DOI: 10.1111/1745-9133.12518

CRIMINOLOGY & Public Policy

ORIGINAL ARTICLE

Effects of restorative justice pre-file diversion legislation on juvenile filing rates: An interrupted time-series analysis

Shannon M. Sliva[1] | **Mark Plassmeyer**[2]

[1] University of Denver

[2] University of Arkansas

Correspondence
Shannon M. Sliva, University of Denver.
Email: shannon.sliva@du.edu

Research Summary: This study uses an interrupted time-series design to analyze the impacts of legislatively supported restorative justice pilot projects in Colorado on overall juvenile filing rates in implementing districts, and it compares those outcomes with nonimplementing districts. On average, pilot districts demonstrate statistically significant reductions in juvenile filing rates during the first year of funding, and significant rates of decline are sustained in the years after initiation. The effects of restorative justice pre-file diversion systems, however, vary by judicial district, and they can be partially explained by differences in implementation.

Policy Implications: Restorative justice pre-file diversion programs accompanied by funding supports and accountability measures—introduced by the Colorado legislature in 2014—are associated with varying declines in juvenile court filings. Implementation matters, however. States considering the use of similar reforms should account for how to best support pilot sites' localized characteristics. Funding restorative justice pre-file diversion presents one mechanism for reducing court involvement, but it should not be offered as a panacea.

KEYWORDS
criminal justice policy, decarceration, diversion, juvenile justice, policy impacts, restorative justice

Restorative justice practices have been posed as a strategy for reducing system involvement and lowering costs. Although there is evidence that restorative justice juvenile diversion programs improve individual-level outcomes for participants, there is little research documenting the system-level effects of restorative justice policy initiatives in juvenile diversion. In 2013, the Colorado legislature authorized the creation of a pilot program, funding restorative justice pre-file diversion programs in 4 of the state's 22 judicial districts. Evaluation reports show that Colorado's restorative justice-based pilot projects have an 8.2% recidivism rate, compared with 12.8% for juvenile diversion programs without a restorative justice component across the state. It is unknown, however, whether the pilot policy has had its aspirational effect of reducing system involvement for juveniles in targeted districts, whether through initial diversion, subsequent reductions in recidivism, or related culture shifts in prosecution practices.

In this study, we use an interrupted time-series design—a commonly employed method for examining policy effects—to analyze the impacts of pilot legislation on overall juvenile filing rates in implementing districts compared with other districts across the state of Colorado. We then offer possible explanations for the patterns evidenced by the time-series models and cautiously articulate implications for policymaking and policy implementation. Finally, we suggest further inquiries that may improve the understanding of how statutory support for juvenile diversion approaches, restorative or otherwise, impact system involvement.

1 | BACKGROUND

1.1 | Juvenile case filing and diversion

Around the turn of the twentieth century, progressive reformers forwarded the juvenile court system as a separate system of justice designed to meet the unique needs of youth (Butts, 2016). Juvenile courts are framed as offering more supportive and rehabilitative responses to crimes committed by youth as a means of disrupting criminal trajectories or preventing more serious crime. Perhaps more realistically, they are characterized by a dual orientation toward punishment and social welfare (Mears et al., 2016). In part prompted by the Juvenile Justice and Delinquency Prevention Act of 1974, juvenile diversion gained popularity in the 1970s as a strategy to reduce system involvement of youth while promoting early intervention and reducing recidivism (Stafford, 2016). Additionally, diversion was seen as a way to reduce court burden (Pogrebin, Poole, & Regoli, 1984).

The National Juvenile Defender Center (NJDC) defines diversion as "any program that is an alternative to the filing of a court petition and that keeps the youth from entering the juvenile court system by referring the child to counseling or other social services" (2019, para. 22). Accordingly, the term is used to describe various informal and formal practices and programs ranging from civil citations to specialty courts to probation (Stafford, 2016). "True diversion" occurs before formal charges are filed, although the term is also sometimes used to describe deferred adjudication or other instances of cases being diverted from formal court processing or punitive sentencing (Ray & Childs, 2015). Cases targeted for diversion are offered an alternative agreement, usually consisting of community service, restitution, restorative justice, and/or individual or family therapy (Schwalbe et al, 2011; Wilson & Hoge, 2013). Upon successful completion of the diversion agreement or contract, a petition—the equivalent to a complaint in an adult criminal case—is not filed. If the agreement is not satisfactorily completed, a petition is filed and the case is processed by the court.

Prefile or precharge diversion has various perceived benefits: It promises to reduce court filings, reduce justice involvement for youth, and improve outcomes related to reoffending. A meta-analysis conducted by Wilson and Hoge (2013) reviewed 45 diversion evaluation studies encompassing 73 programs and found that diversion was associated with reduced recidivism on average compared with conventional justice processes. The literature, however, is far from conclusive. Schwalbe et al.'s (2011) meta-analysis of 28 studies involving experimental comparisons of 19,301 youth discovered that, on the whole, diversion did *not* have a statistically significant effect on recidivism: Rather, recidivism rates for experimental and control conditions were similar. The effects of diversion on recidivism, however, were significant in the instances of family treatment programs, as well as of some restorative justice programs—specifically, those indicating strong fidelity monitoring by researchers (Schwalbe et al., 2011).

Although scholars attempt to discern the efficacy of diversion, they also note that juvenile diversion may do harm (Mears et al., 2016). Rather than reducing system involvement, it may promote intervention where previously none was required, in effect widening and strengthening of the net of State control (Austin & Krisberg, 1981). Net-widening occurs when the system increases interaction with cases that would have otherwise been dealt with informally or dismissed. There is evidence, for instance, that juvenile justice reforms in San Francisco during the 1990s increased the use of diversion and deferred prosecution practices but reduced neither the juvenile detention population nor racial disparities in the population (Macallair & Males, 2004). Other studies of restorative justice diversion programs in Canada (Bonta, Wallace-Capretta, Rooney, & McAnoy, 2002) and Australia (Prichard, 2010) have not found evidence of a net-widening effect. Morris (2002) contended that the net-widening claim is entirely dependent on the focus and implementation of restorative justice diversion programs, such as whether they are used to address serious offenses or only minor offenses.

1.2 | Restorative justice in diversion

As indicated above, diversion agreements may include therapeutic or treatment programs as well as reparations or restorative practices. The use of restorative justice practices is increasingly common in diversion programming. Restorative justice is a paradigm and a set of practices that focuses on the harms caused by crime and other conflicts, and it emphasizes the repair of harm as the central aim of justice (Zehr, 2014). Within the criminal legal system, common restorative justice practices include victim–offender dialogues, community group conferences, and sentencing circles (Umbreit & Armour, 2010). These practices bring together the persons harmed with the persons responsible to engage in dialogue about the harm, its impacts, and the obligations created by it (Zehr, 2014). Conferences may include other community members who have a stake in the issue. Often, they result in a collaborative agreement about what the responsible person will do to repair the harm as much as possible. In court and correctional settings, agreements may be used as an alternative or supplement to traditional requirements of diversion, sentencing, or community supervision (Reimund, 2005).

The use of restorative justice is theorized to have several positive outcomes, including increasing the empathy of defendants who have caused harm, meeting the needs of crime victims for direct accountability, addressing community-level harms, and reducing reoffending (Sliva, Porter-Merrill, & Lee, 2019). Latimer, Dowden, and Muise (2005) completed a meta-analysis of 22 studies examining 35 restorative justice programs implemented at various stages of system involvement. Where victim satisfaction was measured, they found that victims participating in restorative

justice were more satisfied than those in comparison groups in all but 1 of 13 programs. The authors also concluded that, across studies, restorative justice programs have a "moderate-to-weak positive impact" on the satisfaction of offender participants, as well as a clear positive impact on recidivism rates. They further noted, however, the challenge of self-selection bias across many of the studies examined. Sherman & Strang's (2007) more targeted systematic review of randomized trials of restorative justice conferences discovered a "modest but highly cost effective" reduction in recidivism among randomly assigned participants.

As a result of the evidence supporting the effectiveness of restorative justice practices, it is frequently offered as a viable justice alternative with the likelihood of reducing incarceration—both through diverting offenders from the courts and by reducing recidivism rates (Robinson & Shapland, 2008). In short, it is presented as a decarceral strategy or, at least, as a strategy capable of reducing court filings. Between 1988 and 2014, 32 U.S. states adopted one or more statutes supporting the use of restorative justice in their criminal or juvenile codes, with the most common usage—in 21 states—supporting the use of restorative justice for diversionary or pretrial processes (Sliva & Lambert, 2015). In these settings, juvenile defendants meet with direct or surrogate victims (victims of similar crimes in another case) or with community members impacted by the crime to form a restorative agreement. If the agreement is completed to the satisfaction of the participants, the case is not formally filed.

Despite the interest in restorative justice as a policy practice, the small but consistent body of evidence offered for restorative practices is conducted almost entirely at the program level. It is unclear whether statutory or other system-level approaches effectuate theorized outcomes like reducing court-involved or incarcerated populations. Critics of restorative justice as a decarceral or system-shrinking strategy point to the likelihood that restorative justice diversion, like other forms of diversion, can result in net-widening (Morris, 2002).

Certainly, more evidence is needed to understand whether the positive effects of restorative justice diversion programs translate to theorized outcomes, or whether they are confounded by other factors. A likely reason for dearth of research on this topic is the limited opportunity to study the influences of restorative justice practices at a system level. Most jurisdictions use restorative practices in disparate ways. They develop and expand programs at different points in time using different practices and different eligibility standards. Some offer restorative justice practices during pre-file diversion, whereas others offer diversion after filing, or a mixture of the two. Colorado offers a promising opportunity to consider the effects of instating a well-defined restorative justice diversion program across multiple jurisdictions.

1.2.1 | Colorado's restorative justice pilot projects

In 2013, Colorado House Bill 13–1254 established a state pilot program for juvenile pre-file diversion using restorative justice. The bill identified four districts as the pilot sites, establishing two new programs in the 10th and 19th districts and expanding two existing programs in the 12th and 20th. In these sites, district attorneys would identify juveniles facing first-time charges of nontraffic misdemeanors or Class 3, 4, 5, and 6 felonies to screen them for participation. Upon satisfactory completion of the restorative justice diversion program, no charges would be filed. The resulting statute (C.R.S.A. § 19-2-510.5, 2014) mandated the collection of data—with required reporting to the Division of Criminal Justice—to assist the state in determining the ability of the program to reduce costs, lower recidivism rates, and improve the well-being of victims and offenders. Required data points included the number and demographics of juveniles who met the

TABLE 1 Restorative justice pre-file diversion pilot project funding awards

Judicial District	4/1/2014 – 6/30/2014	7/1/2014 – 6/30/2015	7/1/2015 – 6/30/2016
	Actual Funding		
10th Judicial District	$39,290	$12,375	$0
12th Judicial District	$30,245	$155,296	$155,296
19th Judicial District	$16,500	$123,000	$237,904
20th Judicial District	$81,200	$227,850	$205,100
	Population Adjusted per 10k Youth		
10th Judicial District	$9,893	$3,116	$0
12th Judicial District	$25,530	$131,085	$131,085
19th Judicial District	$2,096	$15,626	$30,224
20th Judicial District	$11,837	$33,216	$29,900

program criteria, did or did not participate, reached reparation agreements, and completed the agreements, as well as rearrest rates, and the results of victim and offender satisfaction surveys. Later, in 2015, the Colorado legislature adopted House Bill 15–1091, expanding the eligibility of the restorative justice pilots to allow petty and municipal charges, and enabling district attorneys to use their discretion to waive the first offense limitation.

HB13-1254 was signed into law by the governor on May 28, 2013. Following administrative procedures, the four pilot sites received funding to begin operating their projects on April 1, 2014 (Restorative Justice Coordinating Council, 2016). All sites used a community group conference model, in which juvenile defendants meeting the legislated eligibility criterion were offered the opportunity to participate in a facilitated restorative justice conference with the victim or a victim representative, support persons of both defendant and victim, and other community volunteers. Successful completion was documented by the completion of a restorative agreement developed during the conference.

Despite the common framework, from the first year of funding onward, there was a wide range of variation between jurisdictions in the implementation of the pilot projects. First, as noted, two sites had small existing programs that they were able to expand with funding support from the new statute, and two sites developed new programs using pilot funding. The 12th Judicial District established a referral process with a community provider, whereas the 20th Judicial District expanded an in-house program administered by district attorney's office staff (Sliva et al., 2019). The 19th Judicial District utilized a hybrid model, establishing a county program within a local nonprofit. Pilot sites also received different amounts of funding in their first and subsequent years, based on the need projected by each district (see Table 1). As a result of various factors, including district size, community buy-in, and implementation strategies, districts also served varying numbers of juveniles in the first 2.5 years of the pilot, ranging from 15 in the 10th Judicial District to 82 in the 20th Judicial District. In March 2016, pilot funding to the 10th Judicial District was discontinued because of the low number of juveniles served and primarily unspent funds. As a result, only the remaining three judicial districts are considered as "pilots" in this analysis, not only because implementation in the 10th Judicial District was considered failed altogether, but because the 10th participated in less than 2 years of the 5-year pilot project, serving only 16 youth, before its participation was formally concluded (Omni Institute, 2016).

2 | RESEARCH QUESTIONS

The aim of this study is to examine the influence of statutorily created pre-file juvenile diversion pilot projects in three Colorado judicial districts on overall juvenile filing rates at the judicial district level and to compare these effects with nonimplementing jurisdictions in the state. We focus on juvenile filing rates as the outcome of interest because we are interested in discerning whether the pilot program had a *system-level* effect on court involvement of juveniles in the targeted districts. This is markedly different from measuring a *case-level* outcomes such as agreement completion or re-arrest. We've developed this focus for various reasons. First, case-level outcomes of Colorado's pilot projects have already been documented (OMNI Institute, 2018). Although positive, these case-level outcomes do not ensure system-level effects. Rather, they may be too limited in scope or confounded by problems associated with selection bias or net-widening.

Furthermore, case-level outcomes are proximal, whereas system-level effects are distal. Although measuring system-level effects introduces several limitations, such as ensuring that the effects are caused by the policy under study, this approach also introduces the possibility of capturing complex and distal effects of policy change, such as prompting new organizational behaviors and decision-making in prosecutors' offices, reducing future crimes or arrests by activating community support networks, and so on. Restorative justice is theorized not merely as a program but as a philosophy or, as Umbreit and Carey (1995) put it: "[A] way of thinking, a way of behaving, and a way of measuring" (p. 48).

Here, we attempt to focus on capturing the system-level effects of a policy intended to influence a shift toward restorative justice approaches. Therefore, this study engages with the following questions:

1. What is the effect of statutorily supported restorative justice pre-file juvenile diversion pilot programs on juvenile filing rates in the piloting judicial districts?
2. How do the juvenile filing rates in pilot districts compare with juvenile filing rates in nonimplementing districts across the state during the same time period?

2.1 | Hypotheses

Our hypotheses are as follows:

(1A) Districts implementing pilot programs will have an immediate decline in juvenile delinquency filings at the end of 2014, the year in which funding begins.
(1B) Districts implementing pilot programs will have declines in juvenile delinquency filings in the years post-2014.
(2A) Overall, districts implementing pilot programs will have a greater immediate declines in juvenile delinquency filings than will nonimplementing judicial districts.
(2B) Overall, districts implementing pilot programs will have different trends in juvenile delinquency filings in the years post-2014 than will nonimplementing districts, specifically, greater declines.

3 | METHOD

This study employs two interrupted time series analyses: 1) single-group time-series analyses of pilot judicial districts, comparing juvenile justice filing trends over time before and after the passage of restorative justice legislation in 2014; and 2) multigroup time-series analyses comparing juvenile justice filing trends in pilot judicial districts with nonimplementing districts in Colorado before and after the passage of restorative justice legislation in 2014. This analysis uses publicly available data on juvenile filing rates from the state of Colorado at the judicial district level.

3.1 | Sample

The sample for this analysis consists of 22 judicial districts made up of 1 or more of Colorado's 64 counties. The "treatment" group for this analysis consists of the three judicial districts who implemented restorative justice juvenile diversion pilot programs under legislation created by HB13-1245: the 12th Judicial District, the 19th Judicial District, and the 20th Judicial District. The comparison group consists of the remaining 19 nonimplementing districts. A supplemental analysis is conducted using the 10th Judicial District, which was initially implicated in HB13-1245 but did not successfully implement the pilot project, as a comparison. We identify 2014 as the time variable for the analysis as this represents the first year of funding for the pilot legislation. Although the 12th and 20th districts did have less structured existing programs prior to 2014, the purpose of this analysis is to assess the effect of legislative support and funding—in short, to assess the effects of the policy change, not the programs implicated by the policy change.

3.2 | Data and measures

The data for this study are drawn from the annual statistical reports of the Colorado Judicial Branch (2005–2018) and population reports from the Colorado Department of Local Affairs (2005–2018). We incorporate annual data from 2005 through 2018 as monthly data are not available. This results in 10 observations before and four observations after the 2014 implementation date for the pilot project legislation. The number of observations accommodated by annual data points limits the sensitivity of this analysis but provides a starting point for exploring the research questions we have posed. Available data points do allow us to establish both pre- and post-legislation trends in juvenile delinquency filings per 10,000 youth.

3.2.1 | Dependent variable

The dependent variable in all analyses is the rate of juvenile delinquency filings per 10,000 youth (JDF). A standard variable is constructed using the number of juvenile delinquency filings reported annually in each judicial district. This number is then divided by youth population estimates for residents aged 10–18 in each judicial district, divided by 10,000. We use the ages 10–18 as these are the legal ages that a person can be charged as youth in Colorado.

3.2.2 | Demographic variables

We also use multiple variables gleaned from the Colorado Department of Local Affairs in 2014 as demographic variables to allow for some basic descriptive statistics and to gain a better understanding of any differences in the basic makeup of pilot districts compared with nonimplementing districts at the time of implementation. These variables include population characteristics classically tested as determinants of sentencing and correctional policy in empirical literature, including population density, race, and poverty level, as well as crime rates and incarceration rates (Sliva, 2016). In addition, we account for gender as the role of gender in juvenile justice decision-making is an increasing area of focus in the literature (Bryson & Peck, 2020).

We include the following demographic variables: the percentage of female youth aged 10 to 18 (youth), the percentage of people in the judicial district classified as living in rural areas (where rural = < 2,500 people), the percentage of youth living in poverty, the percentage of Latinx youth, the percentage of Black youth, the percentage of White youth, the juvenile crime rate, the average daily population (ADP) of committed youth from the each district, and the ADP of youth in detention from each district. Both ADP variables are measured as actual numbers.

3.3 | Analyses

3.3.1 | Hypothesis tests

First, to address the likelihood of differences between the pilot districts and nonimplementing districts, we ran *t* tests using SPSS 26 to assess any significant differences in the starting year (2014) demographic makeup of pilot districts and nonimplementing districts. We then conducted single- and multigroup interrupted time-series analyses to test our hypotheses. Lopez Bernal, Cummins, and Gasparrini (2017) recommended that before running an ITSA, researchers specify what type of change they expect to see in the data and when they expect to see it. Researchers must decide whether they expect to see an immediate impact or a delayed impact as a result of onset of an intervention, as well as whether that impact will be a change in the slope/trend of the data or whether it will be a change in the intercept/level of a given outcome. These decisions are based on the available data and factors related to the intervention (Lopez Bernal et al., 2017).

Given that pilot districts received funding for 8 months in 2014 and that our data on juvenile delinquency filings are yearly totals, we hypothesized that there would be an immediate impact in JDF and that JDF would drop significantly in pilot districts at the end of 2014, as well as that after 2014, declines would be sustained or grow modestly. Furthermore, we hypothesized that any immediate decline in JDF would be more pronounced in pilot districts than in nonimplementing districts and that, after 2014, JDF would decline at a greater rate in pilot districts than in nonimplementing districts. This is important as JDF had already been declining in Colorado for at least 10 years prior to 2014 and were likely to continue declining at some level. Also for this reason, we did *not* hypothesize that post-intervention trends would be statistically "steeper" than pre-intervention trends—another common ITSA hypothesis. This is because, on average, neither funding levels nor youth served consistently demonstrate annual increases following initiation of the pilot; therefore, there is no practical implication for this sort of ongoing trend.

All interrupted time-series analyses were run using STATA Version 15 with the user-developed software ITSA (interrupted time-series analysis). This command was developed by Linden (2015, 2018), who has authored papers providing syntax as well as upgrades that make the ITSA software useful for those running interrupted time-series models for both single and multiple groups. We incorporate single-group designs to examine the impact of restorative justice funding within the pilot districts, as well as multigroup designs to assess differences between the pilot district trends and nonimplementing district trends. The ITSA syntax requires that the analyst identify an *intervention period*, a *time variable*, and a *panel* (grouping) *variable*. In our models, the intervention period is the year 2014, the time variable is 1 year (2005–2018), and the variables are grouped by judicial district. The ITSA command offers the option of running ordinary least-squares (OLS) regressions that incorporate Newey-West standard errors that account for heteroskedasticity and autocorrelation. Newey-West standard errors also allow for a straightforward interpretation of the OLS regression results (Linden, 2015). Ultimately, we assess changes in JDF over time and report mean rates, p values, and confidence intervals in the Results section.

The resulting models for the single-group time-series design are expressed in the following form:

$$Y_t = \beta_0 + \beta_1 T_t + \beta_2 X_t + \beta_3 X_t T_t + \varepsilon. \quad (1)$$

Here β_0 is indicative of the original intercept or starting rate of JDF in 2005, β_1 represents the slope or trend of JDF up until 2014, β_2 is the change of JDF in the year immediately after implementation of the pilot program in 2014, and β_3 is the coefficient representing the difference between the slope/trend of JDF up until 2014 and the slope/trend of JDF starting in 2014. Significant p values for β_2 indicate an immediate impact of the pilot legislation, whereas a significant p values for β_3 are indicate the impact of pilot legislation over time (Linden, 2015).

In the case of the multigroup time-series analyses, four more terms are added to the equations for the models that compare JDF in pilot districts with nonimplementing districts. The expanded equation takes the following form:

$$Y_t = \beta_0 + \beta_1 T_t + \beta_2 X_t + \beta_3 X_t T_t + \beta_4 Z + \beta_5 Z T_t + \beta_6 Z X_t + \beta_7 Z X_t T_t + \varepsilon_t \quad (2)$$

The new coefficients represent differences between the slopes and intercepts of the pilot districts and those in the nonimplementing districts Here β_4 is the difference between the starting JDF or the original intercepts in 2005, β_5 is the difference between the slopes/trends of JDF prior to the implementation of the pilot/funding in 2014, β_6 represents the difference in the immediate change in rates of JDF after pilots legislation is implemented in 2014, and β_7 is the difference between the nonimplementing districts and the pilot district slopes/trends of JDF after 2014 compared with the pre-2014 slopes/trends of JDF. Significant p values for β_6 indicate a significant difference in the immediate change in JDF after pilots/funding is introduced in 2014, whereas significant p values for β_7 indicate a pronounced difference between nonimplementing and pilot district slopes/trends of JDF post-2014 compared with the pre-2014 slopes/trends (Linden, 2015).

The ITSA command also offers the option of assessing the significance of the post-intervention slope/trend in the case of the single-group analyses and the ability to compare post-intervention trends for the multigroup analyses. Coefficients for the post-intervention trend(s) are calculated by combining the coefficients of β_1 and β_3. The coefficient for the difference between

post-intervention slopes/trends is calculated by combining β_5 and β_7. This study incorporates both options to assess our second hypothesis.

3.3.2 | Special considerations

Linden (2015) noted that many ITSA designs rely on what are known as naïve designs in that they do not account for differences in the intercept or the slope of comparison groups on the outcome to the intervention period. This is problematic when testing for the effects of an intervention or policy change on any differences between the treated and the control variable in immediate changes in the outcome at the point of intervention (intercept) or the trend (slope) of the outcome compared with the pre-intervention period. Although we were not able to statistically match pre-intervention intercepts (starting rates of JDP on average per year) for either the analysis assessing differences between pilots and nonimplementing districts ($p < .001$) or differences between pilots and the 10th Judicial District ($p < .001$), we were able to ascertain that differences between pre-intervention slopes were not statistically significant prior to the intervention period in 2014 in both the analysis comparing pilots with nonimplementing districts ($p = .19$) and the analysis comparing pilots with the 10th Judicial District ($p = .43$). This improved our ability to assess significant differences in slopes between the nonimplementing district trend and the three pilot judicial districts' trends post-intervention. Our models test for immediate impacts of the legislation, as well as for the differences between the trends in the pilot judicial district post-2014 compared with the nonimplementing district trends of JDF on average per year post-2014.

It is critical that ITSA models be tested for autocorrelation in the error distribution post-analysis, and Linden (2015) pointed to another user package developed for STATA to complete these analyses. ACTEST (Baum & Schafer, 2013) provided important information regarding the existence of autocorrelation using the Cumby–Huizinga test for autocorrelation (Cumby & Huizinga, 1992). This test allows researchers to correct for the presence of autocorrelation in the data. Researchers then specify the highest numbered lag where autocorrelation is present in their models, which accounts for autocorrelation in any of the preceding lags. Accounting for autocorrelation provides accurate estimates, confidence intervals, and a more precise model overall (Linden, 2015). We used ACTEST to assess autocorrelation in all our models and incorporated the appropriate number of lags in each model to account for any presence of autocorrelation.

Although each analysis incorporates multiple hypothesis testing, most are not of concern to our specific hypotheses or are diagnostic in nature; therefore, we did not incorporate corrections for multiple hypothesis tests in our models (Sedgwick, 2014; Steiner & Norman, 2011). Steiner and Norman (2011) stressed that in research using ITSA to test specific hypotheses, it is not necessary to incorporate corrections, particularly when the work is more exploratory in nature and intended to produce a preliminary understanding of a phenomenon or generate hypotheses for future research. This is a result of the chance of missing out on actual positives or increased type 2 errors. Our specific hypotheses in the single-group design of changes in the immediate amount of JDF in 2014 and of declines in JDF post-2014 fit this mold, as do our hypotheses in the multigroup designs, which test for a larger immediate change in JDF in pilot districts and a significant difference of trends in pilot districts compared with nonintervention districts post-2014. In the Results section, we offer explanations of relevant tests along with the output tables.

TABLE 2 Descriptive statistics and t scores

Variable	Pilot Districts ($n = 3$) Mean (SD)	Other Districts ($n = 19$) Mean (SD)	*t* Test
Percent Female Younger Than 18	49.16 (.004)	48.79 (.009)	1.21
Percent Rural	32.81 (31.92)	32.08 (22.89)	–.01
Percent Living in Poverty	21.20 (8.76)	20.09 (8.18)	.27
Average Daily Committed Juveniles	12.40 (6.97)	15.13 (7.84)	–.63
Average Daily Detained Juveniles	4.50 (2.56)	5.15 (2.55)	–.34
Juvenile Crime Rate (Per 100000)	18.79 (14.43)	19.60 (15.16)	–.19
Percent White Younger Than 18	54.67 (14.84)	61.29 (14.88)	–.85
Percent Black Younger Than 18	1.07 (0.53)	2.11 (2.84)	–1.39
Percent Latinx Younger Than 18	40.87 (17.75)	32.28 (14.65)	.92
Total Population Younger Than 18	53052.33 (36041.69)	60781.58 (74107.32)	–.32
JDF during 2014	152.67 (60.62)	153.39 (84.45)	–.02

$^{*}p < .05$. $^{**}p < .01$. $^{***}p < .001$.

4 | RESULTS

4.1 | Descriptive statistics and *t* tests

The descriptive statistics and results of *t* tests comparing the demographics of pilot and nonimplementing districts are available in Table 2. Overall, there were no statistically significant differences between any of the demographic variables in pilot districts versus nonimplementing districts. The nonimplementing districts, however, were somewhat larger and had somewhat larger White populations than did the pilot districts on average.

4.2 | Single-Group ITSA (Individual Pilots)

4.2.1 | Juvenile delinquency filing rates in district 12

In the 12th Judicial District, there is evidence (presented in Table 3) of a significant immediate change (X) in JDF in the first year that funding was available. Here JDF fell by nearly 75 filings (or ~38%) in the first year alone. Our second hypothesis is not supported, however, as the post-2014 slope (T+XT) does not demonstrate a statistically significant decline ($p = .99$); rather, it is nearly flat, with a coefficient of .01. These findings are also visualized in Figure 1, where there is a clear drop at the intervention point, and the slope starting in 2014 is nearly flat (see Figure 1 and Table 3). Therefore, our hypotheses are only partially supported in the 12th Judicial District; there is support for the first, but not for the second, single-group hypothesis.

4.2.2 | Juvenile delinquency filing rates in district 19

In the 19th Judicial District, there is no evidence for a statistically significant immediate change in JDF (X; $p = .16$), although JDF drop by approximately 38 filings (or a little more than 15%) in the

TABLE 3 Results for single-group interrupted time-series analyses

Coefficient	Immediate Change (X)	Post-2014 Slope (T+XT)
	12th Judicial District	
Coefficient	−74.70***	.01
(95% Confidence Interval)	(−118.29, −31.12)	(−8.94, 8.96)
	19th Judicial District	
Coefficient	−38.30	−19.94***
(95% Confidence Interval)	(−94.16, 17.55)	(−23.29, −16.59)
	20th Judicial District	
Coefficient	−21.49*	−2.48
(95% Confidence Interval)	(−42.79, −.19)	(−10.41, 5.45)

*$p < .05$. **$p < .01$. ***$p < .001$.

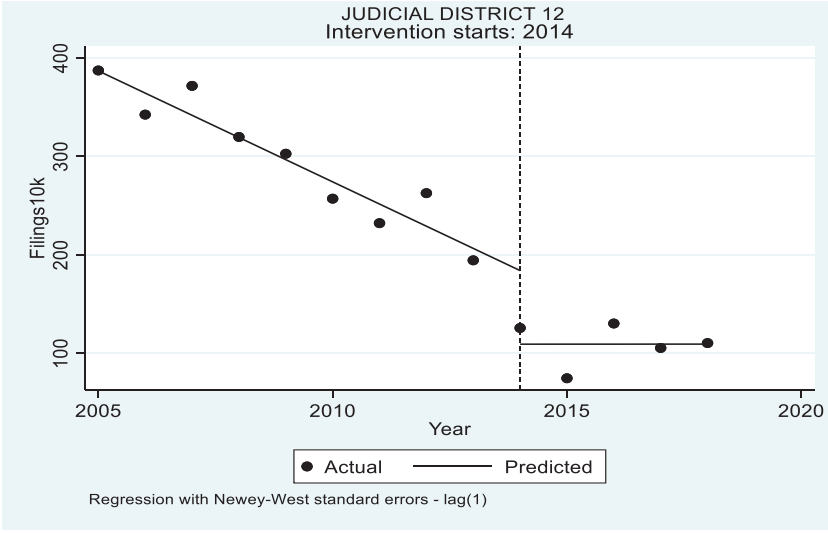

FIGURE 1 Juvenile filing rates in the 12th judicial districts, 2005–2018 [Color figure can be viewed at wileyonlinelibrary.com]

first year of implementation. In the 19th Judicial District, however, the post-2014 slope (T+XT) does demonstrate a statistically significant decline, with JDF declining by an average of nearly 20 filings a year. Here, our first single-group hypothesis is not supported, whereas the second is (see Figure 2 and Table 3).

4.2.3 | Juvenile delinquency filing rates in district 20

In the 20th Judicial District, there is evidence of a statistically significant immediate change in JDF (X) at the end of 2014 where filings dropped by 21.5 (or ~15%). Our first single-group hypothesis is supported; however, our second is only partially supported, as the post-2014 slope (T + XT; $p = .50$) does not demonstrate a statistically significant decline. JDF do decline after 2014, albeit only by ~2.5 per year on average (see Figure 3 and Table 3).

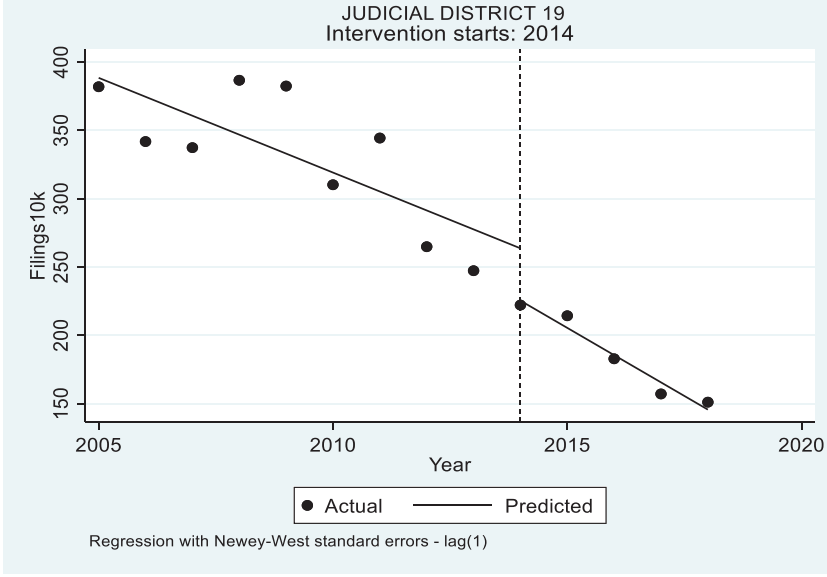

FIGURE 2 Juvenile filing rates in the 19th judicial district, 2005–2018 [Color figure can be viewed at wileyonlinelibrary.com]

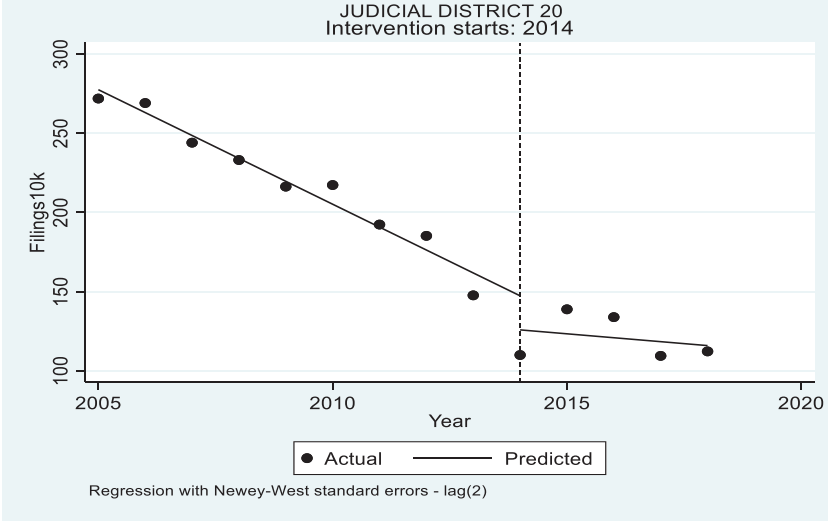

FIGURE 3 Juvenile filing rates in the 20th judicial district, 2005–2018 [Color figure can be viewed at wileyonlinelibrary.com]

4.2.4 | Summary

Overall, our hypotheses related to our first research question were partially supported. In the 12th and 20th districts, the immediate changes both indicated statistically significant declines in JDF. Although the change in the 19th district was not statistically significant, it still represented a sizeable drop, which we consider meaningful. We only found evidence of a statistically significant decline post-2014 in the 19th Judicial District, although data in the 20th Judicial District also

TABLE 4 Results for multigroup interrupted time-series analyses

Coefficient	Difference in Immediate Change (ZX)	Difference in Post-2014 Slope (ZT+ZXT)
	All Pilots vs. Nonimplementers	
Coefficient	−27.53†	−3.29*
(95% Confidence Interval)	(−55.59, .53)	(−6.09, −.50)
	All Pilots vs. 10th Judicial District	
Coefficient	−91.51***	−2.87
(95% Confidence Interval)	(−122.04, −60.97)	(−6.39, .66)

†$p < .10$. *$p < .05$. **$p < .01$. ***$p < .001$.

indicate a steady decline in JDF. Increasingly, scholars recognize the relevance of changes that portray real-world results that might otherwise be dismissed when researchers only rely on effect sizes or significance tests (Ableson, 1985; Thomas, 2017). These results and some possible explanations regarding patterns in the data are expanded on in the Discussion section.

4.3 | Multigroup ITSA (Comparison of pilot trends to nonimplementing trends)

Table 4 depicts the results from the comparison between the average of the pilots and the average of the nonimplementing districts, along with a comparison between the average of the pilots and the 10th Judicial District, which failed to successfully implement a pilot program. It presents differences in immediate change (ZX) and differences in post-2014 slopes (ZT+ZXT) for each comparison (see Table 4).

4.3.1 | Comparison of juvenile delinquency filing trends of the pilot sites (combined) with nonimplementing districts (combined) trends

The analysis testing for any differences between the immediate change in 2014 (ZX) in pilot districts to that of the nonimplementing districts offers some evidence for our first multigroup hypothesis: JDF in pilot districts dropped by more than 27 filings more during the first year of the pilot than they did in the nonimplementing districts. This finding was marginally statistically significant ($p = .05$). Our second multigroup hypothesis was also supported as the differences in post-2014 slopes between the pilots and nonimplementing districts (ZT & ZXT) were statistically significant, with JDF dropping by an average of more than three more each year than in nonimplementing districts after 2014 (see Figure 4 and Table 4).

4.3.2 | Comparison of juvenile delinquency filing trends of the pilot sites (combined) with the 10th Judicial District

As the 10th Judicial District was originally implicated in the policy explored here but was eliminated from the pilot group after the first year, we chose to conduct an additional comparison of this district to the remaining pilots. We found that pilot districts that fully implemented a

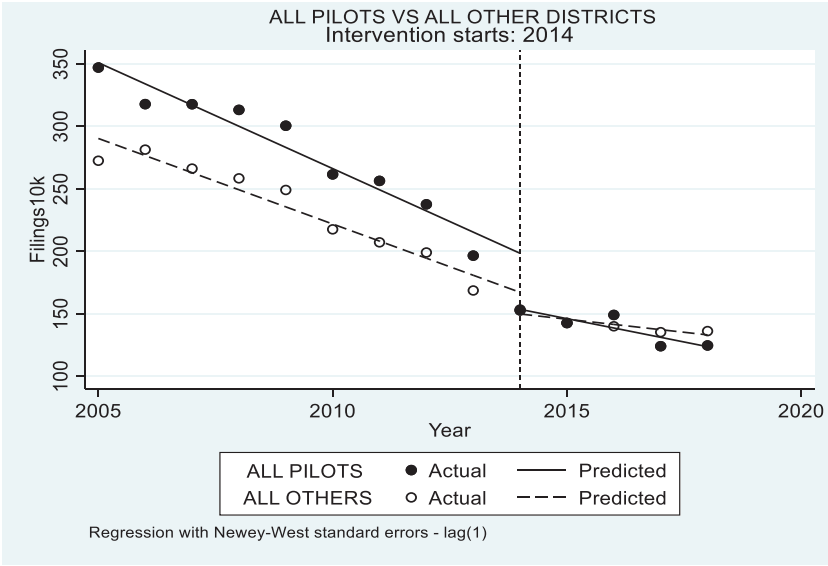

FIGURE 4 Average juvenile delinquency filing rates in pilot districts, 2005–2018, compared with nonimplementing districts [Color figure can be viewed at wileyonlinelibrary.com]

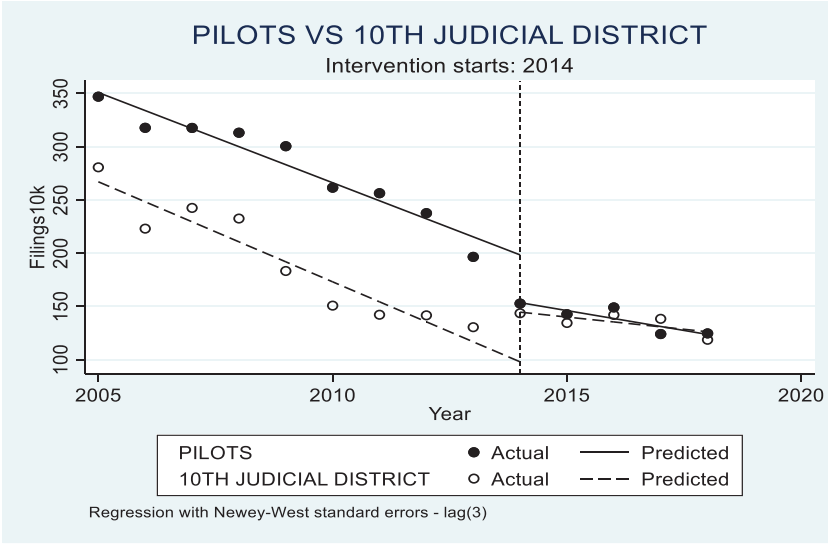

FIGURE 5 Average juvenile delinquency filing rates in pilot districts, 2005–2018, Compared with the 10th judicial district [Color figure can be viewed at wileyonlinelibrary.com]

program had a greater change in JDF, reaching statistical significance, in the first year of implementation (ZX) compared with the 10th Judicial District. Pilot districts exhibited more than 91 less JDF on average than the 10th Judicial District at the end of 2014, the first year of funding (see Figure 5). There is also some evidence that pilot districts produced different trends in JDF after 2014 (ZT & ZXT) as they exhibited a reduction of three more JDF on average per year than the 10th Judicial District; however, it should be noted that this difference was not statistically significant (see Figure 5 and Table 4).

4.3.3 | Summary

Overall, our analyses produced evidence that partially supports our hypotheses. Although we did not find statistically significant differences in all of our tests, there were notable differences, in the hypothesized direction, in both the immediate change in JDF and the post-2014 trends in JDF between pilot districts and nonimplementing districts. In pilot districts on average, there are larger immediate declines in JDF, and JDF decline at a greater rate after 2014 compared with the nonimplementing districts. Disaggregated, there are large differences between outcomes among the pilots, and in particular, the evidence supporting the likelihood of immediate effects is stronger than that supporting the likelihood of sustained effects. In discussing the implications of these results, we acknowledge the realities of smaller effects while following the lead of scholars such as Ableson (1985) and Thomas (2017), who noted that focusing solely on effect sizes or statistical significance may diminish researchers' observation of phenomenon with real-world importance.

5 | DISCUSSION

The aim of this study is to explore the impact of three restorative justice-based pre-file juvenile diversion pilot projects, authorized and funded by the Colorado legislature in 2014, on juvenile filing rates in the target districts. Single-group and multigroup interrupted time-series analyses suggest that the implementation of legislated pilot projects is at least partially associated with declining rates of juvenile delinquency filings (JDF); the strength of the association varies between pilot districts and appears more likely to have immediate than compounding effects.

When legislated pilot projects were initiated in 2014, JDF had been on the decline across Colorado for at least 10 years. Upon the implementation of pilot projects, however, JDF in implementing districts—the 12th, 19th, and 20th—demonstrate an immediate decline on average that is greater than average declines in nonimplementing districts at the same point in time, as well as greater than declines in the 10th Judicial District, which failed to implement the pilot project. Following the initiation of pilots, the three pilot districts maintained an average rate of JDF below the average of nonimplementing districts. Furthermore, as a group, the pilot districts continue to show rates of decline post-2014 that surpass average rates of decline in nonimplementing districts across the state during the same time period. Nevertheless, when disaggregating findings in the single-group analysis, we find that immediate effects in the 12th and 20th judicial districts are primarily responsible for the initial drop in JDF, whereas plummeting JDF rates in the 19th Judicial District post-2014 account for much of the sustained impact of the pilots as a group.

This analysis suggests that the use—or, in some cases, expansion and professionalization—of restorative justice-based diversion in the pilot districts was most often accompanied by an initial decrease in court filings followed by continued, more modest declines in JDF that are still more pronounced than those across the state on average. Figures also reveal, however, that post-2014 slopes are flatter, not steeper, than those preceding policy implementation (see Figures 1, 3, 4, and 5). This analysis does not allow for an explanation of this trend. It may suggest that instituting a new program serving a designated number of defendants resulted in a drop in filings that was sustained in subsequent years but did not expand or result in enhanced, distal impacts as a result of reduced recidivism, preventative effects, or prosecutorial culture shift. As a result, after the first year or two of building up the program, we would expect JDF to remain static. Another possible explanation for this pattern is that districts neared a "floor" in JDF filings, which had

already been falling across the state for more than 10 years. In either case, this analysis offers no compelling evidence that the implementation of state-funded diversion pilots would result in ongoing declines in JDF after the initial startup period, at least without continued increases in funding and populations served.

5.1 | Possible explanations for between-district variation

This study does suggest that the effects of restorative justice pre-file diversion systems vary by judicial district, and historical contextual information about pilot implementation in Colorado offers possible explanations for further study. In particular, the results of this study suggest that it may be important to take into account current trends and practices when predicting the likely effects of policy changes and pilot initiatives. Pilot sites with existing programs, the 12th and the 20th, experienced a greater initial change in 2014 with the infusion of funding from pilot legislation but less subsequent change over time following this initial capital investment. On the contrary, the pilot site launching a new restorative justice pre-file diversion project, the 19th Judicial District, demonstrated a smaller initial impact and a much greater sustained decline in juvenile filing rates. Although all three programs were expanded and formalized by the 2014 legislation, it is important to note that the 12th and 20th Judicial Districts had already begun implementation of restorative justice pre-file diversion at some level that existed in some form as early as the mid-1990s. In particular, the 20th Judicial District had the most well-developed program and had already demonstrated a rate of juvenile filings significantly lower than the state average as early as 2005: 277.49 compared with a state average of 304.28. Therefore, it is reasonable to anticipate that the impact of pilot funding would result in less change among prior adopters than among new adopters of an innovative practice.

These findings can also be considered in light of differential funding levels for each pilot district in year one and over time, based on requests by program administrators. The 19th Judicial District received the smallest amount of pilot funding in year one. In year three, the 19th requested and received nearly twice as much funding as in year two, providing a possible explanation for the greater sustained effect on filing rates. In comparison, the 12th Judicial District requested and received the same amount of funding in years two and three, whereas the 20th Judicial District experienced a slight decline in funding during this same time period (see Table 1). These patterns hold when adjusting the funding levels for population; however, this adjustment also reveals disproportionately higher funding per 10,000 youth in the 12th Judicial District, which does not support this explanation of our findings.

The scale of participation may also be a factor. As of two years following the start of funding, evaluators reported that the 19th Judicial District had served 201 youth; the 20th, 165 youth; and the 12th, 155 youth (Omni Institute, 2016). After 2016, district level numbers were not publically reported. Other implementation factors that are not explored here include informal selection criteria, facilitation and case management approaches, community participation, support for the program by district attorneys, supplemental funding sources, and others. Many of these implementation factors are undocumented or may be hidden (e.g., decision-making processes). Targeted research will be required to undertake a more detailed understanding of policy implementation and to illuminate possible causal pathways that strengthen the initial exploration presented in this study.

5.1.1 | Implications

The results of this study suggest that restorative justice pre-file diversion programs accompanied by funding supports and accountability measures such as those introduced by the Colorado legislature in 2014 can impact system-level justice outcomes like juvenile filing rates at some level, but also that the impact of such policy initiatives is likely to "level off." Here, initial declines in JDF are maintained but not reiterated at similar rates in the following years. With some caution, we suggest that similar state-funded pilots present one mechanism for reducing court involvement but should not be offered as a panacea.

Furthermore, the differential findings between districts suggest that implementation matters and is consistent with prior literature suggesting that variables like who is targeted (Morris, 2002) and who administers the program (Schwalbe et al., 2011) influence the efficacy of restorative justice diversion. States considering the use of similar reforms, such as those piloting innovative pretrial diversion approaches, should consider how to best support pilot sites with different characteristics, as well as how to best track their efficacy. Legislatures or criminal justice administrators initiating such projects should mandate data collection, which extends beyond case completions and recidivism to allow for more complex investigations of cost–benefit assessments, net-widening effects, and long-term impacts, as well as the pathways that facilitate or block desired outcomes. These include system-level outcomes such as filing rates, diversion rates, and incarceration rates, as well as implementation indicators like funding levels, staffing levels, and individual case outcomes.

5.1.2 | Limitations and future research

As with all research, there are limitations to this study. In particular, it is important to consider the possibility of influences on the outcome variable, which are not considered here. For instance, marijuana was legalized in 2012 in Colorado, and retail sales were implemented in 2014, coinciding with the implementation of restorative justice legislation and funding. Juveniles are barred from purchasing recreational marijuana in Colorado, but it is still possible that the availability of legal marijuana more widely may have shielded some juveniles from detection by law enforcement for marijuana-related offenses and impacted juvenile delinquency filings. Although state comparisons in the multigroup analysis address state-level threats to history, we have not conducted a comprehensive historical analysis of policy changes and other contextual features across Colorado's judicial districts.

In addition, even though this study attempts to capture "big-picture" effects of the policy under study, our approach does not control for other district-level factors that may influence outcomes. As a result, we cannot conclusively say that the effects observed are a result of the policy. To address this limitation as much as possible, we compared demographics in the piloting districts with those in the nonimplementing districts and found no significant differences. Other district-level variables, however, might differ significantly between the pilot districts and the nonimplementing districts, but we could not account for them. Future studies should incorporate additional district-level variables to account for other explanations and to identify the pathways of effects captured in this big-picture view.

Furthermore, it is important to note that this study does not address mechanisms of action or tie the identified outcomes to implementation activities in each district. It merely suggests that the policy was associated with an initial drop in filings in the targeted jurisdictions—whether via diversion, recidivism reduction, or other more distal effects like organizational culture shifts or strengthened community networks. Further research incorporating additional variables is needed to discern the pathways of effects. In particular, more attention is needed to understand how implementation characteristics, including funding levels, volume and types of cases served, or case outcomes, of pre-file diversion initiatives affect their impact on system-level outcomes.

Toward this end, it must be noted that one of four originally funded districts did not successfully complete the pilot program. This study focuses on the effects of policy implementation and does not account for the failure of the 10th Judicial District to implement the policy as specified. This must be considered in evaluating the success of the legislation in catalyzing implementation of restorative justice diversion projects.

Finally, we acknowledge that there are a limited amount of post-legislation data points available to establish post-2014 trends, and that long-term effects of the pilots are particularly unclear. These analyses should be revisited in the future to ascertain a more expansive picture of any long-term impact of the pilot programs, as well as the effects of the defunding of pilot sites.

6 | CONCLUSION

A consistent body of evidence supports the effectiveness of restorative justice diversion programs as a mechanism to improve outcomes for victims and offenders. In Colorado, pre-file juvenile diversion projects based in restorative justice have high victim satisfaction rates and low recidivism rates. This study undertakes an exploration of the effects of 2014 legislation that funded restorative justice diversion pilots in 3 of Colorado's 22 judicial districts. Single-group and multigroup interrupted time-series analyses are used to examine juvenile filing rates in each pilot district pre- and post-2014, as well as to compare changes in juvenile filings rates among pilot districts with nonimplementing district trends.

We find that the receipt of funding and related supports from legislated pilot projects is associated with significantly larger reductions in juvenile filing rates in piloting districts during the first year of funding than those occurring across the state in nonimplementing districts during the same time period. In addition, we note that even though two out of three piloting districts demonstrate ongoing rates of decline in juvenile filing rates that are greater than those in nonimplementing districts, the declining trends in pilots post-2014 are flatter, not steeper, than those prior to implementation. Furthermore, the adoption of new practices, as compared with the expansion of existing practices, as well as continued increases in funding may help explain more robust declines in some sites in the years after pilot initiation. These findings offer partial support for the effectiveness of legislation funding restorative justice pre-file diversion projects. They also suggest that the effects of legislative support may be moderated by district-level characteristics, such as existing practices, implementation strategies, and funding levels. We conclude that the efficacy of restorative justice pre-file diversion as a strategy to reduce system involvement is promising and should be further explored.

CONFLICT OF INTEREST STATEMENT

The authors confirm that they have no conflict of interest to declare.

ORCID

Shannon M. Sliva https://orcid.org/0000-0002-9351-8217

REFERENCES

Abelson, R. P. (1985). A variance explanation paradox: When a little is a lot. *Psychological Bulletin*, *97*, 129–133.

Austin, J., & Krisberg, B. (1981). Wider, stronger, and different nets: The dialectics of criminal justice reform. *Journal of Research in Crime and Delinquency*, *18*(1), 165–196.

Baum, C. F., & Schaffer, M. E. (2013). Actest: Stata module to perform Cumby–Huizinga general test for autocorrelation in time series. *Statistical Software Components S457668*, Department of Economics, Boston College. Retrieved from https://ideas.repec.org/c/boc/bocode/s457668.html

Bonta, J., Wallace-Capretta, S., Rooney, J., & McAnoy, K. (2002). An outcome evaluation of a restorative justice alternative to incarceration. *Contemporary Justice Review*, *5*(4), 319–338.

Bryson, S. L., & Peck, J. H. (2020). Understanding the subgroup complexities of transfer: The impact of juvenile race and gender on waiver decisions. *Youth Violence and Juvenile Justice*, *18*(2), 135–155.

Butts, J. (2016). Critical diversion. *Criminology & Public Policy*, *15*(3), 983–989.

Colorado Department of Local Affairs (2005–2018). *Population by single year of age – county* [data set]. Retrieved from https://demography.dola.colorado.gov/population/data/sya-county/

Colorado Judicial Branch (2005–2018). *Annual statistical reports* [data set]. Retrieved from https://www.courts.state.co.us/Administration/Unit.cfm?Unit=annrep

Cumby, R. E., & Huizinga, J. (1992). Testing the autocorrelation structure of disturbances in ordinary least squares and instrumental variables regressions. *Econometrica*, *60*, 185–195.

Latimer, J., Dowden, C., & Muise, D. (2005). The effectiveness of restorative justice practices: A meta-analysis, *The Prison Journal*, *85*

Linden, A. (2015). Conducting interrupted time series analyses for single- and multi-group comparisons. *The Stata Journal*, *15*(2), 480–500.

Linden, A. (2018). Combining synthetic controls and interrupted time series analysis to improve causal inference in program evaluation. *Journal of Evaluation in Clinical Practice*, *24*, 447–453. https://doi.org/10.1111/jep.12882

Lopez Bernal, J., Cummins, S., & Gasparrini, A. (2017). Interrupted time series regression for the evaluation of public health interventions: A tutorial. *International Journal of Epidemiology*, *46*(1), 348–355. https://doi.org/10.1093/ije/dyw098

Macallair, D., & Males, M (2004)..A failure of good intentions: An analysis of juvenile justice reform in San Francisco during the 1990s. *Review of Policy Research*, *21*(1), 63–78.

Mears, D. P., Kuch, J. J., Lindsey, A. M., Siennick, S. E., Pesta, G. B., Greenwald, M. A., & Blomberg, T. G. (2016). Juvenile court and contemporary diversion: helpful, harmful, or both?. *Criminology & Public Policy*, *15*(3), 953–981

Morris, A. (2002). Critiquing the critics: A brief response to critics of restorative justice. *British Journal of Criminology*, *42*(3), 596–615.

National Juvenile Defender Center (2019). *Diversion. Juvenile Court Terminology (website)*. Retrieved from https://njdc.info/juvenile-court-terminology/

OMNI Institute. (2016). *Restorative justice pilot evaluation report*. Retrieved from https://www.rjcolorado.org/_literature_157907/RJ_Pilot_Report_-_July_2016

OMNI Institute. (2018). *Restorative justice pilot evaluation report*. Retrieved from https://www.rjcolorado.org/_literature_181294/2018_RJ_Juvenile_Diversion_Evaluation_Report

Reimund, M. E. (2005). The law and restorative justice: Friend or foe? A systematic look at the legal issues in restorative justice, *Drake Law Review*, *53*

Restorative Justice Coordinating Council (2016). *Annual report on implementation of HB 2013-1254*, Retrieved from https://cdpsdocs.state.co.us/oajja/Restorative_Justice/2016_Annual_Report_HB13-1254.pdf

Robinson, G., & Shapland, J. (2008). Reducing recidivism: A task for restorative justice? *British Journal of Criminology*, *48*(3), 337–358.

Pogrebin, M. R., Poole, E. D., & Regoli, R. M. (1984). Constructing and implementing a model juvenile diversion program. *Youth & Society*, *15*(3), 305–324.

Prichard, J. (2010). Net-widening and the diversion of young people from court: A longitudinal analysis with implications for restorative justice. *Australian & New Zealand Journal of Criminology*, *43*(1), 112–129.
Ray, J. V., & Childs, K. (2015). Juvenile diversion. In (Marvin D. Krohn and Jodi Lane, eds.). *The Handbook of Juvenile Delinquency and Juvenile Justice*. John Wiley & Sons.
Schwalbe, C. S., Gearing, R. E., MacKenzie, M. J., Brewer, K. B., & Ibrahim, R. (2012). A meta-analysis of experimental studies of diversion programs for juvenile offenders. *Clinical Psychology Review*, *32*(1), 26–33.
Sedgwick, P. (2014). Multiple hypothesis testing and Bonferroni's correction. *BMJ*, *349*, g6284. https://doi.org/10.1136/bmj.g6284
Sherman, L. W., & Strang, H. (2007). *Restorative justice: The evidence*. The Smith Institute.
Sliva, S. M. (2016). Determinants of state innovations in American sentencing and corrections policy: A systematic review. *Criminal Justice Policy Review*, *27*(7), 702–722.
Sliva, S. M., Han, T. M., Samimi, C., Golieb, K., McCurdy, J., & Forte, A. (2019). State of the state: Restorative justice in Colorado. Colorado Restorative Justice Council. Retrieved from https://restorativejusticeontherise.org/state-of-the-state-restorative-justice-in-colorado/
Sliva, S. M., & Lambert, C. G. (2015). Restorative justice legislation in the American states: A statutory analysis of emerging legal doctrine. *Journal of Policy Practice*, *14*(2), 77–95.
Sliva, S. M., Porter-Merrill, E. H., & Lee, P. (2019). Fulfilling the aspirations of restorative justice in the criminal system? The case of Colorado. *Kansas Journal of Law & Public Policy*, *28*(3), 457–504.
Stafford, M. C. (2016). New call for assessing the effects of 21st century juvenile diversion. *Criminology & Public Policy*, *15*(3), 949.
Streiner, D. L., & Norman, G. R. (2011). Correction for multiple testing: Is there a resolution. *Chest*, *140*(1), 16–18. https://doi.org/10.1378/chest.11-0523
Thomas, J. M. (2017). Early truancy evaluation: Replication of an evaluation using a regression discontinuity design. *Children and Youth Services Review*, *78*, 150–160. https://doi.org/10.1016/j.childyouth.2017.05.020
Umbreit, M., & Armour, M. P. (2010). *Restorative justice dialogue: An essential guide for research and practice*. New York, NY: Springer Publishing Company.
Umbreit, M. S., & Carey, M. (1995). Restorative justice: Implications for organizational change. *Federal Probation*, *59*(1), 47–54.
Wilson, H. A., & Hoge, R. D. (2013). The effect of youth diversion programs on recidivism: A meta-analytic review. *Criminal Justice and Behavior*, *40*(5), 497–518.
Zehr, H. (2014). *The little book of restorative justice*. New York, NY: Simon and Schuster.

AUTHOR BIOGRAPHIES

Shannon Sliva is an assistant professor in the Graduate School of Social Work at the University of Denver, where she conducts national and local research on the determinants and impacts of sentencing and correctional policy, with an emphasis on restorative and transformative justice solutions. Prof. Sliva tracks state-level restorative justice legislation across the United States, partnering with Colorado practitioners, policy makers, and advocates to document the impacts of leading-edge restorative justice laws and develop recommendations for policy transfer. Her recent work is published in *Social Work, Criminal Justice Policy Review, The Prison Journal*, and *Contemporary Justice Review*, among others.

Mark Plassmeyer received his Ph.D. from the Graduate School of Social Work at the University of Denver and is currently an assistant professor of social work at the University of Arkansas. His research interests are centered on criminal and civil policy's impact on social, economic, and civic opportunities for people with criminal histories, with an emphasis on policies that influence successful reintegration for people with criminal drug records. His other research interests are in drug policy, affordable housing, and the role of social workers in the

political process. Prof. Plassmeyer has recently published work on collateral consequences of convictions in *Criminal Justice Review*, teaching social policy in hyper-partisan times in the *Journal of Social Work Education*, and measuring civic engagement in the *Journal of the Society for Social Work and Research*.

How to cite this article: Sliva SM, Plassmeyer M. Effects of restorative justice pre-file diversion legislation on juvenile filing rates: An interrupted time-series analysis. *Criminol Public Policy*. 2021;20:19–40. https://doi.org/10.1111/1745-9133.12518