Depolicing as Dissent Shirking: Examining the Effects of Pattern or Practice Misconduct Reform on Police Behavior

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Depolicing as Dissent Shirking: Examining the Effects of Pattern or Practice Misconduct Reform on Police Behavior

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Abstract

Theory suggests that bureaucratic actors express opposition to unfavorable organizational and policy changes by acting in ways inconsistent with established rules, norms, and community expectations . Empirical evidence from various professional contexts and geographic locations lends support to the notion that some public employees have indeed engaged in dissent shirking by refusing to perform at their best so as to express work-related dissatisfaction. This research relies on a quasi-experimental design to examine this phenomenon in the context of the police. The study's analysis will be driven by a series of autoregressive integrated moving average models in order to examine the extent to which a form of dissent shirking—"depolicing"—has occurred in jurisdictions investigated by the U.S. Department of Justice under that agency's pattern or practice authority. Despite qualitative support for depolicing under these conditions, this analysis shows no evidence that officers responded to external criticism and intensified oversight brought on by the pattern or practice reform process by policing less proactively. Findings are discussed in terms of both theory and policy.

Theory suggests that bureaucrats may respond to unfavorable changes to their professional environment by dissent shirking, or using their discretionary authority in ways inconsistent with established rules, norms, and community expectations (Brehm & Gates, 1999). Anecdotal evidence from various professional contexts and geographic locations lends support to the notion that some public actors have neglected to perform at their best as a way of expressing work-related dissatisfaction or disagreement with policy (e.g., Barbour, 2010; Troy, 2007; Tucker, 2015).

Many of these examples involve the police. One way this resistance is manifest in the context of law enforcement is in the form of a work slowdown or reduced policing intensity. Officers have

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spoken candidly about their willingness to police less proactively in response to disputes over wages (Bates, 1995; McDonald, 1998; McNish, 1980), working conditions (Osher, 2010), and public criticism (Oliver, 2015; Wolfe & Nix, 2016). This reduction in policing intensity is defined as "depolicing" (Cooper, 2002). The views of one Seattle Police Department officer clearly illustrate the concept: "[p]arking under a shady tree to work on a crossword puzzle is a great alternative to being labeled a racist and being dragged through an inquest, a review board, an FBI and U.S. attorney investigation and a lawsuit" (Leo, 2001, para. 2).

The recent and highly publicized increase in violent crime across many of the country's biggest cities has injected depolicing into the national conversation (Beckett, 2016). Some have theorized that this spike in crime is a direct product of the intense criticism of law enforcement following the shooting of Michael Brown in Ferguson, MO, and the death of Freddie Gray while in the custody of the Baltimore Police Department (Byers, 2014; Goodman & Baker, 2015; Harvey, 2015; Mosendz, 2015). The argument follows that "the onslaught of anti-cop rhetoric" demoralized the police in those cities and across the country, rendering officers less willing to do their jobs aggressively. Criminal actors were emboldened by the absence of robust law enforcement, leading to increased incidence of illegal behavior (MacDonald, 2015, p. 3). Critics have called this proposition "baseless," suggesting that claims of depolicing are more political and ideological opportunism than legitimate policy concern (Coates, 2015).

Despite the intensity of the ongoing public discussion, early empirical analysis has failed to support the contention that public criticism of police behavior has led to depolicing and increased crime (Pyrooz, Decker, Wolfe, & Shjarback, 2015; Rosenfeld, 2015; Wolfe & Nix, 2016).

This article aims to address several relevant and outstanding issues underlying the debate over depolicing, including the employment conditions under which depolicing may occur, officer motivations for engaging in depolicing, and the relationship between depolicing and public safety. In so doing, this research examines depolicing in the context of the U.S. Department of Justice's (DOJ) pattern or practice initiative, the reform mechanism used to address systemic misconduct in several of the country's most prominent law enforcement agencies.

The analysis is driven by a series of interrupted time-series models designed to measure the causal effects of the DOJ's reform process on arrest rates in 10 jurisdictions investigated under the pattern or practice authority. After a review of the relevant theoretical and empirical literature, several research questions and hypotheses are presented. From there, the data and method are reviewed and findings addressed. The article concludes with a discussion of implications for policy and suggestions for future research.

Police Discretion, Oversight, and Pattern or Practice Reform

In 1994, President William J. Clinton signed into law the Violent Crime Control and Law Enforcement Act. The Crime Act, as it is known, is a sprawling piece of legislation designed to address a series of justice policy concerns, including police misconduct. Section 14141 of that law makes unlawful any "pattern or practice of conduct by law enforcement officers . . . that deprives persons of rights, privileges, or immunities secured or protected by the Constitution or laws of the United States" (42 U.S.C. Section 14141). Congress delegated enforcement of this provision to the DOJ, charging the agency with "eliminate[ing] the pattern or practice" by remedying past police abuse and preventing future misconduct involving violations of citizens' civil rights.

Known as the "pattern or practice initiative," Section 14141 has become the federal government's primary mechanism for addressing allegations of systemic police misfeasance (Perez, 2010). Since the law's inception, the Special Litigation Section of DOJ's Civil Rights Division has investigated dozens of pattern or practice allegations, ranging from claims of excessive use of force and biased

policing to violations of search and seizure protocols and mishandling of evidence (Childress, 2015; Rushin, 2014).

The DOJ's initial investigation typically involves a detailed review of department policy and administrative data, interviews with and observations of department staff, as well as an assessment of community perspectives (DOJ, 2015). If this process reveals a pattern or practice of unlawful conduct, the DOJ has legal authority to file civil charges against the jurisdiction in an effort to address the alleged abuse (DOJ, 2015). In nearly all such instances, jurisdictions facing the threat of civil action have chosen to negotiate a settlement rather than challenge DOJ findings in court (Childress, 2015; Rushin, 2014).[1]

Under the terms of these settlement agreements, often referred to as consent decrees (CDs) or memoranda of agreement, the DOJ relies on a well-established template designed to reduce the likelihood of future misconduct. Although the specifics of each settlement vary depending on the nature of the alleged violation, most pattern or practice agreements require affected departments to make several organizational and managerial changes, including (1) strengthen incident reporting and chain of command oversight, (2) develop an internal database designed to track and manage officer behavior, often referred to as an early intervention system, (3) enhance officer training, (4) simplify and clarify the citizen complaint process, and (5) strengthen the jurisdiction's capacity to investigate citizen complaints.[2]

The comprehensive nature of the process has drawn considerable praise from police leaders (R. McNeilly, personal communication, March 3, 2010; C. Ramsey, personal communication, May 20, 2010) and scholars alike (Armacost, 2003–2004; Livingston, 1999), with many lauding its ability to promote lawful and accountable policing without affecting the department's ability to address crime and disorder. In a widely cited essay, Harvard law professor William Stuntz (2006) anointed pattern or practice reform as "the most important legal initiative of the past twenty years in the sphere of police regulation" (p. 798).

The response among rank and file officers in departments facing DOJ oversight has been less sanguine. These officers have argued that added paperwork and other reporting requirements decrease the time available for patrol (Davis, Henderson, & Ortiz, 2005; Fuocco, 1997). They claim that the focus on accountability mandated by the reform creates an unhealthy fear of reprimand and places undue strain on patrol officers (Byron, 2012). This harms morale, adversely affecting the way officers approach problems, and ultimately leads to higher rates of crime (Stockwell, 2001b; Stockwell & Schwartzman, 2001). In fact, union representatives in several pattern or practice jurisdictions have been quoted as saying that the reforms effectively force rank and file officers to police less aggressively and actively resist situations thought to put them at greater risk of citizen complaint or departmental reprimand (Bernstein, 2012; Drake, 2001; Fuocco, 1997). When engaging in this behavior, officers have avoided making discretionary traffic stops and arrests, preferring a more passive, reactive approach to the job designed to minimize both workload and risk (Shi, 2009).

Depolicing as Dissent Shirking

The logic of depolicing is well recognized in the organizational behavior literature. As a form of dissent shirking, this behavior "stems directly from an organization member's opposition to some policy. Not working thus serves as silent protest. Dissent shirking is a common expression of protest over the production of disliked output" (Brehm & Gates, 1999, p. 30). Economists and rational choice theorists have argued that such shirking is in fact an understandable response to policies that increase the cost of on-the-job error or abuse (Prendergast, 2001).

Others have suggested that dissent shirking may not be the product of strategic thinking but instead an emotional response motivated by feelings of anger and alienation ("Ferguson effect," 2014; Sutton, 2015). The views of Anthony Walker, President of the Prince George's County,

Maryland Fraternal Order of Police, are representative: "There is a slowdown [in the way officers are policing], and it's because officers are frustrated with the leadership, with the media and with the overall perception of the agency" (Stockwell, 2001a). Whatever the explanation, news reports from several cities, including Cincinnati (Howlett, 2001; Werthheimer & Adams, 2001), Seattle (Ho & Castro, 2002), Chicago, (Lyderson, 2008), and New York (Celona, Cohen, & Golding, 2014), detail officers' descriptions of depolicing. More systematic research has also uncovered claims of depolicing from officers targeted by heavy external criticism (Oliver, 2015; Wolfe & Nix, 2016).

Much of the recent empirical evidence of depolicing has been produced in response to the hypothesis that public outcry over the death of Michael Brown has both demoralized police officers and emboldened would-be criminals, together leading to higher crime rates. Rosenfeld's (2015) examination of data from St. Louis, MO, found significant increases in both violent and property crime, with homicide data showing the largest spikes, yet little evidence tying these fluctuations to the Brown shooting. Morgan and Pally (2016) conducted a similar analysis of data from Baltimore, MD, another flash point in the debate over depolicing (Oppel, 2015). They found some evidence of rising crime and lower arrest rates in the wake of Freddie Gray's death, but refused to draw the causal link between the two trends, instead attributing their findings to a series of complex organizational and community-level factors. Pyrooz, Decker, Wolfe, and Shjarback (2015) found similar results upon examining crime trends in 81 large American cities.

Another recent study examined the effects of a 2001 riot on police behavior in Cincinnati (Shi, 2009). Results showed significant decreases in arrest rates following the riot. Declines were larger in cases where officers maintained higher degrees of discretionary authority and were subject to greater risk of error, namely, those involving misdemeanor offenses (Shi, 2009). These findings are consistent with research examining the extent to which the Los Angeles Police Department's (LAPD) response to the Rodney King riot affected officer behavior (Prendergast, 2001, 2002). Analysis of monthly data showed that arrest rates for Federal Bureau of Investigation (FBI) defined Part 1 Offenses, and for highly discretionary narcotics arrests, fell in the wake of the Rodney King incident. The author explained the findings in economic terms: accountability-driven reform "may have backfired, by reducing the incentives for officers to do their primary job, namely, confronting and arresting criminals" (Prendergast, 2001, pp. 20–21).

A central goal of the DOJ's reform template is to deter misconduct by increasing both internal and external oversight of officers' discretionary behavior. It is reasonable to conclude that such reforms will increase the cost of errors made by officers under these tightened conditions. Following the logic of rational choice theory, depolicing in these jurisdictions would be an unsurprising response.

There is support for the "emotional backlash" hypothesis as well. In fact, officers from several pattern or practice jurisdictions have attributed incidence of depolicing to what they believe are wrongheaded accusations and unjust persecution by the media, the public, and most of all, the DOJ (Bernstein, 2012; Davis, Ortiz, Henderson, Miller, & Massie, 2002; McEdwards, Lin, Balish, & Palmer, 2001; Spangenthal-Lee, 2011).

In spite of the analysis above, there is scant empirical research that examines the issue of depolicing in pattern or practice jurisdictions and what does exist has produced uneven results. In a 2002 report on DOJ-led reform in Pittsburgh, Davis, Ortiz, Henderson, Miller, and Massie (2002, p. 51) present officer interview and focus group data, suggesting that the CD process made them "less active on the street" and hurt department morale. Yet, a review of various quantitative measures of depolicing, including questionable use of sick days, the rate of annual traffic citations, driving under the influence cases brought, and the ratio of arrests to reported crime, showed no sign of depolicing during the reform period (Davis et al., 2002).

Stone, Foglesong, and Cole (2009) conducted a similar review of the CD process in Los Angeles. As with findings from the Pittsburgh study, officer survey results point to low morale and

widespread incidence of depolicing. Following a detailed empirical review of departmental traffic and pedestrian stop data, as well as arrest quantity and quality, however, the authors conclude that,

> Some depolicing may have occurred in the first two years of the consent decree, when recorded crime rose slightly while enforcement activity, both [traffic and pedestrian] stops and arrests, declined; but there is no sign of depolicing since 2002. Indeed, enforcement activity has increased, with the increase in arrests concentrated on the minor crimes where management policy guides officer discretion. (Stone, Foglesong, & Cole, 2009, p. 30)[3]

Key Terms, Unanswered Questions, and Expected Findings

As the preceding literature review suggests, the empirical research related to depolicing is under-developed. Various fundamental descriptive and analytical questions remain unaddressed.

Which Events Are Likely to Trigger Depolicing?

The DOJ's enforcement of Section 14141 typically begins with a formal investigation of allegations that a department has engaged in a pattern or practice of unlawful conduct (DOJ, 2013). These investigations, which are lengthy and often contentious, bring considerable negative attention upon police departments already suffering from low levels of public trust. As a DOJ investigation in and of itself should not change the cost of officer error, a purely rational actor would find no reason to respond by depolicing.[4] Yet, in light of evidence that some officers felt unfairly maligned by the DOJ's review of their department (Davis et al., 2002) and saw the federal investigation as an unjustified indictment for overblown or nonexistent problems (Streicher, 2008), it would not be surprising to find evidence of emotionally driven depolicing in response to the initiation of a DOJ investigation.

In some cases, the DOJ will release the results of its investigation publicly, either in the form of a findings letter detailing the evidence against the jurisdiction (typically issued if a pattern or practice is found) or a technical assistance letter addressed to the department (usually in cases where no pattern or practice is found). In each instance, these letters are the first public discussion of the allegations underlying an investigation. If a pattern or practice was found, a DOJ findings letter has the potential to reignite public criticism of the department, in the process creating the conditions for emotionally driven depolicing. Conversely, there is no reason to expect depolicing to manifest following the release of findings in jurisdictions not found to have engaged in a pattern or practice of misconduct.

The primary goal of the pattern or practice initiative is the elimination of officer misconduct. The accountability mechanisms—and the increased oversight that characterizes them—at the heart of these efforts are designed to increase compliance with federal law by promoting lawful use of force, racially neutral traffic enforcement, and so on.

Theory suggests that affected officers might push back against the changes brought about by this type of external intervention (Brehm & Gates, 1999). Rational choice proponents argue that dissent shirking is a logical response to a more intense oversight regime (Shi, 2009). The more costly an error becomes, the less likely a street-level agent will engage in the kind of proactive, aggressive policing thought to expose him or her to increased risk of punishment. Both Shi (2009) and Prendergast (2001, 2002) found empirical support for the argument that depolicing occurs as a result of this type of rational thinking. In fact, according to Shi (2009, p. 104), "increased expected penalties for errors caused a 35.6% reduction in misdemeanor arrests" in the wake of a 2001 Cincinnati, OH, riot occurring in the wake of a police-involved shooting. Regardless of the nature of the officer's

motivation, the expectation is that pattern or practice reform will increase departmental oversight, in turn changing an officer's utility function and ultimately lead to depolicing.

How Long Does Depolicing Last?

The field of criminal justice has very little theoretical or empirical insight into the duration of depolicing. The emotional response explanation allows for the possibility that depolicing will subside even if oversight is unchanged. Underlying this view is the assumption that officers are willing and able to overcome the feelings of anger or frustration that motivate depolicing. The narrative surrounding police performance also tends to be fluid, as public criticism and calls for increased accountability are typically short-lived (Chermak, McGarrell, & Gruenewald, 2006; Weitzer, 2002). As the conversation changes, the thinking is that, so too will officers' behavior, as public opinion of the police improves, incidence of depolicing will subside. Stone et al.'s (2009) study found that the low levels of depolicing that occurred in response to the LAPD CD ended after 2 years lending some empirical support to this position.

The rational choice explanation for depolicing seems to suggest that police behavior will remain constant so long as department policy and individual officers' utility functions do not change. Thus, one might expect that arrest rates will not revert to prereform levels unless and until the department either switches back to prereform policies or adopts a less vigilant approach to enforcing reform-driven accountability policies and norms.

At least two additional factors justify these expectations about the time-limited nature of depolicing. First, police leaders in several pattern or practice jurisdictions have denied the existence of depolicing based largely on the view that their department's officers are motivated by fighting crime and the satisfaction of keeping their city safe, not by the individualistic and subcultural concerns thought to drive dissent shirking (Raganella & White, 2004). This anecdotal evidence, which is consistent with a broad literature on bureaucratic public service motivation (Houston, 2000; Perry, Hondeghem, & Wise, 2010), supports the notion that in the long run, the strength of officers' prosocietal orientation will outweigh animosity toward DOJ intervention or departmental policy changes.

Second, pattern or practice reform has led to significant turnover among officers (R. Davis, personal communication, December 18, 2010). It is logical to assume that new officers will have no reason to engage in emotionally driven depolicing, as they were not the subject of the DOJ's intervention and carried no personal attachment to criticism of the department before their hiring. Further, as new officers, they will know nothing of the prereform oversight regime and will develop their own job-related utility functions in the context of pattern or practice reform policies and thus will have no preintervention frame of reference against which to compare postintervention rules and norms.

Analysis

Data

The data set used for this analysis consists of arrest and crime figures gathered from 10 police departments investigated by the DOJ under the pattern or practice initiative. These 10 departments account for 24.4% of the 41 jurisdictions that faced formal investigation between 1994 and 2008 (Childress, 2015). The sample is limited to these 10 departments because of data availability. The other 31 departments were excluded either because they were not found in the FBI's Uniform Crime Report (UCR) database (e.g., the U.S. Virgin Islands, Puerto Rico, and the State of New Jersey) or

Table 1. Key Dates in the Pattern or Practice Reform Process Among Included Jurisdictions.

Jurisdiction	Investigation Initiated	Findings Letter Released	Settlement Enacted	Potentially Confounding Events
Pittsburgh, PA (UOF)	April 1996	January 1997	April 1997	March 1997: Mayor derides DOJ investigation May 1997: Creation of civilian oversight board January 2006: Popular chief not rehired
Montgomery County, MD (RP)	June 1996	November 1999	January 2000	November 2001: Critical report released
Los Angeles, CA (UOF)	August 1996	May 2000	November 2000	July 1991: Christopher commission report issued October 1996: DOJ widens investigation October 2000: Settlement announced in media October 2002: Bratton hired as chief May 2007: MacArthur Park protests
Buffalo, NY (UOF)	December 1997	NA	September 2002	November 1999: Chief complains abt. DOJ process March 2007: Officers threated "blue flu"
Charleston, WV (UOF)	April 1999	NA	NA	NA
Riverside, CA (UOF, RP)	July 1999	NA	NA	NA
Prince George's County, MD (K9)	July 1999	NA	January 2004	April 1999: FBI investigates K9 February 2001: Police Acct. Task Force report issued
Prince George's County, MD (UOF)	October 2000	NA	January 2004	July 2001: *Washington Post* Blue Wall of Silence story
Detroit, MI (UOF)	June 2001	March 2002	June 2003	December 1992: Four officers fired for beating citizen
Portland, ME (UOF)	May 2002	March 2003	NA	January 2002: Chief requests investigation
Bakersfield, CA (UOF)	June 2003	April 2004	NA	NA

Note. Primary reason for DOJ investigation listed parenthetically. UOF = use of force; RP = racial profiling; K9 = Canine-related use of force; DOJ = Department of Justice; FBI = Federal Bureau of Investigation; NA = Not applicable.

missing data rendered their inclusion impossible. Table 1 details the included jurisdictions and provides dates for each of the events under review.

The DOJ determined that 5 of the 10 police departments included those that had engaged in a pattern or practice of unlawful activity, ranging from excessive use of force to racial profiling in violation of the Fourth Amendment. In each case, the affected jurisdiction opted to negotiate the terms of their settlement and accept structured reforms rather than litigate the DOJ's claims. DOJ attorneys found no systemic misconduct in the remaining five jurisdictions.

The impact of pattern or practice reform is measured by tracking changes in jurisdictional arrest levels over time. Much of the public debate over depolicing has omitted any analysis of arrest rates, emphasizing instead the extent to which criticism of police has affected crime (e.g., Frederick, 2015; Pyrooz et al., 2015; Rosenfeld, 2015). This article draws on arrest rates rather than crime for several reasons, the most salient of which is the attenuated (and largely unexplained) relationship between police behavior and crime (e.g., Chamlin, 1991; Weisburd & Eck, 2004). In short, officer behavior is

Table 2. Data Availability for Misdemeanor Arrest in the Ten Included Jurisdictions.

Jurisdiction	Vandalism	Prostitution	Total Drug	Liquor Law	Disorderly Conduct	Drunkenness	Vagrancy
Pittsburgh, PA	X	X	X	X	X	X	NA
Montgomery County, MD	X	X	X	X	X	NA	NA
Los Angeles, CA	X	X	X	X	X	X	X
Buffalo, NY	X	X	X	NA	X	NA	NA
Charleston, WV	X	X	X	X	X	X	NA
Riverside, CA	NA	X	X	X	X	X	X
PG County, MD	X	X	X	X	X	NA	NA
Detroit, MI	X	X	X	X	X	NA	X
Portland, ME	X	X	X	X	X	NA	NA
Bakersfield, CA	X	X	X	X	X	X	X

Note. X = Data available; NA = Not available.

a more direct, reliable indicator of depolicing than are changes to an outcome, like crime, which have been tied to a variety of exogenous social and ecological factors, and may or may not reflect shifts in police activity (Cohen & Felson, 1979; Groff, Weisburd, & Yang, 2010; Levitt, 2004).[5]

Like most police behavior, the decision to effectuate an arrest is a function of an officer's discretionary power. Yet officers do not wield the same level of discretionary authority over all arrest decisions. In some situations, the decision to arrest is determined almost entirely by the officer (Black, 1971; Ohlin & Remington, 1993; Smith & Visher, 1981). Should the officer want to protest changes to working conditions, intensified oversight, or perceived criticism of their job performance, such depolicing will likely manifest in the discretionary response to low-level, misdemeanor offenses (Shi, 2009; Stone et al., 2009).

Felony arrests, on the other hand, are less a function of officer discretion than circumstance and context. For example, it is much more difficult, even impossible, for an officer to engage in protest by ignoring evidence to support the arrest of a car thief or a burglary suspect in the same way an officer may choose to drive past someone loitering or engaging in disorderly conduct. Put another way, officers have much less discretionary authority over felony arrest decisions and higher organizational and societal expectations to pursue such arrests than they do with misdemeanor arrests. As such, felony arrest levels are expected to remain unchanged in the wake of pattern or practice reform.

To examine these hypotheses, data from the FBI's UCRs series were used to create an index from seven misdemeanor arrest categories in order to operationalize depolicing. More specifically, the MISDEMEANOR variable tracks the combined monthly totals of arrests for (1) vandalism, (2) prostitution, (3) drug offenses, (4) liquor law violations, (5) disorderly conduct, (6) public drunkenness, and (7) vagrancy between 1990 and 2010. To test the expectation that depolicing will only manifest in the form of fewer misdemeanor arrests, these data will be compared using the variable FELONY, an index of felony arrest figures, also tracked monthly between 1990 and 2010.[6]

As is denoted in Table 2, the availability and quality of data varied from jurisdiction to jurisdiction, and in certain instances, missing data prevented the use of every arrest category. In cases where data were either completely unavailable or of low quality (as defined by significant instances of missing data), the arrest category was dropped.[7]

Descriptive Findings

Table 3 lists descriptive data on misdemeanor arrests for each jurisdiction, including monthly mean scores for both pre- and postevent periods, as well as the event month itself and the 2-year period following the event.

Table 3. Pre- and Postevent Mean Scores for Misdemeanor Arrest Rates.

Jurisdiction/Event	Sample Mean	Preevent Mean	Postevent Mean	Event Month Total	2-Years Postevent (n = 24)
Pittsburgh, PA	139.68 (n = 252)				
Investigation initiated		149.07 (n = 75)	136.52** (n = 177)	129	131.17
Finding announced		148.80 (n = 84)	135.12** (n = 168)	144	131.13**
Settlement enacted		148.22 (n = 87)	135.18** (n = 165)	149	131.04**
Montgomery County, MD	70.34 (n = 240)				
Investigation		53.12 (n = 65)	76.73** (n = 175)	71	62.98**
Finding		57.32 (n = 106)	80.63** (n = 134)	70	70.33
Settlement		57.53 (n = 108)	80.81** (n = 132)	57	70.21
Los Angeles, CA	375.81 (n = 252)				
Investigation		380.67 (n = 79)	373.59** (n = 173)	392	394.88**
Finding		383.60 (n = 124)	368.27** (n = 128)	363	363.63**
Settlement		383.01 (n = 130)	368.14** (n = 122)	376	361.50**
Buffalo, NY	85.26 (n = 252)				
Investigation		84.43 (n = 95)	85.76 (n = 157)	73	84.54
Settlement		82.86 (n = 152)	88.12** (n = 100)	79	82.67
Charleston, WV	64.58 (n = 240)				
Investigation		75.11 (n = 99)	57.18** (n = 141)	75	72.46**
Riverside, CA	83.23 (n = 240)				
Investigation		73.56 (n = 102)	90.40** (n = 138)	97	88.92**
Prince George's County, MD	95.72 (n = 252)				
Investigation (K9)		95.03 (n = 114)	96.29 (n = 138)	115	97.50
Investigation (UOF)		95.89 (n = 129)	95.54 (n = 123)	93	86.13**
Settlement (UOF)		92.77 (n = 168)	101.61** (n = 84)	75	86.92**
Detroit, MI	120.29 (n = 252)				
Investigation		135.97 (n = 137)	101.62** (n = 115)	110	112.63**
Finding		134.49 (n = 146)	100.74** (n = 106)	122	107.29**
Settlement		127.24 (n = 195)	97.05** (n = 57)	112	102.04**
Portland, ME	36.53 (n = 240)				
Investigation		32.13 (n = 136)	42.27** (n = 104)	31	32.38**
Finding		32.11 (n = 146)	43.38** (n = 94)	42	33.83*
Bakersfield, CA	110.46 (n = 240)				
Investigation		105.04 (n = 149)	119.33** (n = 91)	110	111.54
Finding		105.64 (n = 159)	119.93** (n = 81)	120	109.46

Note. UOF = use of force; K9 = Canine-related use of force.
*$p < .05$. **$p < .01$.

Eight of ten sample jurisdictions showed statistically significant differences between the mean arrest totals prior to the investigation announcement and postinvestigation means. Of those, Pittsburgh, Los Angeles, Charleston, WV, and Detroit saw lasting declines in misdemeanor arrests, consistent with (although not necessarily supportive of) expectations that officers policed less proactively to protest the perceived injustice of the DOJ investigation and the emotional weight of the damage such an investigation caused to their personal and departmental reputation. Conversely, mean scores in Montgomery County, MD, Riverside, CA, Portland, ME, and Bakersfield, CA, increased in the wake of the DOJ investigation, which runs counter to established hypotheses.

The DOJ publicly announced the results of its investigation in six jurisdictions. In three cities, Pittsburgh, Los Angeles, and Detroit, mean postannouncement arrest totals were lower than preannouncement levels; three other jurisdictions, Montgomery County, Portland, and Bakersfield, saw

Table 4. Pre- and Postevent Mean Scores for Total Crime.

Jurisdiction/ Event	Sample Mean	Preevent Mean	Postevent Mean	Event Month Total	2-Years Postevent Mean ($n = 24$)
Pittsburgh, PA	2,499.20 ($n = 240$)				
Investigation		3,307.94 ($n = 63$)	2,210.98** ($n = 177$)	2,409	2,514.46
Finding		3,205.90 ($n = 72$)	2,196.32** ($n = 168$)	2,331	2,570.33
Settlement		3,169.59 ($n = 75$)	2,194.47** ($n = 165$)	2,467	2,565.42
Montgomery County, MD	2,806.42 ($n = 240$)				
Investigation		3,162.54 ($n = 65$)	2,674.14** ($n = 176$)	3,463	3,231.63**
Finding		3,127.62 ($n = 106$)	2,552.33** ($n = 134$)	2,561	2,686.54*
Settlement		3,118.15 ($n = 108$)	2,551.36** ($n = 132$)	2,355	2,702.33*
Los Angeles, CA	19,352.29 ($n = 240$)				
Investigation		27,802.34 ($n = 67$)	16,079.72** ($n = 173$)	22,210	19,482.25
Finding		23,926.25 ($n = 112$)	15,350.07** ($n = 128$)	18,205	18,188.29**
Settlement		23,629.22 ($n = 118$)	15,215.58** ($n = 122$)	17,030	18,340.88**
Buffalo, NY	2,336.34 ($n = 240$)				
Investigation		2,758.30 ($n = 83$)	2,113.26** ($n = 157$)	2,099	2,275.38
Settlement		2,552.22 ($n = 140$)	2,034.10** ($n = 100$)	2,363	2,120.13**
Charleston, WV	536.36 ($n = 228$)				
Investigation		664.05 ($n = 87$)	457.57** ($n = 141$)	550	541.21
Riverside, CA	1,709.96 ($n = 240$)				
Investigation		2,045.06 ($n = 102$)	1,462.28** ($n = 138$)	1021	1,169.25**
Prince George's County, MD	4,193.59 ($n = 240$)				
Investigation (K9)		4,266.83 ($n = 102$)	4,139.45* ($n = 138$)	4,126	4,131.88
Investigation (UOF)		4,241.02 ($n = 117$)	4,148.47 ($n = 123$)	4,405	4,579.79**
Settlement (UOF)		4,337.51 ($n = 156$)	3,926.30** ($n = 84$)	5,090	4,795.25**
Detroit, MI	8,998.66 ($n = 240$)				
Investigation		10,160.70 ($n = 125$)	7,735.57** ($n = 115$)	8,364	7,824.08**
Finding		10,028.28 ($n = 134$)	7,697.07** ($n = 106$)	7,409	7,716.54**
Settlement		9,394.31 ($n = 182$)	7,757.12** ($n = 58$)	9,108	8,327.92**
Portland, ME	416.01 ($n = 228$)				
Investigation		474.69 ($n = 124$)	346.05** ($n = 104$)	368	361.17**
Finding		467.30 ($n = 134$)	342.90** ($n = 94$)	377	348.33**
Bakersfield, CA	142.05 ($n = 240$)				
Investigation		1,125.04 ($n = 149$)	1,643.81** ($n = 91$)	1,575	1,569.54**
Finding		1,150.42 ($n = 159$)	1,658.04** ($n = 81$)	1,455	1,611.83**

Note. UOF = use of force; K9 = Canine-related use of force.
*$p < .05$. **$p < .01$.

postannouncement increases in misdemeanor arrests. These data also provide mixed evidence for the emotionally driven hypothesis. Similarly, of the six jurisdictions to settle with the DOJ, three saw reductions in postsettlement misdemeanor arrests, while three others saw increases.

Table 4 also lists separate mean scores for the 24-month periods following the initiation of a DOJ investigation, the DOJ's release of findings, and settlement. Evidence that monthly misdemeanor arrest totals dropped following the event and then reverted to preevent levels after 2 years would support the notion that depolicing is a temporary phenomenon. One-tailed *t* tests reveal statistically

Table 5. Pre- and Postevent Mean Scores for Felony Arrest Rates.

Jurisdiction/Event	Sample Mean	Preevent Mean	Postevent Mean	Event Month Total	2-Years Postevent (n = 24)
Pittsburgh, PA	132.05 (n = 252)				
Investigation initiated		145.69 (n = 75)	127.03** (n = 177)	141	129.67
Finding announced		144.86 (n = 84)	125.64** (n = 168)	145	135.92*
Settlement enacted		144.54 (n = 87)	125.46** (n = 165)	122	135.17
Montgomery County, MD	130.34 (n = 241)				
Investigation		128.77 (n = 65)	130.18 (n = 176)	114	128.92
Finding		128.15 (n = 106)	132.07* (n = 134)	129	131.79
Settlement		128.18 (n = 108)	132.11* (n = 132)	128	131.53
Los Angeles, CA	248.62 (n = 252)				
Investigation		262.37 (n = 79)	242.35** (n = 173)	264	251.21
Finding		257.42 (n = 124)	240.10** (n = 128)	247	244.08**
Settlement		256.74 (n = 130)	239.98** (n = 122)	236	243.58**
Buffalo, NY	138.01 (n = 252)				
Investigation		139.97 (n = 95)	136.83** (n = 157)	135	137.46
Settlement		138.21 (n = 152)	137.71 (n = 100)	139	131.46**
Charleston, WV	51.15 (n = 240)				
Investigation		59.97 (n = 99)	44.95** (n = 141)	57	51.21
Riverside, CA	98.86 (n = 240)				
Investigation		94.57 (n = 102)	102.04** (n = 138)	102	90.38**
Prince George's County, MD	145.23 (n = 252)				
Investigation (K9)		155.63 (n = 114)	136.64** (n = 138)	177	148.88
Investigation (UOF)		155.31 (n = 129)	134.66** (n = 123)	144	142.21*
Settlement (UOF)		151.01 (n = 168)	133.67** (n = 84)	129	134.83**
Detroit, MI	190.04 (n = 252)				
Investigation		209.94 (n = 137)	166.32** (n = 115)	253	174.17**
Finding		208.08 (n = 146)	165.18** (n = 106)	279	166.96**
Settlement		197.28 (n = 194)	165.79** (n = 58)	263	175.17**
Portland, ME	37.59 (n = 240)				
Investigation		36.52 (n = 136)	38.99** (n = 104)	31	33.75**
Finding		36.32 (n = 146)	39.56** (n = 94)	42	34.33**
Bakersfield, CA	142.05 (n = 240)				
Investigation		137.15 (n = 149)	150.07** (n = 91)	154	145.13*
Finding		137.74 (n = 159)	150.51** (n = 81)	146	145.54*

Note. UOF = use of force; K9 = Canine-related use of force.
*$p < .05$. **$p < .01$.

significant differences in 2-year mean scores, although the direction of these changes varies by event and jurisdiction. In some cases, including Detroit, mean scores in the 2-year window were consistently lower than in nonwindow months. In others, 2-year windows reflected either higher mean totals (e.g., Portland) or a mix between higher and lower scores (e.g., Los Angeles).

Table 5 lists similar descriptive data for felony arrests across the included jurisdictions. As was the case with misdemeanor arrest patterns, most of the included jurisdictions experienced statistically significant differences between pre- and postevent felony arrest levels and within 2-year postevent windows. Felony arrests decreased in some jurisdictions and increased in others.

Tellingly, the nature of changes observed in a jurisdiction's felony arrest rates matched pre- and postevent changes to mean misdemeanor arrest totals. For example, Pittsburgh, Los Angeles, and Detroit each experienced lower levels of misdemeanor and felony arrests following DOJ

Table 6. Using Autoregressive Integrated Moving Average to Model Misdemeanor and Felony Arrests.

Jurisdiction	Misdemeanor Arrest	Felony Arrest
Pittsburgh, PA	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)$ noconstant
Montgomery County, MD	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
Los Angeles, CA	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
Buffalo, NY	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
Charleston, WV	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
Riverside, CA	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
PG County, MD (K9/UOF)	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
Detroit, MI	$(0,1,1)$ constant	$(0,1,1)(0,1,1)_{12}$ noconstant
Portland, ME	$(0,1,1)(0,1,1)_{12}$ noconstant	$(0,1,1)(0,1,1)_{12}$ noconstant
Bakersfield, CA	$(4,0,0)(1,0,0)_{12}$ noconstant	$(4,0,0)$ noconstant

Note. UOF = use of force; K9 = Canine-related use of force.

intervention. Bakersfield and Riverside saw increased misdemeanor and felony arrest levels. No jurisdiction evidenced the expected combination of decreased misdemeanor arrests and increased or unchanged felony arrest levels.

Table 4 lists changes to crime patterns by jurisdiction and pattern or practice event. Nine of ten jurisdictions reported lower levels of crime following DOJ intervention.

In sum, these data reveal a pre- and postevent pattern of arrests that vary by event, crime type, and jurisdiction. These results provide further context to observed changes in jurisdictional arrest patterns and further justify the use of time-series models to more carefully examine the observed trends.

Interrupted Time-Series Analysis

Despite these inconsistent and somewhat unexpected initial findings, an examination of changes to mean arrest rates does not provide sufficient insight into the presence or nature of officer response to DOJ intervention. In fact, interpretation of raw time-series data without the necessary statistical controls in place to account for autocorrelation, seasonal trends, or other relevant events known to influence arrest patterns over time may confound or overstate results (McDowall, McCleary, Meidinger, & Hay, 1980).

The development of autoregressive integrated moving average (ARIMA) models involves an iterative, three-stage process designed to address these issues statistically and thus provides a more valid means of evaluating the impact of a particular intervention (Box & Jenkins, 1976; Tabachnick & Fidell, 2007; Wei, 2007). The first stage of the ARIMA model-building process requires the diagnosis of three structural parameters in order to appropriately fit the time-series data: autoregressive (p), difference (d), and moving average (q; McDowall et al., 1980). The empirical model-building process yielded 20 unique models—one each for misdemeanor and felony arrests made in the 10 included jurisdictions, as is shown in Table 6.[8]

ARIMA models are presented using the following form: $(p, d, q)(P, D, Q)_s + \Theta$. The p term denotes the number of autoregressive components, or lags, in the model, d indicates the number of nonseasonal differences included, and q represents the number of moving averages in the model. P represents the number of seasonal autoregressive terms, D represents the degree of seasonal differences used, and Q reflects the number of seasonal moving average terms. s denotes the seasonal period and Θ represents the model's constant or trend parameter (Chamlin, 1988; Chamlin & Myer, 2009; McDowall et al., 1980). The most common model used here—$(0,1,1)(0,1,1)_{12}$—draws on nonseasonal and seasonal first differences to account for nonstationarity in the series, as well as nonseasonal and seasonal moving averages. Seasonal terms were

excluded from models used to analyze data, like felony arrests in Pittsburgh, where no seasonal trend was observed. The Bakersfield data represented an outlier case, in that both misdemeanor and felony arrest series exhibited significant autoregressive components, but required no correction for nonstationarity or moving averages.

Once derived, these models were used to test for the presence of depolicing in response to three key events in the pattern or practice reform process: (1) DOJ investigation into alleged unlawful behavior, (2) release of DOJ findings, and (3) announcement of negotiated settlement between the DOJ and the affected jurisdiction.

The analysis considers whether the effects of depolicing were immediate or delayed and whether any observed change in arrest levels was permanent or temporary.[9] To measure the duration of depolicing, three variables were created. The first, MONTH, was designed to test the proposition that any depolicing effect is confined to the month in which the event occurred. The event month was coded as 1, while all other months were coded 0. The second, TWO_YEAR, was created to replicate Stone et al.'s (2009) finding that depolicing in Los Angeles began immediately and lasted for 10 years.[10] Twenty-four months following each event date were coded 1, while all cases prior to and after this period were coded 0. A third variable, PERMANENT, tests the notion that depolicing was a permanent phenomenon. Cases preceding the event were coded 0 and those following the event date were coded 1.

ARIMA Model Results

Table 7 displays the results generated by a series of ARIMA models developed to examine how the DOJ's reform process affected misdemeanor arrest patterns. Three unique independent variables enabled consideration of competing hypotheses about the duration of dissent shirking.

The table's first column lists the coefficients for the first intervention—the DOJ's announcement of a pattern or practice investigation. The second and third columns show the results of several models examining how misdemeanor arrest levels were affected by the DOJ's release of investigative findings and the announcement of a jurisdiction's decision to settle with the DOJ.

In short, none of the data presented in these tables indicate a statistically significant relationship between key events in the pattern or practice reform process and monthly levels of misdemeanor arrests made in the 10 included jurisdictions between 1990 and 2010.[11]

Analysis of felony arrest data reveals much of the same: The pattern or practice reform process produced no meaningful statistically significant changes to officer behavior, as is documented in Table 8. The one exception—Montgomery County, MD, following the DOJ investigation—appears to be more a function of anomalous data than depolicing.[12] Models examining the lagged effects of events on arrest rates also generated no significant findings and are thus not listed here. A series of ARIMA models was also developed to examine the extent to which the pattern or practice initiative influenced misdemeanor and felony crime rates in all 10 jurisdictions between 1990 and 2010. There were no statistically significant findings to report.

Discussion

The DOJ's pattern or practice initiative brings to bear both theoretical explanations for dissent shirking in the context of law enforcement: depolicing: (1) an emotionally-driven response to public criticism, a perceived lack of institutional support, or organizational scandal, characterized by flagging officer morale, disgruntlement, or disassociation and (2) a rationally driven response to changes in officer utility functions brought about by reform designed specifically to reduce officer misconduct through increased oversight and accountability.

This research examined whether such expectations are borne out empirically, and the answer was that they are not. An exhaustive review of 20 years of arrest records from 10 jurisdictions

Table 7. The Effects of the DOJ Pattern or Practice Reform Process on Misdemeanor Arrest Rates.

Jurisdiction	Variable	Investigation Announced	Findings Released	Settlement Announced
Pittsburgh, PA	Month	−18.73 (−79.7, 42.3)	10.46 (−54.6, 75.5)	13.87 (−67.0, 94.7)
	2 Years	−0.39 (−21.0, 20.2)	−1.40 (−31.9, 29.1)	4.67 (−35.6, 44.9)
	Permanent	−4.84 (−33.6, 23.9)	−1.51 (−37.1, 34.0)	8.97 (−59.5, 77.4)
Montgomery County, MD	Month	13.69 (−16.6, 44.0)	3.99 (−36.1, 44.1)	−11.41 (−45.6, 22.8)
	2 Years	1.72 (−2.3, 5.8)	0.8 (−7.0, 8.6)	−0.02 (−6.8, 6.7)
	Permanent	−.10 (−9.3, 9.0)	−1.44 (−14.5, 11.6)	−2.56 (−13.1, 7.9)
Los Angeles, CA	Month	−0.1 (−44.3, 44.0)	−21.02 (−95.1, 53.0)	14.94 (−45.2, 75.1)
	2 Years	8.68 (−15.1, 32.4)	−5.50 (−21.6, 10.6)	−8.92 (−21.3, 3.4)
	Permanent	12.5 (−15.4, 40.3)	−10.09 (−31.8, 11.6)	−4.87 (−34.0, 24.2)
Buffalo, NY	Month	−4.70 (−41.3, 31.9)	—	−4.52 (−39.5, 30.4)
	2 Years	0.33 (−6.9, 7.5)	—	0.77 (−5.1, 6.6)
	Permanent	−1.03 (−10.7, 8.7)	—	3.84 (−3.2, 10.9)
Charleston, WV	Month	−4.97 (−57.2, 47.3)	—	—
	2 Years	6.77 (−9.9, 23.4)	—	—
	Permanent	0.20 (−24.1, 24.5)	—	—
Riverside, CA	Month	14.72 (−1.4, 30.9)	—	—
	2 Years	4.24 (−0.7, 9.2)	—	—
	Permanent	0.84 (−9.4, 11.1)	—	—
PG County, MD (K9)	Month	2.41 (−32.0, 36.8)	—	—
	2 Years	2.47 (−8.0, 13.0)	—	—
	Permanent	−5.55 (−25.3, 14.2)	—	—
PG County, MD (UOF)	Month	−2.64 (−34.7, 29.4)	—	3.67 (−33.8, 41.1)
	2 Years	−3.18 (−16.2, 9.8)	—	5.28 (−0.9, 11.5)
	Permanent	−10.89 (−35.5, 13.7)	—	7.63 (−5.8, 21.2)
Detroit, MI	Month	−6.03 (−58.2, 46.1)	11.29 (−36.0, 58.6)	12.0 (−20.1, 44.2)
	2 Years	1.39 (−8.2, 11.0)	2.83 (−5.8, 11.5)	−5.24 (−11.1, 0.7)
	Permanent	−3.1 (−21.2, 15.1)	6.49 (−10.7, 23.7)	3.93 (−5.8, 13.6)
Portland, ME	Month	4.18 (−27.2, 35.6)	3.78 (−51.7, 59.3)	—
	2 Years	2.13 (−8.6, 12.9)	−0.79 (−12.0, 10.4)	—
	Permanent	3.40 (−9.7, 16.5)	0.30 (−16.5, 17.1)	—
Bakersfield, CA	Month	4.42 (−15.5, 24.3)	7.13 (−46.2, 60.5)	—
	2 Years	−1.10 (−17.6, 15.4)	−0.70 (−17.8, 16.4)	—
	Permanent	−0.22 (−20.0, 19.5)	2.15 (−24.0, 28.3)	—

Note. Results are shown in terms of β coefficients, with 95% confidence intervals listed parenthetically. UOF = use of force; K9 = Canine-related use of force.

investigated by the DOJ indicates clearly that neither DOJ investigation, the release of investigative findings, nor a jurisdiction's agreement to submit to DOJ-led reform, was causally related to statistically significant declines in arrest rates.

The results of the time-series analysis largely support the findings from two previous studies that showed little or no quantitative evidence of depolicing in the context of pattern or practice reform (Davis et al., 2002; Stone et al., 2009). They are, however, inconsistent with sentiments expressed by officers who have endured the effects of changes brought on by DOJ-led oversight (Stone et al., 2009; Sutton, 2015). Nor do they conform to the narrative framed by media reports discussing the issue (e.g., Kaste, 2015; Turner, 2015). This tension between anecdotal evidence and statistical analysis is quite similar to the inconsistency at the center of the debate over depolicing. Several possible explanations merit further discussion.

It is possible that both the anecdotal and statistical analyses are accurate, such that officers are comfortable verbalizing discontent, but are less willing to engage in the kind of depolicing that a

Table 8. The Effects of the DOJ Pattern or Practice Reform Process on Felony Arrest Rates.

Jurisdiction	Variable	Investigation Announced	Findings Released	Settlement Announced
Pittsburgh, PA	Month	3.16 (−44.1, 50.4)	11.94 (−29.4, 53.3)	−13.44 (−134.2, 107.3)
	2 Years	−3.50 (−20.0, 13.0)	2.61 (−11.0, 16.2)	−0.58 (−12.1, 10.9)
	Permanent	−0.70 (−19.9, 18.5)	−1.33 (−15.5, 12.8)	−4.30 (−24.7, 16.1)
Montgomery County, MD	Month	−9.42 (−38.3, 19.4)	1.31 (−90.8, 93.4)	−3.67 (−112.1, 104.7)
	2 Years	3.67** (0.6, 7.3)	0.99 (−12.4, 14.4)	−0.14 (−11.0, 10.7)
	Permanent	−8.54** (−16.2, −0.9)	3.65 (−15.1, 22.4)	2.87 (−12.6, 18.3)
Los Angeles, CA	Month	9.63 (−29.6, 48.9)	−0.37 (−47.5, 46.8)	−4.40 (−38.0, 29.2)
	2 Years	−0.16 (−7.0, 6.6)	2.01 (−6.2, 10.2)	0.81 (−6.7, 8.4)
	Permanent	−0.20 (−10.7, 10.3)	0.54 (−9.8, 10.9)	4.40 (−4.0, 12.8)
Buffalo, NY	Month	4.58 (−26.8, 36.0)	—	7.98 (−45.1, 61.1)
	2 Years	2.07 (−3.8, 7.9)	—	−3.52 (−8.6, 1.5)
	Permanent	3.74 (−5.3, 12.8)	—	−1.07 (−8.0, 5.9)
Charleston, WV	Month	1.84 (−33.6, 37.3)	—	—
	2 Years	2.36 (−11.1, 15.8)	—	—
	Permanent	1.34 (−15.1, 17.8)	—	—
Riverside, CA	Month	16.01 (18.5, 50.5)	—	—
	2 Years	−0.36 (−9.2, 8.5)	—	—
	Permanent	10.59 (−7.3, 28.4)	—	—
PG County, MD (K9)	Month	24.13 (−56.7, 104.9)	—	—
	2 Years	2.59 (−13.7, 18.8)	—	—
	Permanent	6.35 (−12.0, 24.7)	—	—
PG County, MD (UOF)	Month	−1.79 (−61.9, 58.4)	—	1.56 (−27.7, 30.8)
	2 Years	−0.24 (−14.8, 15.3)	—	10.44 (2.5, 18.3)
	Permanent	−7.0 (−30.0, 16.1)	—	10.04 (−1.1, 21.2)
Detroit, MI	Month	−0.81 (−68.2, 66.5)	4.54 (−25.5, 34.6)	−1.43 (−20.3, 17.5)
	2 Years	1.15 (−21.2, 23.6)	−1.83 (−12.6, 8.9)	1.29 (−7.6, 10.2)
	Permanent	−0.94 (−35.5, 33.7)	−7.85 (−20.4, 4.8)	9.14 (−0.9, 19.2)
Portland, ME	Month	−0.67 (−35.8, 34.5)	8.37 (−43.8, 60.5)	—
	2 Years	−0.50 (−11.7, 10.7)	−0.21 (−7.3, 6.9)	—
	Permanent	0.83 (−15.4, 17.1)	0.94 (−9.3, 11.1)	—
Bakersfield, CA	Month	11.97 (−20.1, 44.0)	−3.12 (−29.2, 23.0)	—
	2 Years	4.25 (−5.3, 13.8)	−0.97 (−12.3, 10.3)	—
	Permanent	6.55 (−11.3, 24.4)	−9.22 (−60.1, 41.7)	—

Note. Results are shown in terms of β coefficients, with 95% confidence intervals listed parenthetically. UOF = use of force; K9 = Canine-related use of force.
**$p < .05$.

longitudinal analysis of arrest rates might detect. Inconsistency between one's attitude or verbal expressions and their physical behavior is quite common (Ajzen & Fishbein, 2005; Engel & Worden, 2003), and reflective of the reality that anonymously voicing discontent presents less organizational or professional risk than active noncompliance.

Future depolicing research should examine this proposition further, with a particular focus on the extent to which officers' sense of duty and public service motivation stifle the desire to engage in certain types of dissent shirking behavior. Similarly, there is also need for research that considers the extent to which analysis of other potential indicators—changes in sick days taken, retirement, and lateral agency transfers, as well as traffic and pedestrian stops made—uncovers the presence of depolicing in the context of pattern or practice reform.

Such a study would no doubt be complicated by a central challenge of this research: limited data availability. Detailed, publicly available data on the number of citations issued or sick days used will

be difficult to acquire. Indeed, the absence of widely available data—crime and arrest figures—led to the exclusion of a significant portion of the pattern or practice population. This is not to suggest that data were completely unavailable for all 31 omitted agencies. In fact, every excluded jurisdiction submitted some data between 1990 and 2010, but did not do so consistently or with high enough quality to merit inclusion. Even if the comparison was being made between agencies that submitted full data and those that submitted none, the empirical connection between data dissemination, transparency, and organizational corruption remains unproven (Bertot, Jaeger, & Grimes, 2010; Lindstedt & Naurin, 2010).

This limitation must be acknowledged, and with it the possibility that data limitations have biased the sample in favor of jurisdictions less prone to depolicing. At least two salient facts mitigate these concerns, beginning with similarities between the sample agencies and those omitted. First, the substantive nature of the allegations reviewed by the DOJ in sample jurisdictions is nearly identical to those levied against nonsampled departments. So too is the percentage found to have engaged in a pattern or practice of unlawful activity. DOJ intervention, indicative of systemic misconduct and wider organizational dysfunction, occurred in 5 of the 10 sample agencies, very similar to the 48.4% (15 of 31) found in omitted jurisdictions.

The sample is representative not only in terms of substantive composition, but also the number of high risk cases included. Although both Columbus (the only jurisdiction between 1994 and 2008 to challenge the DOJ in court) and Cincinnati (a city where officers spoke openly of depolicing as department leadership undermined DOJ oversight) were excluded, other fraught and publicly oppositional jurisdictions, including Pittsburgh, Prince George's County, and Detroit were included.

In light of these limitations, the results should be interpreted with an appropriate level of caution. Along those lines, the absence of support for the emotional hypotheses should not be read to diminish the importance of reduced officer morale or claims of depolicing. That officers continue to assert that depolicing exists and express emotionally driven dissatisfaction with the circumstances surrounding reform seems to justify a careful assessment of the strategies used by police executives and DOJ staff to communicate with officers about the substance and process of organizational reform.

In fact, low job satisfaction has the potential to interfere with the very goals the DOJ seeks to promote. Proactive efforts to address morale problems would not only facilitate sustainable organizational change (Chanin, 2015), but may help to strengthen police–community relations (Greene, 1989; Wolfe & Nix, 2016). Increased transparency on the part of the DOJ would aid in preventing the alienation of officer groups. The inclusion of rank and file officers at early phases of DOJ intervention, including both the initial investigation and the negotiation of settlement terms, is a worthwhile first step, as is the creation of a forum where affected officers may engage directly with DOJ staff and other relevant stakeholders.

Similarly, these results should not prevent future research on the viability of the rational choice explanation. The notion that police officers would shift their behavior in response to unfavorable changes to working conditions is an important issue, not only as it relates to agencies affected by pattern or practice reform, but police departments across the United States. Over the last several years, departments have increasingly adopted the use of body worn cameras, intensified internal and external oversight, and more aggressive officer discipline to promote officer accountability (Byrne, 2016; Graham, 2016; Mather & Zahniser, 2016). Further understanding of how officers respond to such changes would provide very useful insight for a discipline working to adjust to the heightened public and institutional scrutiny that has come to define the modern conversation around policing in America.

It is also worth considering the prospect that the absence of empirical support for the rational hypotheses is in and of itself an indictment of ability of pattern or practice reform to drive changes to the cost of officer misconduct. Organizational reform is a daunting task, even under the best conditions (Pressman & Wildavsky, 1984); when public actors oppose change, successful

implementation is even more elusive (Mazmanian & Sabatier, 1989; Sykes, 1985). If the reforms had not been institutionalized, officers would not have to adjust their utility functions, obviating the need to engage in rationally driven depolicing. Future research should explore this possibility by examining responses to the reform process among street-level officers and, critically, sergeants and other members of organizational middle management. Deeper understanding of this dynamic will not only shed light on challenges facing pattern or practice stakeholders but will provide a valuable window into how we conceive of and measure dissent shirking in the context of police reform.

Finally, it is worth considering the possibility that neither the rational choice nor the emotional response thesis offers the best explanation for the patterns observed in arrests. Indeed, a more nuanced explanation might be that officers operate within a wider framework of responses that impact the informal ways they engage policing practice. Instead of making cost/benefit calculations or responding emotionally to changes with which they disagree, they might exercise active expression through following formal policy, but not following rules to the letter. This systematic deviation from rules and policy in the form of practice and process deviance might be a strategic but safer way, given DOJ oversight, and reform process implementation, to both share and express dissent. Thus, dissent shirking might be manifest as dissent deviance. In order to explore this possibility, we would need data on whether, like other public agents, police officers under DOJ pattern or practice agreements are differentially compliant with rules, norms, and community expectations, in their practices, regardless of what their arrest data show.

Declaration of Conflicting Interests

Funding

Notes

1. Under Section 14141, the Department of Justice (DOJ) has negotiated with state and local police departments both consent decrees (CD) and memoranda of agreements. Although there are very real legal differences between these two types of settlements, in terms of substance and their effects on officer behavior, the distinction is without difference. As such, in this article, both types of agreement are referenced as "settlement agreements."
2. Compare, for example, *United States of American v. City of Pittsburgh, Pittsburgh Bureau of Police, and Department of Public Safety, Consent Decree* (1997, April 5) with *United States of America v. City of Los Angeles, California, Board of Police Commissioners of the City of Los Angeles, and the Los Angeles Police Department, Civil No. 00-11769 GAF Consent Decree* (2001).
3. It is worth noting here that both the Pittsburgh study and the Los Angeles Police Department report draw conclusions about depolicing based on simple trend analysis of available data, not through the use of multivariate statistical analysis. As a result of various threats to validity, their conclusions should be taken with some caution.
4. It is possible that a DOJ investigation may prompt some organizations to institute reforms to appease federal investigators and avoid a pattern or practice finding. Unfortunately, the decision to do so is seldom made public and thus is not something that can be accounted for in the analysis.
5. It is also possible that a drop in arrests would correspond to (and may be indicative of) a drop in the incidence of crime.
6. The felony arrest index includes monthly arrest figures data for murder, rape, robbery, aggravated assault, burglary, larceny, and auto theft. There were no significant missing data in any of the 10 included jurisdictions.

7. In several instances, missing cases were estimated using Stata 13's interpolation function, including all felony and misdemeanor arrests data for Prince George's County, MD, in 1992; Bakersfield, CA, in 1995; Charleston, WV, in October 2001; and Portland, ME, in June 1995 and July 2008. Detroit, MI, was missing data on arrests for disorderly conduct in 1999. Finally, arrests for vandalism committed in Montgomery County, MD, in June, July, October, November, and December 1994 were missing.

8. Each of the models shown in Table 6 was developed iteratively by examining autocorrelation function (ACF) and partial ACF plots to identify empirically the model of best fit. Durbin–Watson and Box–Ljung diagnostic tests were used to identify autocorrelation in the model residuals and in each case were insignificant at the .05 level, indicating that the chosen models were satisfactory.

9. Unfortunately, technical limitations prevent consideration of whether the onset of depolicing occurs abruptly or gradually (McDowall et al., 1980; Wei, 2007). As a result of these practical challenges, together with work by Shi (2009) and Prendergast (2001, 2002), each intervention is assumed to onset abruptly rather than gradually.

10. It is important to note here that the current state of the depolicing literature provides little if any theoretical or empirical guidance on how to operationalize a "temporary" response to a change in policy or public attitude. As such, the decision to measure the change in arrest rates after 1 month and again after 2 years may indeed not account for more intermediate effects (e.g., those that subside after 6 months). Future research should no doubt engage with this issue.

11. Several additional models were used to examine the possibility that dissent shirking does not begin immediately, but occurs after a delay. These results showed no significant findings either and are excluded for the sake of parsimony. We also exclude the results of models that included variables designed to account for potentially confounding events unrelated to the pattern or practice process, as their inclusion did not affect the outcome of the analysis.

12. Close examination of the jurisdiction's monthly trend data revealed at least two significant and seemingly random dips in felony arrest levels. In June 1998, Montgomery County reported 41 felony arrests, significantly fewer than the 130 reported in May 1998 and the 135 listed for July 1998. Similarly, in October 2010, felony arrests dropped to 38 from 126 in September 2010; in November 2010, the total increased to 122. When these two outlier months are dropped and replaced by predictive statistical averages, the statistical significance listed in Table 8 is no longer evident.

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