of attendance (online or traditional courses) should be considered in future research.

Lastly, 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. Moreover, only officer information was included in this analysis. Studies would also benefit from having measures that capture ecological (e.g., violent crime rate), situational (e.g., call type), and suspect features (e.g., demeanor). Further, from the structural and cultural institutional perspectives, additional officer background (e.g., financial and home environment stability) and organizational factors (e.g., institutional acceptance of violence and race/ethnic profiling) that may confound the association between police-citizen violence and officer education level also warrant future consideration.
Policy Implications

In this study of the impacts of officer education level on violent police-citizen encounters, we observed that college-educated officers were less likely to engage in police shootings, 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 our findings reflect the qualities and characteristics of college-educated officers that enable them to govern while minimizing the loss of life. Based on past research, we conclude that these officers tend to avert situations where potential fatalities are likely through more measured interactions with the public (Fyfe, 2015; Klinger et al., 2016). Our findings suggest that agencies that hire college-educated officers may see a pay off in reduced police shootings and mitigation of associated costs.
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Table 1
Descriptive Statistics of Study Variables (N = 1,104)

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<td>Violent Arrest</td>
<td>.90</td>
<td>-</td>
<td>0-1</td>
<td>1.36</td>
</tr>
<tr>
<td>Citizen Altercation</td>
<td>.78</td>
<td>-</td>
<td>0-1</td>
<td>1.99</td>
</tr>
<tr>
<td><strong>Treatment</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Educational Attainment</td>
<td></td>
<td></td>
<td>1-3</td>
<td>0.91</td>
</tr>
<tr>
<td>High School</td>
<td>.15</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Some College</td>
<td>.55</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Bachelor’s Degree</td>
<td>.30</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td><strong>Covariates</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tenure</td>
<td>11.52</td>
<td>9.32</td>
<td>0-44</td>
<td>3.99</td>
</tr>
<tr>
<td>Age</td>
<td>36.06</td>
<td>9.09</td>
<td>20-66</td>
<td>2.08</td>
</tr>
<tr>
<td>High Rank</td>
<td>.31</td>
<td>-</td>
<td>0-1</td>
<td>0.36</td>
</tr>
<tr>
<td>Routine Citizen Contact</td>
<td>.73</td>
<td>-</td>
<td>0-1</td>
<td>2.72</td>
</tr>
<tr>
<td>Veteran Status</td>
<td>.35</td>
<td>-</td>
<td>0-1</td>
<td>0.27</td>
</tr>
<tr>
<td>Married</td>
<td>.60</td>
<td>-</td>
<td>0-1</td>
<td>0.45</td>
</tr>
<tr>
<td>Male</td>
<td>.86</td>
<td>-</td>
<td>0-1</td>
<td>0.36</td>
</tr>
<tr>
<td>Person of color</td>
<td>.36</td>
<td>-</td>
<td>0-1</td>
<td>1.09</td>
</tr>
<tr>
<td>Investigated</td>
<td>.65</td>
<td>-</td>
<td>0-1</td>
<td>1.09</td>
</tr>
<tr>
<td>Job Stress</td>
<td>1.97</td>
<td>.54</td>
<td>1.14-5.16</td>
<td>11.05</td>
</tr>
<tr>
<td>Job Contentment</td>
<td>4.78</td>
<td>.99</td>
<td>1.18-7.22</td>
<td>11.05</td>
</tr>
<tr>
<td>Organizational Fairness</td>
<td>1.67</td>
<td>.82</td>
<td>-.16-4.05</td>
<td>11.05</td>
</tr>
</tbody>
</table>
### Table 2
Effect of College Education on Police Shootings, Violent Arrests, and Citizen Altercations

#### Panel A
(Doubly robust estimates)

<table>
<thead>
<tr>
<th></th>
<th>Police Shootings</th>
<th>Violent Arrest</th>
<th>Citizen Altercations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>((N = 1,104))</td>
<td>((N = 1,104))</td>
<td>((N = 1,104))</td>
</tr>
<tr>
<td>Education level</td>
<td>(B) (95% \text{ CI})</td>
<td>(B) (95% \text{ CI})</td>
<td>(b) (95% \text{ CI})</td>
</tr>
<tr>
<td>Some College</td>
<td>-0.08 (0.04)*</td>
<td>0.03 (0.04)</td>
<td>0.03 (0.02)</td>
</tr>
<tr>
<td></td>
<td>-0.15, -0.01</td>
<td>-0.05, 0.10</td>
<td>-0.01, 0.06</td>
</tr>
<tr>
<td>College Degree</td>
<td>-0.10 (0.04)*</td>
<td>0.07 (0.04)</td>
<td>0.04 (0.04)</td>
</tr>
<tr>
<td></td>
<td>-0.18, -0.02</td>
<td>-0.01, 0.08</td>
<td>-0.01, 0.08</td>
</tr>
</tbody>
</table>

#### Panel B
(logit estimates)

<table>
<thead>
<tr>
<th></th>
<th>Police Shootings</th>
<th>Violent Arrest</th>
<th>Citizen Altercations</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>((N = 1,104))</td>
<td>((N = 1,104))</td>
<td>((N = 1,104))</td>
</tr>
<tr>
<td>Education level</td>
<td>(B) (95% \text{ CI})</td>
<td>(B) (95% \text{ CI})</td>
<td>(b) (95% \text{ CI})</td>
</tr>
<tr>
<td>Some College</td>
<td>-0.05 (0.02)</td>
<td>0.01 (0.01)</td>
<td>0.01 (0.03)</td>
</tr>
<tr>
<td></td>
<td>0.02, -0.08</td>
<td>0.00, 0.03</td>
<td>-0.02, 0.04</td>
</tr>
<tr>
<td>College Degree</td>
<td>-0.06 (0.02)</td>
<td>0.02 (0.01)*</td>
<td>0.05 (0.03)</td>
</tr>
<tr>
<td></td>
<td>0.03, -0.08</td>
<td>0.00, 0.04</td>
<td>-0.02, 0.03</td>
</tr>
</tbody>
</table>

Notes: *\(p < .05\). **\(p < .01\). ***\(p < .001\)

\(^a\)Standard errors in parentheses
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. Multiple imputation (MI) is a rigorous method for addressing missing data (Rubin, 1987). Under the assumption that missingness is ignorable (i.e., generally “missing at random” [MAR]), MI can produce valid inferences. Although Little’s test rejected the stronger “missing completely at random” (MCAR) assumption, a test of the covariate-dependent missingness (CDM) assumption using high rank, married, male, person of color, and investigated as auxiliary covariates indicates the MAR assumption is plausible (Little’s CDM test: $\chi^2 = 29.27, df = 42, p = 0.93$) (Li, 2013). Accordingly, we employed fully conditional multiple imputation by chained equations (MICE) to iteratively replace missing values (Royston & White, 2011; White et al. 2011).

This process creates multiple datasets so that the standard errors of the estimates reflect 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 (2020) 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 and imputed values were similar.
Model Diagnostics

We checked for multicollinearity between variables, and not surprisingly, tolerance values for the measures age and tenure appeared to be collinear with respective values of .16 and .18. Otherwise, diagnostic tests showed no indication of multicollinearity among covariates (average variance inflation factors [VIF] = 1.9). We ran sensitivity analyses with age and tenure alternatively removed, with no substantive change in the size, direction, or significance of the observed effects. Thus, we include both measures in the final analysis.

Propensity Score Model

For the multivalued treatment, we estimate a multinomial logit treatment model. Let officer \( i \) have a violent police-citizen incident \( Y_i = Y_i(t), t \in T \). 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 modeling strategy. 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, reduces the bias and variance of treatment effect calculations (Brookhart et al., 2006; Pearl, 2011; Rubin & 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 rank, routine citizen contact, veteran status, married, male, investigated, and organizational fairness. The measures age, high rank, 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 & 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 balancing covariates are good proxies of factors that might affect educational attainment among police officers.
The overlap assumption holds that each respondent has nonzero 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 & Robins, 2005; Cattaneo, 2010; Hirano & Imbens, 2001; Linden et al., 2016 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. 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 S2 in the Supplemental Materials shows that the estimated densities have little mass around 0 or 1, suggesting no serious violations of the overlap assumption. Standard mean difference (SMD) and variance ratio (VR) statistics also suggest negligible residual differences in the distribution of the baseline covariates between groups.

Stata software is not set up for statistical diagnostics of propensity score estimation with MI data. Following prior research (Hayes & 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 & 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 S2 in Supplemental Materials). Since correlational designs commonly used in prior research can produce inconsistent and biased results 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, but we focus on the latter in discussing the findings.

**Sensitivity Analysis**

The findings from the main and sensitivity analyses are qualitatively similar but slightly different in terms of statistical significance and effect sizes (see Panel B in Table 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 sizes associated with educational attainment in the doubly robust model are slightly larger, suggesting coefficients are biased downward in the traditional models, although there is greater precision in the estimates. Also, unlike the primary results, we find that the association between a bachelor’s degree and violent arrest is statistically significant. 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 logit regression.
Figure S1
Scree Plot Showing Eigen Values by Factors
Figure S2
Density Plots for Control and Treated Samples
**Figure S2**
Density Plots for Control and Treated Samples (cont.)
Table S1
Polychoric Factor Analysis with Promax Rotation (three factors)

<table>
<thead>
<tr>
<th>Item</th>
<th>Job stress</th>
<th>Job contentment</th>
<th>Organizational fairness</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Feeling hopeless about the future</td>
<td>0.7781</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. Spells of terror or panic</td>
<td>0.7573</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Feeling so restless you couldn't sit still</td>
<td>0.7479</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Crying easily</td>
<td>0.7298</td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Feeling that something bad was going to happen to you at work</td>
<td>0.6718</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6. I feel tired at work even with adequate sleep</td>
<td>0.6375</td>
<td></td>
<td></td>
</tr>
<tr>
<td>7. I am moody, irritable, or impatient over small problems</td>
<td>0.7371</td>
<td></td>
<td></td>
</tr>
<tr>
<td>8. I want to withdraw from the constant demands on my time and energy from work</td>
<td>0.7528</td>
<td></td>
<td></td>
</tr>
<tr>
<td>9. I feel negative, futile or depressed about work</td>
<td>0.7032</td>
<td></td>
<td></td>
</tr>
<tr>
<td>10. I think that I am not as efficient at work as I should be</td>
<td>0.6234</td>
<td></td>
<td></td>
</tr>
<tr>
<td>11. I feel physically, emotionally and spiritually depleted.</td>
<td>0.7952</td>
<td></td>
<td></td>
</tr>
<tr>
<td>12. My resistance to illness is lowered because of my work</td>
<td>0.6677</td>
<td></td>
<td></td>
</tr>
<tr>
<td>13. My interest in doing fun activities is lowered because of my work</td>
<td>0.6417</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
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 S1. Polychoric factor analysis with Promax rotation (three factors) (cont.)

<table>
<thead>
<tr>
<th>Item</th>
<th>Factor Structure</th>
</tr>
</thead>
<tbody>
<tr>
<td>19. Compared to my peers (same rank), I find that I am likely to be more criticized for my mistakes</td>
<td></td>
</tr>
<tr>
<td>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)</td>
<td></td>
</tr>
<tr>
<td>21. When I am assertive or question the way things are done, I am considered militant</td>
<td></td>
</tr>
<tr>
<td>22. Promotions in this department are tied to ability and merit</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Job stress</td>
</tr>
<tr>
<td></td>
<td>Job contentment</td>
</tr>
<tr>
<td></td>
<td>Organizational</td>
</tr>
<tr>
<td></td>
<td>fairness</td>
</tr>
<tr>
<td>19. Compared to my peers (same rank), I find that I am likely to be more criticized for my mistakes</td>
<td>0.5745</td>
</tr>
<tr>
<td>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)</td>
<td>0.5123</td>
</tr>
<tr>
<td>21. When I am assertive or question the way things are done, I am considered militant</td>
<td>0.5148</td>
</tr>
<tr>
<td>22. Promotions in this department are tied to ability and merit</td>
<td>0.5562</td>
</tr>
</tbody>
</table>
23. The administration supports officers who are in trouble 0.5260

24. The department tends to be more lenient in enforcing rules and regulations for female officers -.5089

25. Female officers are held to a higher standard than male officers .4452

26. I feel burned out from my job .5996

27. I feel like I am at the end of my rope .5297

28. I feel I treat the public as if they were impersonal objects .4663

<table>
<thead>
<tr>
<th>Eigenvalue</th>
<th>Percentage of variance (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>10.70</td>
<td>61%</td>
</tr>
<tr>
<td>1.99</td>
<td>11%</td>
</tr>
<tr>
<td>1.34</td>
<td>8%</td>
</tr>
</tbody>
</table>

*Note. N = 982, Factor loadings < 0.4 are not shown (8 of the 36 candidate items did not meet this threshold).*
Table S2
Differences in the Treatment Levels Before and After Weighting on the Propensity Score

<table>
<thead>
<tr>
<th>Covariates</th>
<th>Before weighting</th>
<th></th>
<th>After weighting</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Standard Mean Difference</td>
<td>Variance Ratio</td>
<td>Standard Mean Difference</td>
<td>Variance Ratio</td>
</tr>
<tr>
<td>Tenure</td>
<td>Some College</td>
<td>Bachelor’s Degree</td>
<td>Some College</td>
<td>Bachelor’s Degree</td>
</tr>
<tr>
<td></td>
<td>0.12 (33)</td>
<td>0.13 (33)</td>
<td>0.93 (0)</td>
<td>0.95 (0)</td>
</tr>
<tr>
<td>Age</td>
<td>0.26 (33)</td>
<td>-0.02 (33)</td>
<td>0.90 (0)</td>
<td>0.98 (0)</td>
</tr>
<tr>
<td>High Rank</td>
<td>0.22 (33)</td>
<td>0.18 (33)</td>
<td>1.37 (0)</td>
<td>1.09 (0)</td>
</tr>
<tr>
<td>Routine Citizen Contact</td>
<td>0.27 (33)</td>
<td>0.43 (33)</td>
<td>1.01 (0)</td>
<td>0.93 (0)</td>
</tr>
<tr>
<td>Veteran Status</td>
<td>-0.02 (0)</td>
<td>-0.03 (0)</td>
<td>0.98 (0)</td>
<td>1.02 (0)</td>
</tr>
<tr>
<td>Married</td>
<td>-0.05 (0)</td>
<td>0.72 (33)</td>
<td>1.11 (0)</td>
<td>1.78 (0)</td>
</tr>
<tr>
<td>Male</td>
<td>0.17 (0)</td>
<td>0.13 (0)</td>
<td>0.91 (0)</td>
<td>0.94 (0)</td>
</tr>
<tr>
<td>investigated</td>
<td>0.13 (0)</td>
<td>-0.02 (0)</td>
<td>0.95 (0)</td>
<td>0.98 (0)</td>
</tr>
<tr>
<td>Job Contentment</td>
<td>0.21 (19)</td>
<td>-0.05 (6)</td>
<td>1.02 (0)</td>
<td>1.11 (0)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,104</td>
<td>1,104</td>
<td>1,104</td>
<td>1,104</td>
</tr>
<tr>
<td>Mean values</td>
<td>0.16</td>
<td>0.22</td>
<td>1.00</td>
<td>1.11</td>
</tr>
</tbody>
</table>

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