

Municipal Liability Insurance as Police Oversight: Evidence from Vallejo, California*

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Abstract

Municipal liability routinely translates police misconduct into costs for local governments, yet we know little about whether liability's structure actually changes police behavior. This article studies one underappreciated mechanism: insurance. Private liability insurers can function as decentralized oversight because they price risk, impose deductibles, and provide risk-management services. When a city shifts from pooled risk-sharing to market-based coverage, the fiscal consequences of misconduct become sharper. I examine such a transition in Vallejo, California, which in 2018 left a municipal risk pool after high police liabilities and obtained market-based coverage. Using a generalized synthetic control design with monthly California data, I estimate that Vallejo's violent-crime clearances rose substantially, stabilizing at roughly 30 additional clearances per month relative to the synthetic counterfactual—on the order of a near doubling of the pre-treatment rate. This evidence suggests a shift in resources towards activities that increase the rate of crime solving. That pattern supports the expectation that liability institutions shape ex ante incentives inside public bureaucracies, not merely compensate victims.

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1 Introduction

A central problem in the governance of policing is how to create meaningful accountability for institutions that possess substantial operational autonomy, considerable political influence, and unusually high capacity to resist outside supervision. Police departments are formally embedded within municipal government, but in practice they routinely retain broad independence over training, deployment, discipline, and day-to-day enforcement. That reality has become especially salient over the last decade, as repeated high-profile episodes of misconduct have focused public attention on the monitoring and restraint of police. The institutional tools available to control police behavior, however, remain limited, politically fraught, or both (Cheng 2024, Schwartz 2023). A familiar response has been to rely on direct political or legal oversight, with cities creating civilian review boards, revising use-of-force rules, bargaining over labor contracts, adopting body-worn cameras, or seeking federal supervision through pattern-and-practice investigations and consent decrees. Each of these tools may matter at the margin, and empirical work has begun to illuminate when some reforms succeed (Adams et al. 2025, Braga, MacDonald and McCabe 2022, Mummolo 2018). But they also share a common vulnerability: they depend on forms of political monitoring that are themselves subject to polarization, administrative drift, budgetary constraint, and organized resistance from police departments and their unions. Even when reform begins in earnest, implementation is often contested, uneven, and reversible. It is unsurprising, then, that the public debate over police reform has remained unsettled.

This article examines a different oversight mechanism, one rooted less in electoral politics than in liability and contract. When police misconduct generates civil claims, the legal consequences are almost always converted into financial costs borne by municipalities rather than by individual officers (Schwartz 2016, 2025). In many cities, those costs are buffered by institutional arrangements that dilute the relationship between misconduct and marginal price. Some large municipalities self-insure because they can absorb large payouts directly. Others participate in public risk pools, where liability is spread across many members according to formulas that are only imperfectly tied to any one member's conduct. Still others purchase more conventional liability coverage. From the standpoint of incentives, these arrangements are not equivalent. The form of insurance determines how strongly a city's claims history shapes its present fiscal exposure, whether a third party has reason to monitor municipal behavior, and how much organizational pressure the city faces to alter risky practices before claims arise.

Recent legal scholarship has argued that private insurers can function as powerful but understudied regulators of public police. Because insurers bear substantial financial exposure when misconduct produces payouts, they have incentives to discipline risk through premium setting, coverage decisions, loss-prevention requirements, policy advice, and managerial pressure (Grunwald, Rappaport and Berg 2024, Rappaport 2016*a,b*). Journalistic accounts similarly suggest that insurers sometimes push police departments toward changes that elected officials have been unable or unwilling to impose (Kindy 2022). Yet systematic empirical evidence that insurance actually changes policing outcomes remains scarce. That gap is important not only for understanding police reform but for empirical legal studies more generally. Liability rules are frequently evaluated in terms of compensation and litigation, but the mechanisms through which liability institutions shape organizational behavior are harder to observe. Insurance offers a concrete setting in which to ask whether legal exposure actually produces managerial and behavioral effects inside a public bureaucracy. Can the incentives created by market-based insurance step in to discipline police performance where political oversight is weak, inconsistent, or polarized? That is the question this article tackles.

I provide evidence on that question by studying a sharp institutional transition in Vallejo, California. For decades, Vallejo participated in the California Joint Powers Risk Management Authority (CJPRMA), a pooled municipal risk arrangement in which members contribute to a common fund according to a payroll-based formula. By the mid-2010s, Vallejo had become an unusually costly member because of large liabilities associated in significant part with the police department. Its liabilities were 159 percent of expected levels in FY2016 and 266 percent in FY2017, and settlements involving the Vallejo Police Department accounted for more than 28 percent of the pool's total payments in 2017. When the pool responded by sharply increasing Vallejo's self-insurance retention, the city chose to leave the pool and obtain liability coverage through the CSAC Excess Insurance Authority (EIA), effective July 1, 2018. That change exposed the city to substantially higher and more visible marginal costs, including a much larger deductible and rapidly escalating premium payments. The city simultaneously created a dedicated risk-manager position and transferred risk oversight from the City Attorney's office to a more central municipal role. Taken together, the transition moved Vallejo from a comparatively diffuse risk-sharing arrangement toward one in which the fiscal consequences of misconduct were tied much more closely to the city's own behavior.

I use that institutional change to estimate whether a more market-oriented liability arrangement

altered police performance. The outcome I analyze is the monthly number of violent crimes cleared by each law-enforcement agency in California, as reported in the state’s Open Justice data. Violent-crime clearance is not a complete measure of policing quality, and I do not treat it as such. But it captures a central dimension of police performance—the ability to solve serious crimes—and has the practical advantage of being available at a comparable monthly cadence for a large donor pool of jurisdictions (Bjerk 2022, Leipold 2021). Because Vallejo was the only city in California to experience this specific transition at the relevant time, I estimate the counterfactual trajectory using a generalized synthetic control design (Xu 2017). The results indicate that Vallejo’s violent-crime clearances increased materially after the transition. The effect is not immediate. Instead, it emerges several months after July 2018 and then grows, stabilizing at roughly 30 additional clearances per month relative to the synthetic control—on the order of a near doubling of Vallejo’s pre-treatment baseline. That temporal pattern is itself suggestive: it is consistent with an organizational response that required time to take effect, rather than a purely mechanical reporting change triggered at the moment of transition. Supplementary evidence points in the same direction. External complaints against Vallejo officers fell from 34 in 2018 to 11 in 2019, while department-initiated investigations rose from 6 to 12. Those descriptive patterns do not independently identify a causal mechanism, but they fit a coherent narrative in which the city’s new liability environment sharpened monitoring and altered internal behavior.

The article makes three contributions. First, by identifying an institutional channel through which liability law can shape bureaucratic conduct. The effect of liability does not depend only on formal doctrine or the existence of lawsuits; it depends also, and perhaps primarily, on how risk is financed and managed. Second, it contributes to the literature on policing and accountability by drawing attention to insurance as a form of oversight that operates partly outside ordinary politics. That does not make insurance normatively superior to democratic control, but it does mean that legal and financial institutions can complement or, at times, substitute for more visible reform efforts. Third, it contributes to ongoing debates over municipal governance by showing that pooled and market-based liability regimes generate meaningfully different incentives even when both are nominally forms of “insurance.”

Taken together, these contributions speak to a recurring question in the literature about the real locus of legal effect. Formal doctrine matters, but doctrine operates through organizations that must interpret, finance, and administer legal risk. In the policing context, that organizational layer is especially important, because it is municipalities, not officers, that almost always pay the

monetary price of misconduct. Municipal liability is usually studied through doctrine, litigation, and settlement outcomes. The angle here is different. Insurance sits between a city’s claims history and its budget, and the structure of that insurance — how closely premiums track conduct, how much risk the city retains, how much an outside party monitors it — determines whether formal liability registers as practical pressure on the bureaucracy. The core insight, then, is not simply about privatization or the superiority of markets to politics in some general sense. Rather, the upshot is that we have evidence that where direct democratic control over police is episodic or weak, insurance can create an additional pathway through which legal norms are operationalized inside the bureaucracy. A city manager may ignore diffuse public criticism more easily than a rapidly rising premium, a larger retention, or an insurer’s insistence on risk-management reform. In that sense, insurance can make legal accountability more continuous even when politics remains polarized.

2 Liability, Insurance, and the Governance of Public Police

The starting point for this article is simple. Municipal liability is one of the few legal institutions that imposes material consequences on local governments when police misconduct occurs. But liability by itself does not produce a determinate incentive structure. A city can respond to expected liability in very different ways depending on whether it self-insures, spreads risk through an intergovernmental pool, or purchases coverage from an outside insurer. To understand why that matters, it is useful to distinguish between the formal existence of liability and the institutional machinery through which liability is translated into budgetary and managerial consequences. That distinction, I argue, is where much of the behavioral action lives.

2.1 Why political oversight is often weak

In principle, municipal police departments are answerable to mayors, city councils, courts, and voters. In practice, those mechanisms of accountability are often attenuated. Police departments possess specialized expertise, enjoy public support linked to concerns about safety, and often field powerful unions or associations that resist external intervention (e.g., Cheng 2024). Political leaders face strong incentives to appear supportive of police even when misconduct is salient, because overt budgetary or disciplinary sanctions can be framed as being soft on crime (Fegley and Murtazashvili 2023, Sances 2023). Public demands for accountability also vary dramatically across partisan,

ideological, and local contexts, and evaluations of reform remain highly polarized (Schiff et al. 2024). The upshot is that traditional democratic supervision of policing tends to be inconsistent, especially when the benefits of reform are diffuse and the costs are concentrated.

This political weakness helps explain the appeal of many recent reform proposals. Body-worn cameras, civilian review boards, revised use-of-force policies, and consent decrees all seek to make police behavior more legible and more controllable (Adams et al. 2025, Braga, MacDonald and McCabe 2022, Mummolo 2018). Yet those tools often encounter the same basic problem: implementation depends on local officials and administrators whose incentives are not always aligned with sustained oversight. Even successful reforms may be narrow, episodic, or reversible. Indeed, the Department of Justice recently rescinded several pattern-and-practice findings produced at other police departments, a reminder that even federal supervision is subject to partisan reversal. It is unsurprising, then, that scholars and reformers have become increasingly interested in whether less obviously political institutions can create steadier incentives for police behavior.

2.2 Liability as more than compensation

The law-and-economics intuition behind liability is familiar. If harmful conduct generates legal exposure, actors who expect to bear those costs should have reason to take greater care. But policing complicates that story. Officers themselves rarely internalize the monetary consequences of judgments or settlements; almost universally, municipalities pay (Eisenberg 2015, Goelzhauser 2024, Reinert 2023, Schwartz 2023, 2025). That arrangement may be normatively defensible for a variety of reasons—officers cannot easily afford catastrophic judgments, and qualified immunity limits personal exposure in any event—yet it creates a substantial agency problem. The individual whose conduct generates risk is insulated from most financial consequences, while the municipality that bears those costs may struggle to discipline line-level behavior.

Even at the municipal level, however, the incentive effect of liability varies widely. When a city has deep fiscal capacity, the marginal impact of an additional claim may be muted, especially if costs are absorbed through general revenues. When a city participates in a public risk pool, the relationship between claims and cost may be weaker still, because contributions are often calculated on the basis of payroll, population, or other generalized formulas rather than experience-rated claims. Such arrangements can be sensible from the standpoint of risk sharing, but they also dilute the connection between local conduct and price. By contrast, when a city purchases more market-oriented coverage, it may face sharper deductibles, steeper premium adjustments, and closer

scrutiny from the insurer.

This distinction matters because liability is mediated by insurance. In ordinary private contexts, insurance is sometimes thought to soften deterrence by insulating the insured from losses. But insurance can also generate its own disciplinary regime. Insurers have every reason to reduce claims through underwriting, premium differentiation, exclusions, mandated precautions, inspections, and policy guidance. That is the basic logic behind a long tradition of work on insurers as private regulators. In the policing context, Rappaport’s (2016a) argument is precisely that insurers are well positioned to regulate public police because they possess both the incentive and the practical capacity to impose risk-reducing conditions on departments. The question is whether those capacities are actually exercised, and if so, whether they produce measurable change in police behavior.

2.3 Insurance as decentralized oversight

Three mechanisms are especially important for the argument here. The first is *financial salience*. A market-oriented insurer can raise premiums, impose larger deductibles, or threaten nonrenewal when claims history worsens. Those consequences may not directly discipline line officers, but they are highly visible to city managers, councils, and police leadership. Once liability costs become budgetarily concrete, city officials have reason to press the department to change practices that appear to generate legal risk. The financial stakes are not hypothetical: in 2019 alone, according to records collected by the *Washington Post*, 19 cities paid at least \$398 million in police-misconduct settlements, with individual payments often reaching several million dollars for a single allegation. When those kinds of costs stop being absorbed quietly by a risk pool and begin to show up as line items in the municipal budget, political attention follows.

The second mechanism is *organizational intervention*. Insurers do not merely price risk; they actively try to reduce it. They disseminate model policies, require training, recommend reporting practices, and insist on risk-management infrastructure (Rappaport 2016b, Schwartz 2016). Such interventions are especially important in public settings, where the insured is not a single decision maker but a bureaucracy with many moving parts. If insurers can shape procedures surrounding supervision, use of force, documentation, complaints, and internal review, then liability operates on behavior through organizational change rather than through marginal price alone. What is more, insurer-driven changes can be more attractive for departments to accept than politically driven reform, because they are framed as technical risk management rather than as a response to

electoral pressure.

The third mechanism is *monitoring and information*. A city's claims history and risk profile generate information that can reveal patterns of misconduct or administrative weakness. In a pooled system, that information may matter relatively little to the member if contribution formulas are blunt. In a more market-sensitive arrangement, the same information can trigger scrutiny and intervention. Insurance thus operates as an informational bridge between legal claims, municipal budgeting, and managerial oversight. It connects the downstream consequences of misconduct to the upstream decisions that produce it.

These three mechanisms together imply that not all forms of liability coverage should be expected to have the same effect. A pooled public risk-sharing arrangement can be excellent at spreading catastrophic risk across municipalities while simultaneously generating weak experience rating. A more market-based arrangement need not be fully individualized to sharpen incentives; it need only make the member's own claims history more consequential and bring an outside actor to bear on risk management. That is the contrast exploited in the Vallejo case.

A final conceptual point is worth developing before I turn to the case. Many debates about police accountability presume a basic tradeoff: greater restraint may reduce misconduct, but it also risks chilling effective enforcement (see, e.g., Clark 2024, for a discussion). That concern is important, and a paper about insurance cannot simply assume it away. Indeed, one reason the Vallejo case is analytically useful is that the main outcome examined here is a measure of core policing output, not only a misconduct metric. The possibility that a stricter liability environment can coexist with—or even improve—violent-crime clearances is therefore substantively significant. It suggests that some forms of oversight may redirect effort toward higher-value policing rather than simply constraining activity across the board. Indeed, a wide range of literature has long been interested in whether legal institutions deter, but public-law contexts require a more complicated question: deter what, and with what collateral effects on institutional capacity? A liability regime that discourages unconstitutional or reckless conduct while also improving the quality of serious-case work looks very different from one that simply suppresses police initiative. The Vallejo findings cannot resolve that broader question completely, but they do indicate that stronger liability-linked oversight need not travel only with retrenchment.

2.4 Observable implications

If market-based liability insurance produces sharper incentives than a pooled arrangement, what should we observe? The most direct prediction would concern misconduct itself, but consistent, comparable monthly misconduct data are difficult, if not impossible, to obtain. The setting nevertheless suggests several observable implications. First, if the new liability regime induced city leaders or police managers to shift effort away from risky, lower-value conduct and toward core public-safety functions, then we might observe improved performance on a central policing metric such as violent-crime clearance. Vallejo had long faced allegations not only of misconduct but of policing practices that did not obviously advance serious public-safety goals. A department under pressure to reduce risk might respond by reallocating effort, tightening supervision, or emphasizing higher-quality case work, each of which could plausibly improve violent-crime clearances. Second, if the change produced more active internal governance, we might observe fewer externally generated complaints and more internally initiated investigations. Again, these would be imperfect indicators. A decline in complaints could reflect many things, and an increase in internal investigations could signal either better monitoring or more underlying problems. But taken together, such patterns would be consistent with greater internal attention to risk.

3 The Vallejo Insurance Transition

Vallejo, California, provides an unusually useful setting for studying these ideas because it experienced a sharp and well-documented change in its liability arrangements. For more than three decades, Vallejo was a member of the California Joint Powers Risk Management Authority (CJPRMA), a pool organized to spread liability across public entities. The CJPRMA was founded in 1986, and more than 200 government entities have participated at some point; it runs programs that pool risk and insure general liability, property, and automobiles for member agencies. Joint powers arrangements of this kind are common in California local government and often serve sensible fiscal purposes, especially for smaller jurisdictions that cannot easily self-insure. But from the standpoint of incentives, the CJPRMA structure tied member contributions largely to payroll, using a common formula across all members, rather than tying them closely to member-specific claims. In the event of a covered liability, the pool paid any amount above the member's self-insurance retention. Crucially, this structure weakens the link between any individual member's risk profile and its costs. A department generating disproportionate claims paid the same rate per

payroll dollar as a well-run one.

By the mid-2010s, Vallejo had become an outlier within the pool. The police department had drawn repeated scrutiny for problematic conduct, and journalists had long documented patterns of concerning behavior by Vallejo officers (Bauer 2020). The financial consequences followed accordingly. Under the CJPRMA's governance rules, members whose liabilities exceeded 150 percent of expected levels were subject to review. Vallejo crossed that threshold decisively. In FY2016, its liabilities reached 159 percent of expected levels; in FY2017, they climbed to 266 percent. Settlements involving the Vallejo Police Department accounted for more than 28 percent of the pool's total payments in 2017. Whether one approached the case from the standpoint of legal exposure, organizational dysfunction, or public notoriety, Vallejo was plainly generating unusual risk for the pool and for itself.

In December 2017, the CJPRMA directors voted to raise Vallejo's self-insurance retention from \$500,000 to \$2,500,000, effective July 2, 2018. That decision sharply increased the amount Vallejo would need to absorb before pool coverage applied—a \$2 million annual step change in its fiscal exposure. Rather than remain in the pool under those terms, the city chose to leave. Vallejo was the first member to depart since the City of Williams in 2014, and no others followed until a large group of members exited in 2021. Effective July 1, 2018, Vallejo joined the CSAC Excess Insurance Authority (EIA) and obtained market-based liability coverage. The EIA is an organization that both purchases insurance policies and provides risk-management training and programming to its members, and the transition brought both immediate cost increases and exposure to closer insurer oversight. Vallejo's direct contribution for general-liability coverage rose from \$391,857 in FY 2017–18 to \$852,775 in FY 2018–19, with a \$2 million deductible. By FY 2023–24, the premium had climbed to \$2,448,022. Whatever one makes of the normative desirability of this shift, it plainly made liability fiscally salient to city officials in a way that the CJPRMA formula had not.

The transition also reshaped the way the city managed risk internally. Vallejo created a new Risk Manager position and transferred risk-management responsibilities away from the City Attorney's office toward a more central municipal role. That administrative change is important because it illustrates the broader claim developed in Section 2: insurance can matter by changing governance as well as by changing price. Once liability costs became sufficiently salient, the municipality responded by building an organizational structure devoted specifically to monitoring and reducing risk. The conjecture on which the analysis to come rests is that this bundle of changes—larger deductibles, rapidly escalating premiums, a new insurer-as-overseer, and an internal risk-management

infrastructure—generated novel incentives for the city and department to improve police performance. Prior to joining the EIA, Vallejo officers could engage in reckless behavior without the city or department facing much marginal consequence. After the transition, those same behaviors carried real and growing monetary risk.

This setting is attractive for empirical analysis for three reasons. First, the timing of the institutional change is sharp and well documented, with a clean treatment date of July 1, 2018. Second, the underlying substantive problem—high liability associated with the police department—is visible and independently corroborated. Third, California provides a rich donor pool of law-enforcement agencies reporting comparable monthly crime-and-clearance statistics, all operating within a shared statewide legal environment. Of course, Vallejo is only one case. The inference here is therefore not that every municipality would respond identically to a change in insurance. Rather, the case offers a rare opportunity to examine whether sharper liability-linked incentives can move a police department’s observable behavior in the short to medium run.

4 Data and Research Design

The empirical question is whether the July 2018 transition altered Vallejo’s police performance. Answering that question requires an outcome measure available at a reasonable frequency for a broad set of comparison jurisdictions, a credible strategy for constructing a counterfactual trajectory for Vallejo, and a clear account of what the design does and does not identify. I address each of these in turn.

The primary outcome is the monthly number of violent crimes cleared by each law-enforcement agency in California, drawn from the California Department of Justice Open Justice portal, recorded at the month level. The Crimes and Clearances data record the number of violent offenses—homicide, rape, robbery, and aggravated assault—for which an arrest was made, charges were filed, and the case was turned over for prosecution. I examine the period from July 2016 through December 2019, which provides two years of pre-treatment data and eighteen months of post-treatment data around Vallejo’s July 2018 transition. I end the post-treatment window in December 2019 to avoid contamination from the onset of the COVID-19 pandemic in early 2020 and from a period over which the synthetic-control counterfactual extrapolates outside the plausible support of a count outcome. Violent-crime clearance is not a perfect measure of police quality. Clearance rates can reflect reporting practices, prosecutor screening, and broader patterns in crime as well as police

effort (Bjerk 2022, Leipold 2021). Nor do they capture many dimensions of accountability that animate contemporary debates, such as use of force, procedural justice, racial inequities, or citizen complaints. I therefore treat violent-crime clearances as a limited but substantively meaningful indicator. Solving serious crimes is a core policing function, and if liability-induced organizational reform shifted effort toward higher-value public-safety work, that shift should plausibly be visible in this measure. Moreover, because allegations against the Vallejo Police Department had long centered on low-level, aggressive enforcement of minor offenses, a shift toward serious case work is exactly the margin along which a liability-driven reorganization should operate (e.g., Tyler, Fagan and Geller 2014).

The treated unit is Vallejo. The donor pool comprises the remaining California jurisdictions reported in the Open Justice data, yielding 893 comparison agencies in the main analysis. I do not trim the pool by population, agency type, or baseline clearance level: every reporting California agency other than Vallejo enters as a potential donor, with agencies whose clearance series contain some missing months filled with zeros rather than dropped. Because Vallejo was the only city in the state to experience this precise transition in liability arrangements at the relevant time, a standard panel treatment-control comparison would be inappropriate; there is no natural set of similarly treated units. The empirical challenge is therefore to estimate Vallejo’s counterfactual trajectory absent the change in insurance. To construct that counterfactual, I use a generalized synthetic control framework (Xu 2017). The basic idea is to use untreated jurisdictions to recover a latent-factor model of monthly outcomes and then generate the predicted path for Vallejo absent treatment. The observed post-treatment difference between Vallejo and that synthetic prediction is the estimated treatment effect. Conceptually, the method is well suited to the case because it allows the counterfactual for Vallejo to be built from information distributed across many jurisdictions rather than requiring one or a handful of close matches. The design is especially attractive here for two reasons. First, the donor pool is large and geographically concentrated within one state, which reduces concerns that institutional differences across states are driving the comparison. Second, the treatment is administered to a single unit at a known point in time. In settings like this, generalized synthetic control provides a transparent way to evaluate whether the post-treatment path of the treated unit diverges from what would have been expected on the basis of pre-treatment fit and the common latent structure of the panel. I calculate the effect of the transition using the `gsynth` package in R with parametric inference and bootstrapped standard errors (Xu and Liu 2021). The pre-treatment fit is close. Across the twenty-three months preceding July 2018, the synthetic control

tracks Vallejo’s observed clearances with a root-mean-squared error of 4.68 clearances per month against a pre-period mean of 34.48, a normalized error of 0.14. Pre-treatment residuals have a mean of essentially zero and exhibit no systematic drift, indicating that the interactive fixed-effects model recovers Vallejo’s pre-period series without bias; the near-zero pre-treatment ATTs visible in the right panel of Figure 2 confirm the same fit visually.

Two further specification details are worth stating explicitly. First, the estimator combines two-way (agency and time) fixed effects with an interactive fixed-effects component, the dimension of which is selected by the cross-validation routine built into `gsynth` over the range of zero through five latent factors (Xu 2017). No additional time-varying covariates enter the specification; the counterfactual is constructed from the pre-treatment fit of the fixed effects and latent factors alone. Second, because the interactive fixed-effects estimator pools information across all 893 donors in recovering the latent-factor structure, no single donor is pivotal to the counterfactual, and an agency-by-agency leave-one-out exercise is less informative here than in a classical synthetic-control setting with a small handful of weighted donors. The in-space placebo reported below therefore serves as the principal robustness check on the donor pool, repeating the full estimation procedure with hundreds of non-treated agencies substituted for Vallejo and confirming that the estimated effect is extreme relative to the reference distribution this generates.

A related advantage of the California setting is that the donor pool is composed of agencies operating within a shared statewide legal environment. The agencies are subject to the same broad criminal law, the same state-level reporting system, and similar statewide public debates over policing. That does not eliminate local heterogeneity, of course, but it improves on designs that compare treated jurisdictions to donor units drawn from many states with very different institutional environments. The generalized synthetic control therefore leverages within-state comparability while still permitting a flexible, data-driven construction of the counterfactual. At the same time, the design inevitably carries the limitations of a single treated unit. The synthetic counterfactual is only as credible as the extent to which pre-treatment trends and latent factors provide a basis for predicting Vallejo’s untreated path. The favorable pre-treatment fit is therefore not just a technical detail but central to the inferential argument. Future work would benefit from assembling a multi-state panel of municipal insurance transitions so that the claim developed here can be tested across repeated cases rather than one especially revealing episode.

In addition to the main outcome, I use two descriptive pieces of supplementary evidence drawn from Vallejo’s own public reporting: the annual number of external complaints against officers

and the annual number of department-initiated investigations. These measures are not part of the causal design, and I do not treat them as stand-alone proof. Their value is interpretive. If they move in directions consistent with increased internal monitoring and reduced externally perceived misconduct after the insurance transition, they strengthen the plausibility of an incentive-based account.

Finally, it is important to be explicit about what this design identifies. The synthetic control estimates the effect of Vallejo’s overall institutional transition beginning in July 2018. That transition bundled at least three related changes: withdrawal from the CJPRMA, acquisition of market-based liability coverage through the EIA, and creation of a new risk-management role within city government. From a strict causal standpoint, these features are inseparable, and the design cannot isolate the independent contribution of premium changes, deductibles, insurer oversight, or municipal administrative reorganization. But that bundled nature is also substantively revealing. My main claim is that a shift to a liability regime in which costs were more salient and risk management more active altered the incentives facing the city and the police department. It need not be that one particular contract clause mattered in isolation. And, it is precisely the bundle of changes that constitute the main analysis policy-makers and scholars seek to address: how institutions, not doctrines alone, shape behavior.

5 Findings

Figure 1 summarizes the financial context of the transition. The left-hand panel shows the total payments made by the CJPRMA in response to claims from 2004 through 2023. Payments rose sharply over time, with especially notable growth in the years preceding Vallejo’s departure. The right-hand panel shows Vallejo’s own general-liability contributions. Under the pooled arrangement, those contributions remained comparatively stable, because the CJPRMA’s payroll-based formula generated only gradual increases. After the city moved to the EIA, contributions rose dramatically, more than doubling in the first year and tripling over the following five. The fiscal salience of liability therefore changed in a way that is visible even in aggregate city finances, not merely in the internal accounting of the pool.

Figure 2 presents the main empirical result. The left-hand panel shows Vallejo’s monthly violent-crime clearances over the full analysis window, August 2016 through December 2019. The visible pattern is suggestive but not decisive on its own: clearances drift downward before July 2018

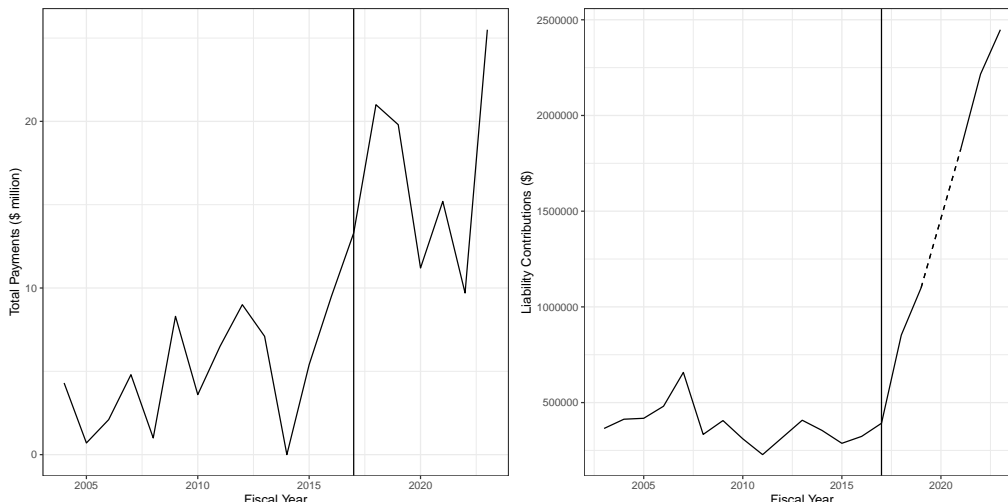


Figure 1: *Finances associated with general liability expenses.* The left-hand panel plots total annual payments by the CJPRMA. The right-hand panel plots Vallejo’s annual contribution to general liability coverage. The substantive point is that Vallejo’s move out of the pool coincided with a sharp increase in its direct fiscal exposure to liability costs.

and then flatten before turning upward. The right-hand panel compares Vallejo to the generalized synthetic control. The post-treatment difference is modest and somewhat noisy in the earliest months, but around month eight after the transition the estimated effect turns both consistently positive and statistically distinguishable from zero, stabilizing at roughly thirty additional violent-crime clearances per month through the end of the analysis window.

This magnitude is substantively large. Vallejo is not Los Angeles or another very large California city, and an increase on the order of thirty violent-crime clearances per month—against a pre-treatment baseline of roughly thirty-four—represents a near doubling of observable police output along this margin. The supporting table reinforces this interpretation. The first seven post-treatment months include substantial noise, with two estimates that are significantly negative. Beginning around month eight, however, the effects turn positive and, for most months, statistically distinguishable from zero in the bootstrap-based inference. Over months 12 through 18, the estimated monthly treatment effects fall consistently in the range of roughly thirty to forty-four additional clearances, with a mean across the stabilized window (months 8–18) of approximately twenty-nine. Table 1 reports the estimated effect of the transition month by month.

The timing of the effect matters for interpretation. If the result were driven solely by an instantaneous reporting change or a discontinuity in data definition, one would expect an immediate jump at the treatment date. Instead, the effect emerges with a lag and then grows. That pattern

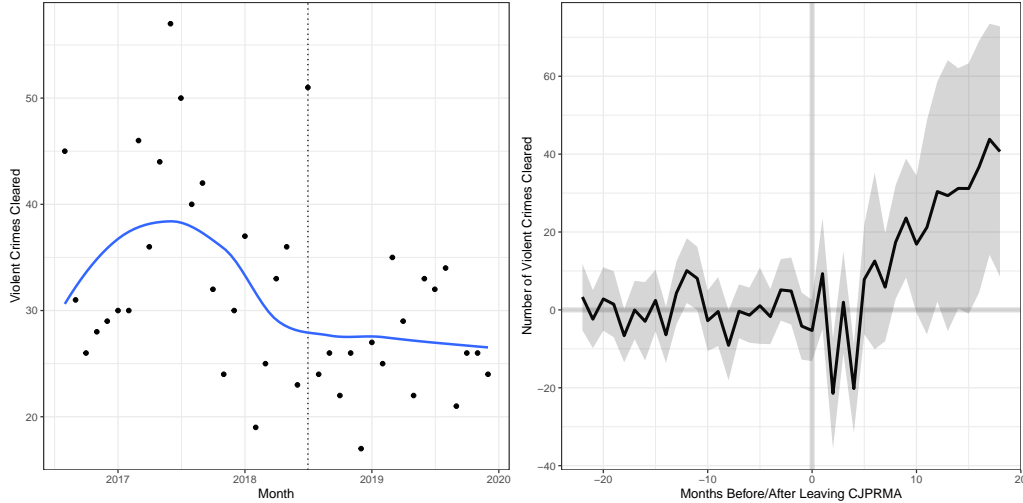


Figure 2: *Violent-crime clearances before and after Vallejo’s insurance transition.* The left-hand panel shows Vallejo’s observed monthly violent-crime clearances over the full analysis window. The right-hand panel shows the estimated treatment effect relative to the generalized synthetic control. The estimated effect turns consistently positive around month eight after the transition and stabilizes at approximately thirty additional clearances per month above the synthetic counterfactual.

is precisely what one would expect if the city’s altered liability environment induced changes in internal governance, resource allocation, supervisory attention, or departmental priorities that took time to influence case clearances. Organizational response, unlike a coding change, has to work its way through a bureaucracy before it shows up in outcomes.

Some (admittedly limited) descriptive evidence points in the same direction. According to the City of Vallejo, the number of external complaints against officers fell from 34 in 2018 to 11 in 2019. Meanwhile, department-initiated investigations—those “launched internally upon authorization of the Chief of Police”—rose from 6 to 12 over the same period. Those numbers are too sparse to sustain a separate causal analysis, and annual comparisons are inherently noisy. (Complaint data is only available for a small number of years and aggregated at the yearly level.) Still, they fit a coherent narrative. In the post-transition environment, fewer complaints emerged from outside the department while more were generated internally, consistent with a department paying closer attention to its own officers and generating fewer externally visible grievances. If so, the improvement in violent-crime clearances did not arise merely because officers became less constrained. Rather, it appears to have reflected a shift toward more focused and better-managed policing.

Months After Transition	Estimated ATT	Standard Error	95% Confidence Interval	<i>p</i>
1	9.302	7.287	(−4.981, 23.585)	0.202
2	−21.429	7.243	(−35.625, −7.232)	0.003
3	1.959	6.664	(−11.103, 15.021)	0.769
4	−20.205	5.802	(−31.577, −8.832)	0.000
5	7.829	7.189	(−6.261, 21.918)	0.276
6	12.536	11.560	(−10.121, 35.194)	0.278
7	5.837	7.085	(−8.050, 19.723)	0.410
8	17.383	7.451	(2.779, 31.987)	0.020
9	23.586	7.785	(8.328, 38.844)	0.002
10	16.880	8.982	(−0.724, 34.484)	0.060
11	21.199	14.030	(−6.300, 48.698)	0.131
12	30.384	14.414	(2.132, 58.635)	0.035
13	29.338	17.748	(−5.448, 64.124)	0.098
14	31.196	15.750	(0.327, 62.065)	0.048
15	31.168	16.404	(−0.985, 63.320)	0.057
16	36.754	16.492	(4.431, 69.077)	0.026
17	43.809	15.140	(14.136, 73.482)	0.004
18	40.673	16.391	(8.547, 72.799)	0.013

Table 1: Synthetic control estimates of the effect of the transition in insurance in Vallejo on the number of violent crimes cleared.

5.1 Placebo tests

The main estimate suggests that Vallejo’s violent-crime clearances rose substantially after July 2018 relative to a generalized synthetic control. Because the design rests on a single treated unit, however, it is important to ask whether the estimated effect reflects a genuine treatment response or an artifact of the synthetic-control procedure applied to a noisy agency-month panel. I address that question with two standard placebo tests.

The first asks whether the same design produces similarly large estimated effects when applied to California agencies that did not experience a change in liability arrangements. I restrict the placebo pool to the 248 agencies with mean monthly violent-crime clearances between five and two hundred over the 2016–2019 window, a range that brackets Vallejo’s pre-treatment baseline of roughly thirty-four clearances per month. For each of these agencies, I re-estimate the generalized synthetic control design treating that agency as if it had transitioned to market-based liability coverage on July 1, 2018. This procedure generates a reference distribution of estimated average treatment effects produced by the same estimator on units that received no treatment.

Figure 3 presents the results. The left-hand panel plots the estimated ATT path for each of

the 248 placebo agencies in grey, with Vallejo overlaid in red. Vallejo tracks inside the placebo cloud during the pre-treatment period and then separates sharply upward in the months after July 2018, rising above the mass of placebo paths. The right-hand panel shows the distribution of mean post-treatment ATTs over months eight through eighteen—the window in which the main effect stabilizes. The placebo distribution is centered near zero (mean = 0.49, standard deviation = 9.45), indicating that the estimator does not systematically produce large spurious effects in this panel. Vallejo’s mean post-treatment ATT of 30.50 clearances per month lies in the extreme right tail of that distribution: only one of the 248 placebo agencies produce a larger estimated effect, yielding an empirical one-sided p -value of 0.008 and a two-sided p -value of 0.016. (the one city with a larger ATT is Fresno, California, which was undergoing a massive crime-reduction effort in 2018 and 2019.) Expressed on the placebo scale, the Vallejo effect is approximately 3.2 standard deviations above zero.

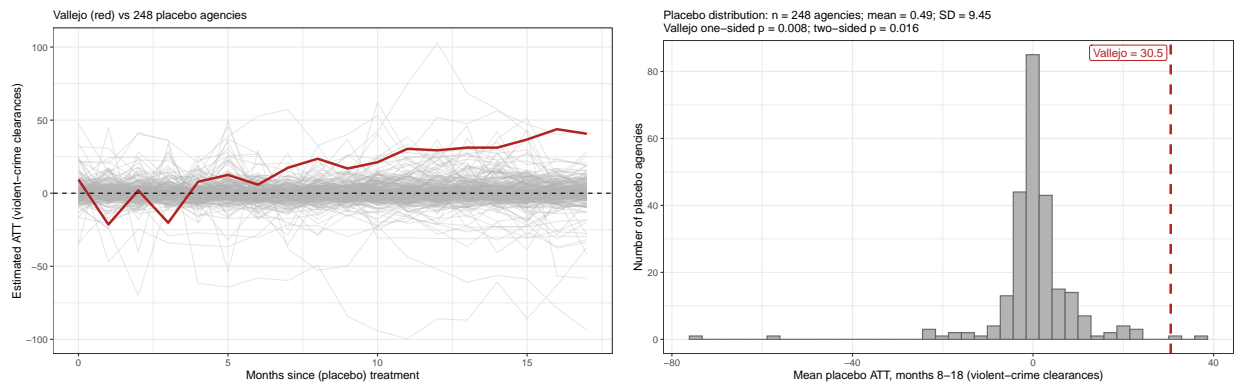


Figure 3: *In-space placebo test*. The left-hand panel plots the estimated ATT path for Vallejo (red) against paths from 248 placebo agencies (grey) treated as if they had transitioned to market-based liability coverage in July 2018. The right-hand panel shows the distribution of mean placebo ATTs averaged over months 8–18, with Vallejo’s value marked by the vertical dashed line. Only one of the 248 placebo agencies produce a larger estimated effect than Vallejo.

The second test restricts attention to data preceding the real treatment date and places a fake treatment on July 1, 2017. Using data from July 2014 through June 2018—all prior to the real intervention—I re-estimate the generalized synthetic control and inspect the estimated ATT path. The results are broadly consistent with the design, with an important nuance. Over the thirty-six months preceding the fake treatment, the estimated monthly ATTs bounce around zero without systematic drift. In the eleven months between the fake treatment date and the real one, however, the estimates trend negative, with an average ATT of -10.7 clearances per month (bootstrap 95% confidence interval: $[-17.8, -3.7]$; $p = 0.003$) and four of the eleven months individually

significant at the 5% level. This pattern indicates that Vallejo’s clearances had drifted below its synthetic counterfactual during the year immediately preceding the real transition—plausibly reflecting the same accumulating organizational dysfunction that drove the city’s extraordinary 2016–2017 liability costs and its eventual exit from the CJPRMA. Because this pre-trend runs in the opposite direction of the post-treatment effect, it cannot explain the main result. If anything, netting out the pre-trend implies a break at July 2018 on the order of +40 rather than +30 clearances per month, which would make the estimate reported above a conservative one.

Taken together, the two placebo tests support the inferential logic of the design. The in-space placebo demonstrates that the estimated Vallejo effect is an extreme outcome relative to a reference distribution built from comparable untreated agencies subjected to the same estimator. The in-time placebo indicates that whatever residual pre-treatment divergence exists works against the main finding rather than for it. A single-case design cannot rule out every conceivable alternative explanation, but these checks substantially reduce the concern that the main estimate is an artifact of the estimator or of pre-existing trends.

6 Interpretation, Mechanisms, and Alternatives

The central empirical pattern is clear: Vallejo’s observed trajectory after July 2018 diverged upward from the synthetic counterfactual. The harder question is what that divergence means. This section develops the interpretation I find most plausible, considers a set of alternative explanations, and addresses the limits of what the evidence can establish.

6.1 From liability shock to organizational response

The most straightforward interpretation is that Vallejo’s move from pooled risk sharing to a more market-sensitive liability arrangement created stronger incentives for city officials and police leaders to reduce risk and improve performance. Under the CJPRMA, Vallejo’s contributions were weakly linked to its own claims experience; under the EIA arrangement, direct costs were both higher and more salient. That shift need not have changed line officers’ incentives directly. Instead, it likely changed the incentives of those with authority over policy, supervision, and departmental priorities. When liability becomes an acute municipal budget issue, city management has greater reason to insist on reforms that reduce misconduct and improve the quality of policing, and the police department itself has more reason to anticipate that pressure and move preemptively.

How, exactly, might that logic produce higher violent-crime clearances? One possibility is reallocation. A department previously devoting time to low-value or risky enforcement could redirect effort toward solving serious offenses, and Vallejo had long faced precisely the allegation that its officers concentrated too heavily on low-level, aggressive enforcement. A second possibility is managerial tightening: better supervision, documentation, and review may improve case quality and thereby raise the probability that violent crimes are successfully cleared. A third is selection and internal discipline. If the department increased scrutiny of officer behavior, reduced tolerance for problematic conduct, or imposed stronger internal controls, that may have improved organizational performance more broadly.

These possibilities are not mutually exclusive. Indeed, the salient feature of the insurance account is that it works through a cluster of managerial pressures rather than a single narrow channel. Insurers can change price, but they can also catalyze administrative changes inside the insured entity. The creation of a dedicated risk manager in Vallejo is consistent with precisely that kind of response, and the concurrence of improved clearances and reduced external complaints suggests that several of these mechanisms were operating together.

6.2 Could the effect reflect worsening crime rather than better policing?

Because the outcome is the number of violent crimes cleared rather than the formal clearance *rate*, one concern is that a rise in crime itself could mechanically produce more clearances. That is a real conceptual issue. If the number of violent incidents rose sharply after treatment, some increase in clearances might simply mirror increased case volume. To address this possibility, consider Figure 4, which reports the total number of reported violent crimes each month before and after the Vallejo transition. Here, we see no evidence of an upturn in violent crime, save perhaps the last two months in the data. However, I also re-estimate the synthetic control using the number of violent crimes as the outcome. Table 2 reports the results, where we see the point estimates are consistently small (far smaller than the number of estimated additional crimes solved) and subject to large uncertainty. Thus, we see no reason to believe that the number of crimes being cleared is due to an increase in the number of crimes committed.

More importantly, the interpretation offered here does not depend on pretending that clearances are a perfect behavioral statistic. It is simply that relative to the synthetic control, Vallejo's observed ability to produce violent-crime clearances improved after the insurance transition. That change is noteworthy in its own right, especially given that the public concern surrounding Vallejo

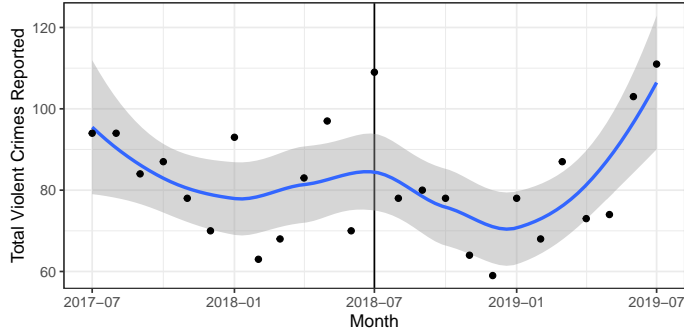


Figure 4: Reported violent crimes in Vallejo, CA, each month, July 2017 through June 2019

had centered on serious departmental dysfunction rather than exceptionally strong performance.

Months After Transition	Estimated ATT	Standard Error	95% Confidence Interval	p
1	10.58	5.85	(-0.88, 22.04)	0.07
2	-16.32	8.26	(-32.50, -0.13)	0.05
3	2.28	6.79	(-11.03, 15.59)	0.74
4	-7.96	6.00	(-19.72, 3.81)	0.19
5	-3.43	7.35	(-17.84, 10.98)	0.64
6	-11.52	9.62	(-30.37, 7.33)	0.23
7	12.21	10.23	(-7.83, 32.26)	0.23
8	15.22	9.50	(-3.41, 33.85)	0.11
9	18.57	10.67	(-2.33, 39.48)	0.08
10	-3.00	10.49	(-23.55, 17.56)	0.78
11	-4.87	15.59	(-35.43, 25.69)	0.75
12	26.33	17.33	(-7.64, 60.30)	0.13

Table 2: Synthetic control estimates of the effect of the transition in insurance in Vallejo on the number of reported violent crimes.

Still, future work should push further on this dimension. Richer analyses could model reported violent crime and clearances jointly, examine formal clearance rates directly where feasible, or incorporate additional operational outcomes such as arrests for violent offenses, use-of-force incidents, or victimization surveys. (For example, we do not know whether the change in the number of clearances is due to a shift of effort *within* the category of violent crime towards easier-to-solve incidents.) The value of the present analysis lies in showing that a liability-regime change corresponded with a substantial upward shift in an important policing outcome. The next step is to unpack more fully which underlying margins moved.

6.3 Could other contemporaneous reforms be responsible?

A further concern is that the treatment bundles several contemporaneous municipal changes. As noted above, that is true: Vallejo’s departure from the CJPRMA, entry into EIA coverage, and creation of a risk-management role all occurred in close temporal proximity. If one wanted to isolate the causal effect of, say, the deductible alone, this design could not do so. Yet the bundled character of the treatment does not undermine the central contribution of the article. In practice, liability institutions operate as bundles. How a city finances risk, the degree to which outside actors monitor that risk, and the internal administrative structures created in response are all part of the institutional effect of liability. That broader institutional package is often the more substantively important object of study, because it is the package—not any single contract term—that municipalities and departments actually face.

Could unrelated municipal reforms happening at the same time have driven the result? It is always possible, and single-case designs cannot eliminate every rival story. But several features of the evidence make a pure coincidence account less persuasive. First, the transition was directly motivated by extraordinary liability associated with the police department; it did not arise out of an unrelated municipal agenda. Second, the supplementary evidence on complaints and internal investigations points toward changes in internal governance rather than toward an unrelated shift in local crime conditions. Third, the lagged treatment effect is what one would expect if administrative implementation takes time, and it is notably harder to reconcile with a mechanical or coincidental account that would predict a sharp discontinuity.

A related alternative explanation is that the observed increase could reflect changes in prosecutorial screening or case processing outside the police department. Because the clearance measure requires not merely an arrest but charging and turnover for prosecution, some portion of the outcome could in principle be influenced by prosecutorial behavior. That possibility should be taken seriously, but it does not obviously undermine the insurance account. If the post-2018 liability environment induced police to produce better-documented, better-supervised, or otherwise stronger cases, then a higher share of cases might be processed successfully by prosecutors as a downstream consequence of improved policing. Interaction with prosecutors, in that case, is not an alternative mechanism but part of the pathway through which organizational improvement manifests in the data (e.g., Clark 2026).

There is also the possibility of symbolic or reputational response. Vallejo’s exit from the pool and

the public visibility of its liability problem may themselves have generated pressure independent of contractual insurance features. Again, the article's claim is best understood at the level of institutional packages rather than microscopic contract terms. A municipality's liability regime includes how claims are priced, how risk is publicly understood, and how city officials respond once that risk becomes impossible to ignore. The reputational dimension of the transition, if it is present, is therefore complementary to, rather than inconsistent with, the broader account advanced here.

6.4 Why might market-based insurance outperform pooled risk sharing?

The broader theoretical implication is not that all private insurance is normatively or practically superior to public pooling. Pools exist for good reasons. They reduce volatility, protect smaller municipalities, and can lower administrative costs. But they may also soften experience rating and thereby weaken the incentives created by liability. Vallejo illustrates how that tradeoff can matter. So long as the city remained within a broad pool and paid on a relatively blunt formula, the marginal budgetary cost of police-generated liabilities was muted, and neither the city nor the department had strong local reason to internalize it. Once Vallejo confronted a much larger retention and a more exposed insurance arrangement, those costs became difficult to ignore, and the institutional response followed.

In ordinary insurance markets, scholars often worry about moral hazard: insurance reduces the insured's incentive to take care because losses are borne by the insurer. The Vallejo case points to a more nuanced lesson. Insurance can generate moral hazard when risk is detached from price, but it can also mitigate moral hazard when pricing, deductibles, and oversight reconnect conduct to cost. As a result, to understand how insurance and liability shape behavior, we need to focus on *how* entities are insured.

6.5 Durability and adaptation

One final interpretive issue concerns durability. Even if the 2018 transition produced real improvement, it does not follow that insurance can permanently transform police culture. Departments may adapt strategically to insurer demands, comply only superficially, or improve along one dimension while continuing harmful practices along another. Vallejo itself remained the subject of controversy in later years, and understanding whether those subsequent controversies were more severe or less severe than they would have been absent the insurance transition is an important question for future research. Those facts should make us cautious about triumphalist claims. The

findings here show that liability institutions can matter. They do not show that insurance is a complete solution to the many problems of policing.

7 Implications for Police Accountability

Municipal liability is usually studied through doctrine, litigation, settlement patterns, or the distribution of legal accountability. Those are all important. But legal exposure is experienced by organizations through financial and administrative institutions, and insurance is one such institution. By showing that a change in insurance arrangements can coincide with a large change in police output, the Vallejo case highlights a point that is often overlooked: the behavioral effect of law depends not only on formal legal rules but on the organizational technologies that translate those rules into incentives. This observation matters beyond policing. Public entities routinely manage legal risk through insurance, self-insurance, and pooled coverage. If those arrangements differ systematically in how strongly they connect behavior to price, then the same formal liability regime can generate very different practical effects across jurisdictions. What this analysis shows us, in part, is that law can in fact shape human behavior, a question at the heart of myriad debates. Part of the answer, we see here, lies in the institutional intermediaries that govern how legal costs are financed.

The findings also matter for debates about police accountability specifically. Discussions of reform often assume a dichotomy between democratic oversight and police autonomy. Insurance complicates that picture. It is neither purely public nor purely private. It is a hybrid form of governance in which a legal obligation to compensate victims creates incentives for a third party to monitor, advise, and discipline a public bureaucracy. That hybrid nature may be especially useful where direct political oversight is weak, contested, or inconsistent. This does not mean that insurance should replace democratic governance. Police departments make distributive and normative choices that cannot be reduced to actuarial risk. Communities care not only about avoiding costly misconduct, but also about fairness, legitimacy, civil rights, and the substantive priorities of law enforcement. An insurer can pressure a city to reduce conduct that generates payouts; it cannot by itself determine what equitable or democratically responsive policing should look like. For that reason, the appropriate way to understand insurance is as a complement to, not a substitute for, public oversight.

The case also carries several concrete policy implications. The first concerns the design of risk-

pooling arrangements themselves. States and municipalities should pay closer attention to whether their existing pools inadvertently blunt accountability. The question is not whether pooling should be abandoned wholesale—it should not be—but whether contribution formulas and retention structures can be designed to preserve more member-specific incentives. A pool that spreads catastrophic risk while still incorporating stronger claims-based pricing is likely to generate better behavior than one that treats high-risk and low-risk members too similarly. California’s own experience with the CJPRMA illustrates the cost of that failure: a pool that did not price Vallejo’s behavior properly ultimately pushed the city out entirely and absorbed substantial losses along the way.

A second implication concerns transparency. Information about municipal coverage terms, claims histories, deductibles, and premium changes is at present scattered across local financial reports and often difficult to assemble. That opacity hinders both academic study and public oversight. Standardized disclosure of municipal liability coverage and claims experience would make it possible to evaluate whether insurance-based incentives are functioning as intended, and it would align naturally with the broader movement toward police transparency in California and elsewhere. States currently moving in the opposite direction—shielding police records and related data from public scrutiny (Pitcher 2025)—risk foreclosing precisely the evidence needed to evaluate whether the reforms they favor are working.

A third implication concerns individualized forms of insurance-based accountability. The Vallejo case lends modest support to proposals exploring professional liability insurance for officers themselves (Ramirez et al. 2018, Ramirez and Pinto 2020). Where financial consequences can be more closely tied to behavior, liability becomes a more effective governance tool. (Of course, that does not mean that officer-level insurance is obviously desirable or administratively simple.) Officer-level proposals would require careful design to avoid perverse consequences—including, for example, the risk that departments shoulder the costs themselves and thereby restore the same moral-hazard problem at a different level. Even so, they deserve serious empirical and normative evaluation, and a pilot program in a willing jurisdiction would generate useful evidence.

A fourth implication concerns the data infrastructure on which any of this work depends. The analysis here rests heavily on violent-crime clearances because they are among the few metrics available consistently across a wide set of jurisdictions. Richer public data on complaints, use of force, discipline, training, and case processing would allow researchers to trace much more precisely how liability institutions shape policing. California has moved toward greater transparency in some respects, but much more remains to be done, and other states have moved in the opposite direction.

Investment in policing data infrastructure is, in this sense, not merely a technical matter or driven by a concern about transparency. Instead, data infrastructure is a precondition for credible reform. Future work should examine variation across insurance arrangements in multiple states, identify the specific contractual and managerial tools insurers use, and study whether insurance-driven reform is durable or merely temporary. Some of the most important questions are comparative: under what conditions do insurers exert meaningful leverage, and when are they too weak, too politically constrained, or too dependent on municipal business to discipline departments effectively? A richer empirical record could substantially improve both scholarship and policy design.

8 Conclusion

This article began with a basic question: can insurance serve as a mechanism of police oversight? The evidence from Vallejo suggests that it can. When the city was forced to leave a pooled risk-sharing arrangement and obtain more market-based liability coverage, the police department's violent-crime clearances increased substantially relative to a synthetic counterfactual. Supplementary descriptive evidence on complaints and internal investigations points in the same direction, suggesting that the transition may have sharpened internal monitoring as well as external fiscal pressure. The broader lesson is that liability institutions do most of their work upstream of any individual claim, by shaping the supervision, training, and managerial attention that determine whether misconduct happens in the first place. Whether liability has that effect depends heavily on how risk is financed and managed. Pooled arrangements can diffuse responsibility; more market-sensitive arrangements can make legal risk more salient and more difficult for local governments to ignore. In that sense, insurance is part of the institutional machinery through which public law is translated into organizational behavior, not just peripheral to the study of police accountability.

That conclusion should be understood with appropriate caution. Vallejo is a single case, the outcome examined here captures only one dimension of policing, and insurance is no substitute for democratic control over public institutions. Even so, the findings indicate that private liability arrangements deserve a more central place in conversations about how law can discipline public power. For scholars of law and society, public law, and empirical legal studies, the case illustrates how seemingly technical features of risk financing can shape the conduct of one of the state's most consequential bureaucracies.

Data and Code Availability

This paper relies on a combination of publicly available data and originally assembled data. Upon publication, all data and code necessary to reproduce the results will be made publicly available at the Harvard Dataverse.

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