

# Next-Generation Policing Research: Three Propositions

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**D**ecades of social scientific labor have been poured into questions about policing and public safety, including a focus on uncovering the myriad racial disparities in policing and throughout the criminal system. Conversely, relatively little research has sought to disentangle policing from public safety. For example, previous research often failed to look at policing outcomes other than crime rates that are important for community well-being, or on how non-police alternatives could contribute to public safety. These research gaps have generated a bias in the research literature in favor of responding to concerns about public safety with status quo policing, perhaps with a few tweaks to existing training and practices.

But policing and public safety are not one and the same. Instead of starting from the presumption that more or better policing is the only route to public safety, many researchers are wondering whether organized community efforts could work better than traditional policing in achieving the goals of building public safety and improving community outcomes.

As one example of an alternative, Devone Boggan started the Peacemaker Fellowship in Richmond, California, as part of his work for the city's Office of Neighborhood Safety in 2009. In 2016, the Peacemaker Fellowship became Advance Peace, an organization that, among other things, runs an extensive mentorship and personal development program that engages individuals who are most at risk of engaging in interpersonal gun violence. The program offers these individuals 18 months of internships, travel opportunities, support navigating social

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services, training and support for life goal-setting, and more and pays them for their participation. While the program engages in programmatic support for individual sustainability, some reports have characterized the work of Advance Peace and similar organizations as a form of friendship, highlighting its deep relational components. For example, Chabria (2017) reported that Boggan “has befriended 84 men considered most likely to kill with a gun” in Richmond (see also Bell 2019). Advance Peace also directly intervenes on episodes that could spiral into violence. The Advance Peace model has established programs elsewhere in California, such as Sacramento and Stockton and is gaining a foothold in Fresno (Hoggard 2020). However, Advance Peace does not work with law enforcement and does not hold building community trust in police as one of its goals. This promising program, which seeks to achieve safety and well-being by centering community, friendship, and support—not policing—has received scant attention from academic researchers.

As another example, Precious Blood Ministry of Reconciliation, a Chicago-based restorative justice organization, similarly builds relationships with young people, with additional focus on young people who have committed harm or who have had some relationship with the juvenile justice system. As an alternative to carceral approaches, some participants have received “Saturday Sanctions,” in which the participant is engaged in self- and community-development programs (VanNatta and Kaba 2013). Today, the organization runs a battery of restorative programs, with focuses on several populations at particular risk of harm. The organization tells its own stories of impact by focusing on individual participants’ experiences (<https://www.pbmr.org/gallery#OurImpactStories>).

In this essay, I offer three propositions for economists and other quantitative social scientists who aim toward a more accurate, comprehensive, and better contextualized research agenda for policing, public safety, and racial inequity: 1) the new public safety research must seek to probe the effects of policing on a range of outcomes, including education, health, community flourishing, and subjective well-being; 2) the new public safety research should evaluate policing alternatives, including community-based strategies of crime deterrence and accountability for harm; and 3) the new public safety research should reach beyond the study of racial disparities to investigate the effects of racism on crime, harm, and disparity in the criminal legal system. In short, the next-generation research agenda on policing and public safety must respond to the deficiencies and limited focus of past research and chart a less parochial course.

**Proposition 1: The new public safety research must seek to probe the effects of policing on a range of outcomes, including education, health, community flourishing, and subjective well-being.**

The traditional datasets used by economists fail to measure many of the collateral costs of policing. The bulk of research on policing and public safety has focused on crime rates (especially felonies), fear of crime, and internal police department

dynamics (stops, arrests, clearance rates, response times, expenditures, and police force size) (Chalfin and McCrary 2017; Raskolnikov 2020). Because of the multiple functions of police in society, and because achieving safety requires more than an exclusive focus on lowering crime rates and making police departments more efficient, scholarship focused on policing and public safety should measure a broader set of outcomes than are usual in this field. The next generation of police research should examine the full range of relationships between policing and other aspects of community life and social structure—including effects on health, segregation, education, urban development, municipal budgets, wealth consolidation, labor relations, liability insurance, and other metrics. Police deterrence of crime, even if successful, may have collateral costs for other metrics of concern.

Some quantitative sociologists have tackled these outcomes, with important implications for conceptual understandings of the consequences of policing beyond crime levels. For example, Legewie and Fagan (2019) examined how aggressive policing, specifically “Operation Impact” in New York City, affected the educational performance of Black and Latinx youth between 2003 and 2012. Legewie and Fagan find a causal relationship between harsh policing and lower educational performance. They use variation in the timing of police surges across neighborhoods and a difference-in-differences approach to show that exposure to surges of police hyper-presence in New York City neighborhoods leads to lower test scores among Black male adolescents, with increasing effect sizes by age. Building from these and other empirical insights, Justice (2021) recently theorized that the effects of policing and incarceration on the educational opportunities and experiences of American children create a form of “hobbling,” or “a social process by which the massification of policing and incarceration systematically compromises the ability of target demographics of American children to enjoy their rights to a free and appropriate public education” (3.1).

American policing and other criminal system researchers have traditionally relied upon five national databases of large-scale secondary data in their analyses of criminal system outcomes: Uniform Crime Reports (UCR), National Incident-Based Reporting System (NIBRS), National Crime Victimization Survey (NCVS), Law Enforcement Management and Administrative Statistics (LEMAS), and Law Enforcement Officers Killed and Assaulted (LEOKA). Researchers have offered a number of critiques of these datasets, mostly focusing on aspects of underreporting and undercounting. For example, UCR offers a wealth of information about reported crime in the United States; however, it is plagued by a serious missing data problem because many police departments do not comply with requests to submit their crime reports to the Federal Bureau of Investigation. This risk seems greatest among police departments in locations where crime might be higher than average (Lynch and Jarvis 2008; Boylan 2019). Similarly, researchers have criticized data from the NIBRS for unrepresentativeness and potential underestimation of crime rates (McCormack, Pattavina, and Tracy 2017). The NCVS, which helpfully allows a deep dive on experiences of victimization to some degree—including otherwise unreported crime—also comes with important

limitations such as declining response rates, the omission of crimes against youth under age 11, underestimation of serial victimization because of rules that sometimes collapse multiple victimizations into a single incident, likely underestimation of rape and sexual assault, and other issues.

These narrow ranges of research and data flow from researchers' presumption that the central function of policing is to reduce and respond to crime. However, decades of research have also shown that policing has played many other latent roles in the American social order. For example, policing has functioned to confine and control socially disfavored groups, both in early American history and in the present (for example, Harcourt 2007; Wacquant 2009), and to protect the property of wealthy groups, including tamping down labor movements seeking fair wages and hours (Fisk and Richardson 2017; Levin 2020). In my own scholarship, I have written about how police policies and practices function to shape families' residential preferences (Bell 2020a) and in this way, reinforce racial residential segregation across cities and suburbs (Bell 2020b; see also Fagan and Ash 2017; Gordon 2020; Kurwa 2020).

Other scholars have extended a similar lens to the mental and physical health outcomes of policing, both for those who directly bear a criminalized status and indirectly for members of their families and communities (Asad and Clair 2018). For example, Sewell and colleagues have found numerous associations between harsh, surveillance-style policing and negative health outcomes. They find associations between chronic stop-and-frisk at neighborhood levels and emergency room use (Kerrison and Sewell 2020); between exposure to lethal police violence and chronic illness like high blood pressure and diabetes (Sewell et al. 2021); and between living in predominantly Black neighborhoods with heavy policing or predominantly White neighborhoods with racially targeted heavy policing, and various negative health outcomes (Sewell 2017). Other examples have emerged, especially in the most recent decade: for example, Geller et al. (2014) provide evidence on associations in survey data of young men in New York City between symptoms of trauma and anxiety and the number of police stops these men had experienced, together with their perceptions of the intrusiveness and fairness of these stops.

Researchers can also turn to new sources of data to investigate community flourishing, community power, and individual well-being. A growing body of scholarship, primarily in positive psychology, aims to develop metrics for individual well-being and community flourishing. For example, the Human Flourishing Program at Harvard University's Institute for Quantitative Social Science has developed a twelve-question survey, along with some additional context-specific tools, for measuring individual flourishing (Harvard IQSS 2021). The twelve questions are intended to reach six central domains of human flourishing: happiness or satisfaction, health (mental and physical), an individual's sense of purpose, an individual's character and virtue, the presence and nature of close interpersonal relationships, and financial/material stability (VanderWeele 2017, p. 8149). VanderWeele (2019), the primary theorist of this particular human-flourishing

measure, has also developed potential measures of community flourishing, building from six domains: individual flourishing (to be measured using the aforementioned twelve items), positive relationships, good leadership, healthy community practices, community satisfaction, and a sense of community mission. He argues that the other five domains could be measured through an additional 20-item questionnaire (VanderWeele 2019, pp. 258–60). Researchers aiming to recognize the latent functions of policing through their research practices might incorporate some of these individual- and community-flourishing measures into the evaluation of policing, police reforms, and public safety-oriented state and community interventions.

Other scholars are also developing frameworks and tools that form the basis of an alternative set of metrics for policing and public safety outcomes. For example, Sampson (2012) reports evidence from the Project on Human Development in Chicago Neighborhoods, which gathered data through surveys, interviews, data on neighborhood physical conditions using video technology, and responses to events, and combined it with other available data on health, crime, housing, violence, and population. Sampson draws from that data to develop measurable concepts such as collective efficacy, legal/moral cynicism, and other key outcomes that should be of central concern in studies of policing. Cohen et al. (1998) discuss how to use surveys like the Stress-Related Growth Scale and the Posttraumatic Growth Inventory to measure “thriving,” by which they mean the response to a specific stressful event. They also discuss how to validate self-reported data and how to carry out such assessments for groups and communities. Small (2009, 2017) offers examples of how to estimate the size and strength of social support networks and other measures of social capital, important potential outcomes for studies that might explore how policing affects the expansiveness and strength of social bonds. A variety of authors have looked at relationships between policing and measures of voting or civic engagement. For example, Drakulich et al. (2017) find that being stopped by police tends to increase voting, but experience of incarceration tends to decrease it. Walker (2020) combined data from different national surveys to argue that the experience of having a loved one or family member who has contact with the criminal system can increase voting, mobilizing people to use their political voice to change political and legal conditions. Laniyonu (2019) finds that concentrated policing across areas of New York City “was associated with reductions in voter turnout in the 2006 and 2010 midterm elections, it was associated with higher rates of turnout in the 2008 presidential election . . .” This insight offers suggestive evidence of a combination of chilling effects and mobilizing effects on voters that varied depending on the role of policing in local political debates.

It is important for researchers to embark on this expanded body of quantitative work with a rich understanding of the theories that would link policing to these outcomes in other domains. As one example, it may not make sense to expect significant changes or variation in civic engagement or collective efficacy related to a single intervention in a year or less, given the depth of legal estrangement and marginality in communities studied.

**Proposition 2: The new public safety research should evaluate policing alternatives, such as community-based strategies of crime deterrence and accountability for harm.**

Economists, like other social scientists, rarely research community-based crime deterrence efforts. This empirical neglect biases the evidence base that informs policy debates. Past research on public safety across social science disciplines has overwhelmingly focused on the capacity (or lack thereof) of police to deter crime and reduce crime rates. We are now a generation into a body of research that has come to treat the capacity and necessity of police to prevent crime as a truism (Meares 2014; Sharkey 2018). Indeed, the question of whether police can and do prevent crime is so well-worn that it is no longer treated as a research question but as a fact. That fact has given birth to an array of investments into policing that focuses on deterrence rather than response, including a multitude of for-profit companies that provide surveillance and predictive algorithms that assist police in crime prevention (Brayne 2020; Ferguson 2017). It is worth pausing a moment to reflect on how we have arrived at this point.

For much of the 20th century, quantitative research failed to find a connection between police and crime prevention (Meares 2015). In the early to mid-1990s, scholars invested serious energy debunking “the myth of the police,” arguing that “[t]he police do not prevent crime . . . Experts know it, the police know it, but the public does not know it” (Bayley 1994, p. 3). Much of the 20th-century policing research didn’t examine policing strategies aimed at reducing crime before it occurred, but policing strategies in response to crime that had already occurred. For example, in a nod to the baseline 20th century project of professionalizing police forces and solidifying an occupational identity and purpose among police officers, much policing research examined the structure and efficiency of departments (for example, Reiss 1992). Research on these topics continues in the 21st century (for example, Cihan, Zhang, and Hoover 2012; Vidal and Kirchmaier 2018). Yet now, an expansive body of research on algorithmic policing and crime deterrence suggests, for example, that targeted policing in more condensed “hot spots” is an effective preventive policing strategy (for example, Braga, Papachristos, and Hureau 2014). More generally, preventive policing, once deemed impossible, has become a universally accepted aspiration of police departments.

Since the late 1990s and early 2000s, in the wake of the “Great Crime Decline” of the 1990s, scholars have been trying to sort out how substantial and central a role policing played in that drop in crime. In this journal, Levitt (2004) gives his evaluation of ten possible factors, claiming that increased numbers of police played a determinative role in the 1990s decline in crime, but better policing strategies did not. Some book-length treatments of reasons behind the Great Crime Decline include Blumstein and Wallman (2000), Roeder, Eisen, and Bowling (2015), and Sharkey (2018). They all offer considerable nuance but also acknowledge ongoing uncertainty about the precise reasons for the crime decline. A number of studies provide suggestive evidence that various approaches to policing were among a

number of factors in the 1990s crime decline, along with others like the rise and fall of the crack epidemic and the density of community organizations. By the early 2000s, some economists advocated uncritically for putting more police on streets, based on their belief in a straightforward causal relationship between the flooding of cities with more police on the streets in the 1990s and reduced levels of crime (for example, Ludwig and Donohue and 2007). They neglected the racialized costs of this strategy, as they did not account for outcomes like broken community bonds, loss of interpersonal and institutional trust, or other social and political costs of heavy, concentrated policing.

To be sure, it is plausible that during the 1990s crime decline, increases to police funding and new methodologies of policing contributed to reduced crime—while also causing myriad collateral harms. Moreover, as sociologist Patrick Sharkey (2018) has shown, local community organization presence also had a causal relationship with those declines during the 1990s and 2000s in cities throughout the United States (Sharkey 2018; see also Sharkey, Torrats-Espinosa, and Takyar 2017). Sharkey's work uses the formation of nonprofits focused in other areas, like arts and humanities, medical research, and environmental protection as an instrumental variable for the formation of nonprofits related to violence, crime, and community-building, thus allowing a causal estimate. Surprisingly little social scientific research in sociology, criminology, or economics has similarly focused on the role of community-based organizations in crime reduction or community-based alternatives to policing and prisons. Part of the reason is that there has never been a robust, well-funded, and consistently supported network of community organizations that engage in violence reduction and public safety efforts, which might leave the impression that policing must be the primary way to reduce violence, while blinding researchers and analysts to the capacity for community organizations to play a more central role if they were better and more consistently funded, supported, and evaluated.

The primary alternative to policing that has received thorough attention from researchers are violence interruption programs, such as CureViolence, CeaseFire, Safe Streets, Save Our Streets, and hundreds of others, which enlist the work of trusted community members to interrupt encounters that could become violent without their support and negotiation. The basic idea of this violence prevention approach is to treat violence before it occurs by detecting and mediating conflict before it escalates and to do this with community members who have a type of expertise, skill, and credibility that police officers lack. These violence interruption programs have shown an array of promising results for reducing violence over more than a decade of research (Abt 2019; Braga and Pierce 2005; Milam et al. 2013; Slutkin et al. 2015; Webster et al. 2013; Whitehill et al. 2014).

But there are a number of other community-based programs or alternatives to traditional policing that remain largely unstudied, even though some of them are becoming models for other jurisdictions across the nation. For example, CAHOOTS (Crisis Assistance Helping Out on the Street) started in Eugene, Oregon, to send two-person clinical response teams to aid people in mental health crisis, without relying on armed police officers. Although the program has existed for more than

three decades, in summer 2020 it gained national attention and became the model for numerous pilot programs—in San Francisco, Denver, Rochester, Toronto, and more. Eugene’s CAHOOTS program is funded and overseen by the police department, but some other emerging programs are funded and managed separately from police. Despite its long duration—even longer than the violence interruption programs mentioned above—CAHOOTS has never been rigorously evaluated. There are also rich debates over, among other things, how to measure its diversion rate (Gerety 2020). There is a dearth of information and modeling of police-free crisis response, though one hopes that will change as more cities embrace these approaches.

Similarly, little rigorous research examines the effects of interventions targeted directly at conditions that produce crime. One example is Advance Peace, described in the introduction, which aims to tackle the lack of career opportunities and economic resources that drive participation in crime. Chicago’s Rapid Employment and Development Initiative (READI) program offers one promising approach for intervening in the criminogenic conditions of poverty, housing insecurity, low economic opportunity, and trauma while laying groundwork for empirical measurement of this approach. READI works with six community organizations to offer participants who are people deemed at risk of participation in violence one year of transitional employment, cognitive behavioral therapy, and professional development. READI partnered with the University of Chicago Crime Lab early and is operating as a randomized control trial to measure an array of program outcomes. While there is some research on the effectiveness of restorative and transformative justice projects that aim to respond to and heal individuals and communities after episodes of violence without using prisons and police, more is needed to help these programs be effective as they evolve and scale up (Dixon 2020; Sered 2019).

Some local governments have expanded their public safety and crime reduction efforts beyond policing as well. For example, the New York City Mayor’s Office of Criminal Justice has pioneered “NeighborhoodStat,” a community-led strategic plan to address specific social, economic, and environmental conditions affecting crime across 15 public housing developments (Pearl 2019). NeighborhoodStat has shown some potential effects on crime, though the plan and the evaluation process are still in relatively early stages (Delgado et al. 2020).<sup>1</sup> “NeighborhoodStat” is a reference to a famous data-gathering effort, CompStat, pioneered in New York City in the mid-1990s under Commissioner William Bratton, which is now used in many police departments nationwide (Weisburd et al. 2003). CompStat measures complaints about crime from the public, police arrests, and issuance of summonses. Most police departments using this approach hold weekly meetings, open to the public, where officers learn about and reflect upon the CompStat numbers as a police management strategy. Though CompStat was initially lauded (for example,

<sup>1</sup>The author has served as a paid member of an advisory committee for the evaluation of this program, which is part of the Mayor’s Action Plan for Neighborhood Safety.



O’Connell 2001), it has faced recent criticism both because of the limited scope of data collected and because it incentivizes officers to search, engage, arrest, and perhaps to distort crime statistics (Bronstein 2015; Lu, Yang, and Thomas 2020; Thomas and Wolff 2020). In addition, CompStat is run by the police department and focuses on evaluation of police. In contrast, NeighborhoodStat is run by the Mayor’s Office in collaboration with the community and aims to evaluate a complex set of alternative investments in public safety, ranging from increased outdoor lighting to exercise programs to youth employment opportunities.

Okechukwu’s (2021) research on community-based safety measures in mid-20th century Brooklyn provides one example of the potential for an expanded scope of research on community-based safety measures. Okechukwu probes archival materials and oral histories to describe four different non-police organizational strategies that Black community members in the Brooklyn neighborhoods of Bedford-Stuyvesant and Crown Heights used to produce public safety: externally focused community patrol, internally focused community patrol, building physical refuge for hyper-vulnerable populations, and “othermothering” (older women serving as caring “eyes on the street” to observe and keep communities abreast of threats) (see also Collins 2002; Jacobs 1961 [2016]). Okechukwu analyzes each strategy, exploring how well (if at all) they fit with contemporary visions of abolition of the police. This work exposes both the possibilities and limitations of community-based security measures. While Okechukwu’s work is qualitative, quantitative research could also have value for exploring other aspects of these questions, such as the relative long-term efficacy of different longstanding community-based strategies as compared with each other.

One Million Experiments, a collaborative project between two police and prison abolitionist organizations, Project Nia and Interrupting Criminalization, is collecting snapshots of community-led safety efforts across four categories—mutual aid, alternatives to calling 911 during emergencies, support (such as healing circles and financial support), and community events (One Million Experiments 2021). The project also produces a newsletter that does periodic deep dives on particular community safety projects. One Million Experiments does not seek to evaluate these projects, but to expand the imaginations and agendas of readers about projects they could attempt in their own communities.

These examples are seeds for economic research on non-carceral approaches to violence reduction and response to harm. Scholars should examine the mechanisms and characteristics of organizations that can affect crime reduction. Perhaps some types of crime are better deterred by alternative organizations than by traditional police. Perhaps increases in crime in the short term would be offset by crime reductions in the longer term. Perhaps there would be heterogeneous effects of organizations on crime and well-being depending on neighborhood and municipal characteristics. Scholars should also continue to investigate how non-carceral ecological interventions might reduce crime, building on preexisting research on greening, street lamps, and more (Doleac and Sanders 2015; Garvin, Cannuscio, and Branas 2013).

At a basic level, what sociologist Robert Sampson (2012) has termed “collective efficacy” may account for how the presence of community organization seems to produce lower violent crime rates. A community’s sense of mutual trust and informal social control over what happens in their neighborhoods can perhaps build protective factors that soften the criminogenic effects of social and economic neighborhood disadvantage. Indeed, rather than passively accepting the underlying assumption that police departments and policing techniques in more-or-less their current form are a natural and inevitable feature of a public safety agenda, researchers should investigate of the accuracy of those assumptions. If the policy goal is improved public safety along with other aspects of community flourishing more generally, community-based organizations may well have a much larger role to play.

**Proposition 3: The new public safety research should reach beyond the study of racial disparities to investigate the effects of racism on crime, harm, and disparity in the criminal legal system.**

Economists arrived relatively late to the study of racial disparities in the criminal system compared other social sciences (for example, compare Becker 1968 with Du Bois 1904; see also Bushway and Reuter 2008). Yet once economists entered the fray, they began to have great influence over the methods, assumptions, and outcomes of interest in policing and public safety research. Economists have explored racial disparities in policing along multiple dimensions in papers that have guided the way for other quantitative social scientists who attempt to isolate discrimination as a cause of racial disparity at particular junctures in the criminal system continuum (for example, Antonovics and Knight 2009; Anwar and Fang 2006; Coviello and Persico 2015; Goncalves and Mello 2021). Economists have also shed light on the effects of police officer race on racially disparate outcomes (for example, Donohue and Levitt 2001). Some research has focused instead on structural features, such as urbanity and racial demographics, to understand racial disparities in policing (Hoekstra and Sloan 2020; Ross 2015).

In some permutations, this style of research has laid groundwork for deeply consequential changes in law and policy meant to address those racial disparities: for example, it can be useful to know the details of how racial disparities in traffic stops operate to identify particular ways to reduce those disparities. However, this style of research on racial disparities—focused as it is on specific types of interactions with the criminal justice system, as reported by the criminal justice system—is routinely oblivious to the social, political, and economic context of race. Economists’ analytic approaches to studying racial disparities lead them to miss many of the institutional mechanisms through which racism arises in the criminal legal system (as discussed in this journal by Small and Pager 2020). To convey that context, sociologists sometimes use the term “structural racism,” which can be defined as “a social system in which race is a central principle of

social organization that serves to sort individuals into positions of relative advantage and disadvantage based on their racial category” (Merolla and Jackson 2019: 2; see also Gee and Ford 2011; Powell 2008).

The interpretation of quantitative results of racial disparities focusing on specific steps in the policing process has led to some contentious debates over claims that a given set of police data does or does not reveal officer racial bias. Researchers should be much more attentive to the limitations of their data, their choice of a theoretical lens through which they interpret their data, and the pressures to report results on racial disparities in a provocative, iconoclastic way.

As one example, quantitative psychologists Johnson and colleagues published an influential 2019 article in the *Proceedings of the National Academy of Sciences*, which stated (in the article’s statement of significance): “White officers are not more likely to shoot minority civilians than non-White officers” (Johnson et al. 2019, p. 15877). After criticism for making this claim based on the data and estimation strategy used (Knox and Mummolo 2020), and after a back-and-forth in letters, Johnson and colleagues issued a correction and reframed their finding in this way: “As the proportion of White officers in a fatal officer-involved shooting increased, a person fatally shot was not more likely to be of a racial minority” (Johnson et al. 2020a p. 9127). Although this correction stated their core finding in a more careful if perhaps less publicly digestible way, Johnson and colleagues ultimately retracted their article altogether because they believed that their “work has continued to be cited as providing support for the idea that there are no racial biases in fatal shootings, or policing in general” (Johnson et al. 2020b, 18130).

Perhaps the most well-known social science controversy on interpreting data on racial disparities in policing involves the research by Fryer (2019). Fryer’s research concludes, based on data from ten major cities, there are racial disparities in officers’ uses of physical force like handcuffing, pepper-spraying, and other non-lethal engagement, but there was no racial disparity in the likelihood of being shot by police. As commenters pointed out when working-paper versions of the study became available, these data were on people who had a police encounter. Thus, key aspects of racial inequality in uses of force, such as the higher rates at which Black people are stopped by the police in the first place (Knox, Lowe, and Mummolo 2020), as well as other tricky aspects of studying police interactions through data are not taken into account (Goff et al. 2016). Accordingly, the study cannot shed light on a central pathway through which racism affects policing (for example, Epp, Maynard-Moody, and Haider-Markel 2015). Fryer (2020) followed up in a June 2020 op-ed in the *Wall Street Journal*. After expressing “dismay” because many readers have misinterpreted or misappropriated this finding “as evidence that there is no racism in policing, that football players have no right to kneel during the national anthem, and that the police should shoot black people more often,” Fryer explained that from his viewpoint as an economist, his study cannot speak to the impact of racism on its outcomes: “Racism may explain the findings, but the statistical evidence doesn’t prove it. As economists, we don’t get to label unexplained racial disparities ‘racism.’”

Of course, there are better ways to address the problem of unexplained racial disparities than leaving them untheorized. Researchers can study policing outcomes through a lens that employs theoretically informed metrics for racism. Part of what it means to study racism is to draw upon substantive theories on race and the law. For example, Rojek, Rosenfeld, and Decker (2012), examined racial disparities in the traffic stops in St. Louis, Missouri. They observed interaction effects of officer race, driver race, and the racial composition of the neighborhood: In predominantly White neighborhoods, stops of Black drivers were more likely to result in a search, especially when the officer was White. However, in predominantly Black neighborhoods, White officers were more likely to search cars with White drivers. The researchers did not merely report this finding; they drew upon Donald Black's theory of law and suggested that both outcomes on racial disparities are the consequences of both racial profiling and residential segregation.

Health researchers have been at the forefront of moving beyond just providing evidence on racial disparities in outcomes, and are starting to examine associations between health and racial disparities that themselves are a legacy of racially unjust institutions (Asad and Clair 2018; see also Kohler-Hausmann 2019). As one of an increasing number of examples, Boen, Keister, and Aronson (2020) examine associations between aspects of the racial wealth gap and health outcomes. They find that "savings, stock ownership, and homeownership consistently improve health, but debt is associated with worse health, even after adjusting for total net worth." They also find that the correlations between different kinds of financial assets and health vary by race. In another study, Boen, Kozlowski, and Tyson (2020) look at correlations in school-, family-, and individual-level data between indicators of perceptions of safety in schools and health outcome both in adolescence and after leaving school in young adulthood. Lukachko, Hatzenbuehler, and Keyes (2014) consider state-level indicators of racial disparities in employment, education, political participation, and sentencing treatment by judges (including incarceration, capital sentencing, and disenfranchisement) to examine the effects of structural racism on heart attacks. In this style of work, we see possibilities of studying the effects of racism on health by examining the effects of a central outcome of structural racism, such as wealth inequality or educational disadvantage, and we can also imagine creating a multifaceted measure that aims to capture an array of aspects of racial marginality and examining its effects on health.

Researchers can also study public safety outcomes through a lens that employs theoretically informed metrics for structural racism. Drawing from this work, the next generation of quantitative research on policing and public safety needs to account for structural racism, not only in interpretation of research outcomes, but also as it manifests in the data itself.

This type of critical eye is also needed for exploring racism within police organizations, especially police culture and networks. The groundbreaking work of Wood, Roithmayr, and Papachristos (2019) use social network analysis of "big data" not to study police-generated crime data, but instead to study the officers themselves, their networks, and police misconduct complaints. They find that police misconduct in

Chicago is not random, but networked, meaning that officers tend to engage in behavior that leads to a misconduct complaint in groups. They also find associations between misconduct incidents and police demographics, such as age and race— younger officers receive more civilian and departmental misconduct complaints than older officers, and White officers are somewhat more likely than Hispanic and Black officers to have received at least one complaint (p. 13). Studying networks may help quantitative sociologists to get a handle on dynamics that influence culture within organizations, including the networked quality of police misconduct. Police culture is sustained by an array of professional narratives: narratives of dangerousness (Sierra-Arévalo 2019), narratives of “bad” segregated neighborhoods (Moskos 2008), and so forth. Quantitative researchers should explore opportunities to operationalize these narratives in understanding how racism produces racial disparities in policing.

## **Next Steps**

While this article maintains that increased research and evaluation of alternative projects would be germane to policy debates, it is important to use the tools of social science with circumspection and humility. It may be that some norms of “evidence-based policymaking,” which base normative decisions about good policy on clear, countable results, are in some ways out of step with creative efforts to “reimagine” public safety. Evidence-based policymaking, at least as currently conceived, is often backward-looking and timid. Reimagination is forward-looking and definitionally bold. This difference may come into play in the evaluation of community-based projects. Inevitably, many of these programs, at least initially, will be riddled with mistakes—perhaps even fail (for example, Madden, Leeds, and Carmichael 2020). This process of trial and error is at the heart of reimagination. As Ejeris Dixon (2020, 19) has explained, “We have to be accountable enough to continue our experiments, to measure them, to hold ourselves to high standards, and to believe in them.”

After all, embarking upon experiments for societal improvement is neither utopian nor unprecedented. American history is littered with institutions that were once deemed abject failures that might have been discarded if not for belief in them beyond data. Ironically, police departments themselves can be viewed as an emblematic example of such evolution. Early police departments were often initially riddled with corruption and unprofessional. Until research in the 1990s, researchers were unable to provide a persuasive statistical argument that the police reduced crime. It was only after a radical rethinking of police work—not constrained by preexisting evidence—that new realities could emerge.

Thus, researchers who embark upon collaborative research on community-based safety projects must be careful about how to interpret what may appear to be “failure.” Statistical failure may not mean that the project is fundamentally valueless. Along similar lines, some community organizers have criticized traditional quantitative research paradigms for deploying research strategies that misunderstand the

complexity of organizations' goals and desired impact, instead externally imposing logics of traditional measurement where alternative evaluative logics, perhaps based on narrative, might be more valuable (for example, Keene, Keating, and Ahonen 2016; Roe 1994; Rogers 2008; van Wessel 2018). Qualitative evaluations that draw from participant narratives have allowed researchers to understand outcomes of programs and policies that were not previously anticipated or that are difficult to measure, to explore unanticipated mechanisms of how and why approaches succeed or fail, and to explore nuances that quantitative tools cannot capture. Qualitative evaluations can also give voice and power over data-gathering to communities themselves, serving broader democratic goals and rectifying epistemic injustices (or wrongs against people "in their capacity as a knower" (see Fricker 2007, p. 1), which can be potent for marginalized communities in criminal system policymaking processes.

Beyond using mixed-methods research, both qualitative and quantitative, in evaluation—a strategy that has become standard—some researchers have gone further by embracing community-based participatory research as part of rigorous evaluation processes, enlisting the collaborative effort of both qualitative researchers and community members. The belief is that the data are better analyzed, with fewer framing and interpretation risks of the sort outlined above, by triangulating multiple types of data and by staying tightly connected to the population being studied throughout the research process. Social scientists should approach evaluative research with awareness that, while quantitative research is a valuable tool, it should never be an exclusive tool in moral and political debates over public safety.

Over the past few decades, academic public safety researchers have been much more aggressive about partnering with police departments than with community organizations. Social scientists tend to receive esteem for reaching data-sharing agreements with governmental agencies, including police departments, that allow the researchers to clean and analyze administrative data. Leading social science journals often publish and prominently feature articles that emerge from academic-police department partnerships. The esteem directed toward scholarship that emerges from such partnerships comes in part because of the increasing influence of "big data," both within policing and in the study of it (for example, Brayne 2020; Desmond, Papachristos, and Kirk 2016). Perhaps the quintessential example of such partnership is the storied Chicago Crime Lab, mentioned above, which has roots within the Chicago School of Sociology that was famous for treating the city as a laboratory.

Of course, analysis of police data can be of high value for understanding racial disparities, police budgets and staffing, and other aspects of policing. However, research that uses police data has severe limitations. Especially with respect to crime data, police data tell scholars about the world as it is understood and created by the institution that collects the data. Much crime goes unreported. Police presence is not equal across space, and in some places—especially White and suburban communities—crime does not consistently receive a police response (Jacques and Wright 2015). A current University of Chicago sociologist Robert Vargas (2020) has offered a trenchant critique of the Chicago Crime Lab for its history of research partnerships and deep entanglements with the Chicago Police Department. In Vargas's

view, such partnerships have led to flawed research that seeks to “improve cities by managing Black individuals instead of ending the police violence Black communities endure.” For these and other reasons, research that relies on police-created data is not more valuable, more scholarly, or more deserving of funding and attention than research on community-based modes of deterrence and accountability.

Pursuing research on alternatives to traditional, carceral crime deterrence measures will also demand that researchers engage in new forms of partnership. There are numerous community-based efforts afoot to increase community security and address harm, but most public safety researchers treat these efforts as if they are invisible or unworthy of rigorous study. It should be admitted that, in the past, the professional rewards for academics who consider pursuing empirical research on community-based alternatives to policing and public safety have often been scant. Quantitative scholars might be hesitant to make the time and energy investments to pursue a partnership with community-based organizations in part because their data, if the organizations have it, require working with smaller sample sizes. Research produced in partnership or collaboration with community organizations may be labelled as “activist” scholarship, perhaps even imposing career costs for its practitioners (Cancian 1993; Warren et al. 2018). However, collaborating with community groups is important work that the academy has the capacity to increase. Examples like the aforementioned READI program, with its evaluative collaboration with University of Chicago researchers, support this point. Yet, much more of this work is needed outside of the large university research labs. Scholars in the academy, including those who review articles, evaluate grant proposals, and vote on faculty appointments, should reflect upon and reorient our assumptions about what constitutes a laudable institutional partnership and rigorous scholarly research. Funders, including the federal government, might also have a role to play in facilitating this research by funding the alternative harm prevention and response work of community groups in ways that would better facilitate data-gathering and causal identification.

The current political and moral conversation over public safety is shallow and truncated because it is impossible for policymakers and activists to have well-informed debates about costs and benefits, tradeoffs, predicted outcomes over time, or potential unintended consequences of various changes. For example, if pursuing alternatives to policing along with a corresponding decrease in police funding would produce a sharp increase in homicide rates, the utility calculus might be different than if these approaches would produce a slight uptick in property crime in wealthy areas while increasing well-being in marginalized areas.

In addition, some recent reductions in police spending happened for reasons unrelated to changes in public safety policy. The city of Oakland, California, has been used as an example of the perils of cutting police funding and investing in alternatives—even though the 2020 police budget reduction in Oakland was taken up as an austerity measure because of municipal fiscal crisis, not to embrace a political goal of reimagining public safety. Indeed, the Oakland police budget reduction occurred alongside a reduction in the fire department’s budget, and some line items cut from the police budget were actually cuts from alternative measures to

promote public safety that were funded through the police department, such as Operation Ceasefire mentioned earlier (Clayton 2021; Sciacca 2021). Research is sorely needed to compare police defunding when it is accompanied by investment in various alternatives and when it is not so accompanied.

This essay has offered three propositions for next-generation research on public safety, which is already now underway. In a process accelerated in the wake of the murder of George Floyd in June 2020, social scientists have finally started to question the role of our own research in perpetuating the status quo, our uncritical acceptance of professional tropes about the value of police data, our ideas about the best ways of measuring public safety, and more. This paper offers an entrée into a broader plan for a social science that not only sees those status quo biases in our work, but actively seeks to root them out. Perhaps most importantly, next-generation policing and public safety research must remain circumspect and humble. Our expertise, while vast in some ways, is limited in others (Simonson 2021). Sometimes, the data will simply be insufficient to provide complete answers to the burning questions of our day. At those times, we must pause and reflect on where our findings fit within a larger ecosystem that is examining and rethinking policing, the criminal system, and their transformation.

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