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Does racial impact statement reform reduce Black–White disparities in imprisonment: Mixed methods evidence from Minnesota

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Abstract

Increasingly scholars have argued that, if the United States is to reduce Black–White disparities in incarceration, it is necessary to move away from race-neutral efforts and ensure that policies consider race. Despite this perspective, criminal legal policies have almost exclusively been race-neutral, with one general exception at the state level: racial impact statement reform. Although racial impact statement reform exists now in 10 states, no scholarship has empirically examined the implications of this approach for racial disparities in imprisonment. Using a mixed methods approach, we begin to fill this gap by examining the implications of Minnesota's racial impact statement reform on Black–White imprisonment rate disparities. Our quasi-experimental results do not suggest that Minnesota's reform reduced Black–White disparities in imprisonment. Our legislative analysis suggests that the null effects we observed were likely due to the fact that racial impact statements are responses to legislation that has already been proposed, and that the legislation proposed in Minnesota was not sufficient to significantly address Black–White imprisonment disparities, regardless of the extent to which these statements impacted the votes of legislators.

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1 | INTRODUCTION

Since the mid-1970s, the United States has built a system of punishment that incarcerates the largest number of people in the world (Travis et al., 2014; Walmsley, 2018). However, over the last 10 to 15 years, there has been a growing recognition that the US experiment with mass incarceration has gone too far and that change is needed (Enns, 2014; Gottschalk, 2015). As a result, there have been a significant number of reforms enacted (Beckett et al., 2016; Clear & Frost, 2014). In most instances, these reforms have seemingly been race-neutral and have almost exclusively focused on reducing punishment for nonviolent offenses (Beckett et al., 2016; O'Hear, 2017). These reform efforts have led to some successes. Overall, the United States' imprisonment rate has declined by about 10% from 2010 to 2020, but in some states declines have been more dramatic (Gottlieb et al., 2021; Sabol & Johnson, 2022). Still, even with these changes, the United States remains the world leader in incarceration (Walmsley, 2018).

The current approach to reform has clearly led to reductions in incarceration. In recent years, Black–White disparities in incarceration have begun to decline, but the evidence is mixed on whether reform efforts have played a role (Sabol & Johnson, 2022). For instance, one study (Gottlieb et al., 2021) found no evidence that California's reform efforts (often viewed as a race-neutral reform model) have reduced disparities and some evidence that they may have even increased disparities (although see Lofstrom et al., 2020). However, what cannot be argued is that, even with current reform efforts, the United States continues to have substantial Black–White disparities in incarceration, with Black individuals imprisoned at about 5 times the rate of White individuals (Nellis, 2016).

As a result, many scholars have argued that, to reduce disparities in incarceration, it is imperative that we move away from race-neutral efforts and ensure that policies consider race (London, 2011; Mauer, 2011; Schlesinger, 2011; Van Cleve & Mayes, 2015). Despite this call, criminal legal policies have almost exclusively been race-neutral, with one general exception: 10 states have enacted racial impact statement legislation or have sentencing commissions that provide racial impact statements (Porter, 2021). While racial impact statement policies vary, all provide mechanisms for legislators to receive projections about the impact potential legislation will have on racial disparities in imprisonment prior to voting on the legislation (Porter, 2021). Despite the fact that racial impact statement legislation is becoming increasingly prominent (6 states have passed legislation since 2019 and 6 others have proposed legislation), no studies have empirically examined the impact of these policies on imprisonment rate disparities (Porter, 2021).

In this study, we begin to fill this gap by examining the impact of Minnesota's racial impact reform efforts on Black–White disparities in imprisonment. We focus on Minnesota for a few reasons: (1) It was the first state to provide racial impact statements (beginning in 2008), so there is ample time since the reform effort for any impacts to take effect; (2) Minnesota has consistently produced racial impact statements after the reform was enacted (this is not true of other states, such as Connecticut, which has produced very few); and (3) Minnesota's sentencing commission makes racial impact statements readily available to the public, which allows us to examine potential mechanisms that may explain why Minnesota's reform has or has not impacted Black–White imprisonment disparities.

To assess the impact of Minnesota's reform on its Black–White disparities in imprisonment, we employed a mixed methods approach. First, we created a panel dataset that captures two separate measures of Black–White imprisonment disparities (a ratio and a gap measure) from 2000 to 2019 at the state level. Using a quasi-experimental synthetic control approach for each of these two outcomes, where we matched the treated state (Minnesota) to the weighted combination of states that most closely matched Minnesota's pre-treatment trends in the outcome of interest, we found little evidence that Minnesota's racial impact statement reform had any impact on Black–White disparities in imprisonment. Then, to assess why the reforms had little

impact on these disparities, we examined the content of each racial impact statement from 2008 to 2019 and the legislative outcomes (whether the bill passed or not) that followed. This legislative analysis provides suggestive evidence that these null effects were largely driven by the fact that racial impact statements are responses to legislation that has already been proposed, and that the legislation proposed in Minnesota was not sufficient to significantly address Black–White disparities (regardless of how legislators voted).

2 | LITERATURE REVIEW

2.1 | Building mass incarceration

The United States began its experiment with mass incarceration in the mid-1970s (Travis et al., 2014). Since then, the US incarceration rate has more than quadrupled, and the country now incarcerates more people than any other country in the world (Travis et al., 2014; Walmsley, 2018). This growth was driven primarily by changes in policy (not by changes in crime) that led to increased admissions for nonviolent offenses and increased sentence length, particularly for violent offenses (Raphael & Stoll, 2013; Travis et al., 2014). Many of the policy shifts during this period sought to reduce the discretion of criminal legal actors to ensure that people charged with the same crime received similar sentences and served similar amounts of time (Oleson, 2011).

Mandatory minimum sentencing, three strikes legislation, and truth in sentencing were all particularly important for the growth in incarceration (Travis et al., 2014). While each of these changes were seemingly race-neutral (the Sentencing Reform Act of 1984 for instance even explicitly stated that sentence enforcement must be "entirely neutral as to race"; §5H1.10), they in-fact produced racially disparate outcomes (Travis et al., 2014). For instance, in the year 2000, the Black imprisonment rate was more than 8 times the White imprisonment rate (Sabol & Johnson, 2022). The racially disparate impact of these policies occurred for at least two reasons. First, these policies created an architecture that ensured that sentence length was often significantly impacted by criminal history. Since Black individuals are more likely to have a criminal background than White individuals, people of color are more likely to receive severe mandatory minimum sentence or qualify for a third strike than a White person without or with a lesser criminal background (Mauer, 2010).

Second, these policies did not eliminate discretion; instead, they just shifted discretion from judges to prosecutors (Bjerk, 2005; Gertner, 2002). While it is difficult to fully ascertain the extent to which prosecutors drive racial disparities, it is rather clear that prosecutors' implicit biases have been associated with increases in the incarceration of people of color (Mauer, 2010). Prosecutors have been found to employ various stereotypes that are associated with viewing Black defendants as dishonest or innately dangerous and are more likely to consider Black defendants as presumed to be guilty or having capacity to have committed the crime in question (Prasad, 2017; Sloan, 2019). Given that prosecutors have the ability to negotiate pleas that work around mandatory parameters, this kind of racial bias among prosecutors exacerbates racial disparities in imprisonment (Bjerk, 2005).

While implicit biases among judges and prosecutors constitute a significant challenge within the criminal legal system, these are merely symptoms of a much larger problem of institutional racism. Institutional racism undergirds the entire structure of an organization and simultaneously overlooks how its practices and policies may directly impact, and often harm, people of color (Anthias, 1999; Better, 2008; Murji, 2007). Institutional racism may amount to "unwitting prejudice, ignorance, thoughtlessness, and racist stereotyping" which seeks only to disadvantage historically marginalized communities (Anthias, 1999, p. 1). Institutional racism within the criminal legal system is perhaps among the most salient explanations for extant racial

disparities in every facet of the system (Chaney, 2015; Cunneen, 2006; Rucker & Richeson, 2021; Toldson, 2020; Walker, 2020). For instance, Lynch (2011), through a case study of policing practices in Cleveland, further illustrates the practical manifestations of institutional racism. Lynch's (2011) study demonstrates how law enforcement practices can disproportionately target historically marginalized communities. Lynch (2011) highlights the ways in which institutional practices, even when race-neutral on their surface, can have deeply racialized impacts. Furthermore, Haney Lopez (1999) suggests that institutional racism permeates the conduct of judicial officers and contributes significantly to racial discrimination of those who encounter the criminal legal system. Only by thoroughly examining the deeply ingrained practices and norms within the criminal legal system, perpetuated by institutional racism, is it possible to identify and implement strategies to rectify the systemic inequalities and reverse the social damage (Beckett, 2022; Forman, 2017; Haney Lopez, 1999).

2.2 | Efforts to roll back mass incarceration

Since the incarceration rate peaked in 2008, there has been a recognition that the scope of mass incarceration was too great (Enns, 2014; Gottschalk, 2015). As a result, policymakers have passed a significant amount of legislation that has aimed to roll back mass incarceration (Beckett et al., 2016; Clear & Frost, 2014). These reform efforts have largely taken a race-neutral approach aimed at reducing the scope of incarceration without specifically targeting the racial disparities that persist (Beckett et al., 2016; O'Hear, 2017). At the sentencing stage, many states, led perhaps by California, have focused on reducing the use of incarceration for nonviolent offenses and technical violations (Beckett et al., 2016; Author). In the case of California, the reforms were successful at reducing incarceration overall, but there is mixed evidence as to whether they impacted racial disparities in a positive or negative way (Gottlieb et al., 2021; Lofstrom et al., 2020).

A second set of reforms has focused on reducing or ending the use of monetary bail and determining whether an individual should be detained pretrial based on risk. These types of approaches have been enacted in a number of states including New Jersey, New York, California, Kentucky, and Illinois (Preston & Eisenberg, 2022). There is little evidence regarding the impact of this approach on incarceration rates overall, as well as racial disparities in incarceration. However, research shows that pretrial detention and high bail amounts increase incarceration and racial disparities incarceration, which suggests that ending monetary bail may reduce both incarceration rates overall and disparities in incarceration (Donnelly & MacDonald, 2018; Sutton, 2013). On the other hand, the risk assessments that are often replacing monetary bail may contain elements (such as criminal history) that are more prominent among people of color, so it is quite possible that these approaches may not have any positive impact on reducing disparities and may even exacerbate disparities (Stevenson, 2018).

Since the United States began efforts to reduce incarceration some progress has been made. Overall, the prison population has declined by about 11% since 2010 and Black–White disparities in imprisonment fell about 40% from 2000 to 2020 (Sabol & Johnson, 2022). With that said, much work remains to be done. The United States still incarcerates people at the world's highest rate and disparities remain stark: Black individuals are imprisoned at about 5 times the rate of White individuals (Nellis, 2016; Walmsley, 2018). The persistence of these disparities has led a number of scholars to argue that criminal legal reform policies need to consider race, rather than take a seemingly race-neutral approach (London, 2011; Mauer, 2008; Schlesinger, 2011; Van Cleve & Mayes, 2015).

2.3 | Racial impact statement reforms

While almost all criminal legal reform efforts have been facially race-neutral, the exception at the state level is racial impact statement reform. Since 2007, 10 states have enacted racial impact statement legislation or have sentencing commissions that provide racial impact statements: Minnesota's sentencing commission began providing statements in 2008, Iowa and Connecticut passed legislation that took effect in 2009, Oregon passed legislation that took effect in 2015, New Jersey passed legislation that took effect in 2018, Colorado and Florida passed legislation that took effect in 2020, and Maine, Maryland, and Virginia adopted legislation in 2021 (Porter, 2021). In each of these cases, the legislation provides a mechanism for quantitative forecasts on the impact of criminal legal policy changes on racial disparities in imprisonment to be produced for legislators prior to votes on legislation (Gottlieb, 2022; Porter, 2021).

2.4 | Sentencing structure and reforms in Minnesota

Minnesota's sentencing guidelines were established for felony offenses in 1980 and onward, which currently serve as a comprehensive framework aimed at standardizing sentencing decisions (MSGC, 2023a). By utilizing a grid system, these sentencing guidelines contextualize sentencing parameters based on the legal circumstances at the time of the offense, offense severity, and criminal history to determine presumptive sentences (MSGC, 2023a). Despite efforts to enhance sentencing equity and consistency, the guidelines invoke elements of subjectivity by delineating exceptions under broadly defined conditions where it is permissible to depart from the presumptive sentences for "identifiable, substantial, and compelling circumstances." (MSGC, 2023a). Additionally, statutory provisions mandate minimum sentences for specific offenses, such as murder and felony DWI, reflecting a legislative intent to impose stringent penalties for severe crimes.

Within this larger context, racial impact statement reform first took effect in Minnesota at the start of 2008. In addition to being the first state to attach racial impact statements to criminal legal legislation, Minnesota's racial impact statement reform is unique in that the policy was not enacted because of legislation (Erickson, 2014; London, 2011; Porter, 2021). Instead, in response to significant racial disparities in incarceration, the decision to produce racial impact statements was made by the Minnesota Sentencing Guidelines Commission (MSGC) (Erickson, 2014; London, 2011). This was an understandable decision, given that Black–White disparities in imprisonment were particularly stark in the years leading up to racial impact statement reform. For instance, in 2007, the year before the MSGC began producing racial impact statements, the Black–White imprisonment rate ratio in Minnesota was nearly 11 to 1 (i.e., Black people were nearly 11 times more likely to be imprisoned than White people) and the Black–White imprisonment rate gap indicated that the Black imprisonment rate was more than 1000 per 100,000 people higher than the White imprisonment rate (Authors' calculations using prison data from the Bureau of Justice Statistics National Prisoner Statistics Program and Bridged Race Population Data from CDC Wonder).

From 2008 to 2019, the MSGC has produced 24 racial impact statements (MSGC, 2023b). These statements are produced purely for informational purposes (London, 2011). As such, they do not make recommendations about whether a bill should be passed, nor are legislators required to take specific steps/actions in instances when disparities are projected to increase (Erickson, 2014; London, 2011). These statements tend to be brief (roughly 1–2 pages), although they have become more detailed over time (MSGC, 2023b). In each instance, Minnesota's racial impact statements begin by describing the racial composition of the general population in the state, as compared to those with felony convictions and those who are imprisoned (London, 2011). Then, the statement makes a projection about how the policy will impact those with felony convictions and imprisonment rates (London, 2011). Last, in most instances, the statements project whether the proposed law will exacerbate, mitigate, have no

effect, or an unclear effect on imprisonment disparities (the imprisonment rate ratio) between each specific non-White racial group and White indivduals (London, 2011).

2.5 | *Theory*: Why Racial Impact Statements Might or Might Not Impact Black–White Imprisonment Rate Disparities

As Verma (2015) documents using California as a case study, the extent to which criminal legal reform has its intended effects depends in part on how the reform is interpreted by front line actors. In the case of Minnesota's racial impact statement reform, the key frontline actors are the state legislators. While no studies have empirically examined the impact of racial impact statement reform on Black–White disparities in imprisonment, if legislators choose to interpret this reform as necessitating them to strongly consider the implications of state criminal legal policy proposals on Black–White disparities, racial impact statement reform is far more likely to achieve the aim of reducing these imprisonment disparities.

There are a few other mechanisms through which racial impact statement reform could lead to reductions in Black–White imprisonment disparities. First, it may be that many legislators want to reduce Black–White disparities in imprisonment, but are unaware of how specific bills will impact those disparities. By providing objective projections about the implications of potential legislation, racial impact statements provide these legislators with a tool that makes it easy for them to vote their values (Erickson, 2014; London, 2011; Mauer, 2008). Second, it may be that many legislators are in favor of reducing Black–White disparities in imprisonment, but that Black–White disparities may not always be front and center in their mind when considering legislation (e.g., they may be focused on potential crime reduction impacts or how the proposed law impacts imprisonment overall). In this instance, by providing objective projections about the implications of proposed legislation on Black–White disparities in imprisonment, racial impact statements are priming these legislators to focus on Black–White disparities, increasing the likelihood that they will vote in ways that reduce disparities (Mauer, 2008).

However, as suggested by Verma (2015), there is no guarantee that legislators will interpret racial impact statement reform as necessitating them to actually take strong corrective action to reduce Black–White disparities. In fact, experimental framing and priming studies suggest that racial impact statement legislation has the potential to increase Black–White disparities. Specifically, a growing body of evidence suggests that when individuals are primed with information that highlights racial disparities, this either has no effect on their support for criminal legal policy or leads to backlash and makes individuals (especially White people) more likely to support punitive criminal legal policies and less likely to support criminal legal reforms (Gottlieb, 2017; Hetey & Eberhardt, 2014; Peffley & Hurwitz, 2007; Wozniak, 2020). Therefore, it is quite possible that by priming disparities, racial impact statements will perversely increase the likelihood that legislators support legislation that increases Black–White disparities in imprisonment and to vote against legislation that reduces these disparities.

Finally, it is also possible that racial impact statement reform will not have any impact on Black–White disparities in imprisonment. Specifically, racial impact statements provide information, but they are a reactive tool in that they are responses to legislation that has already been proposed (Erickson, 2014; London, 2011; Mauer, 2008). As such, the impact of these statements is not just driven by how legislators respond to an impact statement in a future vote. The impact of these statements is also driven by the type of legislation that is proposed to begin with. If legislators are proposing legislation that is small in scope or that is rarely projected to substantially impact Black–White disparities, racial impact statement reforms, by definition, are limited in their ability to impact Black–White disparities. If legislators do not interpret racial impact statement reform as necessitating them to address Black–White disparities, it is certainly possible that this reform will have no impact on the type of legislation that they propose (Verma, 2015).

2.6 | The current study

In the current study, we seek to answer two questions: (1) What has been the effect of Minnesota's racial impact statement reform on Black–White disparities in imprisonment and (2) What explains the impact (or lack thereof) of Minnesota's racial impact statement reform on Black–White imprisonment disparities? Based on the discussion above, we do not have a clear expectation as to whether Minnesota's reform impacted Black–White imprisonment disparities, and if so, in what direction.

3 | METHOD

3.1 | Data

For our quantitative analyses, where we examined the main effect of Minnesota's reforms on Black–White disparities in imprisonment, we used data from the Bureau of Justice Statistics (BJS) National Prisoner Statistics (NPS) Program. We used this data to capture the number of Black and White people in prisons in each state in a given year (from 2000 to 2019) (Bureau of Justice Statistics, 2021). Since states vary in the size of their populations, raw numbers of people imprisoned do not adequately capture an individual's risk of experiencing imprisonment. Therefore, to adjust raw numbers to differences in state population size, we used bridged race population estimates from CDC Wonder (2023). For the second part of our analyses, where we examined potential mechanisms, we collected data on racial impact statements from the Minnesota Sentencing Guidelines Commission's (2023b) website. To examine whether bills with racial impact statements ended up being passed, we collected information from the Minnesota Revisor's (2023) website.

3.2 | Quantitative measures

Our quantitative analyses focused on two variables measuring Black–White imprisonment disparities: the Black–White imprisonment rate ratio and the Black–White imprisonment rate gap. First, we began by creating separate imprisonment rates for Black and White people. The Black imprisonment rate in a given state in a given year was captured using the following equation: $BlackPrisonRate_{yst} = ((BlackPrisonIncar_{yst})/(BlackPop_{yst})) * (100,000)$, with $BlackPrisonIncar_{yst}$ representing the number of Black people in prison in a state in a given year and $BlackPop_{yst}$ being an indicator of the size of state's Black population in a given year. Therefore, this Black imprisonment rate number is equal to the total number of Black people in a given state and year that were incarcerated in state prison per 100,000 Black population. We did not include individuals in Federal prisons because Federal prison numbers should only be minimally impacted (if at all) by state-level policy. Then, we used the same formula to capture the White imprisonment rate: $WhitePrisonRate_{yst} = ((WhitePrisonIncar_{yst})/(WhitePop_{yst})) * (100,000)$. Therefore, this White imprisonment rate number is equal to the total number of White people in a given state and year that were incarcerated in state prison per 100,000 White population.

After creating these race-specific rates, we then created two measures which capture Black–White disparities in imprisonment. We created the first, the Black–White imprisonment rate ratio, using the following formula: $Black–WhiteRatio_{yst} = BlackPrisonRate_{yst}/WhitePrisonRate_{yst}$. Therefore, the $Black–WhiteRatio_{yst}$ indicates how many times more (less) likely a Black person is to be imprisoned than a White person in a given state and year. The second measure, the Black–White imprisonment rate gap, was created using the following formula: The $Black–WhiteGap_{yst} = BlackPrisonRate_{yst} − WhitePrisonRate_{yst}$. Therefore, the $Black–WhiteGap_{yst}$ describes the absolute difference between the Black and White imprisonment rates.

We restricted our analyses to 45 states. Oregon, Iowa, Connecticut, and New Jersey were excluded because they, too, had mechanisms for producing racial impact statements during this period. Alaska was also excluded because it did not have complete data on Black and White imprisonment rates for all years in the study period, since our empirical approach, the Synthetic Control Method, requires a balanced panel.

3.3 | Analytic strategy

3.3.1 | Main quasi-experimental analyses

To examine the impact of racial impact statement reform on Black–White disparities in imprisonment, we treated Minnesota as a quantitative case study. To try to make causal inferences and minimize differences in pre-reform trends in imprisonment between Minnesota and control states, we adopted a quasi-experimental synthetic control approach (Abadie et al., 2015; Gottlieb et al., 2021; Bartos & Kubrin, 2018; Lofstrom & Raphael, 2016).

In many instances, researchers adopt a difference in difference framework to evaluate the impact of policies. However, the key assumption behind difference in difference analyses, that trends in the outcome of interest are parallel and would continue to be parallel if the policy was never implemented, is often violated or not plausible because it is difficult to find a state or group of states whose average pre-treatment trends are parallel to the pre-treatment trends in the outcome in the treated states (Bartos & Kubrin, 2018; Huntington-Klein, 2021). When the parallel trends assumption is not plausible, it is difficult to attribute changes in the difference between treated and control states to the policy being assessed (Huntington-Klein, 2021).

The synthetic control method extends the difference in difference approach and weights potential donor (control) states so that trends in the outcome of the control states most closely match trends in the treated state (Abadie et al., 2015; Bartos & Kubrin, 2018). To create this weighted combination of nontreated states, the synthetic control method employs a data-driven approach (timeinvariant, non-negative, and add to one) to states that are not treated (Abadie et al., 2015; Bartos & Kubrin, 2018). At this stage of the analysis, two types of predictors can be included: (1) prior values of the outcome and (2) other covariates that are known to be associated with the outcome (Bartos et al., 2020; McCleary et al., 2017). In this case, we know little about the causal factors that conclusively impact Black–White imprisonment rate disparities. Moreover, imprisonment rates generally change slowly (imprisonment is determined both by passed and current policies and trends) unless there is a significant policy shock. Therefore, we include all pre-treatment values of the outcome of interest, since these prior outcome values are likely to be most important in determining future outcomes; this approach also has the added benefit of maximizing pre-treatment fit (Bartos et al., 2020; Ferman et al., 2020; McCleary et al., 2017). We do not include other covariates because, when all pre-treatment outcome values are included, additional covariates do not contribute to the selection of the synthetic control and should not be included (Ferman et al., 2020; McClelland & Mucciolo, 2022). Since the synthetic control method creates a control group that most closely matches the treated state in pre-intervention outcome trends, the difference in the outcome of interest between the treated state and its control group is viewed as the effect of the policy if the pre-intervention fit is good (Bartos & Kubrin, 2018). A key assumption underlying this approach (the logic is similar in other quasi-experimental approaches as well) is that without the implementation of the reform, the treated group (in this case Minnesota) would have enacted similar policies and followed a similar outcome variable trend to its synthetic control group in post-reform years. This assumption is untestable (since the reform was in fact implemented), but is plausible, especially when pre-treatment trends are similar across treatment and control groups.

For the traditional version of the synthetic control method to be appropriate, it is important for pre-treatment outcome trends in the treated group to be closely matched to pre-treatment

outcomes in the synthetic control. When this is not feasible, bias-corrected synthetic control approaches have been developed that rely on regression to estimate how much bias is introduced into synthetic control estimates due to poor pre-treatment fit and then use that estimate to de-bias the traditional synthetic control estimate (Abadie & L'Hour, 2021; Ben-Michael et al., 2021; Wiltshire, 2022). No clear guidelines that have been established to determine whether pre-treatment fit is good, but we use the following requirements: (1) The control group is less than 1% different from the treatment group in the year prior to the treatment and (2) Over the course of the pre-treatment period, the control group is less than 1% different on average than the treatment group.

We began our analyses by examining the impact of Minnesota's reform on the Black–White imprisonment rate ratio. Since Minnesota's reform was first implemented in 2008, a synthetic control group was created to most closely match pre-intervention trends in the Black–White imprisonment rate ratio from 2000 to 2007. Unfortunately, as demonstrated in Table A1, the pre-treatment fit was not good for this analysis. Specifically, while the difference between Minnesota and its synthetic control was small in the last pre-treatment year (.663%), the average difference during the course of the pre-treatment period was 9.338%. Therefore, for this analysis we employ a bias-corrected synthetic control approach, using OLS regression to estimate how much bias is introduced by poor pre-treatment fit and then using that estimate to de-bias the synthetic control results (Wiltshire, 2022).

Then, we examined the impact of Minnesota's reform on the Black–White imprisonment rate gap. To do so, we created a synthetic control that most closely matched pre-intervention trends in the Black–White imprisonment rate gap from 2000 to 2007. As shown in Table A2, the pre-intervention fit was excellent; the synthetic control differed from the treatment by an average of .133%, while this difference was .195% in the last pre-treatment year. When the pre-treatment fit is excellent, the traditional synthetic control approach is preferred to bias-corrected approaches, so we use the traditional synthetic control method for this analysis (Ben-Michael et al., 2021; McClelland & Mucciolo, 2022).

Determining whether associations are statistically significant when using the synthetic control method requires an approach that is different than what is used in traditional regression analyses (Abadie et al., 2015). Instead of using coefficients and standard errors to determine statistical significance, the synthetic control method relies on placebo-in-space estimates (Abadie et al., 2015; Bartos & Kubrin, 2018; Galiani & Quistorff, 2017). In short, we repeated the process described above and created a synthetic control group for each potential control state in each analysis. Despite not being treated, we act as if each control state was treated at the same time as Minnesota. We then rank each state by the size of its effect. For each analysis, the size of the effect is equivalent to the ratio of the post-treatment root mean squared prediction error (RMSPE) over the pre-treatment RMSPE (Galiani & Quistorff, 2017; Wiltshire, 2022). If 95% of the control units have smaller post-treatment effects than Minnesota, the racial impact statement reform had an effect that is not likely spurious and the association is statistically significant (Galiani & Quistorff, 2017).

We conducted each analysis in Stata 17 (StataCorp, 2021). For our analysis of the effect of Minnesota's reforms on the Black–White imprisonment rate ratio, we used the allsynth command to employ the bias-corrected synthetic control approach (Wiltshire, 2022). We used the synth_runner command for our analysis of the effects of Minnesota's reform on the Black–White imprisonment rate gap (Galiani & Quistorff, 2017).

3.3.2 | Supplemental quasi-experimental analyses

One key challenge with imprisonment data is inconsistent reporting of Latinx ethnicity. During the course of the study period, 14 states failed to report Latinx ethnicity in at least 1 study year. Minnesota, unfortunately, was missing data on Latinx ethnicity in 4 of the first 5 pre-treatment

years (2001–2004), and did not begin consistently reporting Latinx ethnicity in the imprisonment data until 2005. Not capturing the population of Latinx individuals who are imprisoned can be problematic, and lead to overestimation of White imprisonment rates and underestimation Black–White disparities (Nellis, 2016). To test whether our results are driven by differences in Latinx ethnicity reporting, we conducted two sets of robustness checks. First, we ran each of our analyses, but with a different set of covariates. Specifically, we included a covariate for each year that captures whether Latinx imprisonment data was missing. To do so, we had to exclude some of the pre-treatment measures of the outcome (otherwise the missing Latinx imprisonment covariates would have no explanatory power; McClelland & Mucciolo, 2022). In this case, we included pre-treatment outcome values for every other year beginning in 2001 (2001, 2003, 2005, 2007; McClelland & Mucciolo, 2022). Using this approach we no longer have a good pre-treatment fit for Black–White imprisonment gap analysis, so we employ the bias corrected synthetic control approach, rather than the traditional synthetic control that we employed in our main analyses. Second, we conducted each of the analyses described above, but on a sample that is restricted to the years 2005–2019 and to the 34 states (Minnesota and 33 potential control states) that reported Latinx ethnicity in each of those years. In these analyses, our only covariates are pre-treatment measures of the outcome, as was the case in our main specifications. While this approach addresses concerns about differential reporting of Latinx ethnicity, a limitation of this approach (and why we treat these as supplemental rather our primary specification) is that it reduces the length of the pre-treatment period to 3 years, which limits our ability to capture the extent to which our control matches Minnesota's pre-treatment trends in the outcomes of interest (Abadie et al., 2015).

3.3.3 | Assessing mechanisms

To examine why Minnesota's racial impact reform did or did not impact Black–White disparities in imprisonment, we examined the 24 instances during the study period in which racial impact statements were produced. Specifically, we examined a number of key issues: (1) To what extent was proposed legislation projected to impact the Black–White imprisonment rate ratio and imprisonment rates overall; (2) Did bills that were projected to reduce the Black–White imprisonment rate ratio fare differently (i.e., did it pass) than bills that were not projected to do so? And (3) Did bills that were projected to reduce imprisonment overall fare differently (i.e., did it pass) than bills that were not projected to do so? Since racial impact statements are reactive and are only produced after a bill has been proposed, the impact of these statements on Black–White disparities in imprisonment is dependent on: (1) The voting response of legislators to the information provided in racial impact statements and (2) The type and scope of legislation that legislators are proposing in the first place. By examining how legislation fared and the content of racial impact statements, we provide suggestive evidence about the role that each of these mechanisms play in explaining our quantitative findings.

4 | FINDINGS

4.1 | Quantitative results

Our first analysis examines the association between Minnesota's racial impact statement reform and Minnesota's Black–White imprisonment rate ratio. As we discussed previously, Table A1 demonstrates that, for this analysis, the synthetic control does not closely match Minnesota's outcome trends prior to the treatment. Therefore, we conduct this analysis using the bias-corrected synthetic control approach. As Figure 1 shows, there is no evidence that Minnesota's reform led to a reduction in the Black–White imprisonment rate ratio. In fact, in all post-

treatment years, except for 2009, the Black–White imprisonment ratio was higher in Minnesota than in its bias-corrected synthetic control. For instance, at the end of the study period in 2019, the Black–White imprisonment rate ratio was approximately .29 higher than its synthetic control and Minnesota continued to imprison Black individuals at more than 8 times the rate as White individuals. However, bias-corrected placebo-in-space estimates suggest that none of these associations were statistically significant. In the 12 post-treatment years, bias-corrected p-values for the difference between Minnesota and its bias-corrected synthetic control ranged from a low of .16 in 2013, 2014, and 2015 to a high of .64 in 2008. Therefore, our findings suggest that racial impact statement reform had no statistical impact on the Black–White imprisonment ratio in Minnesota.

While the Black–White imprisonment rate ratio is most commonly used to document imprisonment disparities and is the measure of disparity assessed by Minnesota's racial impact statements, the Black–White imprisonment rate gap is an alternative metric that is also sometimes used. Therefore, in our second analysis, we examine the impact of Minnesota's reform on Minnesota's Black–White imprisonment gap. As we documented previously in Table A2, the pre-treatment fit between Minnesota and its synthetic control was quite good in this instance. Therefore, for this analysis, we rely on the traditional synthetic control approach. As shown in Figure 2, we find no evidence that Minnesota's racial impact statement reform led to reductions in the Black–White imprisonment rate gap. In fact, in each post-treatment year, with the exception of 2008, the Black–White imprisonment gap was higher in Minnesota than in its synthetic control. For instance, in the 2019, the last year of study period, the Black–White imprisonment rate gap was 66 per 100,000 persons greater in Minnesota than in its synthetic control. However, placebo-in-space estimates suggest that none of the associations are statistically significant. In the 12 post-treatment years, p-values for the difference between Minnesota and its synthetic control ranged from a low of .20 in 2015 to a high of .29 in 2008. Therefore, our findings suggest that racial impact statement reform had no statistical impact on the Black–White imprisonment rate gap in Minnesota.

FIGURE 1 Bias-corrected synthetic control estimate of the effect of Minnesota's racial impact reform on the Black–White imprisonment rate ratio. Minnesota's Synthetic Control = Pennsylvania = .19, Utah = .134, Wisconsin = .676. Solid line represents the difference in the Black–White ratio between Minnesota and its bias-corrected synthetic control, with positive numbers indicating that Minnesota's ratio is higher than its synthetic control and negative numbers indicating that Minnesota's ratio is lower.
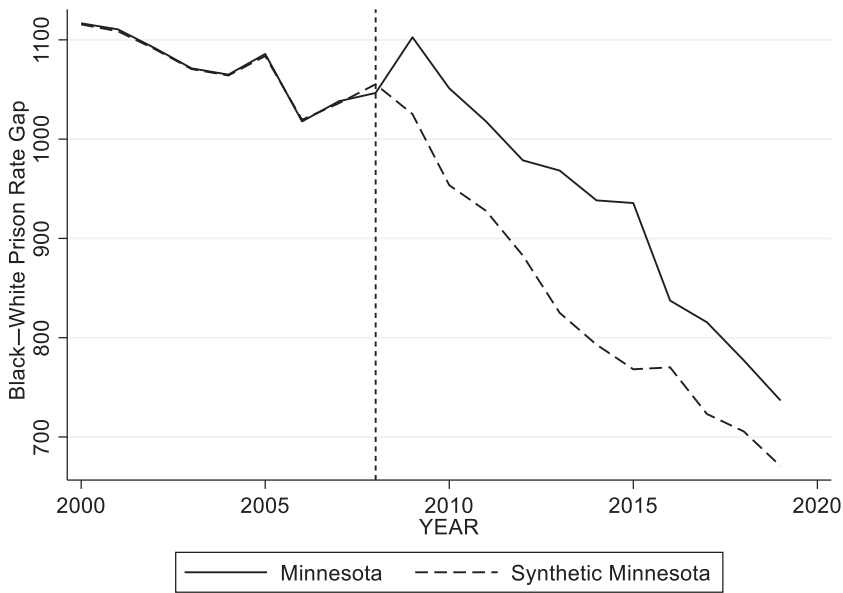
FIGURE 2 Synthetic control estimate of the effect of Minnesota's racial impact reform on the Black–White imprisonment rate gap. Synthetic Minnesota: Florida = .026, Georgia = .037, Massachusetts = .083, Mississippi = .082, North Dakota = .108, South Dakota = .176, Tennessee = .452, Texas = .035.

4.2 | Supplemental quantitative results

As described in the analytic strategy section, one potential concern with our analyses is that there is differential reporting of Latinx ethnicity in imprisonment data across states, and that this differential reporting could bias Black–White imprisonment rate disparity estimates. Unfortunately, Minnesota did not consistently start reporting Latinx ethnicity until 2005. Therefore, we conducted two additional sets of analyses to assess whether our results were driven by differential reporting of Latinx ethnicity. First, we conducted each synthetic control analysis described above, but adjusted our covariates to capture whether Latinx data was missing for that state in each pre-treatment year. These models also included pre-treatment outcome values for the years 2001, 2003, 2005, and 2007. Second, we conducted each synthetic control analysis described above, but restricted the sample to the years 2005–2019 and the 34 states (Minnesota and 33 potential control states) that reported Latinx ethnicity in each of those years.

Generally, these results (reported in Figures A1–A4) are quite consistent with the findings discussed above, which suggested that Minnesota's reforms had no impact on Black–White disparities. There is one exception. In the analyses restricted to the years 2005–2019 and to the 34 states (Minnesota and 33 potential control states) that reported Latinx ethnicity in each of those years, the Black–White imprisonment ratio analysis suggests that from 2008 to 2012 Black–White disparities were actually significantly higher in Minnesota than its synthetic control ($p < .05$), but that this effect faded out over time; from 2013 to 2019 there was no statistical difference between Minnesota and its synthetic control in the Black–White imprisonment rate ratio. Notably, none of our supplemental analyses provide evidence that the reforms reduced disparities in imprisonment between Black and White individuals, and each analysis suggests that by the end of the study period there was no association between Minnesota's reforms and Black–White imprisonment rate disparities.

4.3 | Mechanisms

Taken together, our quantitative analyses suggest that Minnesota's racial impact statement reform likely had no impact on Black–White disparities in imprisonment. However, these analyses provide no evidence as to why these statements did not lead to reductions in Black–White disparities. To examine this further, we reviewed each of the 24 racial impact statements that were attached to a bill during the study period and tracked whether those bills became law.

Before describing these findings, we wanted to highlight some characteristics and limitations of Minnesota's racial impact statements themselves. First, the type of disparity that is being assessed in Minnesota's racial impact statements is the Black–White imprisonment ratio. Projection is not made about the impact on the Black–White imprisonment rate gap. Second, prior to 2018, racial impact statements often did not provide information on magnitude of shifts in imprisonment, which makes it difficult to infer how large an impact a proposed bill might have on Black–White disparities. Third, there were a few instances where the statements either indicated Black–White disparities would not be impacted or did not make a specific projection about whether it would exacerbate or alleviate disparities, when the numbers they provided suggested that either Black or White people might disproportionately benefit/be harmed by a policy. It is possible that this is because the changes in the aggregate would be very slight (since the policy itself impacted few people), but in some instances this was not clear. Fourth, in many instances, it was also often unclear how long it would take for the projection to be realized.

With those caveats aside, we next highlight a few points (illustrated in Table 1) that shed light on possible mechanisms. First, we find little evidence to suggest that our findings are driven by backlash or that the racial impact statements backfired and led legislators to vote in ways that lead to greater disparities. Specifically, we find that 2 (33%) of the 6 bills projected to reduce the Black–White imprisonment rate ratio passed, compared to 5 (28%) of the 18 bills that were projected to increase the Black–White imprisonment ratio or have no effect or an unclear effect on the Black–White imprisonment rate ratio. Second, the biggest dividing line appeared to be around whether bills would reduce or increase imprisonment, with legislators less likely to pass bills that increased punitiveness. Of the 6 bills projected to reduce imprisonment, 4 (67%) passed; all of these bills were expected to reduce punitiveness around drugs, but 3 were projected to exacerbate Black–White imprisonment rate ratios. By contrast, of the 18 bills that increased punitiveness, only 3 (18%) passed, two of which focused on sex crime enhancement (both were projected to reduce the Black–White imprisonment rate ratio) and one on firearm possession enhancement (which was projected to increase the Black–White imprisonment ratio). Third, although estimates of the overall number of people impacted were often missing from racial impact statements, in many cases the scope (i.e., the number of people impacted) of the reforms appear to be quite small, so by definition these reforms would have minimal impact on Black–White disparities. For instance, the two bills that passed and were projected to reduce Black–White disparities were both projected by the Sentencing Guidelines Commission to impact fewer than 60 prison beds annually.

Taken together, this suggests an important fourth point: A key limitation to what racial impact statements can accomplish, with respect to reducing Black–White imprisonment disparities, is that they are limited by the legislation that is proposed. In this case, only a quarter of the bills proposed were projected to reduce the Black–White imprisonment rate ratio (all by small amounts). Notably, reducing imprisonment appeared to be something that legislators were on-board with (especially for drug offenses), yet 0 of the 24 bills that we examined were projected to both reduce imprisonment and the Black–White imprisonment rate ratio.

TABLE 1 List of bills, bill outcomes, and racial impact statement projections.

Bill origin	Final status	Brief description of proposed policy	Estimated effect on imprisonment	Estimated effect on Black–White disparity
House File No. 2013 (LY: 2019)	Failed to pass	Marijuana offenses thresholds adjusted, and penalties provided.	MSGC staff estimated that the bill would likely result in an eventual prison reduction of 16 beds.	Essentially no projected impact on Black–White imprisonment disparities. White share projected to go from 47.4% to 47.3%, with Black share remaining unchanged at 34.2%.
House File No. 812 (LY: 2019)	Failed to pass	Criminal sexual conduct offenses for persons in authority positions over juveniles expanded.	MSGC staff estimated that the bill will result in one additional prison sentence a year for a first-degree criminal sexual conduct (CSC), and an eventual prison bed impact of 12 beds.	No projected impact on Black–White imprisonment disparities.
House File No. 689 (LY: 2019)	Failed to pass	Probation length for certain offenses modified, and court-granted early termination and discharge of probation clarified.	MSGC staff estimated that prison-bed demand will be reduced by 85 beds as a result of this bill. Any impact would be delayed by at least 5 years.	Would increase Black–White imprisonment disparities by a little. White share projected to decline from 47.4% to 47.1% and Black share projected to increase from 34.2% to 34.4%
House File No. 480 (LY: 2019)	Failed to pass	Criminal sexual conduct definitions amended, criminal sexual conduct offenses expanded for persons in current or recent positions of authority over juveniles, affirmative defenses to criminal sexual conduct charges eliminated, and statute of limitations removed for felony criminal sexual offenses.	MSGC staff estimated that the bill will result in 52 new felony offenders annually, and 104 new prison beds.	It was projected that there would be no change in Black–White imprisonment rate disparities.
House File No. 89 (LY: 2019)	Failed to pass	Child pornography penalties increase; mandatory minimum sentences establishment; sentencing guidelines commission modification of sex offender grid requirement.	MSGC staff estimated that this bill will result in the need for 55 prison additional prison beds.	Slight decline projected in Black–White disparities. Share of White population would go from 47.4% to 47.6%, while share of Black people would go from 34.2% to 34.1%.
Senate File No. 111 (LY: 2019)	Passed	Sexual conduct crimes provisions modification.	MSGC staff estimated that the bill will eventually result in 55 new prison beds.	Projected to slightly reduce Black–White imprisonment disparities, Share of White population would go from 47.4% to 47.6%, while share of Black people would go from 34.2% to 34.1%.
Senate File No. 2445 (LY: 2019)	Failed to pass	Second degree criminal sexual conduct crimes presumptive sentences increase.	MSGC staff estimated that the bill will likely result in an eventual prison increase of 350 beds annually.	Would likely reduce Black–White disparities a bit. White share would likely increase from 47.4% to 47.7% while Black share would likely decline from 34.2% to 33.5%.
House File No. 2964	Failed to pass		The bill is projected to increase overall imprisonment with 183 new felony offenders	The projected impact of this policy on Black–White imprisonment disparities is unclear.

TABLE 1 (Continued)

Bill origin	Final status	Brief description of proposed policy	Estimated effect on imprisonment	Estimated effect on Black–White disparity
(LY: 2018)		Predatory offender registration for soliciting a [sex worker] required, and penalties for soliciting a [sex worker] enhanced.	annually, resulting in 28 additional prison sentences each year and ultimately impacting 22 prison beds.	
House File No. 2904 (LY: 2018)	Failed to pass	Child pornography offenses penalties increased, mandatory minimum sentences created, and Sentencing Guidelines commission directed to modify the sex offender grid.	The bill would necessitate 73 additional prison beds: 47 for non-prison sentences shifting to prison and 26 for longer sentences, based on assumptions regarding child pornography offender characteristics.	Would likely reduce Black–White disparities by small amount. White share would likely increase from 46.6% to 47%, while Black share would likely decline from 35% to 34.7%.
Senate File No. 2699 (LY: 2018)	Passed as part of omnibus bill	Child pornography offenses penalties increase, sex offenses stays of sentence or adjudication reporting; parent child reunification after sexual abuse restrictions authorization; invasion of privacy penalty increase and offender registration; pornography and sex trafficking connection data collection; penalty assessment modification.	The bill is projected to lead to 58 offenders annually with higher severity level sentences. Nine will shift from probation to prison, and nine will receive longer prison terms, requiring 31 additional annual prison beds.	Would very slightly reduce Black–White disparities but by increasing punitiveness. White share would increase from 46.6% to 46.7%, while Black share would decline from 35% to 34.9%.
House File No. 34 (LY: 2017)	Failed to pass	Minnesota Public Safety Personnel Protection Act created, and penalties for obstructing emergency responders increased.	The bill may result in 1543 more people receiving felony sentences, requiring 506 additional prison beds.	This would likely policy would reduce Black–White disparities by a little bit, but unclear by how much.
House File No. 2557 (LY: 2016)	Failed to pass	Minnesota Public Safety Personnel Protection Act created, and penalties for obstructing emergency responders increased.	Would result in an increase in imprisonment by increasing the number receiving either an executed prison sentence or a jail sentence, but unclear by how much.	Unclear what impact would be on Black–White imprisonment disparities.
Senate File No. 3481 (LY: 2016)	Passed	Controlled substance crimes thresholds modifications; marijuana plants and controlled substance trace amounts possession new offenses creation; lower level controlled substance crimes mandatory minimum sentences elimination; community justice reinvestment account establishment.	It is estimated that various policy changes in this bill will eventually reduce the need for prison beds by 664 beds.	The policy is projected to increase Black–White imprisonment disparities. Our calculations suggest that the White share would decline from 46.6% to 45.4%, while Black share would increase from 35% to 36%.
Senate File No. 878	Passed	Adds ammunition to the provisions prohibiting certain persons from possessing firearms. Those with a prior conviction for a crime of	Imprisonment is projected to increase by greater punitiveness around possession of ammunition, but unclear by how much.	The amendment is projected to exacerbate Black–White imprisonment disparities, but unclear by how much.

(Continues)

TABLE 1 (Continued)

Bill origin	Final status	Brief description of proposed policy	Estimated effect on imprisonment	Estimated effect on Black–White disparity
Amend# SCS0878A-550 (LY: 2015)		violence, possession of ammunition will be subject to the same 5-year mandatory minimum as possession of a firearm.		
House file No. 285 (LY: 2013)	Failed to pass	The bill would add felony assault in the fifth degree, felony domestic assault, and domestic assault by strangulation to crimes of violence. It would also add language to certain persons (felons and juveniles) not to possess firearms, that prohibits possession of ammunition as well as firearms.	Bill projected to increase imprisonment rates by increasing the number of crimes labeled violent. Specifically, it may result in the need for an additional 9 to 34 prison beds per year.	Bill would likely increase Black–White disparities, although only slightly because of small number of people impacted annually.
House File No. 1665 (LY: 2012)	Failed to pass	This bill amends the list of offenses defined as crimes of violence in Minn. Stat. § 624.712.	The number of people imprisoned would be likely to increase, but unclear by how much.	Black–White disparities also likely to increase, but unclear by how much.
House File No. 306 (LY: 2011)	Failed to pass	Emily's Law established, adult certification and extended jurisdiction juvenile prosecution age lowered for juveniles, and violent juvenile offense defined.	Would lead to very slight increase in imprisonment (only 12 people from 2001 to 2009 were impacted).	Would likely very slightly increase Black–White disparities.
Senate File No. 2018 (LY: 2009)	Passed as part of omnibus bill	Omnibus criminal justice, public safety, corrections, judiciary and controlled substance policy provisions modifications.	The reforms in totality would reduce imprisonment. 305 people would move from prison to probation and that 367 would have sentenced reduced.	Would likely increase Black–White imprisonment disparities, but unclear by how much.
House File No. 1596 (LY: 2009)	Passed as part of omnibus bill	Marijuana plant possession crime established, and marijuana sale and possession amounts modified.	Combined thresholds for marijuana and methamphetamine/cocaine would result in 11 fewer people in prison and 40 with reduced sentences.	No projected impact on Black–White imprisonment disparities.
House File No. 1199 (LY: 2009)	Failed to pass	Felony domestic assault and domestic assault by strangulation defined as crimes of violence.	Would increase overall imprisonment but unclear by how much.	Would likely increase Black–White imprisonment disparities but unclear by how much.

TABLE 1 (Continued)

Bill origin	Final status	Brief description of proposed policy	Estimated effect on imprisonment	Estimated effect on Black–White disparity
Senate File No. 2790 (LY: 2008)	Passed as part of omnibus bill	Miscellaneous corrections provisions modification.	By deferring judgment for certain drug offenses, should lead to reduction in imprisonment, although unclear by how much.	MSGC staff state the policy would not impact Black–White imprisonment disparities. However, data provided suggest it would increase Black–White imprisonment disparities, but the magnitude is unclear.
House File No. 3175 (LY: 2008)	Failed to pass	Attempted robbery penalty increased, robbery involving a motor vehicle crime created.	Would increase imprisonment, but unclear by how much.	Would increase Black–White disparities in imprisonment, but unclear by how much.
House File No. 3101 (LY: 2008)	Failed to pass	Domestic abuse no contact order violations criminal penalty increased.	Would result in slight increase in imprisonment, with 47 additional individuals a year receiving executed prison sentences.	This policy would very slightly exacerbate Black–White disparities.
House File No. 2949 (LY: 2008)	Failed to pass	Salvia divinorum sale or possession established as a crime.	This policy would increase imprisonment overall, but unclear by how much.	Unclear how it would impact Black–White disparities.

5 | DISCUSSION

In recent years, a large number of seemingly race-neutral criminal legal reform efforts have been enacted (Beckett et al., 2016; Clear & Frost, 2014). Despite these efforts, substantial Black–White disparities in imprisonment persist (Sabol & Johnson, 2022). As a result, scholars have increasingly called for race-conscious criminal legal reforms (London, 2011; Mauer, 2008; Schlesinger, 2011; Van Cleve & Mayes, 2015). Racial impact statement reform has, to our knowledge, been the only state level criminal legal reform that specifically aims to consider race, and there are now 10 states with mechanisms that allow for the production of racial impact statements when criminal legal bills are proposed (Porter, 2021). However, to our knowledge, no empirical scholarship has examined the extent to which racial impact statement reform has impacted Black–White imprisonment disparities.

In this paper, we began to fill this gap by examining two questions: (1) What has been the effect of Minnesota's racial impact statement reform on Black–White disparities in imprisonment and (2) What explains the impact or null effects of Minnesota's racial impact statement reform on Black–White imprisonment disparities? To answer question 1, we relied on state level panel data and a quasi-experimental synthetic control approach. Our findings from these analyses suggest that it is likely that Minnesota's racial impact statement reform has not had an impact on Black–White imprisonment disparities, regardless of whether disparity is measured as a Black–White imprisonment ratio or gap.

To answer question 2, we conducted a legislative analysis of the content of Minnesota's racial impact statements, as well as whether there were any clear patterns in the racial impact statements that predicted bill passage or failure. Our results from this analysis suggest that legislators do not appear to be voting against bills because they will reduce Black–White disparities or voting for bills because they will increase or fail to reduce disparities. Therefore, providing racial impact information does not appear to be harmful toward efforts to reduce Black–White disparities and does not appear to lead to backlash, at least among Minnesota's legislators. Instead, the reform appears to have had minimal impact on Black–White disparities because it is by definition, reactive. Since racial impact statements serve informational purposes only and are produced after a bill has been proposed, their impact on Black–White disparities is greatly impacted by the types of criminal legal bills that are proposed. Notably, only 25% of the bills examined were projected to reduce Black–White disparities in imprisonment. Of those, 0 were projected to reduce the imprisonment rate, a critical issue given that legislators were far more likely to pass bills that reduced rather than increased the imprisonment rate. And, the two bills that were projected to reduce Black–White disparities and passed were very small in scope and unlikely to have a substantial aggregate impact, with each projected to impact fewer than 60 prison beds annually.

In evaluating the significance of these findings, it is important to consider the study's limitations. First, our findings are restricted to Minnesota. Nine other states have since enacted racial impact statement reform, and it is certainly possible that findings would be different in other contexts (Porter, 2021). Second, unfortunately, state level imprisonment data inconsistently reports Latinx ethnicity, which has the potential to bias estimates of Black–White disparities (Nellis, 2016). To address this issue, we conducted two sets of robustness checks and our findings were generally quite consistent with our main specifications. However, we cannot rule out with certainty that these issues with Latinx reporting are impacting our findings. Third, while we did not find evidence that Minnesota's reforms reduced Black–White disparities in imprisonment, it is certainly possible that the reforms reduced Black–White disparities for other criminal legal outcomes. For instance, our analyses did not allow us to look at how racial disparities in punishment were impacted post-reform for specific offenses. It is certainly possible that the lack of aggregate effects could mask progress across specific offense types (e.g., disparities in imprisonment for drug offenses). Fourth, our findings focus on the state level implications of

Minnesota's racial impact statement reform. While this level of analysis is appropriate (since all racial impact projections were for state level policies and were state level projections), it is certainly possible that it masks local variation in the implementation of state laws (Verma, 2015).

Last, the synthetic control approach relies on the assumption that without the implementation of the racial impact statement reform, Minnesota would have followed a similar path to its synthetic control group in the outcomes of interest in post-reform years. This assumption is untestable. It is certainly possible that some synthetic control states enacted reforms that Minnesota would not have enacted (without racial impact statement reform) that may have reduced disparities and, that this, in turn, is masking potentially positive impacts of Minnesota's racial impact statement reform. However, we do not believe this to be the driving force behind our null findings for two reasons. First, our analysis of legislation that was proposed and passed in Minnesota suggests that only a small share of bills were projected to reduce Black–White disparities. Moreover, the bills that were projected to reduce disparities tended to be small in scope and were projected to impact a small number of people. Therefore, there is little reason to believe that the totality of Minnesota's legislative actions post-reform would lead to reductions in Black–White disparities. Second, prior scholarship also suggests that reforms enacted in synthetic control states are unlikely to explain the null findings. Specifically, Lerman and Mooney (2022) show that states that have reduced their prison populations the most (presumably those that made the most substantial reforms) did not see larger reductions in racial disparities in imprisonment than states that failed to reduce their prison populations.

Despite these limitations, we believe our paper has important implications for criminal legal reform efforts. First, racial impact statements serve an important informational function. By providing information on how proposed bills are expected to impact imprisonment, as well as racial disparities in imprisonment, these statements have the potential to enable legislators to make more informed decisions (Erickson, 2014; London, 2011; Mauer, 2008). However, based on our review of racial impact statements in Minnesota, we have a few suggestions for how to ensure that these statements meet this potential. Specifically, each statement should make projections not just about whether a bill is expected to impact disparities, but what the magnitude of the projected effect would be. These magnitudes should be reported both for incarceration ratios and gaps, and for a number of time frames (e.g., how are Black–White imprisonment rate disparities projected to be impacted by this bill 1 year after passage, 5 years after passage, and 10 years after passage, etc.). Moreover, these statements should be made readily available to the public, so that they can hold legislators accountable for how they vote.

Second, while racial impact statements serve an important informational purpose, advocates and policymakers should not rely on this approach as a primary means to ensure that Black–White imprisonment disparities are reduced. The reactive nature of this approach, by definition, means that the ability of racial impact statements to reduce Black–White imprisonment rate disparities is significantly impacted by the nature of the bills that are proposed (regardless of how important a role the statements play in voting patterns of legislators). Therefore, advocates who care about reducing Black–White imprisonment rate disparities (in conjunction with imprisonment rates) need to shape bills before they are proposed to make sure that they are addressing Black–White disparities.

And the types of bills advocates need to push for would represent a significant departure from the status quo. Namely, reforms would need to move away from efforts that purely focus on nonviolent offenses (Gottlieb et al., 2021; Rehavi & Starr, 2014). Instead, reforms would need to change how we punish violence, and reduce/eliminate sentence enhancements for prior criminal legal involvement (Hester et al., 2018; Rehavi & Starr, 2014). Whether legislators would be willing to get behind these reforms is separate question. It is unclear why these types of reforms have not been proposed in Minnesota—whether these types of changes are not palatable to legislators, whether legislators are just not aware of how impactful these changes would be or whether legislators are not being pressured by members of their community to enact this

type of change. However, to both reduce imprisonment and reduce Black–White disparities in imprisonment substantially, these are the types of changes to the criminal legal system that would need to be undertaken.

DATA AVAILABILITY STATEMENT
The manuscript relies on publicly available data. Information on how to access data from each source is provided in the manuscript.

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APPENDIX

T A B L E A 1 Pre-treatment Black–White imprisonment rate ratios in Minnesota and its synthetic control.

	Minnesota	Synthetic Minnesota
2000	16.727	14.857
2001	14.561	13.345
2002	13.102	12.558
2003	11.111	11.956
2004	10.137	12.113
2005	10.979	11.269
2006	10.758	11.246
2007	10.799	10.870

Note: Minnesota's Synthetic Control = Pennsylvania = .19, Utah = .134, Wisconsin = .676.

T A B L E A 2 Pre-treatment Black–White imprisonment rate gaps in Minnesota and its synthetic control.

	Minnesota	Synthetic Minnesota
2000	1116.694	1115.575
2001	1110.576	1108.668
2002	1091.726	1090.690
2003	1071.303	1070.568
2004	1064.982	1064.155
2005	1085.826	1083.765
2006	1017.829	1019.511
2007	1038.182	1036.155

Note: Synthetic Minnesota: Florida = .026, Georgia = .037, Massachusetts = .083, Mississippi = .082, North Dakota = .108, South Dakota = .176, Tennessee = .452, Texas = .035.
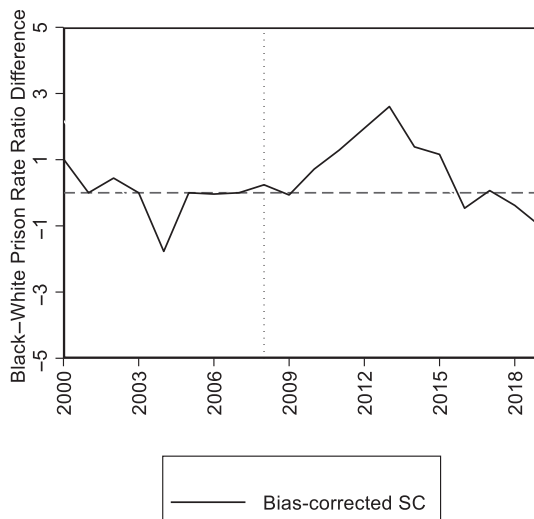

F I G U R E A 1 Bias-corrected estimates of effect of Minnesota's reform on the Black–White imprisonment rate ratio with covariates capturing missing Latinx data. Minnesota's synthetic control = Pennsylvania = .393, Utah = .10, Wisconsin = .507.
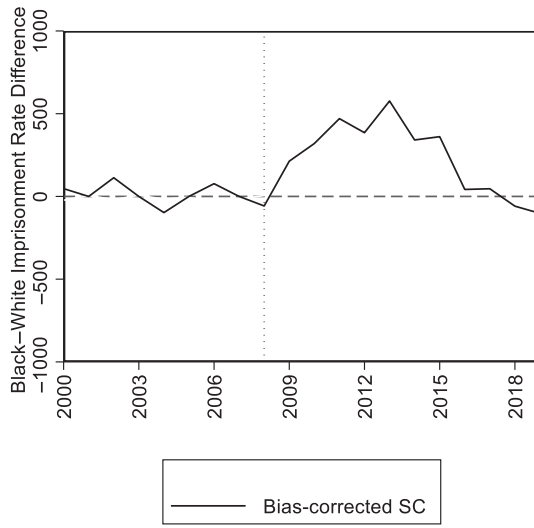
FIGURE A2 Bias-corrected estimates of effect of Minnesota's reform on the Black–White imprisonment gap with covariates capturing missing Latinx data. Minnesota's synthetic control = Delaware = .170, Hawaii = .199, Massachusetts = .203, New Hampshire = .220, New Mexico = .040, South Dakota = .168.
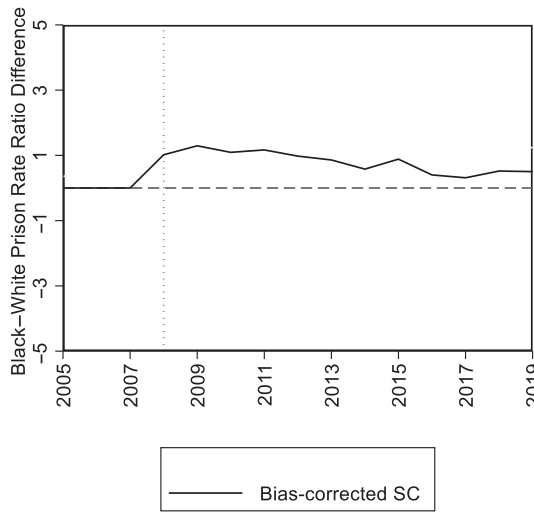

FIGURE A3 Bias-corrected estimates of effect of Minnesota's reform on the Black–White imprisonment ratio on sample without missing Latinx data from 2005 to 2019. Minnesota's synthetic control = Pennsylvania = 1.
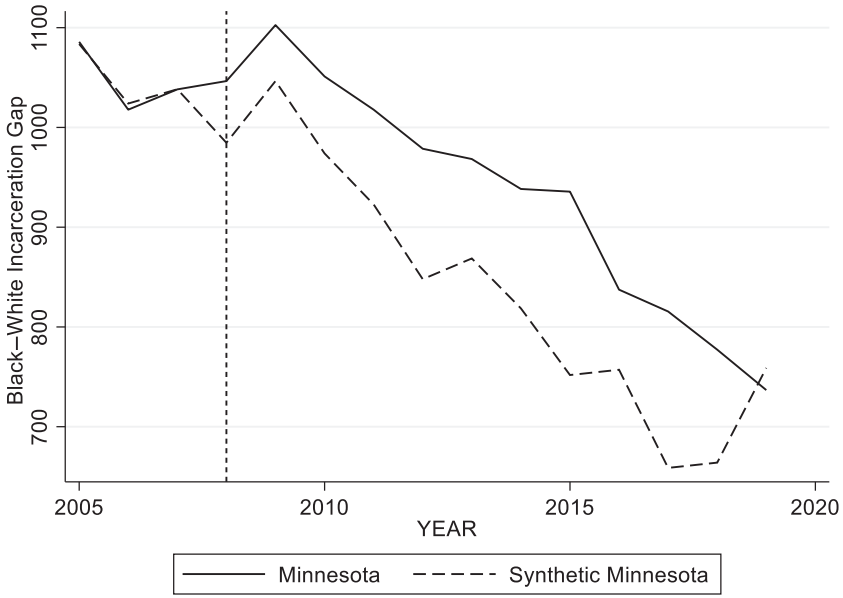
FIGURE A4 Synthetic control estimates of effect of Minnesota's reform on the Black–White imprisonment ratio on sample without missing Latinx data from 2005 to 2019. Minnesota's synthetic control = Florida = .267, Hawaii = .486, Texas = .248.