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Abstract

Objectives: Test the immediate and sustained impact of suspending a police K9 program on officer injury, suspect injury, and suspect resistance rates.

Methods: A large municipal policing agency housing one of the oldest K9 programs in the U.S. suddenly terminated the program at the close of summer 2020. We exploit this change as a natural experiment to test three hypotheses related to rates of injuries (officer and suspect) and resisting arrest. We use Bayesian modeling in an interrupted time series analysis to measure the immediate and long-term effects of the K9 apprehension program's suspension on our hypotheses.

Results: The sudden suspension of K9 apprehension was not associated with a statistical increase in officer or suspect injury, or suspect resistance, during felony arrests.

Conclusions: As police agencies, communities, and stakeholders renegotiate the risks of policing, constraints on using police K9s are unlikely to impact aggregate officer or suspect safety negatively.

Keywords: Deterrence, Injuries, K9s, Natural Experiment, Police, Officer Safety

1. INTRODUCTION

"Take a BITE out of crime!"
McGruff, the Crime Dog (Ad Council, 1978)

First used in the United States in 1907, K9s are among the oldest and most recognizable police tools. The bite of a K9 is universally feared, and police agencies have long claimed that such fear keeps officers safe and reduces suspect resistance (Chapman, 1990; Dorriety, 2005; Hart et al., 2000). Critics disagree, arguing that K9s increase suspect injuries and constitute a form of racially disparate policing (Mesloh, 2006; Schiavone, 2018; Wasilczuk, 2022). Despite prolonged use, the scholarship on K9s is scant, with most research examining the medical implications of bites, or olfactory studies on the ability of the dogs to detect narcotics, explosives, and cadavers. Scholars have renewed calls for empirical tests of police procedures, specifically concerning use-of-force (Lum & Koper, 2017; McLean, Stoughton, et al., 2022). Policing must subject even its "best practices," and hallowed traditions to empirical testing (Weisburd & Neyroud, 2011).

We respond to those calls by conducting a natural experiment in which a large city suddenly terminated one of the nation's oldest police K9 patrol programs in the summer of 2020. This policy change allows us to test three claims: that K9 teams reduce officer injuries, reduce suspect resistance, and either reduce or increase suspect injuries. For all outcomes, we base our measures on rates per felony arrest, as the agency in question has a policy restricting K9 apprehension deployments to felony incidents. Our results push back on claims from critics and proponents of K9s alike, as the sudden disappearance of K9 operations did not significantly affect officer injuries, suspect injuries, or resisting arrest rates. Given the prevalence of K9 apprehension in U.S. policing, we close with calls to improve empirical research in this area, to help police executives balance risk in police operations, and to inform ongoing debates about the proper role of police use of force.

2. POLICE K9: BACKGROUND AND IMPACTS

Dogs have served as the guardians and domesticated partners of humans for at least 11,000 years (Bergström et al., 2020). Historical records show dogs were used in the Roman and French empires to serve in combat and as personal protection. Chapman (1990, pp. 9–10) reports that a dog in Ancient Greece recognized two fugitives who had murdered a slave as the earliest record of a K9 "detective." In contemporary agricultural societies, dogs are still used for a variety of reasons, including hunting and protection (Pacheco-Cobos & Winterhalder, 2021).

The first specialized *police* K9 program began in 1899 in Ghent, Belgium (Chapman, 1990). This early program was robust, employing 37 dogs to police a population of approximately 175,000 residents. The Ghent model soon spread to other European countries and the United States. By 1908, the earliest U.K. police K9 program was established by the North Eastern Railway Police, who imported Belgian-trained dogs to deal with disorderly sailors during shore leave (Chapman, 1990). Police in the U.S. adopted K9s in the early 1900s. An early *New York Times* (1907) article profiles the Ghent police K9 program as the model to bring K9s to the New York Police Department. Contemporaneous newspaper accounts claim that the NYPD K9 program immediately reduced crime by 50% and "practically eliminated those crimes which are

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common to residential sections" of the city (quoted in Chapman, 1990, p. 17). Cities soon established police K9 programs across the country.

Police K9 programs in the U.S. take on two dominant configurations: detection and apprehension. The current study only focuses on the claimed effects of apprehension K9s[i], which track and detain suspects with the potential of a bite occurring during the incident. The potential bite separates pointy-eared K9s from their floppy-eared brethren, the bloodhound, which police also use for tracking but do not bite. The other main form of K9 programs is for olfactory detection. These overlap with apprehension K9s; in fact, many apprehension K9s are "dual trained," meaning they can track and detain suspects and simultaneously provide an olfactory detection capacity. Some agencies have established a concurrent but unrelated "facility dog" program to build rapport with victims and witnesses (Spruin et al., 2020).

As noted, most research on police K9s is unrelated to their use as a police weapon. Instead, there is a relatively robust research literature concerned with their olfactory abilities (Jezierski et al., 2016; Kokocińska-Kusiak et al., 2021), whether it be for cadavers (Komar, 1999; Riezzo et al., 2014), explosives (Lazarowski et al., 2020) and arson (Scott, 2014), suspect scent lineups (Ferry et al., 2019), or narcotics detection (Abdel Fattah & Gharib, 2020). Relatedly, researchers have investigated the impact of handler beliefs on K9 detection performance (Lit et al., 2011, 2019).

A smaller body of evidence related to police K9s comes from medical studies, typically describing injuries sustained by suspects from K9 bites[ii]. Loder and Meixner (2019) survey national data from 2005-2013 and estimate[iii] there are 3,661 annual emergency room visits for police K9 bite injuries, which accounted for approximately 1% of all such visits for any dog bite injury. Though representing a small fraction of the total dog bites in the U.S., police K9 bites tend to produce more bites and catastrophic injuries compared to non-police dog bites. K9 bites require more hospital time, invasive testing, and operations (Meade, 2006; Pineda et al., 1996; Snyder & Pentecost, 1990). Hutson, et al. (1997) survey medical records to test the impact of a K9 policy change in Los Angeles. Reviewing 790 police K9 bite records, the authors find that the change from "find and bite" to a "bark and hold" policy resulted in massive decreases in the numbers of bites, from 639 (1988-1991) to just 66 (1992-1995). Court rulings have been instrumental in producing such police K9 policy changes, and civil suits have established the necessity of careful monitoring of K9 apprehensions and bite ratios by agencies (see *Chew v. Gates*, 1992; *Kerr v. City of West Palm Beach*, 1989).

The criminological literature on K9s is scant, especially considering how use-of-force has long been a focus of study (Alpert & Dunham, 2004; Alpert & Smith, 1994) and continues to have high salience in public concern (Mourtgos & Adams, 2020). Some studies include K9 force as a secondary focus, such as in a BWC study finding that police specialty units (including K9 teams) generally use more force compared to non-specialty patrol units, and that BWCs significantly reduced force among those specialty units (Gaub et al., 2020, 2021). Hickey and Hoffman (2003) review six years of records in a single large suburban department and develop descriptive measures of bite ratios. They show that 14% of suspects apprehended (i.e., found) by K9s were bitten, and that non-white suspects were less likely to be bitten than white suspects.

The Boundaries of K9 Claims

Given the paucity of empirical research in this area, many untested claims regarding police K9s exist. Consistent from the beginning of their use in policing, proponents have claimed that K9s have a deterrent effect – they reduce crime, and they reduce resistance and assaultive behavior from suspects confronted by police. We briefly spell out these common claims here, although we leave most untested in the current

study, hoping that other researchers will find value for future research. In his history of the North American police K9, Chapman (1990, p. 104) links to five main advantages of using K9s in police work:

1. Psychological intimidation to reduce disorderly behavior in crowd/protest situations
2. Deterring potential criminals and thereby reducing crime
3. Detecting suspects at night, and thereby assisting in arrest
4. Officer protection
5. Public relations

There is precious little empirical evidence to support any claims, whether by proponents or critics of police K9 programs. The gap in the research record is real, and so our task is to lay out "what we know" and link it to what we call "suggestive evidence" to develop hypotheses. In the following section, we re-introduce the testable claims and focus on the empirical evidence. First, however, we briefly outline common claims about K9s, although they are outside the scope of the current study.

Police and K9 advocates often claim that K9s have a deterrent effect. In this context, deterrence claims are sometimes generalized (K9s reduce crime) and, at other times, specific (K9s reduce suspect resistance and assaultive behavior). Following recent school shootings, one breeder and trainer of K9s made the deterrence appeal directly, stating (Koberg, 2022, para. 9), "I've watched them as a deterrence in our militaries, I've watched them as a deterrence in our families, I've watched them as a deterrence in our law enforcement… I'd like to see it in every school system." The deterrence claim is perhaps vividly remembered by those exposed to McGruff the Crime Dog – a cartoon character created by the National Crime Prevention Council in 1978 – who urged viewers to "Take a BITE out of crime!" (Office of Justice Programs, 2002). McGruff is considered among the most successful ad campaigns of its time, and "became a cultural hallmark of empowerment against crime" (Melillo, 2013, p. 167).

One study finds that homes with dogs (i.e., pets) experienced less property crime than homes without dogs (Grooms & Biddle, 2018). This study was not on police K9s but does suggest a general deterrent effect of dogs. The general claim that K9s are "crime fighters" is a commonly used one when agencies seek public donations to fund the purchase of a new K9 (Walby et al., 2018). Chapman (1990, p. 218) provides a bulleted list of the types of crimes K9 teams have helped reduce the incidence of, including felonies such as[iv]: child molestation, sexual assaults, burglaries, robberies, and vehicle thefts. Chapman offers multiple anecdotal reports from police officers and chiefs regarding the purported deterrence of K9s. The claimed deterrent effect is so strong that in one story, the mere fake barking of a police officer was enough to cause a hidden burglary suspect to surrender. In another, some 60 members of the Hell's Angels motorcycle gang chose to peacefully leave Manteca, California, after realizing the police force had two K9s.

One common belief is that K9s benefit the public perceptions of the police department, or that they are an effective public relations tool. One survey of K9 officers found that 99% of respondents believed that "the presence of the dog increased the public's perception of their competence and enhanced public opinion and respect for officers" (Hart et al., 2000, p. 199). This firmly-held belief runs counter to recent experimental evidence that officers pictured with K9s were viewed more negatively (Sandrin et al., 2022).

We reiterate that the claims above are entirely untested. One police chief quoted by Chapman (1990, p. 108) declared that (emphasis provided): "the crime prevention aspect of…police dogs is *immeasurable*." Perhaps he is right, although we suspect there are causal identification strategies that can assess deterrence effects, and welcome future empirical tests. However, for the current study, we are most interested in their purported benefit to officer and suspect safety, and effects on suspect resistance.

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Testing K9 Claims – Injuries and Resistance

The claims we test in this paper are more important than the anecdotal hyperbole of K9s having the ability to scare off outlaw motorcycle clubs or child sex offenders. Specifically, we aim to assess Chapman (1990) and others' claims that K9 programs improve officer safety, improve suspect safety, and reduce the likelihood of suspects resisting the police.

Officer Safety: A primary justification for K9 apprehension programs is that they are a valuable source of officer safety. Chapman (1990, p. 111) notes that officer safety is the most important benefit of police K9s: "While the police dog is of great utility in routine police work its primary responsibility is to protect the handler and other officers." This common belief is documented across the small literature on police K9s. Hart et al. (2000) found that 91% of their K9 officer respondents endorsed the belief that K9s have an important or very important role in protecting them. They found that more than a third of officers (38%) believed their K9 partner had saved their lives, while many others felt the K9 had saved them from injury. The authors further find that overwhelming numbers of K9 officers (95%) believe their department was more effective due to a K9 program, although defining "effective" in this context was unclear.

Agencies are motivated to preserve the health and safety of their officers, and injury is commonplace in police work. Approximately 160 officers are assaulted in the U.S. every day, meaning that upwards of 10% of officers in the country are assaulted yearly (White et al., 2019). Additionally, the many police K9s killed in the line of duty offer more evidence of the extremes of danger that K9 teams can face. Many K9 deaths occur in situations involving officer protection, either of their handlers or by acting in place of human officers who might otherwise be harmed (ODMP, 2022).

The claim that K9s reduce officer injuries has never been empirically tested. However, some supporting evidence was found in a study that tested the effects of a more restrictive Taser policy. Womack et al. (2016) show that when Dallas police implemented a more restrictive Taser policy, there was a slight increase in officer injuries. Other research has similarly found a reduction in officer injuries following the introduction of Tasers (Smith et al., 2007; Taylor & Woods, 2010). It might be that the sudden removal of K9s could produce similar increases in officer injuries.

Suspect Safety: Related to officer safety are police claims that the presence or psychological intimidation of K9s increases safety for arrestees. In other words, we might consider that the bite ratio (Mesloh, 2006) by definition has more "no bite" counts than bite counts[v]. The counterfactual in these cases relies on the assumption that *absent the K9*, suspects would likely choose to resist more often and aggressively, creating an officer's response to that resistance.

There is empirical support for the idea that ending combative situations earlier is safer for suspects and officers. Dorriety (2005, p. 93) states this claim as one of force replacement: "the use of police dogs actually lessens the likelihood that officers might have to resort to deadly force." In other words, K9s pre-empt the need for deadly force by ending situations earlier or providing an alternative to lethal force. Previous studies have found that introducing other serious forms of police force produces safer outcomes for suspects. For example, introducing the Taser was associated with fewer and less severe suspect injuries (Smith et al., 2007; Taylor & Woods, 2010). If proponents of police K9s are correct, the sudden end of a K9 apprehension program will result in higher rates of suspect injuries.

However, critics of police K9s make the opposite claim: they believe K9s produce suspect injuries that might not otherwise occur. For example, some scholars suggest that police use K9 bites in a racially disproportionate manner (Mesloh, 2006; Wasilczuk, 2022; Wolf et al., 2009), although other studies find

non-white suspects are less likely to be bitten compared to white suspects (Campbell et al., 1998; Hickey & Hoffman, 2003). Measuring such bias is extraordinarily difficult and subject to active scholarly debate (Tregle et al., 2018). However, if true, one way bias should appear is in surplus, unnecessary injuries to suspects. Like the claims of K9 proponents, critics' claims find some support in research on Tasers, where researchers find court restrictions on Taser use is correlated with fewer suspect injuries (Boehme et al., 2021).

Schiavone (2018) creates a theoretical rationale for excessive numbers of bites. She writes that the fear of K9s is so great that suspects are compelled to defend themselves from the K9, thereby creating a bite that would not otherwise occur. If true, and "in light of the human behavioral fear response," Schiavone (p. 619) asks whether any use of the apprehension K9 could be considered reasonable under the law. This theory would predict, therefore, that the sudden absence of a K9 unit would create a safer felony arrest environment for suspects. Schiavone's theory would be supported by finding a lower rate of suspect injuries in the post-removal period.

Psychological Intimidation – the "Or Else" of Felony Arrests: Bittner (1970) theorized that police represent the "or else" of society, representing the ability to coerce or compel behavior across a wide variety of dimensions, including the use of physical force. For many K9 handlers and writers, the K9 represents the sharpest non-lethal edge of coercive police operations. The dogs are psychologically intimidating, and their presence proves to the suspect that immediate compliance is the only conduct sure to save them from serious injury (Chapman, 1990). Suspect resistance is the key driver of police decisions to use force (McLean, Alikhan, et al., 2022), and this hypothesis links to police assertions that the presence of a K9 can reduce suspect injuries by reducing the likelihood of resistance.

Hypotheses

We are unaware of any quantitative evaluation of the claimed benefits of K9s in policing, despite their prevalent use in U.S. policing for over 115 years. As covered earlier, there are no known empirical studies of the exact claims studied here, namely that the loss of a K9 program will result in increased officer injuries, suspect injuries, and resistance to arrest. Guided by relatively little literature, we derive the following three testable hypotheses based on the claims documented across practitioner, media, and scholarly sources reviewed above:

(H_1) The **Officer Protection Hypothesis**: The rate of officer injuries per felony arrest increases after the program termination.
(H_2) The **Suspect Safety Hypothesis**: The rate of suspect injuries per felony arrest increases after the program termination.
(H_3) The **"Or Else" Hypothesis:** The rate of suspect resistance per felony arrest increases after the program termination.

3. Measures and Method

The studied agency's patrol dog program was suspended without notice on August 12, 2020, by order of the city's mayor. Our agency tracks data in a manner that allows us to test the effect of suspending the patrol dog program on officer and suspect injuries, and resistance during felony arrests. We use Bayesian modeling in an interrupted time series analysis of daily measures to assess the immediate and long-term effect of the K9 apprehension program's suspension on our three hypotheses. We leverage a natural experiment to infer causal links between the lack of K9s and the outcomes of interest.

Design

Experimental techniques, particularly randomized controlled trials (RCTs), are widely considered the gold standard in research (Todak et al., 2018). Regarding the current study, one might imagine a more perfect experiment on the impact of K9 units in policing. For example, unconstrained by agency management or institutional review boards, researchers might design a study in which K9 units are deployed on a randomized schedule across blocked precincts, as done in body-worn camera research (Braga et al., 2022). One obvious objection to an RCT is the simple fact that none has been conducted in the K9 context, lending support to the idea that the difficulty may be insurmountable. Furthermore, even a relatively large collection of RCTs suffers when the *goals* of the technology or intervention are unclear (Alpert & McLean, 2018; McLean, Stoughton, et al., 2022). Though we support future evaluation of K9s using RCTs, the absence of research in this area has left the goals of K9 programs undefined, especially given the results of the current study, as we report later.

We take advantage of a natural experiment. The pre-intervention time series begins on January 1, 2018, and ends on August 11, 2020. The post-intervention time series begins on August 12, 2020, and ends on December 31, 2021. Accordingly, the pre-intervention series is 954 data points, with a post-intervention length of 507 days.

Agency Context

We base our study in a large municipal police department serving a state capital city in the United States. The agency services an estimated 200,000 residents living in a mixed urban core and suburban service area. Approximately 600 full-time sworn officers work for the agency. The studied agency housed one of the oldest modern-era K9 programs in the country, beginning before 1960 (Chapman, 1990). Before the suspension, the K9 program housed approximately seven full-time apprehension handlers and three bloodhound handlers. The agency is comparable to the national average regarding officers' sex and most racial categories, except for employing relatively more white and fewer black officers. Further, the studied department is accredited by the Commission on Accreditation for Law Enforcement Agencies (CALEA) and all agency use-of-force policies comply with CALEA standards.

The Natural Experiment

The agency suspended its patrol dog program on August 12, 2020, following allegations that one of its K9s had unlawfully bitten a black man during an arrest for aggravated domestic violence. Media coverage of the incident occurred several months after the occurrence (April 2020), overlapping with the summer of policing protests in response to the murder of George Floyd. Like other major U.S. city police departments, the agency already faced heightened public scrutiny and political pressure to reform practices. Although the initial incident had cleared internal review and had occurred four months earlier, the city's mayor and police chief responded to the media story by announcing the immediate suspension of the agency's K9 program. In addition, all K9 handlers were placed on leave while all K9 bites in the previous two years were investigated. The policy was directed by the city's mayor, with the public support of the chief of police. The policy took effect immediately, and was not pre-announced, removing any anticipatory effects.

The implementation shock is a critical component of our causal identification strategy, as it allows us to leverage a natural experiment and identify treatment effects by comparing the pre- and post-shock periods. When a series of measures is suddenly and unexpectedly interrupted by a policy shock at an identifiable and non-repeating point in time, time-series analysis can leverage a natural experiment to

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causally identify the effects of the shock (Shadish et al., 2002). Researchers have used similar design strategies to investigate the effects of police layoffs (Piza & Chillar, 2020), the effect of body-worn cameras on citizen fatalities (Miller & Chillar, 2021), the effect of mandatory sex assault kit testing on rape arrests (Mourtgos, Adams, Nix, et al., 2021), and how alcohol licensing variation contributes to hospital admissions and crime (Vocht et al., 2017).

Measures

We cannot make a simple pre-post comparison of injuries and resisting arrest following the termination of the K9 program because the frequency of situations requiring a K9 varies over time. The agency restricts K9 deployment to felony apprehensions. Accordingly, felony arrests are the circumstances in which an apprehension dog could have been used and possibly prevented an officer or subject from being injured, or potentially deterred an individual from resisting arrest.

The number of felony arrests changes naturally across time. This variation was even more prevalent following the George Floyd protests beginning mid-year 2020. Like much of the U.S., the studied city saw a substantial increase in violent crime through the remainder of 2020 and 2021. Further, there were rapid increases in the number of officers voluntarily leaving large agencies (Mourtgos et al., 2022a), as well as significant changes in call response times and the distribution of officer activity (Mourtgos et al., 2022b; Nielson et al., 2022). We address these measurement challenges by constructing each outcome as a rate per felony arrest, using three rate variables to test our hypotheses: the rate of officer injury per felony arrest, the rate of subject injury per felony arrest, and the rate of subject resistance per felony arrest.[vi]

Table 1 provides descriptive statistics and naïve pre-post intervention differences. A few observations stand out. First, injuries to both officers and suspects are relatively rare, if considered a function of overall police activities and contacts. Second, subjects are more likely to be injured than officers. Third, while resisting during a felony arrest does not occur in most cases, over the four years, resistance occurred during felony arrests approximately 17% of the time. All three variables increased when making naïve pre-post intervention comparisons, although the changes were minor. While the rates remain low, the percent change for each measure is not insignificant. For officer injury per felony arrest, the increase was 16.7%. For subject injury per felony arrest, the increase was 19%. The resistance rate per felony arrest increase was much lower at 1.8%.

While these percentage changes may suggest an increase in injuries and resisting arrest after suspension of the apprehension k9 program, as stated before, a simple pre-post comparison is naïve and inappropriate. First, we are comparing different lengths of time in the pre- and post-intervention periods. Second, a simple pre-post comparison cannot account for any present trends. The primary question is whether the observed increase in officer injuries and decrease in subject injuries—albeit small—significantly different from the rates expected in the counterfactual. We answer that question in the next phase of analysis.

Table 1

	Mean	SD	Pre-Intervention Mean	Post-Intervention Mean	Pre-Post Difference

Officer Injury/Felony Arrest	.012	.065	.012	.014	+ .002
Subject Injury/Felony Arrest	.022	.085	.021	.025	+ .004
Resists/Felony Arrest	.167	.187	.166	.169	+ .003

4. Analysis

We proceed with Bayesian modeling and interrupted time series analysis (ITSA). Our research questions and data are appropriate for ITSA analysis. We have longitudinal data about officer injury, subject injury, and resisting arrest, all calculated as rates per felony arrest. Further, we have an intervention affecting an entire population (i.e., all officers making arrests at the studied agency) at a specific time. When a series of measures is interrupted by an intervention at a specific point in time, ITSA is appropriate (Shadish et al., 2002).

We use Bayesian modeling because these methods assume a probability distribution for each parameter coefficient, rather than assuming one 'true' population parameter. The ability to acknowledge and leverage uncertainty through Bayesian inferential methods allows researchers to better guard against false-negative results, inflated false-discovery rates, and inflated effect sizes (Barnes et al., 2020).

An ITSA model typically includes four key parameters: (1) the outcome variable, (2) a continuous variable indicating time since the start of the observational period, (3) a dummy variable designating observations before and after the intervention, and (4) a continuous variable indicating time passed since the intervention. An ITSA model estimated in this manner allows us to investigate whether the intervention had no effect, had only an immediate effect, had only a sustained long-term effect, or had an immediate and sustained effect (Lecy & Fusi, 2022).

The equation is represented as follows:

$$Y = \beta_0 + \beta_1 T + \beta_2 \gamma + \beta_3 \tau + \varepsilon$$

Where $Y = outcome$; $T = Time$; $\gamma = Intervention\ Point$; $\tau = Post - Intervention\ Time$; $and\ \varepsilon = Error$

Due to the distribution of the dependent variables, we estimate a hurdle lognormal model for each hypothesis.[vii] Before estimating each model, we assessed all three time series for stationarity. The Augmented Dickey-Fuller test was significant for all three dependent variable time series ($p < .01$), indicating that correcting for non-stationarity was unnecessary. Next, we specified prior distributions for each model, using weakly informative priors in each model for the intercept, sigma, and hu (binary hurdle process) parameters. Flat priors were used for T, γ, and τ [viii]. ITSA models were then estimated for all three hypotheses.[ix]

Figure 1 plots the resulting model estimates for each response variable. The figure visualizes a decreasing injury rate for both officers and subjects across the entire period (panels A and B, respectively). The resisting trend is level across the entire period (panel C). One can also observe a separation of the pre- and post-intervention trends for all three models.
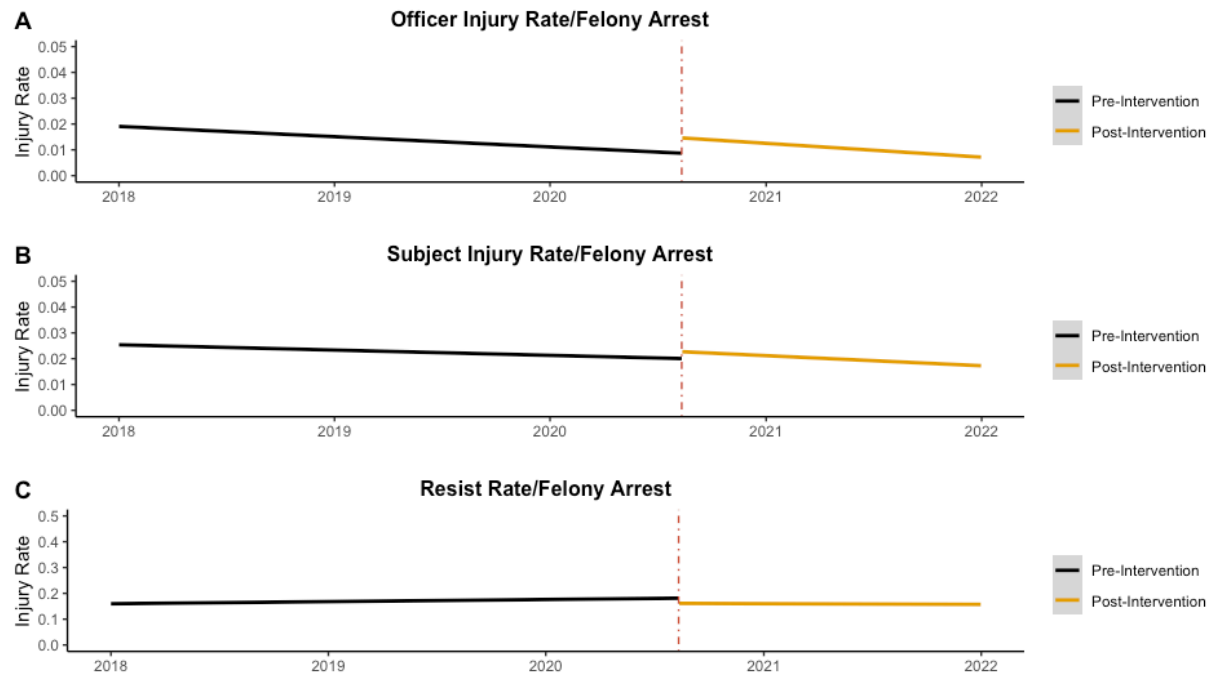
Figure 1: Interrupted Time-Series Estimate Plots

While the plots indicate a possible immediate effect of the policy intervention, one must examine the parameter estimates to see if the effect is of any substantive degree. Table 2 presents parameter estimates for all three models and confirms no significant effect post-K9 suspension.

Table 2: Parameter Estimates (All Models)

	Coef.	**Est. Error**	**95% CI**
Officer Injury			
Time	-.001	.000	[-.001, -.000]
Immediate Effect	.471	.271	[-.062, 1.006]
Sustained Effect	-.001	.001	[-.002, .001]
Intercept	-1.408	.714	[-1.751, -1.068]
sigma	.584	.044	[.506, .675]
hu	.932	.007	[.919, .945]
Subject Injury			
Time	-.000	.000	[-.001, .000]
Immediate Effect	.124	.163	[-.200, .441]
Sustained Effect	-.000	.000	[-.001, .001]
Intercept	-1.787	.109	[-2.002, .109]
sigma	.551	.029	[.498, .611]

hu	.869	.009	[.852, .886]
Resisting			
Time	.000	.000	[-.000, .000]
Immediate Effect	-.119	.088	[-.290, .051]
Sustained Effect	-.000	.000	[-.001, .000]
sigma	.619	.014	[.592, .647]
hu	.349	.013	[.324, .374]

The 95% credible intervals cross zero in all three models for the immediate and sustained effect parameters. While the coefficients are positive for an immediate effect in both injury models, the changes were not larger than a .95 probability of random chance. The sustained effect in both injury models are negative, but again not larger than a .95 probabilty of random chance. The immediate and sustained effect parameters in the resisting model are both negative, but do not indicate a probability of an effect beyond random chance. In sum, the results of the analysis do not indicate the discontinuation of the apprehension K9 program had a discernable short- or long-term effect on rates of officer injuries, subject injuries, or resisting arrest.

5. Discussion

Much of what we "know" about police K9s is received wisdom passed down among police officers. This knowledge is inherently valuable, as what police know and experience is not always directly translatable to the empirical record (Fleming & Rhodes, 2018). Furthermore, the gap between claims and evidence was created by researchers, not police themselves, and if collective responsibility is to be assigned, it should lay at the doors of the academy. Bittner (1970, p. 240) warned against similar scholarly complaints about police record keeping: "It is all too easy to fall into an attitude of supercilious critique concerning the poverty of data. The fact is that neither the police nor functionaries in other practical endeavors should be expected to keep records that would make it convenient for scholars to study them."

With that history in mind, this study is a first step in addressing the gap between claims about K9s and the empirical record. Neither critics nor supporters of police K9s will find much to celebrate. Our results reject all three hypotheses. The sudden loss of K9 apprehension was not associated with a decrease in injuries to suspects, an increase in officer injuries, or an increase in resistance during felony arrests. No outcome saw significant change in the immediate or long term.

The most conservative interpretation of the results is that there is no statistically significant link between the sudden termination of the K9 apprehension program and officer injuries, suspect injuries, or suspect resistance. There is a great conviction within the law enforcement K9 community that these programs provide more effective policing by increasing officer safety, reducing suspect injury, and deterring suspect resistance. We were not able to detect any such effect on any measure. We offer two theories for why we might be seeing null results.

First, it may be that K9s prevent a specific category of officer injury, namely injuries to K9 handlers, while not impacting non-handlers. K9 handlers are placed in more dangerous situations—ones where a suspected felon is alluding apprehension—and therefore have an increased risk of injury compared to other officers. Since the safety increase (specific only to K9 handlers) is offset by the greater risk of injury

from tasks assigned, K9 handlers are no more or less likely to be injured than non-handling officers. In the current study, K9 handlers were necessarily no longer K9 handlers, and were no longer placed into situations that K9 handlers face, including those that might result in injury. Therefore, with no possibility of injury, the model could not find injury. Put simply, if there is an effect, it is a self-fulfilling one. Even if this were the case, it would not provide convincing justification for establishing a K9 team, absent some other untested outcome.

Another possibility is that alternative police weapons suffice as replacements in situations that might give rise to suspect and officer injury. The vast majority of agencies in the U.S. operate with a use-of-force policy based on a continuum of force in response to resistance levels (Mourtgos, Adams, & Baty, 2021). The bite of a K9 is legally non-lethal force (*Matthews v. Jones*, 1994; *Robinette v. Barnes*, 1987), though most agencies consider the K9 bite the most serious less-lethal force (Dorriety, 2005). We can only speculate, but it may be that officers faced with situations that counterfactually would involve deploying a K9 were instead able to rely on some combination of other tools (baton, oleocapsicum spray, Taser, hands, etc.) and tactics. This alternative suggests a hydraulic theory of force, where the absence of one weapon is immediately compensated for with another. Such a phenomenon would not be surprising. Patrol officers routinely make those decisions when a K9 team is unavailable or delayed to the point where other options are selected. Perhaps the policy shock of losing the K9 option was not so shocking at all, and officers continued to find creative ways to fulfill their role as the community's "or else" (Bittner, 1970).

We note that the rate of officer assaults in the immediate aftermath of the policy change could be considered marginally significant at the α=0.1 level ($b = 0.471, s.e. = 0.271$), though in the longer-term analysis, there is no significant association. This result implies that constraints on, or removal of, K9 teams may require an adjustment period for officers to become more comfortable with alternative options, but does not impact officer injuries in the long term. When deciding to decommission K9 apprehension programs, the safest course of action would be a slow implementation, perhaps by simply not replacing K9s as they retire. Linked to the hydraulic theory above, such a course would give time for officers to adapt. Given our other results, constraints or even the removal of K9 operations would not be expected to fundamentally change injury rates amongst officers and suspects, or resisting arrest rates.

5.1. Limitations and Future Research

The empirical research has not caught up to the competing narratives surrounding police K9s. Our results should be interpreted as providing context to those debates, but not resolving them. While the study's design offers a relatively strong causal inference for the null effects, we caution against overinterpreting the results. Like many studies, we are limited to investigating the impact within a single agency, which complicates generalization. A strong theoretical basis would help grant validity across variation in agency contexts, as when "contexts differ across sites – public versus private interactions, field versus laboratory observations – theory is required to generalize from one to the other" (Coppock, 2018, p. 11). We do not provide deep theoretical insights in this study, but point to the need for them, as theory would help predict the effects of K9s across agency sites.

Notably, the conclusions drawn from this analysis can only answer the narrowly posed research questions in the aggregate. Our level of analysis cannot speak to individual interactions where a patrol dog could prevent a specific officer's injury. We caution against interpreting the findings as dismissing officers who believe their K9 partner has saved them from severe injury or even death. While the results indicate no adverse aggregate impact on officer injuries during the studied period, there are undoubtedly situations where a K9 reduces the likelihood of officer injury. Other scholarship has called for rethinking use-of-force

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continua in policing to balance the risk of injury to police and those they contact (Mourtgos, Adams, & Baty, 2021). Our study supports those calls and suggests a rebalancing of the risk curve, not a flattening of it.

Finally, analyses with different outcomes of interest may provide different results, just as additional data may draw different conclusions. We have provided a review of K9 claims earlier that can serve as fertile ground for future research. Additionally, no statistical model or research design is conclusive, and the ones utilized here should not be construed as such. We welcome more robust designs capable of making clearer causal inferences. The most welcome such design would be to use RCTs to randomly distribute K9 teams. This could be done by randomizing which precincts (within large agencies) receive K9 deployments, to dampen the assumptions of RCTs that can occur if randomization is at the call-for-service level (see Choi, Michalski, and Snyder 2022 for recent coverage). Other potential designs exist, and it is well outside the scope of this article to lay out the specific experimental design to be used.

6. Conclusions

Chapman (1990, p. 217) begins the final chapter of his book by surmising that more agencies will likely be adopting K9s to "help cut crime and render cities, citizens, and officers safer." However, these claims were not rooted in scientific evaluation, if only because no such evidence was available at the time. We have presented a starkly different picture of the effect of police K9s than that painted for the last century or more of their use. Mclean et al. (2022, p. 17) review the evidence on various ways to impact police use of force, and find that, "The current state of the literature on police use of force can best be summarized as a wealth of ideas with a lack of support." We agree and complement their review with evidence that the claims of both critics and proponents of police K9s are empirically unsupported. Contradicting the claims of K9 proponents, the sudden disappearance of K9s did not increase suspect resistance or officer injuries during felony arrests. At the same time, contradicting critics who claim K9 apprehension teams are overzealous, it did not result in a sudden decrease in suspect injuries either.

We end by emphasizing what Weisburd (2003) calls the "moral imperative" to provide experimental evidence in criminological research. Such an obligation is clearly in play in the context of police K9s. On one hand, as seen in our earlier review, K9s are claimed to be critical tools for saving officers from injury and death, de-escalatory tools to prevent more severe forms of police force, and generalized deterrents for serious felonies (Chapman, 1990; Dorriety, 2005). On the other hand, critics also trade in non-empiric claims that police K9s are furry extensions of a terroristic police state bent on perpetuating white supremacy (Wasilczuk, 2022), or tools to sustain the capitalist order (Wall, 2014), or that officers wield the K9s without proper legal and tactical restraint (Marshall Project, 2020).

To be clear, our results cannot possibly resolve these debates. The natural experiment offers a strong design, and our results are an important contribution to the scientific literature on police K9s. However, it would be reckless policymaking to overweight a single study compared to the combined bulk of what Fleming and Rhodes (2018) call the "local knowledge" and "craft" developed in a community of police practitioners over a century (or more, if one takes into account human experience with dogs). As a first step in evaluating the effect of K9s on policing outcomes, our findings improve the knowledge base available to police practitioners faced with near-constant calls to use evidence in their policymaking while protecting their officers, communities, and agencies.

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7. Endnotes

[i] For the purposes of our paper, we are solely interested in the purported benefits and effects of apprehension trained K9 teams, sometimes called "patrol K9s." We use "police K9" as shorthand for the remainder of this paper. However, we suggest that given the paucity of research, all of the roles K9s play in policing are excellent potential sites for future research.

[ii] Though exceptions exist, one unfortunate aspect of the police K9 research literature is a unique focus on only one geographic area – Los Angeles County – and one specific hospital (USC). This focus is due to data availability, in that Los Angeles County Sheriff K9 unit data became available and was the focus of early legal inquiry. The current study is one of the few to situate itself outside of Los Angeles County.

[iii] Loder and Meixner (2019) use weights from observed K9 bites (n=581) to develop a national estimate of bites (N=32,951), produced over a study period of nine years (2005-2013).

[iv] Chapman uses less modern language in his list of K9-deterrable crimes. Here we have taken the liberty of translating his list to crime categories recognizable by most researchers and practitioners.

[v] Construction of a bite ratio is itself a contested ground. Here, we are using the definition of a bite ratio to mean the number of K9 bites divided by the number of arrests where the K9 was assisting.

[vi] In the current study, felony arrest means arrests where, at a minimum, someone is arrested for a new criminal offense. That is, if someone is arrested solely for the existence of an outstanding warrant, those arrests are not captured in the data. This is out of necessity with data limitations. The studied agency requires officers to obtain an additional separate case number for a warrant(s) arrest when someone is arrested for a new criminal charge *and* a warrant. Because of this, and due to database limitations, there is no way to disaggregate which warrant arrests were solely for warrants and which warrant arrests were in conjunction with an arrest for a new criminal charge. While this reduces our sample size, the remaining number of felony arrests across four years is still large enough for analysis (N=9,578). Further, we have no practical or theoretical reason to believe that arrests affected solely for warrants systematically differ from arrests for new criminal charges.

[vii] Hurdle models are synonymous with zero-inflated models in that a distribution has a significant amount of zero values that, if not accounted for, will over- and under-predict values. A lognormal distribution is often utilized in Bayesian estimation with continuous variables whose logarithm is normally distributed, as is the case with our data. A hurdle lognormal model is a mixture model in which a logistic regression model predicts whether a value is zero or not (the hurdle), and lognormal model estimates values that are not zero.

[viii] Weekly informative priors typically have little influence on final parameter estimates. They are useful, however, in constraining parameters to reasonable ranges. Further, in large enough samples (as we have here), the data will overwhelm the prior distribution, regardless of the prior values. For transparency, the following weakly informative priors were used for the intercept, sigma, and hu parameters in each respective model:

Officer Injury Model:
1. Intercept = Student t (3, -2.3, 2.5)
2. Sigma = Student t (3, 0, 2.5)
3. Hu = beta(1, 1)

Subject Injury Model:
1. Intercept = Student t (3, -2.3, 2.5)
2. Sigma = Student t (3, 0, 2.5)
3. Hu = beta (1, 1)

Resistance Model:
1. Intercept = Student t (3, -1.4, 2.5)
2. Sigma = Student t (3, 0, 2.5)

3. Hu = beta (1, 1)

[ix] Ten thousand iterations of four Markov-chain Monte Carlo (MCMC) chains were sampled to estimate the posterior distributions. All chains converged with Rubin-Gelman statistics of one and adequate effective sample sizes for each parameter in each model. Posterior predictive models were examined, indicating good model fits.

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