Do Female Officers Improve Law Enforcement Quality?
Effects on Crime Reporting and Domestic Violence Escalation

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Abstract

We study the impact of the integration of women in US policing between the late 1970s and early 1990s on violent crime reporting and domestic violence escalation. Along these two key dimensions, we find that female officers improved police quality. Using crime victimization data, we find that as female representation increases among officers in an area, violent crimes against women in that area, and especially domestic violence, are reported to the police at significantly higher rates. There are no such effects for violent crimes against men or from increases in the female share among civilian police employees. Furthermore, we find evidence that female officers help prevent the escalation of domestic violence. Increases in female officer representation are followed by significant declines in intimate partner homicide rates and in rates of repeated domestic abuse. These effects are all consistent between fixed effects models with controls for economic and policy variables and models that focus exclusively on increases in female police employment driven by externally imposed affirmative action plans resulting from employment discrimination cases.

Keywords: Women in policing, occupational sex segregation, affirmative action, crime reporting, domestic violence, intimate partner homicide.
JEL Codes: J16, J78, K14, K31, K42, N92, I12

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"I definitely think they need more female officers and every crime scene should have a female officer. Not to be left alone with a male officer. ... It's all about the approach by the police."

- Female assault victim quoted in Spohn and Tellis (2012; p. 400).

I.   Introduction

Although law enforcement remains a male-dominated occupation, a dramatic and lasting demographic shift occurred between the late 1970s and the early 1990s. Over this period, the share of female officers in major US police departments nearly tripled from 3.4% to 10.1%.[1] This increase in female representation in law enforcement occurred in the wake of the landmark 1964 Civil Rights Act, which prohibited sex discrimination in employment, and during a period in which women increased their labor market participation and newly entered several non-traditional occupations (Goldin 2006; Blau, Brummund, and Liu 2012).

Even against this backdrop of female progress, women's integration in law enforcement was remarkable. The occupation imposes physical and emotional demands on workers and is strongly associated with masculine traits of power, protection and authority. As a result, the employment of female officers has been controversial from the start (Martin and Jurik 2007). Opponents point to the fact that women are, on average, smaller and weaker than men to argue that they are less capable at policing. They also argue that incorporating female officers in a department undermines unit cohesion and makes male officers less productive. The strongest opposition to increasing female representation is directed at cases in which it is achieved through the enforcement of employment anti-discrimination laws and implementation of affirmative action (AA) plans. In those cases, the particular concern is that departments respond to external pressures to include women by lowering standards for female applicants, which lowers average officer quality and can itself lead to resentment and conflict among officers. Early proponents of female officers argued that population-wide sex differences were not relevant to the select sample of individuals who sought work in law enforcement and emphasized areas of similarity between the sexes. More recently, rather than arguing that female officers are equal substitutes for males, advocates for women in policing have argued that women make distinctive contributions to police production. In particular, advocates point to police interactions with female assault victims, and especially domestic violence victims, as domains in which female officers can be especially effective (Lonsway et al. 2003).[2]

This notion that increasing women's representation in local law enforcement helps other women in their area echoes theories of improved role modeling from greater female representation in positions of authority (e.g., Athey, Avery and Zemsky 2000; Carrell,

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[1] According to data in the Uniform Crime Reports (United States Department of Justice 2009), the average female officer share among county and municipal police departments with over 100 officers increased from 3.4% in 1976 to 10.1% in 1994. Subsequent growth was much slower; by 2011, women represented on average only 11.1% of officers at such departments (United States Department of Justice 2011). Table 1 reports female officer shares in our estimation samples.

[2] Female assault victims often prefer to interact with female officers (as in Spohn and Tellis 2012, quoted above). This preference may arise because they find it less difficult to disclose sensitive personal information to female, as opposed to male, officers. It may also reflect differences in how male and female officers interact with female victims (Buzawa and Buzawa 1996).

Page and West 2010).[3] It also echoes political science theories of representative bureaucracy that increasing minority representation among civil servants helps the minority population (Keiser et al. 2002; Meier and Nicholson-Crotty 2006). The notion is also reflected in arguments by organizations such as the National Organization for Women for promoting women in positions of power in all spheres: economic, political, cultural, and within government bureaucracies. Their aim is not only to address past discrimination but to "effect change in these institutions from within."[4]

This research addresses the controversy by providing empirical evidence on the effects of the integration of female officers between the 1970s and 1990s on the quality of local policing. Motivated by arguments about the potential advantages of female officers in handling violent crimes against women and domestic violence, we define our two primary quality measures as the rates at which these crimes are reported to police and the rate of escalation of domestic violence. In order to capture effects on overall officer quality and to detect tradeoffs between improving policing for different groups of victims, we also test for effects on other crimes and victims. Our estimation approach involves two phases. In the first, we study all changes in the female shares among police officers, while in the second we build on the results of Miller and Segal (2012) and focus on increases in female representation induced by externally imposed AA plans. Using these two approaches allows us to accomplish two goals. First, we can provide stronger causal evidence about the effects of female integration, and second, we can examine whether or not it matters how female integration is achieved.

Our analysis begins with a quality measure based on the rate at which crimes are reported to police. Because crime reporting is an essential input into the production of law enforcement services, underreporting of crimes is a major policy concern. In particular, violent crimes against women are thought to be severely underreported to police (Tjaden and Thoennes 2006) and increased reporting rates for these crimes is one of the key achievements cited by the White House in assessing progress on the issue since the 1994 Violence Against Women Act (VAWA; Title IV of the Violent Crime Control and Law Enforcement Act of 1994, Public Law 103-322).[5] Furthermore, crime-reporting rates provide a gauge for the degree of trust that people in an area have in their local police and proxy for their assessment of police quality.

Increased reporting of domestic violence and more effective police handling of reported cases should in turn reduce escalation rates of domestic violence. Crime incidence will decrease if potential offenders are deterred by their greater chances of incurring police involvement and criminal penalties or if direct police intervention in households with abuse changes the behavior of offenders or victims. However, because increased reporting will inflate official crime statistics for most crimes (and paradoxically appear to lower police quality), our crime rate measures are either based on direct victimization data or limited to homicides, where non-reporting is not a concern.[6] Specifically, we measure domestic violence escalation using intimate partner homicide rates and longitudinal victimization survey data on repeated incidents of domestic violence.

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[3] In our context, female officers may serve as role models of a sort for female assaults victims, demonstrating to them that women need not be powerless.

[4] Quoted from the National Organization for Women's 1998 Declaration of Sentiments, online at http://www.now.org/organization/conference/1998/vision98.html.

[5] See, for example the "Factsheet: The Violence Against Women Act" made available at http://www.whitehouse.gov/sites/default/files/docs/vawa_factsheet.pdf (downloaded August 18, 2013) following the March 7, 2013 re-authorization of the Act.

[6] The exception is in Online Appendix Table 6A, where the outcomes are rapes and assaults reported to police. Notwithstanding our concerns about reporting bias in police data, we consider these other outcomes for completeness.

Our empirical analysis reveals that increasing the female share of officers improves the quality of policing for both of our primary quality measures. First, we find evidence that higher shares of female officers in an area increase the reporting rates of violent crimes against women in that area.[7] Such effects are absent for male victims; there are also no increases in reporting arising from growth in the female share of civilian police employees.[8] Second, we find that increasing female officer representation prevents domestic violence escalation. Specifically, we find that higher local shares of female officers lower the probability of future domestic violence following incidents of abuse and reduce subsequent intimate partner homicide rates for female victims. We also find reductions of intimate partner homicide rates for male victims, as do studies of domestic violence policy changes (Iyengar 2009; Aizer and Dal Bó 2009). The literature attributes these effects for male victims to a reduction in the number of cases of battered women killing their abusers. All of our models include location and year fixed effects, as well as controls for local area economic, social and policy changes (including schooling, labor force participation and the female share in non-traditional occupations). Our findings are also all consistent between the direct approach based on female employment shares and the approach based on AA exposure.

Our results contribute to the economics of crime literature on domestic violence by analyzing the role of police demographics and by introducing the novel outcome measures of crime reporting and repeated domestic abuse. Previous research considers the effects of policies mandating arrest (Iyengar 2009) or prosecution (Aizer and Dal Bó 2009) in domestic violence cases and the impact of states' adoption of unilateral divorce laws (Dee 2003; Stevenson and Wolfers 2006). Domestic violence rates are also shown to be elevated by higher gender pay gaps (Aizer 2010), higher unemployment rates (Tertilt and van den Berg 2012), and unexpected football losses (Card and Dahl 2011).

Outside of the issue of domestic violence, this paper also contributes to the literature on the effects of police staffing and policies on crime outcomes. Much of that literature focuses on the size of the police force to measure deterrence effects (Levitt 1997; McCrary and Chalfin 2013). Studies also evaluate particular innovations in police practice, such as the use of information technology (Garicano and Heaton 2010) and DNA databases (Doleac 2011). By studying the sex composition of the force, we consider an unexamined dimension of police staffing on crime outcomes. Our interest in the effects of changing police demographic characteristics is shared with studies that relate officer race and ethnicity to arrest rates for different demographic groups (e.g., Donohue and Levitt 2001; Antonovics and Knight 2009). However, unlike that literature, which is primarily concerned with relationships between officers and suspected offenders, this paper centers on interactions between police and victims. That literature also focuses on police behavior as the outcome of interest, while our outcomes are victim reporting behavior and crime rates.

Furthermore, in Section 6, we study exogenous variation in police demographics driven by employment discrimination litigation and find evidence of positive effects on policing quality. Although our previous estimates include both fixed effects and time-varying local controls to eliminate the main potential sources of spurious correlations, the AA estimates

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[7] This finding is consonant with the result in Iyer et al. (2012) relating increased female political representation in India to higher rates (per population) of reported violent crimes against women.

[8] To detect tradeoffs in improving policing for different groups of victims and to provide a wider range of quality measures, we also investigate the effects of female officer representation on the rates of assaults per population against female or male victims (measured using the victimization survey) and the rate of assaults and rapes per population reported to the police. As reported in Online Appendix Tables 5A and 6A, we find no significant effects. Limitations of these additional outcome measures are discussed in Section 5.2.

provide the additional benefit of focusing on increases in female police representation that were induced by court interventions. Hence, these estimates do not reflect situations in which local police responded to political pressure to increase female representation,[9] but instead focus on situations in which the local police continued to restrict their hiring of female officers (even relative to national hiring trends, as indicated by the pre-trend Figure 1) and were litigated for ongoing employment discrimination. Previous studies of police diversity and crime outcomes that have used AA as a source of variation find mixed results. McCrary (2007) studies race and finds no effects on reported crime, arrest, or clearance rates. Lott (2000) associates increased diversity in sex or race with higher reported crime rates, but that study suffers from well-noted methodological problems.[10] Our paper is the first in the AA literature to examine reported and unreported crimes, to study reporting as an outcome, and to examine violent crimes against women and intimate partner violence.

Finally, by studying the impact of women's initial integration in policing, this research provides new insight into the economic and social effects of increasing female representation in a traditionally male dominated occupation that involves power and authority in which women remain underrepresented. The finding that female representation in law enforcement has significant effects on policing quality expands on previous studies that document significant effects of female representation, whether achieved through quotas or driven by changes in market forces alone, in other traditionally male dominated occupations, such as political leadership (Chattopadhyay and Duflo 2004; Iyer et al. 2012) and business leadership (Matsa and Miller 2011, 2012, 2013).

The next section provides background and describes hypothesized mechanisms. Section 3 discusses the main data sources. Section 4 presents the empirical approach and results from the analysis of female officers and crime reporting; Section 5 does the same for domestic violence escalation. Section 6 presents estimates of the effects of affirmative action. Section 7 concludes.

II.   Women in Policing: Background, Identification, and Hypotheses

A.   Initial Integration and Identifying Variation

Through the 1960s, US local law enforcement officers were almost all male. A small number of female officers (or "policewomen") worked in certain departments, but even these select women operated in a limited functional sphere and were often segregated into designated Women's Bureaus.[11] They were granted less authority than male officers and

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[9] The concern about police responding to direct local pressure to hire more women is most problematic for our estimates if the pressures to hire female officers occurred in conjunction with pressure to improve handling of violence against women. We present two pieces of evidence in the paper for why that is not likely. First, during the time period under study, policy advocacy addressing violence against women did *not* include hiring female officers among the desired reforms. Second, using the best available data, we find no significant relationships between changes in female officer shares and other police department policies related to domestic violence towards the end of our sample period. See Section 4.2 for details.

[10] Specifically, the main outcome variable (crime rate) is included as a control variable in the first-stage regression that predicts police diversity. The results for female officers are statistically insignificant when this error is corrected; see footnote 22 in Lott (2000) and page 540 in Holzer and Neumark (2000).

[11] Eisenberg (2009) characterizes the "policewoman" job at the Seattle Police Department in the 1950s as "social work with a gun and a badge" (p. 17), but also notes that policewomen were involved in a range of

were generally barred from patrol duties and opportunities for training and promotion (Martin and Jurik 2007).

Much of this occupational segregation can be explained by the relatively low supply of female police officers during the period. Female labor force participation was low overall and employed women were mainly concentrated in traditionally feminine occupations (Goldin 2002, 2006; Blau, Brummund, and Liu 2012). Law enforcement's masculine associations with power and authority, together with its physical and emotional demands and unpleasant and dangerous working conditions, were additional deterrents for most women. Nevertheless, even the low supply of potential female officers sometimes exceeded demand for their services, and capable women who sought to enter the profession often faced opposition from hiring departments and potential coworkers. The same stereotypes and beliefs about women's unsuitability for police work that suppressed labor supply created demand side obstacles as well.[12]

Female integration in US law enforcement started in the 1970s. Our analysis starts in the late 1970s, when outcome data are first available, and runs through the early 1990s, before the major federal legislation on violence against women. During this period, the female share of sworn officers nearly tripled from 3.4% to 10.1%. One likely reason for the increase in female representation during our sample period is the shift in gender norms and attitudes that increased women's labor supply overall and especially in non-traditional occupations. National laws against employment discrimination likely also contributed. The 1963 Equal Pay Act banned pay differences based on sex, Title VII of the 1964 Civil Rights Act prohibited sex discrimination in employment, and the 1972 Equal Employment Opportunity Act extended the latter to cover government agencies. Court decisions clarified that these protections rendered illegal not only policies that explicitly treat the sexes differently, for example, by blocking women from patrol duties or promotion competitions, but also policies that have a disparate impact on women (and are not closely linked to job performance), such as height and weight standards for hiring (as in *Dothard v. Rawlinson* in 1977; or educational requirements, as in *Griggs v. Duke Power Co.* in 1971). Female representation in law enforcement was also advanced through federal incentives, such as the requirement in the 1973 Crime Control Act that departments have in place Equal Employment Opportunity programs to be eligible for funding.

The timing and extent of female integration varied across departments, which is what enables us to estimate its impact on crime reporting and domestic violence escalation after including location and year fixed effects.[13] We control for changing local factors, such as increasing female empowerment and improving economic conditions, and for sub-national policy changes that may have coincided with female integration in law enforcement and

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activities, such as making drunk-driving arrests, playing undercover roles in sex crime cases, and providing security for parades and celebrity appearances.

[12] Although pressures from market competition might be expected to eliminate discriminatory labor practices and correct biased perceptions in the private sector (Becker 1957), those pressures are weaker for the public sector workforce, and updating of beliefs may be slow if employers have little experience upon which to update their beliefs. Of course, even in the private sector and with unlimited experience, statistical discrimination can be self-reinforcing (and accurate on average) if employers have low expectations from female candidates and women under-invest in relevant (but untested) skills in response (Coate and Loury 1993). Market competition will also not eliminate discrimination when the source is co-worker tastes, but will instead lead to segregation. In policing, male officers frequently resisted having female peers or commanding officers (Eisenberg 2009), possibly because a female presence would undermine or "pollute" (Goldin 2002) the masculinity and prestige of their occupation.

[13] Anecdotal evidence suggests that the exact timing and depth of the integration of female officers was affected by random local shocks in the preferences of police or city leaders, the quality of female candidates, and by idiosyncratic catalyzing events, such as a 1968 scandal involving corrupt male vice detectives in Seattle (Eisenberg 2009; p. 55).

also affected domestic violence reporting or escalation rates. In our preferred estimation approach for violent crime reporting rates, we compare changes between female and male victims. In Sections 4 and 5, we use falsification and robustness checks to verify that the remaining variation in female representation in our models is exogenous. In Section 6, we present estimates based on an alternative approach that exploits plausibly exogenous variation in female integration from externally imposed AA following employment discrimination cases against particular departments in different years.[14] Studying AA rather than female representation may yield more reliable estimates, but it also has its limitations. In particular, the estimates from AA-induced gains may have limited external validity for predicting effects of female representation outside of AA because the effects of increased female representation achieved specifically through AA may differ from the average effects from all causes. This could happen if AA departments were forced to hire less qualified women or if they faced more internal resistance from male officers. Hence, evidence from each of these approaches is informative in its own right and is an essential part of this study.

B.   A Different Shade of Blue? Hypothesized Effects of Female Integration

Debates about the role of women in policing, both scholarly and popular, frequently center on how ability differences between male and female officers affect their modes of interaction with suspected offenders. Opponents emphasize that women are smaller and physically weaker than men, while proponents argue that they tend to have better social and communication skills and are less likely to use force against civilians (for more detailed arguments and evidence, see Rabe-Hemp 2008). By contrast, this paper examines potential effects of gender differences in interactions not only with crime perpetrators *but also with their victims*. This sub-section describes two hypothesized mechanisms related to the interactions between officers and victims by which increases in female officer shares can improve policing outcomes for female assault victims and a third potential mechanism with ambiguous predictions for policing quality.

Our first hypothesized mechanism entails female officers employing a distinctive approach in their police work. Specifically, we consider the theory that female officers have different attitudes about violence against women in general and about domestic violence in particular and that they handle these cases differently than do male officers.[15] Sex differences in officers' attitudes regarding domestic violence were likely to have been especially important during the time period of our analysis because the criminal justice system, as a matter of policy, tended to minimize its importance.[16] Indeed, a major

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[14] The history of AA in policing and its proximate impact on employment outcomes is described in more detail in McCrary (2007) and Miller and Segal (2012), who show large increases in Black representation in law enforcement following these targeted interventions. Miller and Segal (2012) also find smaller, but still economically and statistically significant, incremental increases in female police employment in lower-ranked officer positions. For excellent summaries of the literature on the effects of AA on employment, see Donohue and Heckman (1991), Holzer and Neumark (2000).

[15] Because of the high degree of non-random selection into policing (both self-selection and screening) and the pressures felt by many female officers to conform to the masculine police culture, it is unlikely that average sex differences in abilities or preferences in the general population (documented in large psychology and economics literatures, summarized in Croson and Gneezy 2009) will be mirrored exactly among officers. Nevertheless, some gender differences may persist, possibly because of differences in motivation for entering the profession: in a recent study, female recruits to the NYPD rated "the opportunity to help people" significantly higher than males did (Raganella and White 2004).

[16] This was reflected, for example, in the fact that marital rape was not a crime in any state until 1975 and in court decisions such as the 1989 Brooklyn state supreme court case in which Dong Lu Chen was sentenced to 5 years of probation after killing his wife (by smashing her skull with a hammer) because she had been

objective of the 1994 VAWA was "to encourage States, Indian tribal governments, and units of local government to treat domestic violence as a serious violation of criminal law" (Public Law 103-322, Section 2101(a)). Individual police officers often viewed domestic violence cases negatively and frequently failed to respond to domestic violence calls. Newmark, Harrell and Adams's (1995) evaluation of a successful officer training program in Texas found that, even after officers had been trained in how to handle domestic violence cases, 15% of surveyed domestic violence victims still reported that law enforcement had been called but never arrived.[17]

Although it is natural to imagine that female officers might react differently in these cases, the notion of a female policing style is controversial in the criminology literature. Some studies find evidence of gender differences in attitudes and behaviors among police officers, particularly relating to domestic violence. For example, Buzawa and Buzawa (1996) report that domestic violence victims describe female officers as "more understanding, showing more concern, and providing more information about legal rights and shelters" (p. 61). However, their review of the literature leads them to conclude that the evidence "tentatively supported the existence of attitudinal and behavioral differences" between male and female officers but that "this theory is still generally unproven by empirical research" (p. 61). More recent studies also report mixed evidence of gender differences among officers (Poteyeva and Sun 2009). A distinct feature of our study is that it measures the impact of female integration on departments, rather than comparing male and female officers in a department at a single point in time. This enables us to also capture any spillover effects that female officers have on the attitudes and behavior of their male coworkers (analogous to the effects on male colleagues of female judges in Boyd, Epstein and Martin 2010 and female corporate directors in Adams and Ferreira 2009).

Under this first mechanism, female representation can improve reporting rates *directly* by increasing the likelihood that police respond to victims' requests for assistance and also *indirectly* by increasing the willingness of victims to contact police. This indirect effect is part of our second hypothesized mechanism, which is based on the behavior of crime victims.

Female victims of violent crimes often prefer to discuss the incidents with female rather than male officers (e.g., Jordan 2001).[18] This preference may be a reaction to differences in how male and female officers interact with them, but it can exist even if male and female officers have similar attitudes and behaviors. Openly discussing the sensitive and personal details of a violent crime incident may be especially difficult for female victims interacting with male officers. Female assault victims and male and female officers

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sexually unfaithful. Police policy also reflected these attitudes. Police training manuals from this period contain guidelines for handling domestic violence cases with a minimal level of intervention (e.g., the 1968 manual for the International Association of Police Chiefs advises police officers that their "sole purpose" is to "preserve the peace" and they should attempt to "pacify [the] parties" and use arrest only as "a last resort" and the 1974 Oakland police department manual describes the role of the police as more of a "mediator and peacemaker" than enforcer of the law).

[17] In a well-known case, the San Jose police department was sued for the wrongful death of Ruth Bunnell, who was murdered by her husband: the police did not respond to her call for help, even though she had contacted the police 29 times in the prior year. Another widely publicized case was *Thurman v. City of Torrington*, brought by Tracey Thurman against the city police department in Torrington, Connecticut, after her abusive husband beat her almost to death in 1983.

[18] Jordan (2001) also reports that women find forensic physical examinations less traumatic when the examiner is female. Outside of criminal investigations, there is also evidence of similar gender preferences in the medical context, in which female patients often prefer to receive their care (in general, and especially for obstetrics and gynecology) from female physicians (Reyes 2006).

mention this preference in several interviews reported in Spohn and Tellis (2012).[19]

Under these two mechanisms, female officers will increase the rates at which violent crimes against women are reported to police and the completeness of those reports. The increased frequency and depth of crime reporting, together with greater concern and action from police officers, should in turn reduce the likelihood of future violent crimes against women. There may be general deterrence effects that lower violence against women as this type of criminal activity becomes more likely to be punished. There may also be reductions in escalating violence for victims of domestic abuse if police interventions are effective or lead victims to receive other support services.

It is worth noting that the second mechanism relies on female victims' having some knowledge of the demographic composition of their local law enforcement. Women may learn about their local police through interactions, while reporting a crime or being interviewed as a witness or suspect, or through observation. The importance of direct interactions for inference about changes in female shares among officers suggests that the effect of female officers should be larger for crimes (such as domestic violence assaults) in which the same person is likely to be victimized repeatedly. In such cases, victims who report incidents to police learn about how the local police respond, and their decision to report subsequent incidents (and what information to provide to police) will likely be affected by the quality of their previous interactions. Nevertheless, even without previous direct interactions with police, women are generally aware of the sex mix around them (Castillo, Leo and Petrie 2013) and can observe increases in the female share of officers performing routine duties, such as patrolling and responding to complaints, in their area (Meier and Nicholson-Crotty 2006). Female officers may be especially noticeable because their presence is unusual in the male-dominated occupation.

Separate from these two gender-related mechanisms, female representation could also affect law enforcement quality by affecting average police officer quality. If departments had been hiring their best candidates through the 1970s regardless of gender, and they achieved female integration in the 1980s by lowering standards for female applicants, possibly because of external pressure, then average officer quality would drop as more women were hired. If, instead, integration resulted from greater supply of highly qualified female candidates, then average officer quality would weakly increase. Quality will also improve if female integration is achieved by removing discriminatory barriers that cause departments to reject women for less capable men.

Even if officer quality increases, however, ongoing sex discrimination and resistance from peers, supervisors, and subordinates can undermine the effectiveness of even highly talented female officers. This resistance may be especially problematic for women hired or promoted at departments with active affirmative action plans. Because of its different predicted impact on officer quality and potential effects on police morale, our analysis of AA in Section 6 may not be directly comparable to the analyses in Sections 4 and 5. Hence, we argue that both the direct and AA-based estimation approaches are independently informative.

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[19] The examples in Spohn and Tellis (2012) include the quote at the opening of this paper. Another victim said, "I don't feel like men are emotionally equipped to deal with this kind of thing. I don't see cops as being, I mean, I think they try to do the best they can, but they come off as abrasive and ask about your personal life. … I think a female officer would be more compassionate and would communicate in a more sensitive manner." (p. 391). One male officer in that study said, "The only part is that being a male [female victims] don't want to discuss every sexual act that happened … so I have to bring in a female." (p. 232). Female officers said, "Being a female, I think that they can relate to me and feel comfortable opening up" (p. 233) and "I think that it helps that I am a female in terms of dealing with the victims but it hurts when dealing with the suspect. … It helps being a soft-spoken female. I tell them I'm not there to judge…" (p. 224).

Our empirical analysis is focused on testing the predictions of the first two sex-specific mechanisms that greater female representation will improve policing outcomes for violent crimes against women, and particularly for domestic violence, but will not affect outcomes for other crimes or victims. These predictions also align with the theory of representative bureaucracy in political science positing that demographic diversity among public sector employees (passive representation) shifts policy outcomes to benefit the minority group (active representation).[20] By contrast, the third mechanism about officer quality is not specifically related to gender and is compatible with either improvements or deteriorations in policing quality for female and male victims of violent crimes and other crimes. We accommodate this third possible mechanism, and the potential quality tradeoffs between victims, by also assessing quality outcomes for assaults against men and property crimes. However, without direct evidence on officer characteristics, we will not be able to directly study the third mechanism. Findings of positive effects that are limited to violence against women and domestic violence escalation will support the conclusion that first two mechanisms are present in our data, but will not be enough to definitively rule out changes in officer quality under the third mechanism.

III.   Main Data Sources and Variables

This section describes our data sources and procedures for constructing our two main outcome variables–crime reporting and intimate partner homicides–and our main explanatory variable for the female officer share. Summary statistics are reported in Table 1. The control variables, other outcomes, and AA data are described in the text as they appear in Sections 4 through 6.

A.   Data on Crime Reporting

Our data source for crime reporting is the National Crime Victimization Survey (NCVS), conducted by the Bureau of Justice Statistics (BJS) since 1973. Specifically, we use the NCVS files for the 40 largest MSAs in the country, available starting in 1979 (US Department of Justice, Bureau of Justice Statistics 2007).[21] This survey provides unique data on crime incidents, reported or unreported to police, on a nationally representative sample of about 40,000 to 50,000 housing units. Household members 12 and older are interviewed regarding crime incidents twice a year for 3 consecutive years. Participants are asked screening questions for possible crimes. Any question that elicits a positive response is followed by additional questions that gather details about the nature of the incident, including *whether it was reported to the police*. Crimes include both completed and attempted assault, robbery, purse snatching, pickpocketing, burglary, and theft. Changes to the survey in 1992, aimed at addressing complaints that the survey was not gathering complete information about sexual assaults and domestic violence, limit the

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[20] See Keiser at el. (2002) for theoretical foundations and evidence on the link between active and passive sex representation in the educational sphere and Meier and Nicholson-Crotty (2006) for a summary and evidence in the case of female police officers and rape.

[21] These data are publicly available to researchers through the National Archive of Criminal Justice Data (NACJD) at the Inter-university Consortium for Political and Social Research (ICPSR). Data are only for the core counties within each MSA.

comparability with later years, so we focus on crimes before 1991.[22] We focus on this earlier period primarily because that is the period of the initial and more rapid growth in female police representation. It also predates the federal VAWA of 1994, the landmark policy on the topic that could affect our outcomes (other policy changes during our sample period are discussed in Section 4.1). The main limitation of focusing on this period, however, is that we are not able to consider reporting of rape and sexual assaults. Prior to the re-design, the NCVS contained no specific screening questions about sexual assaults (though respondents could offer information about rapes and attempted rapes in response to the general question about assaults).

Although victimization studies provide crucial information that is not available in police reports, crime may still be underreported in these surveys. In particular, scholars have noted the lower implied incidence rates of domestic violence in the NCVS as compared to the VAWA survey (Tjaden and Thoennes 2000) and argued that survey methods might be a cause. We are unable to explicitly account for this source of underreporting in our sample (lacking data, for example, about the sex of the interviewer or mode of interview). The estimated effects should properly be interpreted as reflecting incidents of the type that would be reported in the NCVS.

B.   Data on Intimate Partner Homicides

Data on homicides come from the Supplemental Homicide Reports (SHR) within the Uniform Crime Reporting (UCR) program (United States Department of Justice 2009). The main limitation of the UCR for measuring crime incidence is that only crimes reported to police are included.[23] This can lead to biased estimates for crimes with changing reporting rates, but should not be a concern for homicides. Among homicides, we use information on the relationships between victims and offenders to identify intimate partner homicides (in which the victim was a current or former spouse, girlfriend or boyfriend of the offender).[24] The SHR include information on the relationships between victims and offenders starting in 1977. Because we are especially interested in the initial period of female integration, we use homicide data starting in 1977. However, to avoid potential confounding effects related to local policy changes in anticipation (or as a result) of the VAWA, we end the sample period in 1991. Because our model incorporates a one-year lag between domestic violence reporting and homicides, this end year is consistent with the end year of 1990 in the analysis of NCVS data on reporting rates.

Our main outcome variable is the county-year *IPHRate*, computed by dividing the intimate partner homicide (IPH) count for adult victims (age 18 or older or missing age)

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[22] Major changes to the survey included adding multiple questions and cues about crimes committed by family member, intimates and acquaintances and about rapes and sexual assaults as well as broadening the scope of covered sexual incidents (rape, attempted rape, verbal threats, unwanted sexual contact without force). See <http://www.icpsr.umich.edu/icpsrweb/NACJD/NCVS/redesign.jsp> and

<http://www.bjs.gov/content/pub/pdf/ncsrqa.pdf> for more information about the break in the series in 1992 associated with the NCVS re-design.

[23] Another concern about UCR data relates to incomplete reporting by police. For example, as noted in Stevenson and Wolfers (2006), several states do not report any murders in some years. Our analysis excludes observations from states that report no murders for the entire state that year as we believe that these cases reflect missing data. They are Florida between 1988 and 1991; Rhode Island in 1977 and 1979; Maine in 1987; Kentucky in 1988; and Maine and Iowa in 1991.

[24] We exclude homosexual relationships from our definition of intimate partners. These homicides are rare in the data (between 3 and 5 cases per year nationally for female victims and 38 to 72 for male victims) and including them has negligible effects on the estimates. In cases with multiple victims, we use the relationship with the first victim (the only one in the data). For multiple offenders, we count the crime if there had been an intimate relationship between the victim and any of the offenders.

by the population in the county. Using the county (rather than state) as our main geographic unit allows us to examine relationships with the local shares of female officers. The county also provides a larger and more stable geographic unit than the police department's service area and is more comparable across locations and over time.[25] IPH is relatively rare and most counties report none in most years (87% of observations for male victims and 82% for female victims; Table 1). We reduce the share of observations with zero IPH deaths (to 40% for male victims and 24% for female victims) by restricting our estimation sample to the largest counties, with over 150,000 in population in every year of the sample period. This restriction also helps address the concern that our estimates might otherwise be unduly influenced by large fluctuations in IPH rates from small changes in IPH counts among small counties.[26] We also use the SHR data to compute the total number of *non-intimate partner* homicides by sex in each county-year, which we use as a control for overall violent crime rates in an expanded estimation model.

C.   Data on Police Demographics

Our primary data source for police employment information is the Law Enforcement Officers Killed or Assaulted (LEOKA) file within the UCR. This file contains counts of officers and civilians of each sex at each reporting police department. We create aggregate employment measures at the MSA-level for the reporting analysis and county level for the IPH analysis. Our key explanatory variable, *FemaleOfficerShare*, is the ratio of the weighted numbers of female officers to total officers, where we weight departments based on the size of the population they serve (a variable in the UCR).[27] This measure accounts for the fact that the departments within a geographical area that serve larger populations and have larger numbers of sworn officers are more likely to be the relevant departments for crime victims. For falsification checks, we also define the variable *FemaleCivilianShare* in an analogous manner using the weighted counts of civilian employees. In creating these measures, we noted a few outliers that are clearly data entry errors, and before aggregating the data, replaced them with interpolated values.[28]

Over the period of our analysis, the *FemaleOfficerShare* variable increases dramatically in both estimation samples: going from 5% to 13% in the crime victimization sample and from 3% to 10% in the sample of large counties used for the IPH analysis (Table 1, Panel C). Nevertheless, the great majority of police officers are male throughout the sample period. For example, the female share of officers is under 27% across all county-year observations in the IPH sample and under 11% in 90% of observations. The estimates in this paper therefore reveal the effects of integrating female officers to a limited degree and are not likely to apply equally to increases in female officer shares to equal representation

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[25] Repeating the main IPH specifications at the police department level for the largest departments (serving populations above 75,000 in all years) also yields similar results for the OLS analysis in Section 5 and the IV analysis in Section 6 (Online Appendix Table 7A).

[26] The results are also robust to alternative population cutoffs. For example, Column 1 of Online Appendix Table 3A reports estimates with a population threshold of 50,000 that are similar to the main estimates.

[27] When different departments serve the same (city or county) population we first merge their data by summing the numbers of officers and civilians of each gender.

[28] The complete list is as follows. In 1977 and 1979, the number of female civilian employees reported for the NYPD is zero even though in the years before and after it is above 1,500. Also at the NYPD, the number of female officers is zero in 1979 even though it is 294 in the year before and 539 in the year after. In no other year in our sample period is there as large a change in the number of female officers that is then reversed. In the St. Louis Police Department in 1981, the number of female officers is reported as 358, which is more than 5 times as many as in the years before or after. In each of these cases, we replaced the suspect values with the average from the two adjacent years.

or beyond.

IV.   Effects of Female Officers on Crime Reporting

Our first outcome variable is the rate at which violent crimes are reported to police. We test the prediction from the first two hypotheses in Section 2 that female officer shares will increase crime reporting by female, but not male, victims. We start with reporting because it captures a key input in the police production function and the trigger for intervention.[29] Reporting rates also proxy for the trust that victims have in police. Despite its importance, crime reporting has rarely been studied as an outcome in the economics literature.

A.   Estimation and Results for Crime Reporting

This section presents estimates of the effects of female officer representation on crime reporting rates from fixed effects models. Our empirical specification starts with the following form:

(1) CrimeReported$_{ijt}$ = β_{SF}FemaleOfficerShare$_{jt}$ + βX_{ijt} + α_j + τ_t + ε_{ijt}

The unit of observation is a crime incident and the outcome *CrimeReported$_{ijt}$* is an indicator variable set to 1 if crime *i*, committed in geographic area (MSA) *j* and year *t*, was reported to police. The main explanatory variable, *FemaleOfficerShare$_{jt}$*, is the female share among officers in that locality and year; α_j and τ_t are fixed effects for MSA and year; and X_{ijt} is a set of controls for victim demographics, crime features and public policies. We estimate linear probability models using ordinary least squares and find that nearly all of the predicted values of the outcome fall within the range from zero to one. To allow for arbitrary serial correlation and correlated errors across victims within an MSA, we cluster standard errors at the MSA level.

Using a sample comprising all assaults against women, we first estimate the basic fixed effects model with no additional covariates in Column 1 of Table 2. The coefficient of 1.04 on *FemaleOfficerShare* indicates that each percentage point increase in female officer share in an MSA is associated with an equal percentage point increase in crime reporting by female assault victims in that MSA: a sizable increase in reporting when considered in absolute terms or relative to average reporting rates for female assault victims (49%; Table 1). In this table, as in the remaining tables in the paper, we only report estimates for the main variables of interest; tables with coefficients for all control variables can be found in Online Appendix B.

In Columns 2 to 4 of Table 2, we report estimates from models that add increasing sets of control variables. Our choice of controls is motivated by previous research on crime reporting and incidence of violence against women. The first factor we consider is women's educational and economic progress in an area, which could affect both female labor supply in policing and also domestic violence rates (Aizer 2010) and reporting decisions. Column 2 includes controls for female educational and labor market outcomes

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[29] Buzawa and Buzawa (1996; pp. 76-77) argue that reporting of domestic violence to police was important even before the VAWA reforms, as it was associated with lower rates of future violence: they interpret this as evidence that even the "classic" police response helped reduce repeated victimization. We discuss escalation of domestic violence and repeated violence in the next section. More recent evidence on the importance of reporting is in Carrell and Hoekstra (2012), which finds benefits from reporting of domestic violence to authorities that extend to exposed children and their peers at school.

(mean earnings, the fraction employed, and average years of schooling) at the MSA level from the Current Population Surveys (CPS; King et al. 2010)[30] and controls for individual-level information about the victim's race (Black), ethnicity (Hispanic), and education (years of schooling and an indicator for missing values) from the NCVS. We also control for increasing female empowerment during the period as measured by the female share of workers in non-traditional professional occupations, defined, as in Bailey, Hershbein and Miller (2012) to exclude nursing and teaching, at the MSA level. Another factor addressed in Column 2 is the potential correlation between female integration in law enforcement and state or local policy reforms aimed at improving police handling of violence against women. The landmark national policy on the issue, the 1994 VAWA, was enacted after our sample period and does not directly affect our estimates. However, earlier policy changes could have affected outcomes in our sample period, and are therefore included as controls. Specifically, we account for policies aimed at domestic violence or shown in the literature to have affected its incidence or severity: funds for police training awarded under the 1984 Family Violence Prevention Services Act (Newmark, Harrell and Adams 1995); a state-year level control for the generosity of welfare benefits based on the maximum AFDC payment to a single mother with two children (Nou and Timmins 2005); and state-year level controls for unilateral divorce laws (Dee 2003; Stevenson and Wolfers 2006), mandatory arrest laws (Iyengar 2009), and no-drop prosecution policies (Aizer and Dal Bó 2009).[31] Although several of these covariates are related to reporting rates, their inclusion reduces the main estimate by only a small amount.

Starting in Column 3, we categorize crime incidents based on the relationship between the victim and offender. We define the variable *Domestic* as an indicator for intimate partner violence, in which the attacker (or one of the attackers in case of multiple offenders) is a current or former spouse or boyfriend of the victim's. This applies to 18 percent of the observations in the sample of assaults against women. There are only 142 domestic violence cases with male victims in the NCVS (constituting 1.5% of all assaults against male victims) and so we are unable to examine reporting rates for them. We exclude these cases and thus, in practice, the *Domestic* variable is defined only for female victims. Because the policies listed above are mainly expected to affect reporting for domestic violence cases, we also add interaction terms between each of the policies and *Domestic*. The negative interaction effect between *Domestic* and no-fault divorce (in Online Appendix Table 2B) could mean that battered wives are less likely to report abuse to police if they can obtain a divorce easily without asserting it as a cause. Interestingly, we find a negative estimate for police training programs, but only for *non*-intimate partner violence.[32] Column 4 adds incident-level controls for additional crime characteristics from the NCVS: crime severity, its interaction with attempted versus completed status, and indicators for multiple offenders and an offender that was known to the victim but was not

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[30] When data are not available for the MSA we use the data for the state (weighted by county population in cases where the MSA includes counties from more than one state). We follow this procedure with other variables (such as policy reforms) that are only available (or defined) at the state level.

[31] Aizer and Dal Bó (2009) study the 50 largest cities in the US. Using their data, we coded each MSA (or county, in Section 5) as having a no-drop policy if any city within that MSA had one. We assigned values of zero for cities not included in Aizer and Dal Bó (2009).

[32] Although it is important to include these policy controls to rule out possible confounding effects, our estimates for these policies may not be representative of their full or long-term effects. This is particularly true for mandatory arrest laws, which are very rare in our sample. The only states with such laws in place by 1990 are Connecticut, Iowa, Missouri, and Nevada and only Missouri affects our MSA sample. While No-drop policies were more prevalent, they affected less than 8% of the observations in our MSA sample and only 88 domestic violence cases. Another criminal justice policy that changed in recent decades in the spread of "three-strikes" laws mandating long sentences, but these policies do not vary in our sample.

their intimate partner.[33] Our main estimate remains stable.

In the final column of Table 2, we explore the prediction from Section 2.2 that reporting of domestic violence will be especially responsive to female officer shares. We find strong confirmation of this prediction in the large (1.16) and significant positive coefficient on the interaction term between the domestic violence indicator and the female share. The estimates in Column 5 indicate that a 7.4 percentage point increase in the female officer share (corresponding to the increase in average female officer share over the sample period; Table 1) increases reporting of intimate partner violence by 13.6 percentage points (5.1 + 8.5) and reporting of other assaults against female victims by a more modest 5.1 percentage points.

In contrast to these strong relationships for female assault victims and female officers, we do not detect significant relationships for male assault victims, or female civilian shares, or for the civilian share among police employees.

We estimate the relationships for male victims in part as falsification checks implied by the first two gender-specific mechanisms in Section 2, in part to test for overall effects on officer quality under the third mechanism, and in part to assess tradeoffs that may exist in improving quality for different types of crimes. The lack of an effect for male victims (Table 3, Column 1) indicates that increases in the female share of officers do not deter male victims from turning to the police or harm the general perceptions of local police quality. It shows that the increase in reporting in Table 2 is specific to female victims and not caused by changes in overall trust in police or reporting propensities among all victims of violent crimes. This pattern fits the predictions of the two gender-based mechanisms discussed in Section 2 but is less consistent with the third mechanism relating to changes in officer quality.

The remaining columns in Table 3 show that the increase in reporting is related to the increase in female officer representation in particular, and not related other features of police employment. Columns 2 and 3 show that the female share among civilian police employees does not predict increased reporting of violence against women (in general or for domestic violence).[34] Similarly, the results in Columns 4 and 5 rule out effects of another change in police employment during the period, namely, the increase in the civilian share among police employees (which would affect the overall female share because civilian employees are far more likely to be female; see Table 1). Taken together, the estimates in Tables 2 and 3 show that the meaningful relationship is between female officer shares and reporting by female assault victims.

In Table 4, we incorporate the main estimates for women and the falsification check for men into an expanded version of the model that we estimate on a pooled sample of assault victims of both sexes. Thus, we estimate a model of the form:

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[33] Felson et al. (2002)'s examination of victims' stated reasons for reporting or not reporting domestic violence charts their complex motivations and concerns. On the one hand, domestic violence victims often perceive their assaults as more serious than assault by strangers because of the higher chance of recurrence. On the other hand, they feel heightened privacy concerns that inhibit reporting.

[34] Using police employment data from confidential EEO-4 reports (obtained from the Equal Employment Opportunity Commission), and using linear interpolation to fill in missing years (data are only available for 1977, 1980, 1984, 1985, 1987, 1989, and 1989 in our sample period) we also investigated incremental effects of increasing the share of female officers in *higher ranks* (in the professional or managerial job categories; see discussion in Miller and Segal 2012). However, we did not find consistent and statistically significant differential effects beyond increasing the female share among all sworn officers. Researchers interested in obtaining access to EEOC data should contact Ronald Patrick Edwards.

$$(2)\ CrimeReported_{ijt} = \beta_{SF}FemaleOfficerShare_{jt} + \beta_{SF\_F}\ Female \times FemaleOfficerShare_{jt}$$
$$+\ \beta_{SF\_D}\ Domesic \times FemaleOfficerShare_{jt}\ + \beta X_{ijt} + \alpha_j + \tau_t + \varepsilon_{ijt}$$

The insignificant coefficient on *FemaleOfficerShare* reflects the lack of an effect for male victims (similar to the model in Column 1 of Table 3, but with the restriction that the controls have the same coefficients for men and women, and with additional interactions between policies and *Domestic*). The main variable of interest is the product *Female×FemaleOfficerShare*, which measures the increase in reporting by female victims *relative to male victims* when the female officer share increases. This interaction is positive and significant (in Column 1), which confirms the first result of Table 2. Because the expanded model includes reporting rates for male victims as a counterfactual for unobservable changes in local factors that affect reporting rates for all victims, the shift to a comparison by victim sex enables us to include a richer set of fixed effects. The effect of female officers remains substantial in predicting reporting of crimes against female victims (relative to male victims) with a complete set of MSA by year fixed effects (Column 2). The large and significant coefficients on *Domestic×FemaleOfficerShare* in Columns 3 and 4 of Table 4 show that the second main finding of Table 2, a substantially larger impact of female officers on reporting of domestic violence, is also confirmed in the comparisons with male victims (with either MSA and year fixed effects or MSA by year fixed effects).

In the same spirit as the comparison by victim sex, we also test an alternative identification strategy that compares the effects of female officer shares on crime reporting of violent versus property crimes, using a pooled sample of assaults against female victims and burglaries (defined as unlawful or forcible entry or attempted entry of a residence that usually, but not always, involves theft or attempted theft) of households with female members (aged 12 or older). We find similar results (reported in Online Appendix Table 1A) using the property crime counterfactual for local trends in reporting propensities. The female officer share is significantly related to reporting of assaults, and particularly domestic violence, against female victims, but not to reporting of burglaries. The absence of an effect of female officers on property crimes again suggests that the third mechanism, from overall changes in officer quality, is less important than the first two. As with the male-female comparison, the inclusion of MSA by year fixed effects does not alter these results.

B.   *Alternative Hypotheses for Reporting Results*

The results in the previous section paint a consistent picture of female officers increasing reporting of violent crimes against women, but they may not capture the casual effect of female officers if police departments made other changes that would increase reporting around the same time that they hired more women. This section explores, and reports evidence against, the main possible alternatives that could generate changes in policing contemporaneous with increased female representation, but not caused by it.

One possibility is that female shares are related to department size. This would happen if all departments equally wanted to hire more women, but growing departments were able to do so more rapidly and extensively. Because it is possible that having more officers per population could increase reporting (if police are less time-constrained and more visible and available to citizens), we include this variable as an additional control in our main models (from Columns 1 and 3 in Table 4). The officers per population variable has no

independent effect on reporting rates for male or female assault victims overall and the estimates from the expanded model in Column 1 of Table 5 show no change in the *Female×FemaleOfficerShare* estimates (Panels A and B) or the interaction *Domestic×FemaleOfficerShare* (Panel B).

The next alternative we consider is that police departments increased their hiring of female officers specifically to handle an increase in assaults against women reported to them. In that case, the increase in female representation would properly be an outcome rather than an explanatory variable, and our estimates would likely be biased upward (if trends in reporting continued). We address this concern by computing the annual rates of reported assaults (total or domestic violence) per population by victim sex and MSA using the NCVS data. Adding these values, lagged by one year, as controls in our model does not affect the main estimates for *Female×FemaleOfficerShare* or *Domestic×FemaleOfficerShare* (Column 2 of Table 5).[35] Similarly, Column 3 shows the stability of our estimates to adding controls for the previous year's homicide rates in the MSA. Intimate partner and non-intimate partner homicides are included separately. The former is not related to reporting; the latter has a positive association.

In the next column of Table 5, we consider the possibility that changes in female officer shares are related to the severity or nature of crime in the local area. The concern here is that MSAs with more homicides or that are more affected by the crack epidemic are less likely to hire female officers and also have lower crime reporting rates for assaults and domestic violence. Column 4 shows the stability of our main estimates in a model that includes the current period non-intimate partner homicide rate and the Fryer et al. (2013) city-level index for the crack cocaine epidemic in that year.[36]

In Column 5 of Table 5, we revisit the issue of changing gender attitudes that may increase both female representation in law enforcement and reporting of crimes against female victims. The robustness of the main estimates to controls for the victim's own education and local area controls for female education, employment, and average earnings, as well as the female share in non-traditional occupations, argues against an overall story about female empowerment. We confirm this further by expanding the model to include measures of sexist attitudes held by men and women (at the Census-region and year level) from the General Social Surveys (Smith, Marsden and Hout 1979-2010).[37] We find significant associations between sexist attitudes and crime reporting, but the main estimates are not affected by these new controls. Column 6 shows the results are also robust to including all of the extra controls from Table 5 in a single model.

The final alternative hypothesis we consider is that departments hired more female officers at the same time that they established designated units for domestic violence cases or victim assistance or that they instituted policies for handing domestic disturbances. This is related to the hypothesis examined in Columns 2 and 3 of Table 5 that increased reporting of crimes against women triggered the desire to hire more female officers, but does not require a specific trigger from previous cases. Instead, the reason may have been

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[35] In a separate analysis, we also find no evidence to support the idea that female officer shares tend to increase after increased reporting of these crimes.

[36] These data were downloaded from <http://scholar.harvard.edu/fryer/publications/measuring-crack-cocaine-and-its-impact> on March 9, 2012. For each MSA we use the value for the largest city in that MSA. Two MSAs (Nassau-Suffolk and West Palm Beach-Boca Raton) have no city data, so we use the state values (for NY and FL, respectively). The index starts in 1980; we use 1980 values for the years before 1980.

[37] Specifically, we created indicator variables for negative responses to each of the questions: "If your party nominated a woman for President, would you vote for her if she were qualified for the job?" and "Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?" The sexism variable is the sum of these indicators (so higher values represent more sexism). Values are interpolated linearly for years in which data are unavailable.

random (local political pressure), but as long as the increase in female representation was contemporaneous with the institution of other programs, our estimates will capture both the effects of female officers and the effects (if any) of those programs. Our main data sources do not allow us to consider this question directly, so we use data on local police departments from the two earliest waves of the Law Enforcement Management and Administrative Statistics (LEMAS; US Department of Justice, Bureau of Justice Statistics 1987, 1990) surveys.

Using the balanced panel of departments surveyed about these programs in 1987 and 1990, we test for any associations between the programs and the female employment shares at the department, after controlling for year and department fixed effects and the size of the department (measured by numbers of officers and civilians) and of the population served. The results, reported in Panel A of Table 2A in the Online Appendix, show no significant individual associations between the individual programs – having a designated unit for domestic violence or victim assistance (overall or full-time), or having a formal policy for handling domestic disturbances – and female employment shares. Columns 1 and 2 show this for female officer shares and Columns 3 and 4 do this for female civilian shares. The F-tests on the joint significance of the programs also fail to reject the hypothesis of no association. Panel B of the table confirms the lack of an association using alternative measures for the programs of interest. Columns 1 and 3 use an indicator for having any program (to further address concerns that adoption of the different programs may be correlated) and Columns 2 and 4 have indicators for having 1, 2 or 3 programs (with the omitted group being no programs) to capture variation in program intensity. The absence of significant associations (and inconsistent signs of the relationships), as well as the results in Columns 2 and 3 of Table 5, likely reflects the fact that hiring female officers was not widely advocated as means to improve police responses to violence against women during our sample period. In fact, the 1994 federal VAWA has no specific provisions related to female officers.[38]

The results in Table 5 and Online Appendix Table 2A provide additional support for interpreting the estimates in Section 4.1 as reflecting the impact of increasing the female share of officers.[39] We provide further support for this interpretation in Section 6 when we instrument for the female share of officers using externally-imposed AA. Before turning to those estimates, we first measure the relationship between female officers and domestic violence escalation rates.

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[38] The only parts of Public Law 103-322 (the law that contains the VAWA) that relate to female officers in particular are the requirements (in Sec. 1702) that departments applying for Title I grants "provide assurances" that they will "to the extent practicable, seek, recruit, and hire members of racial and ethnic minority groups and women in order to increase their ranks within the sworn positions in the law enforcement agency" and (in Sec. 200107) that states participating in the Title XX Police Corps program "make special efforts to seek and recruit applicants from among members of all racial, ethnic or gender groups." The main provisions of the VAWA relate to: federal penalties for sex crimes and federal grants for crime prevention, victim assistance programs (including establishment of the national hotline and support for shelters and community-based programs), promotion of policies that increase domestic violence arrests, and training and educational programs (for police, prosecutors, judges, court personnel). The VAWA also includes new data collection, research and confidentiality requirements, and modifies evidentiary rules for sex offense cases.

[39] In separate regressions, we also confirmed the robustness of the results to the tests in Table 5 using the identification approach in Equation (1) on the sample of only female victims (used in Table 2).

V. Effects of Female Officers on Domestic Violence Escalation

Having uncovered a strong relationship between female officers and domestic violence reporting in the previous section, we now examine the effects of this improved contact with police on the escalation of violence between intimate partners. If police intervention is generally effective at reducing escalation rates, then we should expect to find lower escalation rates resulting from the increase in reporting induced by female officers. We expect further reductions in escalation if the *likelihood* or the *quality* of the police response improves with additional female officers. Alternatively, if officer quality diminishes as the female share increases, even the positive effects on crime reporting may not translate into lower crime rates and could even increase escalation if police involvement triggers retaliation. Motivated by this theoretical ambiguity, and the policy importance of preventing escalation, we examine the empirical relationship between female police representation and domestic violence escalation in this section. Our primary measure of escalation captures the extreme negative outcome of intimate partner homicide while our secondary measure captures repeated incidents of domestic violence reported in the NCVS.

A.   *Estimation and Results for Intimate Partner Homicides*

We examine the relationship between female officer representation and intimate partner homicide rates using an empirical specification of the following form:

(3) $IPHRate_{sjt} = \beta_{SF}FemaleOfficerShare_{j,t-1} + \beta X_{j,t-1} + \gamma Z_{j,t} + \alpha_j + \tau_t + \varepsilon_{sjt}$

The unit of observation is a county-year and the outcome $IPHRate_{sjt}$ is the number of intimate partner homicides (IPH) per 100,000 population in county j and year t with victims of sex s. In keeping with the literature on intimate partner homicides, we separately consider both male and female victims.[40] Because the mechanism for the hypothesized effect (preventing escalation of violence) is expected to occur over time, we use the previous year's female share among police officers in the county as our main explanatory variable.

We control for potentially confounding factors by including covariates, also lagged by one year, that capture economic conditions (for both sexes, to account for potential victims and offenders, available at the state-year level in the CPS)[41] and the same public policies used in the reporting analysis in Section 4. We account for potential differences in domestic violence escalation and reporting rates by county size and race with controls for lagged county population and lagged population share White. We also follow the literature on crime outcomes and control for the lagged value of number of police officers per

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[40] The reason that improved policing of domestic violence against women is expected to reduce homicide rates for male victims is that battered women sometimes kill their abusers in self-defense or in defense of their children (Saunders 2002). Justifiable homicides are included in the SHR data we study, though they are not included in summary totals of homicides published from the UCR. In addition, although our reporting estimates in Section 4 are limited to female domestic violence victims, it is worth noting that male victims of domestic violence often report very low satisfaction with the police. Therefore, it is possible male victims of domestic violence would find female officers more compassionate or more likely to take them seriously (as in the first mechanism in Section 2), which could increase reporting and reduce escalation for male victims as well. We are unable to examine reporting rates for domestic violence with male victims in the NCVS because there are only 142 such cases in our sample.

[41] Before 1977, the CPS data only identify large states separately. For counties in smaller states in 1976, we use the mean values for their state group.

population. Together, these lagged controls compose $X_{j,t-1}$. α_j and τ_t are county and year fixed effects. We estimate the IPH rate model in levels, and not the logarithmic or log-odds transformations sometimes used in crime rate regressions because of the relative infrequency of IPH: even in our preferred estimation sample of large counties, about 40% of all county-year observations have zero IPH counts for male victims. In all regressions, we cluster standard errors at the county level to allow for arbitrary serial correlation and weight observations using the county population.

Across the range of specifications in Table 6, we find a negative and significant relationship between the previous year's female share of officers in the county and the current year's IPH rates for both female (Panel A) and male (Panel B) victims. Column 1 shows estimates from the fixed effects model without covariates, estimated on a sample of all counties with population over 150,000 for every year in the sample period (1977 to 1991). The coefficients imply that a 6 percentage point in increase in the female officer share (corresponding to the increase in the average female officer share over the sample period; Table 1) leads to a decline of 0.087 deaths per 100,000 population for women (a 13.8 percent decline relative to the sample mean of 0.63 in 1977; Table 1) and 0.13 for men (22.2 percent of the mean rate of 0.60 in 1977). Column 2 reports similar estimates after including the basic set of control variables.

The estimates in Column 3 are from our preferred specification, which includes the contemporaneous controls in $Z_{j,t}$: the county's non-intimate partner homicide rate (similar to Aizer 2010) and the (state-level) crack cocaine index (Fryer et al. 2013) to account for county-specific changes in overall violent crime rates.[42] Column 4 then adds region-by-year fixed effects (for each of the nine Census divisions) to account for arbitrary time trends in unobservable region-specific factors. The inclusion of all of these controls has only minor effects on the main estimates, increasing the magnitude of the coefficient for female victims to −1.5 and decreasing it for male victims to −1.8.

Online Appendix Table 3A reports additional robustness checks that confirm the main estimates. The main coefficients are very similar (slightly larger for female victims) when the model is estimated without population weights (Column 2; compare to Column 3 of Table 6). This consistency across weighting schemes suggests that the model is well-specified and effects are relatively homogeneous over our sample (Solon, Haider and Wooldridge 2013). The remaining columns of the table show significant negative effects of female officers on the *number* of IPH deaths (Column 3) and the natural logarithm of the IPH death rate (which excludes county-year observations with zero deaths; Column 4) and from a Tobit specification for the main outcome, the *IPHRate* (Column 5). Finally, the placebo test in Column 5 of Table 6 (in the spirit of the test in Column 2 of Table 3) finds no significant association between the female share *among civilian police employees* and intimate partner homicide rates for victims of either sex.[43]

This evidence indicates that the change in the sex composition of law enforcement during the late 1970s and 1980s had a meaningful impact on reducing the ultimate escalation of domestic violence, possibly because of the substantial increase in reporting rates and changes in police officer behavior. The finding of statistically significant effects on IPH rates for victims of both sexes, and not only for women, is also present in Iyengar

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[42] We use the state-level values of the crack index for the county-level analysis to include all counties. As mentioned above, because the crack index starts in 1980, we use 1980 values for the years before 1980.

[43] When we repeat the placebo test from Column 4 of Table 3 using the lagged share of civilian workers, we also find it to be uncorrelated with IPH rates.

(2009).[44] That feature of the results is nevertheless notable for our sample period when IPH rates were initially similar between the sexes (the 1977 rates are 0.63 and 0.60 for women and men, respectively), but constituted very different shares of total homicides (about 33 percent for women and less than 9 percent for men). During our sample period, IPH rates also declined much more for men (to 0.29 in 1991, less than 3 percent of all homicides with male victims) than for women (to 0.58 in 1991).

B. Estimation and Results for Repeated Intimate Partner Violence

The results of the previous sub-section show a strong link between increased female officer shares and lower homicide rates related to domestic violence. In this sub-section, we present evidence on an intermediate outcome that captures a potential channel for this effect. In particular, using NCVS data, we focus on women who report experiencing domestic violence at least once in the sample period. The question that we ask is if the female share of officers in the area at the time of each incident affects the likelihood of the woman being assaulted again by an intimate partner during her period of participation (up to 3 years) in the NCVS.[45]

Building on the regression framework used for reporting in Section 3 (Equation 1), we alter the dependent variable from *CrimeReported* to *RepeatOffense* and estimate this equation:

$$(4) \quad RepeatOffense_{ijt} = \beta_{SF}FemaleOfficerShare_{jt} + \beta X_{ijt} + \alpha_j + \tau_t + \varepsilon_{ijt}$$

The sample includes all domestic violence offenses (intimate partner assaults with female victims) and the new outcome captures future offenses after the current one.[46] The main explanatory variable is still *FemaleOfficerShare_{jt}*, the female share of police officers in the area. We start by using the controls for individual, incident, and local area factors (as these can affect both reporting rates and repeat violence) from the reporting regressions in Section 4. Then, we add the crime controls used in the previous section (i.e., officers per population, population, crack index, and non-intimate homicides in the MSA, and the economic conditions of males in the MSA). As above, standard errors are clustered at the MSA level to allow for arbitrary correlations across incidents and over time within the MSA.

We find a negative and significant effect of the female officer share in determining the likelihood that a domestic violence offense is repeated while the respondent is in the survey. The size of this estimate is meaningful: a 7 percentage point increase in female officers (the overall increase in the NCVS sample) is associated with a 23 percentage point reduction in repeated domestic violence rates (in Columns 1 and 2 of Online Appendix Table 4A for the basic model and model with additional crime controls).

A possible concern with these estimates is that domestic abuse that occurs after individuals exit from the NCVS sample is not observed. The estimates will be not be biased by random variation in the timing of abuse relative to the end of the household's

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[44] Aizer and Dal Bó (2009) only find statistically significant effects for male victims. Stevenson and Wolfers (2006) find significant IPH effects for female victims and statistically significant intimate partner violence effects for "severe violence" against male victims.

[45] For individuals interviewed after 1987 our measure of future domestic violence assaults includes those occurring after 1990 (as long as they are reported in the NCVS).

[46] When multiple domestic violence incidents are reported against a single victim within the same month, we use the record number variable (RECSEQ) to define a unique order, assuming that lower numbers are assigned to earlier incidents.

sampling window. However, attrition before the end of the 3-year sampling window can be related to female officer shares. Attrition is a meaningful indicator of reduced escalation if it is caused by women leaving their abusive partners. If attrition is instead caused by couples or families moving together because of factors (like increased police intervention) related to increased female officer shares, the negative estimates may not reflect actual reductions in domestic violence assaults. We address this concern by restricting the sample to observations of households interviewed at least once more after the focal domestic violence incident. The conclusions are unchanged (Column 3 of Online Appendix Table 4A). Thus, the results of this exploratory analysis support the relevance of one channel for the effect of female officers on IPH rates, namely, preventing ongoing violence and escalation within households.

It is natural to ask if NCVS data can also be used to investigate the effects of female officer share on rates of violent crimes across MSAs and over time. We report estimates in Online Appendix Table 5A. Column 1 reports a negative and significant effect for overall domestic violence rates, consistent with the escalation results in this section. However, the small sample of total domestic violence incidents (1,146 observations, on average fewer than 3 observations per MSA-year) may not produce reliable measures of aggregate trends. We consider other violent crimes in Columns 2 and 3 of the table, but again, these aggregate estimates may be unreliable because of the small numbers of underlying observations. We find insignificant effects of female officer on total assaults for either female or male victims. Online Appendix Table 6A reports estimates using UCR data on rapes and assaults reported to police. In addition to the limitation that increased reporting rates will inflate crime rates, these data are not sufficiently detailed to identify assaults against women (until 2012, the definition of rape required a female victim) or domestic abuse. Though the point estimates are negative, we find no statistically significant effects of female officer shares on reported rapes or assaults. When considered alongside the increase in reporting by female assault victims (and no change for male victims), the UCR estimates do suggest that increasing female officer shares lead to declines in overall rates of assaults. They do not suggest the presence of overall officer quality effects or tradeoffs.

VI.   Effects of Affirmative Action on Police Quality

This section revisits the results of the previous two sections using an alternative identification approach that focuses on increased female representation induced by externally imposed AA. As discussed above, these estimates have the potential advantage of being more reliably identified than the estimates in the previous sections, but they may also reflect a different underlying relationship if AA itself has direct effects on police quality, separate from the effects of increased female officer shares. The reasons for direct effects, discussed in Section 2, include a possible drop in average officer quality or greater opposition from male officers, when the female officer share is increased under external pressure.

We base our AA exposure measures on the legal database in Miller and Segal (2012).[47] Our key variable of interest is *YearsAAOn*, defined as the difference between the current

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[47] We did need to modify the legal database in Miller and Segal (2012) slightly for this paper. First, we limited the sample to county and municipal departments (excluding state police). Second, after confirming (as in Miller and Segal 2012) that the share of female officers increases after litigation, even without externally imposed AA plans, we grouped these departments in the AA group. If we repeat the regressions treating the litigated only departments separately, the results for AA are essentially identical. Third, we added 3 previously

year and the start year of AA (or litigation year for cases that did not lead to explicit plans) for the earliest plan in the area (MSA or county). *YearsAAOn* does not continue to accrue additional years of AA exposure after the AA end date of the latest plan. We include all litigation based on employment discrimination and the resulting AA plans. It is important to note that these cases are all based on employment discrimination, specifically related to hiring, promotion or termination practices; the cases are not about police misconduct or other civil rights violations. Including the full set of plans provides useful variation in the database, but may cause us to under-estimate the impacts of AA that is targeted on female employment. Finally, because the Miller and Segal (2012) legal database covers only the largest 429 police departments (479 including state police), it does not include all of the departments used the current analysis. We assign zero years of exposure to departments with no information, which may also bias our estimates downwards if we are treating some litigated departments as un-litigated.

In order to produce estimates that are comparable to those in the prior sections, we implement our alterative identification approach by estimating IV versions of the equations presented in the previous sections (Equation 2 for reporting, Equation 3 for IPH rates, and Equation 4 for repeated domestic violence), instrumenting for the main explanatory variable of *FemaleOfficerShare*, and in Equation 2, for its interactions with female victim and domestic violence indicators, with years of AA exposure (*YearsAAOn*) and the appropriate interaction terms. The samples and controls are unchanged from those described in Sections 4 and 5.

For each estimation sample, we first confirm the first stage impact of AA exposure in increasing female officer shares found in Miller and Segal (2012). These estimated coefficients on the *YearsAAOn* variable are all positive and highly statistically significant. In the crime reporting sample in Table 7, the coefficient is 0.0025, with a cluster-robust standard error of 0.00068 (t-statistic of 3.7 and F-statistic of 13.8). The first stage estimate is similarly positive and significant on the sub-sample of domestic violence incidents used to measure repeated abuse Online Appendix Table 4A (coefficient of 0.0029, cluster-robust standard error 0.00077, t-statistic of 3.8 and F-statistic of 14.2, for *YearsAAOn* in Column 4). Each additional year of AA is also linked to a significant increase in the female officer share in the IPH sample in Table 8, where the coefficient on lagged *YearsAAOn* is 0.0031, with a cluster-robust standard error of 0.00047 (t-statistic of 6.6 and F-statistic of 43.5). These strong test results for the effects of AA exposure on female officer shares suggest that weak instrument bias is unlikely to be driving our IV estimates.[48] We also confirmed that all of the main results are unchanged in sign and

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excluded departments to the AA group: Santa Ana (CA), Orange County (FL), and Detroit (MI). Santa Ana and Orange County were excluded from Miller and Segal (2012) because the protected group was based on ethnicity, which is not a basis for exclusion in this paper. The Detroit Police Department had a well-known (and litigated for reverse discrimination) *voluntary* race-based plan that was not externally-imposed for the study of Black employment. The department also operated under externally-imposed plans for female employment during the sample period (e.g. *Schaefer v. Tannian*). Because our focus here is on gender, rather than race, we include Detroit in the AA group.

[48] Furthermore, all of our models are just-identified, with the same number of instruments as endogenous variables, which also limits concerns about weak instruments. Because there is no clear-cut procedure for testing for weak instrument bias in the case of non-i.i.d. errors, we are not able to conduct formal tests on our sample. For the case of clustered standard errors, in addition to the single-equation F-statistics reported in the text, we computed the Kleibergen-Paap rk Wald F-statistics that also take into account the number of endogenous variables: these values are around 40 for the IPH models in Table 8, 5 to 7 for the reporting models in Table 7, and 14 in the repeated domestic violence model in Table 4A. However, there is no theoretical basis for applying the Stock and Yogo (2002) critical values derived for the i.i.d. case or even applying the "F > 10 rule of thumb." Across all of our models, the Cragg-Donald Wald F-statistic, computed assuming i.i.d. errors, is always above 100 and well above the Stock-Yogo critical values (when such values

significance in reduced form versions of the models, which provides further reassurance that bias from weak instruments is not the source of the significant IV estimates reported below.[49]

Our IV estimates for crime reporting outcomes (in Columns 2 and 3 of Table 7) are qualitatively unchanged from our OLS estimates. The IV models use our preferred identification approach, in which male assault victims provide counterfactuals, corresponding to the OLS models in Columns 1 and 3 of Table 4. In the IV models, we instrument for the three potentially endogenous variables (*FemaleOfficerShare*, and interactions with *Female*, and *Domestic*) with three AA variables (*YearsAAOn*, *Female×YearsAAOn*, and *Domestic×YearsAAOn*). The IV coefficients imply a 1 percentage point AA-induced increase in female officer shares increases reporting of all assaults against women by 1.3 percentage points (statistically significant at the 10 percent level) and increases reporting of domestic violence assaults by 4.5 percentage points (1.3 + 3.2, statistically significant at the 1 percent level).

The reductions in IPH rates from OLS models in Table 6 are also repeated in the IV models in Table 8. These IV estimates are from the same county-year sample (with more than 150,000 population in the sample period). The IV estimate for lagged *FemaleOfficerShare* in the IPH regression for female victims is −4.1 (standard error of 1.4; Column 2) in the basic model (corresponding to the model in Column 1 of Table 6) and −3.1 (standard error of 1.6; Column 3) in the expanded model with additional crime controls and region-by-year fixed effects. Similarly for male IPH victims, the IV estimate is −4.2 in the basic model (Column 4) and −3.9 in the expanded model (Column 5). In addition to supporting the OLS findings for the main outcomes, the corresponding IV model also confirms the results of the exploratory analysis presented in Section 5.2 on repeated domestic violence as a channel for the escalation results. The estimates are reported in Online Appendix Table 4A. Column 5 reports the IV estimate of −8.1 (standard error of 3.4) for *FemaleOfficerShare* in the main equation for repeated violence.[50]

One potential concern about the IV analysis based on years of AA exposure is that AA itself might have occurred following increasing trends in female officer shares (reflecting trends in female labor supply) or in domestic violence reporting or incidence. We consider this potential explanation by testing for spurious placebo effects of AA in the years *before* AA starts, first, using an expanded reduced form version of our parametric model with separate linear trends for the years before and after AA, and second, in a non-parametric model that allows for arbitrary non-linear trends around AA.

Neither type of analysis suggests that effects of AA reflect the continuation of preexisting trends. In the parametric reduced form analyses of crime reporting IPH rates and repeated domestic abuse, the years *after* AA variables (*YearsAAOn* and interactions with victim sex and crime type for the crime reporting models) are always statistically significant (and share the same sign as the IV estimates) but the years *before* AA variables never are. This pattern tightens the empirical link between AA implementation and the main outcomes of interest.

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are available, which is not the case for the model with 3 endogenous variables and 3 instruments used in Column 2 of Table 7).

[49] As discussed, for example, in Chernozhukov and Hansen (2010), reduced form estimates are unbiased even when instruments are weak.

[50] Similar to the OLS estimates, Online Appendix Table 5A also reports a marginally significant (at the 10.1% level) reduction in domestic violence (but no effects on total assaults) incidence using NCVS data and Online Appendix Table 6A reports negative but statistically insignificant declines in reported rapes and assaults. Section 5.2 describes the data limitations that apply to these estimates.

The non-parametric analyses allow us to explore trends in outcomes in the years immediately preceding and following AA initiation, without imposing a linear relationship. We depict the results in Figures 1 and 2, for the NCVS and IPH samples, respectively. Each curve plots the set of point estimates for indicator variables for small groups of years before or after AA starts. We generally use 2-year bins, but use 4-year bins to avoid small cell sizes in the repeated abuse sample for observations more than 2 years from the litigation date. The figures show several patterns. First, for all of the outcomes, they confirm the lack of pre-trends from the linear model in the years closest to AA. Second, they support the general pattern of increasing effects with more years of AA exposure. The figures also show that female officer shares were not growing faster in departments before they were sued for discrimination. In the crime reporting sample, there appears to be a relative decline in female office shares before AA that is reversed under AA.[51]

Across all of the outcomes explored in this paper, the AA estimates are always consistent in sign and significance with the OLS estimates and generally larger in magnitude. The falsification analyses in Sections 4 and 5 indicate that the reason for the difference is not that the OLS estimates are biased by correlations with omitted variables. Instead, the larger IV estimates may result from heterogeneous treatment effects. One reason that the average effect of AA-induced growth might be larger is if increases in the share of female officers have a larger impact on departments with recent histories of employment discrimination. Another potential reason for the larger IV magnitudes is measurement error police employment (as discussed in McCrary and Chalfin 2013).

In either case, the consistency of the results between estimation approaches strengthens the causal interpretation of the evidence in this paper that female officers improved police quality for female assault victims and victims of intimate partner violence. It also shows that quality improvements were achieved even when the increase in female representation was a result of external pressure. These results provide empirical evidence that AA can improve quality in certain circumstances, as suggested by recent laboratory experiments, such as Niederle, Segal and Vesterlund (2013) that finds that gender-based AA increases the willingness of highly qualified women to enter competitions.

VII. Conclusion

This paper studies the effects of female integration in the traditionally male dominated occupation of law enforcement in US localities between the late 1970s and early 1990s. Using national panel data on crime victimization and homicide reports from the Department of Justice, and a variety of identification strategies, we estimate the effects of increasing the female share of officers on police quality related to violence against women and domestic violence. Our primary quality measures capture two key dimensions: crime reporting rates and domestic violence escalation. We find that increasing female shares among officers, but not among civilian police employees, improves outcomes along both dimensions. Our investigations of other crimes and victims uncover no significant effects of female officers on overall assaults or reported rapes. All of the findings are robust to studying increases in female officer shares induced by exposure to externally imposed AA. The results may be attributable to gender differences in officer preferences or productivity. They provide new evidence that an aspect of police employment policy affects the

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[51] Separate estimates also reveal no apparent effect of AA exposure on the share of women in non-traditional occupations in the area, in either parametric or non-parametric models, which tightens the link between AA and police employment.

behavior of crime victims and potential offenders.

Our results also suggest that police departments will respond more effectively to sexual assault and domestic violence cases if they continue to hire female officers and ensure that victims are able to request that those officers conduct their interviews and handle their cases. Although this paper does not directly examine the effects of policies or staffing procedures within departments that affect the assignment of female officers to assault cases for certain victims, our findings of overall effects of increasing female officer shares provide evidence supporting the presence of the underlying mechanism that motivates such policies. To the extent that these results apply outside of the US context, they may also support initiatives to hire more female officers as part of broader efforts to reduce violence against women, such as those recently undertaken in India (e.g., Crilly 2013).

Finally, our finding of quality improvements from externally imposed AA does more than provide an alternative identification strategy for estimating of the overall effects of female officers. First, it suggests that AA in employment can help members of protected groups other than those directly affected. The finding also implies that the equilibrium share of female officers at police departments targeted for interventions was sub-optimal, at least with respect to the outcomes examined in this paper. Our finding of improvements in these outcomes from the AA-based models shows that increasing female representation is still effective when achieved through externally imposed plans or quotas. Police departments operating under these plans may not have been convinced of the merits of hiring more women. Nevertheless, their outcomes improved along several key dimensions.

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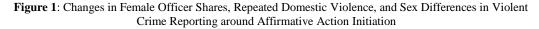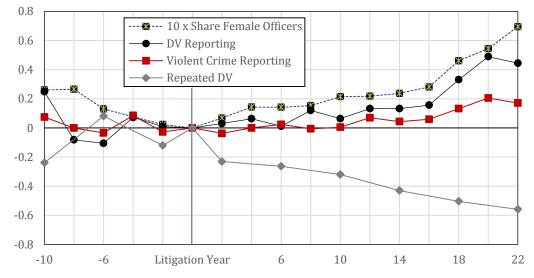
Figures

Figure 1: Changes in Female Officer Shares, Repeated Domestic Violence, and Sex Differences in Violent Crime Reporting around Affirmative Action Initiation

Notes: The figure depicits point estimates for two-year bins before and after affirmative action initiation (litigation year is the omitted category) for crime reporting and female officer shares from regression models estimated on the NCVS sample of violent crimes. Crime reporting estimates are for interactions between year bins and an indicator for either female victims of any violent crimes or of domestic violence. Male domestic violence victims are omitted from the sample. The estimates for the female share of officers are multiplied by 10 for readability. Repeated DV estimates are from the subsample of DV incidents; because of smaller cell sizes, we use 4 year bins for incidents more than 2 years before or after the litigation year.

Figure 2: Changes in Female Officer Shares and Intimate Partner Homicide Rates around Affirmative Action Initiation
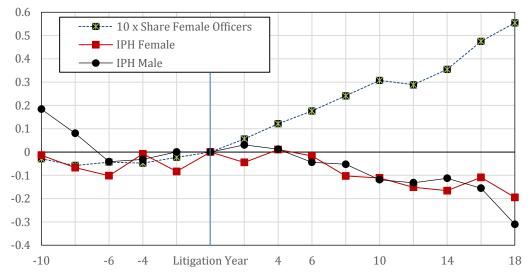

Notes: The figure depicits point estimates for two-year bins before and after affirmative action initiation (litigation year is the omitted category) from regression models estimated on the county-year panel of intimate partner homicide (IPH) rates per 100,000 population. The estimates for the female share of officers are multiplied by 10 for readability.

Tables

Table 1: Summary Statistics

Panel A: Fraction of Assaults Reported to the Police

		All	Male	Female	Female Domestic
Fraction reported	1979 - 1990	0.44	0.41	0.49	0.55
	1979	0.43	0.42	0.46	0.48
	1990	0.44	0.38	0.54	0.69
Observations		15, 319	9,037	6,282	1,146

Source: NCVS MSA Sample. Years: 1979-1990. Unit of observation is a crime incident.

Panel B: Intimate Partner Homicides (IPH)

		Counties with population above 150,000 in all years		All counties	
		Male	Female	Male	Female
IPH per Population	1977-1991	0.442	0.633	0.402	0.566
	1977	0.598	0.631	0.536	0.569
	1991	0.287	0.576	0.270	0.530
Observations with Zero IPH		1,494	881	39,209	37,258
[Population-Weighted Fraction]		[0.26]	[0.13]	[0.49]	[0.38]
Counties		255	255	3,084	3,084
Observations		3,732	3,732	45,032	45,032

Source: Supplemental Homicide Reports. Years: 1977-1991. Unit of observation is a county-year.

Panel C: Police Employment

	Reporting Sample			IPH Sample		
	All	1979	1990	All	1976	1990
Female Officer Share	0.086	0.053	0.127	0.063	0.032	0.095
Female Civilian Share	0.648	0.627	0.677	0.656	0.608	0.686

The "Reporting Sample" is the sample of all assaults (against male and female victims) used in Panel A above. The "IPH Sample" is the sample of counties in Panel B above with population above 150,000 in all years.

Table 2: Female Officer Shares and Crime Reporting by Female Assault Victims

Dependent variable: Was the crime reported to police? (Yes = 1)

	(1)	(2)	(3)	(4)	(5)
Female Officer Share	1.047**	0.950**	0.990**	0.931**	0.691*
	[0.479]	[0.449]	[0.446]	[0.419]	[0.386]
Domestic × Female Officer Share					1.155**
					[0.458]
MSA and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Victim and Local Area Controls	No	Yes	Yes	Yes	Yes
Domestic Indicator and Interactions	No	No	Yes	Yes	Yes
Crime Controls	No	No	No	Yes	Yes
Observations	6,282	6,282	6,282	6,282	6,282
R^2	0.018	0.026	0.030	0.065	0.065

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors clustered at the MSA-level in brackets.

The *Domestic* indicator is set to 1 if the victim is female and offender is the victim's current or former husband or boyfriend. Assaults against male victims by intimate partners (n=142 observations) are excluded from the sample. Victim Controls are: Black, Hispanic, Years of Schooling, and Missing Education. Local Area Controls are: Divorce Law, Mandatory Arrest Law, Police Training, No-Drop Policy, Maximum AFDC Benefits, Mean Earnings (MSA-Gender), % Employed (MSA-Gender), and Mean Years of Schooling (MSA-Gender), share female in nontraditional occupations (MSA). Domestic Interactions are with: Divorce Law, Mandatory Arrest Law, Police Training, No-Drop Policy, and Maximum AFDC Benefits (to a single mother with 2 children). Crime Controls are: Multiple Offenders, Known (Not-Domestic) Offender, Attempted Attack, Completed Simple Assault with Injury, Attempted/ Completed Aggravated Assault or Rape, and Attempted×Attempted/Completed Aggravated Assault or Rape.

Table 3: Falsifying Exercises for Crime Reporting by Male and Female Assault Victims

Dependent variable: Was the crime reported to police? (Yes = 1)

	(1) Male	(2) Female	(3) Female	(4) Female	(5) Female
Female Officer Share	-0.175				
	[0.523]				
Female Civilian Share		-0.010	0.012		
		[0.102]	[0.112]		
Domestic × Female Civilian Share			-0.121		
			[0.151]		
Share Civilians				0.213	0.211
				[0.234]	[0.236]
Domestic × Share Civilians					0.014
					[0.200]
MSA and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Victim, Crime, and Local Area Controls	Yes	Yes	Yes	Yes	Yes
Domestic Indicator and Interactions	No	Yes	Yes	Yes	Yes
Observations	9,037	6,282	6,282	6,282	6,282
R^2	0.089	0.065	0.065	0.065	0.065

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the MSA-level in brackets.
The *Domestic* indicator is set to 1 if the victim is female and offender is the victim's current or former husband or boyfriend. Assaults against male victims by intimate partners (n=142 observations) are excluded from the sample. See Table 2 notes for variables in Victim, Crime, and Local Area Controls, and Domestic Interactions.

Table 4: Comparisons by Victim Sex of Crime Reporting by Assault Victims

Dependent variable: Was the crime reported to police? (Yes = 1)

	(1)	(2)	(3)	(4)
Female Officer Share	0.080		0.059	
	[0.427]		[0.422]	
Female × Female Officer Share	0.838***	0.778**	0.657**	0.618**
	[0.291]	[0.293]	[0.280]	[0.288]
Female × Domestic × Female Officer Share			1.089**	1.004**
			[0.457]	[0.455]
MSA and Year Fixed Effects	Yes	Yes	Yes	Yes
MSA × Year Fixed Effects	No	Yes	No	Yes
Victim and Crime Controls	Yes	Yes	Yes	Yes
Domestic Indicator and Interactions	Yes	Yes	Yes	Yes
Local Area Controls	Yes	No	Yes	No
Observations	15,319	15,319	15,319	15,319
R^2	0.068	0.098	0.068	0.098

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the MSA-level in brackets.
The *Domestic* indicator is set to 1 if the victim is female and offender is the victim's current or former husband or boyfriend. Assaults against male victims by intimate partners (n=142 observations) are excluded from the sample. See Table 2 notes for variables in Victim, Crime, and Local Area Controls, and Domestic Interactions.

Table 5: Testing Alternative Hypotheses for Increased Reporting

Dependent variable: Was the crime reported to police? (Yes = 1)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Assault Reporting						
Female Officer Share	0.157	0.240	0.224	0.292	0.122	0.479
	[0.449]	[0.440]	[0.372]	[0.400]	[0.409]	[0.437]
Female × Female Officer Share	0.836***	0.699**	0.796***	0.838***	0.806***	0.726**
	[0.291]	[0.281]	[0.283]	[0.279]	[0.288]	[0.296]
R^2	0.068	0.069	0.068	0.068	0.068	0.071
Panel B: Assault and Domestic Violence Reporting						
Female Officer Share	0.131	0.216	0.202	0.269	0.101	0.442
	[0.442]	[0.438]	[0.369]	[0.397]	[0.405]	[0.434]
Female × Female Officer Share	0.657**	0.473*	0.593**	0.645**	0.628**	0.491*
	[0.280]	[0.269]	[0.271]	[0.265]	[0.277]	[0.271]
Female × Domestic × Female Officer Share	1.079**	1.309**	1.197**	1.150**	1.075**	1.366***
	[0.454]	[0.488]	[0.461]	[0.464]	[0.460]	[0.499]
R^2	0.068	0.070	0.068	0.069	0.068	0.072
MSA and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Victim, Crime and Local Area Controls	Yes	Yes	Yes	Yes	Yes	Yes
Domestic Indicator and Interactions	Yes	Yes	Yes	Yes	Yes	Yes
Police Officers per Population	Yes	No	No	No	No	Yes
Lagged Reported Assaults and Domestic Rates	No	Yes	No	No	No	Yes
Lagged IPH and Non-IPH Homicide Rates	No	No	Yes	No	No	Yes
Current Non-IPH Homicide Rate	No	No	No	Yes	No	Yes
Crack Index (linear and squared)	No	No	No	Yes	No	Yes
Male and Female Sexism in Region	No	No	No	No	Yes	Yes
Observations	15,319	13,669	15,206	15,143	15,319	13, 493

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the MSA-level in brackets.

The *Domestic* indicator is set to 1 if the victim is female and offender is the victim's current or former husband or boyfriend. Assaults against male victims by intimate partners (n=142 observations) are excluded from the sample. Column 2 and 6 omit observations missing lagged reported assaults and domestic violence rates; Columns 3, 4 and 6 omit observations with missing homicide data (for Florida in 1988 to 1990). See text for details about the sexism and crack index. See Table 2 notes for variables in Victim, Crime, and Local Area Controls, and Domestic Interactions.

Table 6: Female Officer Shares and Intimate Partner Homicide Rates

Dependent variable: Intimate Partner Homicides per 100,000 population

	(1)	(2)	(3)	(4)	(5)
Panel A: Female Victims					
Lagged Female Officer Share	-1.446***	-1.474***	-1.544***	-1.528***	
	[0.382]	[0.395]	[0.408]	[0.393]	
Lagged Female Civilian Share					-0.059
					[0.073]
R^2	0.562	0.566	0.569	0.588	0.567
Panel B: Male Victims					
Lagged Female Officer Share	-2.202***	-2.096***	-2.271***	-1.812***	
	[0.587]	[0.605]	[0.581]	[0.578]	
Lagged Female Civilian Share					0.023
					[0.096]
R^2	0.607	0.617	0.622	0.652	0.617
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Census Region (9)×Year Fixed Effects	No	No	No	Yes	No
Lagged Local Area Controls	No	Yes	Yes	Yes	Yes
Non-IPH Homicide Rate and Crack Index	No	No	Yes	Yes	Yes
Observations	3,732	3,732	3,732	3,732	3,732
Number of Counties	255	255	255	255	255

*** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the county level in brackets.

Sample is restricted to counties with population above 150,000 in all years. Observations are weighted by county population. The non-IPH homicide rate control is for female victims in Panel A and male victims in Panel B. See text for details about the crack index. Local Area Controls are: Officers per Population, County Population, White Population Share, Maximum AFDC Benefits (to a single mother with 2 children), Divorce Law, Mandatory Arrest Law, Police Training, No-Drop Policy, Mean Male Earnings, Male % Employed, Male Years of Schooling, Mean Female Earnings, Female % Employed, Female Years of Schooling, share female in non-traditional occupations.

Table 7: Affirmative Action Estimates of Crime Reporting by Assault Victims

Dependent variable: Was the crime reported to police? (Yes = 1)

	(1) First Stage Dep. Var.: Share Female Officers	(2) IV	(3) IV
Years of AA Exposure	0.003***		
	[0.001]		
Female Officer Share		-1.021	-0.961
		[1.199]	[1.174]
Female × Female Officer Share		1.913**	1.334*
		[0.846]	[0.751]
Female × Domestic × Female Officer Share			3.230**
			[1.312]
MSA and Year Fixed Effects	Yes	Yes	Yes
Victim, Crime and Local Area Controls	Yes	Yes	Yes
Domestic Indicator and Interactions	Yes	Yes	Yes
Observations	15,319	15,319	15,319
R^2	0.900		

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors clustered at the MSA-level in brackets.

The *Domestic* indicator is set to 1 if the victim is female and offender is the victim's current or former husband or boyfriend. Assaults against male victims by intimate partners (n=142 observations) are excluded from the sample. See Table 2 notes for variables in Victim, Crime, and Local Area Controls, and Domestic Interactions.

Table 8: Affirmative Action Estimates of Intimate Partner Homicide Rates

Dependent variable: Intimate Partner Homicides per 100,000 population

	(1)	(2)	(3)	(4)	(5)
	First Stage Dep. Var.: Share Female Officers	IV	IV	IV	IV
Victim Sex		Female	Female	Male	Male
Years AA On	0.003***				
	[0.000]				
Lagged Female Officer Share		-4.177***	-3.137*	-4.273**	-3.959***
		[1.404]	[1.617]	[1.745]	[1.467]
County and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Census Region (9) ×Year Fixed Effects	No	No	Yes	No	Yes
Non-IPH Homicide Rate and Crack Index	No	Yes	Yes	Yes	Yes
Lagged Local Area Controls	Yes	Yes	Yes	Yes	Yes
Observations	3,732	3,732	3,732	3,732	3,732
R^2	0.856				
Number of Counties	255	255	255	255	255

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors clustered at the county level in brackets.

Sample is restricted to counties with population above 150,000 in all years. Observations are weighted by county population. The non-IPH homicide rate control is for female victims in Columns 2 and 3 and for male victims in Columns 4 and 5. See text for details about the crack index. See Table 6 notes for variables in Local Area Controls.

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