Reforming Child Protection Services:

The Effect of Differential Response on Subsequent Maltreatment for Reported Children

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Caitlyn Keo*

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Abstract

Traditionally, *all* child protection services (CPS) reports are investigated to formally determine whether the alleged child maltreatment can be confirmed. This conventional investigative response (IR) to all reports has been met with concerns that investigations can be adversarial for children and families, particularly for those at the intersection of poverty and suspected child maltreatment. Therefore, many U.S. states have adopted differential response (DR) systems. In a DR, "dual track," system, the agency reserves the IR track for reports in which the child appears to be at imminent risk of subsequent maltreatment. For lower risk reports, the agency uses an alternative response (AR) track. While services to families are typically provided in the AR track, there is concern that, without an investigation, children could be more at risk for future maltreatment. Using child maltreatment data from the National Child Abuse and Neglect Data System for 2004 to 2019 and the staggered adoption of DR reforms over time. I assess how DR affects subsequent maltreatment for reported children, and I find that DR policies on average increase the likelihood of a re-report yet not the likelihood of a re-report with substantiation. These findings provide evidence for how popular DR reforms affect subsequent maltreatment as proxied by further reengagement with CPS given a re-report.

Key Words: Child Welfare, Child Protection Investigations, Reforms, Public Policy

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* Department of Applied Economics, University of Minnesota. Email: ckkeo@umn.edu.

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1. Introduction

Traditionally, child protection services (CPS) agencies respond to all reports of child maltreatment with a formal investigation. However, many agencies and practitioners have expressed concerns about how traditional investigative responses can be inflexible and adversarial. In response, many agencies have done away with the one-response-fits-all approach to handling CPS reports. To meet the needs of families, an alternative response may be more appropriate at times than an investigation. Shifts away from one-track investigative CPS systems towards dual-track systems recognize that child and family needs are diverse and complex. Yet, simultaneously balancing the requisites for child safety and family preservation can be delicate which brings forth concerns.

Most U.S. states have reformed CPS with a differential response (DR) system, which separate child maltreatment reports into two (or more) response tracks. In a dual-track CPS system, these tracks are generally known as an alternative response (AR) track and an investigative response (IR) track. In a DR system, criteria are established in which the traditional IR track is usually restricted to reports for children who have been severely maltreated or who appear to be of moderate to high risk of re-reports (i.e., in imminent danger for further abuse or neglect). The non-investigative AR track is established for reports that are assessed to be low to moderate risk and are addressed through services to the family. Reforms for dual tracks are intended to allow CPS to engage with families in a way that moves away from a solely punitive and adversarial stance towards a more collaborative and strengths-based approach.

To date, there is limited long-term research that assesses the outcomes of differential response compared to traditional child welfare models. To study DR policies across states over a longer period, I leverage large amounts of data on child maltreatment reports from the National Child Abuse and Neglect Data System (NCANDS) for 2004 to 2019. To proxy subsequent

maltreatment for reported children, I use re-reporting and re-reporting with substantiation as my primary outcomes of interest. To gain an understanding of these outcomes, I specifically look at states that adopt DR between 2004 and 2019.

In order to understand the effect of DR reforms on child maltreatment, I first analyze child and case characteristics associated with assignment to differential response. I limit my data to report-child observations in periods with DR reforms and I use a linear probability model (LPM) to predict placement on the AR track. Several characteristics of reports affect the probability of AR vs DR assignment, including who makes the report. Reports made by education, child care, substitute care personnel, and medical and mental health personnel are more likely to receive AR response relative to reports made by social services personnel. Prior victims, children reported due to physical abuse or sexual abuse, and younger children (<5 years old) are less likely to receive an AR. Ceteris paribus, race and ethnicity also plays a role in who receives an alternative response.

The main empirical analyses address analyzes changes in subsequent maltreatment in states that adopt DR policies versus those that don't. I leverage the staggered adoption of DR reforms over time to estimate average treatment effects on the treated (ATET) and use difference-in-difference (DD) strategies to understand how DR policies affect re-reporting and re-reporting with substantiation. I find that DR policies increase the likelihood of a re-report within 6 months, 12 months, and 3 years. Disaggregated results show that this change is driven by children who would be considered medium-risk and high-risk in the absence of the policy based on propensity score weighting from the periods without DR. When the data is disaggregated by race, the increase the likelihood of a re-report is driven by White children and children of two or more races. Yet, on average, there is no effect DR on re-reporting with substantiation within any of the three bounded time intervals, even across risk tercile and across race.

CPS agencies that are reformed for DR vary in how often they use alternative responses. To increase our understanding of how AR track use influences the subsequent maltreatment outcomes of interest, I use instrumental variable (IV) methods to estimate a local average treatment effect (LATE) of AR versus investigation on re-reporting. I find that increasing the use of AR is associated with higher likelihoods of a re-report within 6 months, 12 months, and 3 years. However, increasing the use of AR is associated with a lower likelihood of re-reporting with substantiations within 6 months, 12 months, and 3 years.

Contributing to the existing literature in public economics and child welfare, this paper provides evidence as to how DR reforms in child welfare affect safety outcomes for reported children. To date, there is limited long-term research that assesses the safety outcomes of DR systems compared to traditional child welfare systems. This lack of evidence can make it challenging to evaluate the effectiveness of the DR approach fully. This study contributes to the literature by using administrative CPS data at the child-level. Earlier studies used aggregated state-level data on re-reporting rates (e.g., Johnson-Motoyama et al., 2023; Piper, 2017). The state-level findings are informative, but microdata and further analysis are of importance to understand the nuanced effects of DR policies on children of various characteristics. I also provide evidence across numerous states over a relatively long period, which improves upon state and county-specific program evaluations which are often completed in shorter durations or during pilot programs (e.g., Fuller et al., 2013; Murphy et al., 2013).

2. Background on Child Welfare Systems

The child welfare system encompasses a wide range of responsibilities entrusted to local authorities whose primary goal is to ensure the safety, permanency, and well-being of children.

Responding to allegations of child maltreatment is one of the foremost tasks in child welfare. In response to maltreatment allegations, the relevant jurisdiction– referring to the power, right, or authority to interpret and apply the law– differs across states. There are 39 states with state-administered CPS systems, while 9 states have county-administered systems, and 2 states have hybrid systems. The District of Columbia is also considered a state-administered system (Weigensberg et al., 2022). Throughout this study, I use the terms 'jurisdiction' and 'agency' interchangeably to discuss CPS.

At intake, CPS agencies are responsible for all allegations of child maltreatment (abuse and neglect).[1] These allegations are called referrals, and they are often made by phone through a 24-hour hotline. In 2019, 36 states processed referrals through state-centralized screening systems rather than county-centralized screening systems (Weigensberg et al., 2022). If a referral meets the state and local criteria for CPS involvement it is screened-in, and if a referral does not meet these criteria, it is screened-out. The referrals that are screened-in are then referred to as 'reports.'

2.1 Stages of Child Welfare in a Traditional Investigative System

In a traditional CPS system, *all* reports of maltreatment are investigated (see the left panel A of Figure 1). Given federal law, a CPS investigation of a maltreatment report necessitates a formal finding that either confirms the maltreatment allegation(s) (therefore called a substantiated report) or determines that no maltreatment occurred (i.e., an unsubstantiated report). A report is substantiated when the CPS investigation determines that there is confirmation of reasonable cause to believe that at least one child on the report has been abused or neglected. A report is

[1] Federal law (42 U.S.C.A. § 5106g) broadly defines child maltreatment to include abuse and neglect. Abuse refers to harmful actions inflicted upon a child, such as physical or sexual abuse, while neglect pertains to the failure to provide a child with safe and consistent care. Following the broad federal definition of child maltreatment, narrower more concrete definitions of maltreatment which elicit CPS involvement differ at the state level and change over time (Child Welfare Information Gateway, 2014).

unsubstantiated when an investigation determines that no child maltreatment occurred. This determination might occur when there is insufficient evidence under state law or agency policy to support the alternative conclusion (Child Welfare Information Gateway, 2003). Unsubstantiated reports require no further action on behalf of CPS.[2]

A substantiated report results in two common forms of CPS intervention. One option is for the caseworker to recommend removing the child(ren) from their home and placing them in foster care, often while CPS provides services to the family (parents or guardians). The primary goal in a foster care scenario is family reunification or another permanent placement, such as kinship care, guardianship, or adoption, if reunification is not feasible. The second option is for the caseworker to recommend allowing the child(ren) to remain in their home while providing services to the family. In either scenario, the overall objective is to ensure child safety and strengthen families.

2.2 Reforming Child Welfare to Allow for Differential Response

Over the last few decades, CPS in most U.S. states have transitioned to a differential response (DR) system, also known by several other names, including multiple response and family assessment.[3] In 2019, 30 states had DR systems, with 25 operating at the state level and 5 at the county level (Weigensberg et al., 2022). This can be seen in Appendix Figure B1. Additionally, by 2019, 9 states and the District of Columbia had previously implemented DR, sometimes as a pilot program, and later discontinued DR in favor of a traditional investigative system.[4]

[2] An important note regarding many CPS agencies is that if a referral is screened out, or a report is unsubstantiated, future re-referrals or re-reports of suspected child maltreatment cannot reference the details of the previous allegations that did not become substantiated reports. In other words, previous unsubstantiated allegations (including screened-out referrals) hold no weight in the decision of substantiation for the current report.
[3] Note, that state legislation proceeds reforms and implementation of DR.
[4] Reasons for discontinuation generally include funding or resource limitations (e.g., the Alaska DR program was discontinued in 2009 due to the loss of legislative funding allocated for the program), inconsistent implementation, leadership changes, and safety concerns following high-profile child fatalities (Alaska Citizen Review Panel, 2015; Child Welfare Information Gateway, 2014; David Scharfenberg, 2015).

CPS agencies that are reformed for DR separate child maltreatment reports into two (or more) response tracks: alternative response (AR) and investigative response (IR). In a DR system, criteria are established such that the IR track is usually restricted to moderate- to high-risk reports for children who have been severely maltreated or who appear to be at risk of re-reports (i.e., in imminent risk for further neglect or abuse). Then, the non-investigative AR track is established to work with lower-risk families and address the underlying causes of their maltreatment report with referrals to services. The non-investigative AR track does not require a formal finding for the alleged maltreatment. Hence, by construction, reports on the AR track cannot be substantiated.

Reforms focus on the underlying reason for the suspected maltreatment allegation and largely aim to address the intersection of poverty and suspected maltreatment. Specifically, active reform uses the AR track to focus on meeting the needs of families through services in a way that promotes family well-being and self-sufficiency (Piper et al., 2019). The AR track can be used to deliver a continuum or battery of services including, but not limited to, parenting, mental health, and substance abuse interventions.

While there are common features of DR systems across jurisdictions, DR is not a uniformly defined reform, and its implementation varies. In some states, the determination for the appropriate track (IR or AR) for maltreatment referrals is made at the time of screening to screen out to AR, whereas in other states, a determination is made after a report is screened in. Among the 30 states with DR in 2019, 17 states make the track determination only after a report is screened in, 6 states make the determination only *at the time of screening to screen out to AR*, 6 states allow for both (determinations at screening *and* after a report is screened in), and 1 state uses another process (Weigensberg et al., 2022). In this study, I will focus on DR reforms that make the track determination only *after a report is screened in*. Panel B of Figure 1 shows a standard DR example

for an agency that has one AR track and makes the track determination after a report is screened. Other differences in DR implementation are discussed in Appendix C.

2.3 Differential Response, Child Safety, and Re-involvement with CPS

Critics of CPS reforms often argue that DR does not provide the same level of child safety as traditional investigative, one-track systems. A limited number of studies have used aggregated state-level data to explore the effect of DR on safety outcomes and found reductions in child welfare system involvement (Fluke et al., 2019; Johnson-Motoyama et al., 2023; Piper, 2017; Shipe et al., 2022). For instance, Piper (2017) utilized NCANDS data and found that re-reporting rates for AR cases were lower than IR cases when fewer than a third of the accepted reports for CPS intervention were assigned to the AR pathway. Another study, which analyzed data from six states between 2004 and 2013, discovered that increased utilization of the DR pathway led to a decrease in the overall rate of substantiated re-reports (Fluke et al., 2019).

3. Data and Descriptive Analysis

3.1 Data on Child Maltreatment Reports

In this study, I use the restricted National Child Abuse and Neglect Data System (NCANDS) Child File datasets for 2004 through 2019.[5] This administrative data is voluntarily collected from state agencies and disseminated by the National Data Archive on Child Abuse and Neglect (NDACAN) which is located at Cornell University (Children's Bureau, 2019b). Once the annual Child Files

[5] These Child Files are based on submission years which are fiscal years. Throughout my analysis, I acknowledge the year (and month) of the report based on the calendar year (month) the report was received rather than the fiscal year data was submitted. This is derived from the 'report date' which is the month, day, and year that the responsible agency was notified of the suspected child maltreatment. Thus, I keep all reports that were made in the calendar years 2004 to 2019. This drops the reports that were submitted to the Children's Bureau in 2004 or later but were received by CPS as early as 2001.

have been appended, these data are harmonizable over time and across states. The data consist of the demographics of reported children (e.g., age at the time of report, race, Hispanic ethnicity), the reasons for the maltreatment report, the investigation or assessment disposition which indicates placement on the IR and AR track, the reporters' characteristics (e.g., social services, medical or mental health personnel, family and friends), and whether the child has previously been a victim.

In the NCANDS data, there is detailed information that is specific to each reported child since observations in NCANDS are report-child level. In other words, observations are unique within a state using a report ID (ID meaning identifier) and child ID. Any given alleged maltreatment report (identified by report ID) may correspond to one child or several children (identified uniquely by child ID). For most of the NCANDS, child IDs are linkable within each state across years,[6] allowing for viable panel or repeated cross-sectional methods. Linkable child IDs are essential for assessing subsequent maltreatment (measured by re-reporting) as a primary outcome of interest, which is discussed in detail in section 3.4.

For models with re-reporting as the outcome, consideration of foster care placement is important because of potential censoring: children in foster care may be less likely to be re-reported, assuming they are in a safe placement. While the NCANDS Child Files include an indicator for foster care services (i.e., placement), concerns about data quality led me to merge specific variables from the Adoption and Foster Care Analysis and Reporting System (AFCARS). The collection of the AFCARS foster care files is mandated, requiring state agencies to report to the Children's Bureau twice annually. These data are also disseminated by NDACAN (Children's Bureau, 2019a). Using the state foster care identifier (variable *stfcid*) which is available in both NCANDS and AFCARS, the foster care variables that I merge are, 1) an indicator for foster care

[6] However, there are several breaks in child ID linkage that are documented by NDACAN. In Appendix A, I discuss how I deal with breaks in child IDs in constructing my sample.

placement, and 2) the length of time in care (in years; continuous values are defined based on 'days in care'/365).

To construct my analytic sample of maltreatment reports, I first make systematic state-year exclusions and then I make child exclusions across states (see Appendix A). Specifically, in state years with linkable child IDs, I restrict the sample to children ages 0-17 with reports where 'neglect,' 'physical abuse,' 'emotional or psychological abuse,' or 'sexual abuse' were one at least one of the (up to four) documented reasons for the report.[7] Then, among states with DR, I limit to DR systems that begin DR between 2004 and 2019 (dropping the always treated states), that do not discontinue the use of DR before 2019, and that only make the track determination after a report is screened in.[8] After the child exclusions and the state-year exclusions, my sample is constructed with 18,458,785 report-child observations from 21 states (including the District of Columbia). Note, while this study covers reported children between 2004 and 2019, I have an unbalanced state-year panel.

3.2 Covariates for Local Demographic and Economic Factors

Socioeconomic factors such as income, education, employment, and social support are known to be associated with child maltreatment. For example, there have been consistently observed relationships between low household income and child abuse and neglect (Berger et al., 2017; Berger & Waldfogel, 2004; Paxson & Waldfogel, 1999, 2003). Socioeconomic determinants of child maltreatment further include parental work status (Paxson & Waldfogel, 1999) and unemployment (Brown & De Cao, 2018). Accordingly, empirical research on child maltreatment

[7] Most states officially recognize four major types of child maltreatment: neglect, physical abuse, sexual abuse, and emotional or psychological abuse. Appendix Figure B1 shows the number of annual reports-child observations by reasons for report.

[8] This is because NCANDS data do not provide the appropriate universe to analyze states that screen out to AR. In those states, it would be more appropriate to use data on referrals to CPS, rather than data on CPS reports.

across disciplines has explored the effects of policies that address the socioeconomic factors related to maltreatment – predominately maltreatment characterized as physical abuse and neglect (e.g., Berger et al., 2017; Ginther & Johnson-Motoyama, 2017; Raissian & Bullinger, 2017; Rittenhouse, 2023).

I rely on two additional datasets for 2004 to 2019 to account for county- and state-level differences in socioeconomic factors in my analyses. First, I construct county-level covariates for demographics using one-year samples from the American Community Survey (ACS) (Ruggles et al., 2019). Using the ACS, I construct the state log of the child population; the share of the population that is non-Hispanic black, non-Hispanic Asian, non-Hispanic other race, and Hispanic any race; and the share of children living below the poverty threshold. Some counties in the ACS are masked, in which case I compute the residual characteristics of the rest of the state. Then, I use the state-year covariates constructed by the University of Kentucky National Welfare Data (2023) to account for potential socioeconomic factors that may affect maltreatment. I created the state log of population; the state log of average personal income; the log of the state minimum wage; the log of Supplemental Nutrition Assistance Program (SNAP) benefits for a family of three; and the log of Temporary Assistance for Needy Families (TANF) benefits for a family of three. I also use the state unemployment rate.

3.3 Differential Response Treatment Variables

In this study, I take advantage of the fact that I know different levels of treatment related to DR: state-level treatment and child-level treatment. Child-level treatment indicates that a reported child is placed on the AR track. I create a binary treatment variable for placement on the 'AR track' that is equal to one if the NCANDS report disposition is 'alternative response disposition-victim' or

'alternative response disposition-non-victim.'[9] Then, placement on the 'AR track' equals zero if there was one of the other report dispositions since all other dispositions would have been concluded on the IR track. Of course, child-level treatment is conditional on state treatment.

State-level treatment indicates that a state is reformed for differential response either at the state level or the county level. Following the methods of Johnson-Motoyama et al. (2022), if any report in that state year received a placement on the 'AR track,' the state is coded as utilizing DR that year. This state-level variable is binary and equals one in the post-DR period. The treatment variable is equal to zero for both the pre-DR period for states that eventually reform or in the states that never reform for DR since no child has received an AR placement that year. I cross-checked this method with the SCAN Policies Database 2019.[10] Among the 21 states in the sample, 8 states (including the District of Columbia), have (ever) implemented DR during the period of this study. Appendix Figure B2 shows DR treatment status by year for states in my sample.

3.4 Outcome Measures

There are different ways to proxy for subsequent child maltreatment. Using administrative CPS data, the commonly used measures include re-referrals (i.e., a referral or report, followed by another referral), re-reporting (i.e., a report, followed by another report), or recurrence (i.e., revictimized as indicated by a substantiated report, followed by another substantiated report). Since NCANDS does not include screened-out referrals, I cannot look at re-referrals for this study.

[9] A report disposition is the final determination that is concluded from the CPS response to a report of child maltreatment. Dispositions categories include substantiated; indicated or reason to suspect; alternative response disposition-victim; alternative response disposition-not a victim; unsubstantiated; unsubstantiated due to intentionally false reporting; closed-no finding; other; and, unknown or missing. If there are multiple children on the report, the report disposition takes on the most severe of the dispositions. For example, a report that is "substantiated" means that at least one child on the report was found to be a victim.

[10] The State Child Abuse and Neglect (SCAN) Policies Database 2019, compiles data on state definitions and policies related to the surveillance of child maltreatment incidences and associated risk and protective factors for the calendar year 2019 (Weigensberg et al., 2022).

Furthermore, since reports cannot be substantiated on the AR track, I do not look at recurrence. Instead, I focus on re-reporting as my primary outcome of interest. Linkable child IDs in NCANDS (which were briefly discussed in section 3.1) are essential so that I can measure re-reporting.

A re-report commands another CPS response. In the literature, it is common to proxy for subsequent maltreatment by identifying when a reported child is later re-reported within a given interval of time. The most important or relevant interval of time remains unclear; various intervals of time (e.g., 6 months, 12 months, 18 months, 60 months) are used in impact evaluations and empirical studies (e.g., Antle et al., 2009; Baron et al., 2023; Cuomo & Carrión, 2011; Putnam-Hornstein et al., 2021; Shipe et al., 2022).

For my first set of outcomes, I measure a child maltreatment re-report with an indicator for when the current report is followed by a subsequent report within 6 months, 12 months, and 3 years. The intervals are calculated based on report dates. To account for potential censoring for intervals of longer duration, the 6-month and 12-month re-report indicators are created for reports from 2004 to 2018, and the 3-year re-report indicator is calculated for reports from 2004 to 2016.

Substantiation is exclusively possible on the IR track. Another important indication of subsequent maltreatment includes re-reporting with substantiation. For my second set of outcomes, I measure re-reporting with substantiation within 6 months, 12 months, and 3 years. These outcomes also account for potential censoring: the 6-month and 12-month re-report with substantiation indicators are created for reports from 2004 to 2018, and the 3-year re-report with substantiation indicator is calculated for reports from 2004 to 2016.

3.5 Descriptive Statistics: Trends in the Outcomes

The re-reporting and re-reporting with substantiation outcomes in this study are bounded within 6 months, 12 months, and 3 years. The proportion of reported children who are re-involved with

CPS via a subsequent screened-in report increases as the duration window increases. Figure 2 shows the trends in the outcomes by year. In Panel A, there is a slight incline over time for the proportion of reported children who get re-reported across all three intervals. Looking at the most recent years of the data, in 2018 about 22 percent of reported children were re-reported within 6 months and 31 percent were re-reported within 12 months. In 2016 nearly 50 percent of all reported children were re-reported within 3 years. In Panel B, there is persistence in the trends of re-reporting with substantiation across intervals and increasing the interval of time does not increase the proportion of reported children as greatly.

Appendix Tables B1 and B2 show further descriptive analyses for key variables that are included in analyses. Specifically, within states that adopt DR policies between 2004 and 2019, Appendix Table B1 shows the characteristics of report-child observations separately before and after the policy. Then after the policy, it further disaggregates the IR and the AR track observations. Then, Appendix Table B2 shows the characteristics of report-child observations that are investigated in state-years with no DR reform (conventional investigative CPS structure) and state-years that are post-DR reform (CPS with DR reform).

3.6 Descriptive Analysis: Predictors of an Alternative Response

The DR approach generally allows CPS agencies to respond differently to maltreatment reports based on factors such as the type of maltreatment and the severity of the report. To show what predicts an alternative response, I use a linear probability model (LPM) where the dependent variable is the binary child-level treatment, AR track equals one, in periods with DR. Figure 3 is a coefficient plot that shows the predictors of an AR track placement from my preferred specification which includes child and report characteristics (coefficients shown), and conditions on local demographic and economic factors, and fixed effects (coefficients not shown). This preferred

specification is column 3 of Appendix Table B3. Reports made by education, child care, substitute care personnel (5% level), and medical and mental health personnel (10% level) are more likely to receive an AR, relative to reports made by social services personnel. For negative associations, prior victims, children reported due to physical abuse or sexual abuse, and younger children (<5 years old) are less likely to receive an AR. Moreover, Black, and Asian or Pacific Islander children are less likely to receive an AR, relative to White children, as are Hispanic children relative to non-Hispanic children. This affirms research from Texas which showed that assignment to the AR track was largely driven by child race, although I cannot also assert that it is driven by neglect and poverty as found by Choi et al. (2021).

3.7 Descriptive Analysis: Predictors of Re-Reporting in Periods with No Differential Response

This study focuses on subsequent child maltreatment for reported children. Before I propose my empirical strategy to assess how the adoption of DR policies affect subsequent maltreatment, I want to first look at predictors of my outcomes in the absence of DR reforms. In periods with no DR, meaning pre-DR and never DR state-years, I assess the predictors of subsequent maltreatment using a LPM where the dependent variable is a binary re-reporting, or reporting with substantiation, outcome. To model each outcome separately, in Appendix Table B4 I include controls for child characteristics and report characteristics, local demographic and economic factors, and state and year fixed effects.

Across columns, positive and significant predictors of re-reporting and re-reporting with substantiation include being a prior victim, neglect, psychological and emotional abuse, foster care services, and length of time in care (in years) interacted with foster care. Physical abuse is associated with a higher likelihood of being re-reported, but not necessarily re-reported with substantiation. Older children (>5 years old) are less likely to be re-reported and less likely to be

re-reported with substantiation. Relative to White children, Black, American Indian, Alaskan Native, and Asian or Pacific Islander children are less likely to be re-reported, and Black and Asian or Pacific Islander children are less likely to be re-reported with substantiation. Children on substantiated reports are less likely to be re-reported in 6 months, 12 months, and 3 years, but are marginally more likely to be re-reported with substantiation in 3 years.

The models that pool across state and year in periods with no DR reforms, meaning they do not include state and year fixed effects, are used to create the re-report propensity scores (Appendix Table B4). These propensity scores will be used in later analysis.

4. Empirical Strategy

4.1 Difference-in-Difference: The Effect of Differential Response Policies on Re-reporting

How do differential response policies affect re-reporting? To quantify the impact of DR policy (treatment), I leverage the variation in timing, and I measure the average treatment effect on the treated (ATET) using differences-in-differences (DD) methods. The general form of the DD model with variation in treatment timing is expressed as:

$$Y_i = \alpha_1 DR_{s(i)t(i)} + \alpha_2 X_i + \alpha_{s(i)} + \alpha_{t(i)} + \varepsilon_i \quad (1)$$

where, Y_{ist} denotes the re-reporting outcome for report-child i. Year t refers to the year the initial report was screened in. $DR_{s(i)t(i)}$ is a binary variable that indicates whether report-child i is in a state s that had the DR reform in year t. X is a vector of control variables. State fixed-effects, $\alpha_{s(i)}$, account for time-invariant differences between states, such as differences funding, child welfare practices, or differences in definitions of child maltreatment. The year fixed effects (indicators), $\alpha_{t(i)}$, control for the common temporal trend in specific calendar year *t,* which relates to the year

of which the report was screened in by the agency. Finally, ε_i is the error term with standard properties.

In my analytic sample, I have 21 state clusters in total: 13 control states and 8 treated states. Having relatively few treated state clusters may raise potential problems that invalidate the conventional cluster robust inference. Primarily, the concern is that when there are too few clusters, the t- statistics tends to over-reject (Cameron et al., 2008; Cameron & Miller, 2015; Mackinnon & Webb, 2017, 2018). Therefore, I model equation (1) using wild cluster bootstrapped t-statistics to create confidence intervals (Roodman et al., 2019). The p-values from the wild bootstrapped t-statistics are clustered by state with 1000 repetitions (Roodman, 2022). While the coefficients and the t-statistics are the same as they would be in a cluster robust model, the bootstrapping of the t-statistics changes the p-values (which reveal the significance) since it uses the bootstrapped distribution rather than the normal distribution. Compared to the asymptotic normal distribution, the bootstrapped distributions lead to better approximations of my outcomes (Cameron et al., 2008; MacKinnon et al., 2023).

4.2 Instrumental Variables: The Effect of Alternative Response Utilization on Re-reporting

CPS agencies that are reformed for DR vary in how often they use alternative responses.[11] To understand how this variation affects my outcomes of interest, I show how alternative response utilization affects re-reporting. For this analysis, I present results from instrumental variable (IV) regressions in which the state treatment (i.e., the existence of DR policy that year) is used to instrument child-level treatment (i.e., the placement of a child's report on the AR track). The first stage ensures that variation in the instrument is only derived from variation in AR utilization across

[11] Alternative response utilization by state year can be seen in Appendix Table B5.

states and over time to then subsequently estimate the unbiased local average treatment effect (LATE) of AR on subsequent maltreatment (i.e., re-reporting).

The appropriateness of this IV approach relies on two assumptions: (1) the DR policy is predictive of variation in AR utilization; (2) the DR reform only affects safety, as measured by re-reporting, through its effect on use of the AR track, so the DR policy is uncorrelated with the error term. Weak identification tests demonstrate that the state treatment is a strong instrument. The second assumption is reasonable because the primary purpose of the DR reform is to change CPS interactions for families on the AR track. Thus, it is improbable that DR (the instrument) would also belong in the equation simultaneously. However, it is possible that states with greater utilization of the AR track differ from states with lower or no utilization on characteristics associated with child maltreatment re-reporting. Even after accounting for possible confounding effects of time and location, variation in AR utilization is still a function of maltreatment risk. Child characteristics, maltreatment characteristics, and socioeconomic factors are likely to be correlated with unobserved report characteristics.

I estimate fixed-effects two-stage least squares (2SLS) regressions that are pooled across years. The first stage model is a LPM that is specified as:

$$AR_i = \beta_1 + \beta_2 DR_{s(i)} + \beta_3 X_i + \beta_{s(i)} + \mu_i \quad (2)$$

where β_2 is the coefficient for the effect of DR on the AR track. The outcome, AR_i, is a binary variable equal to one when report-child i is placed on the AR track, and equal to zero when placed on the IR track. The model also includes the vector of control variables, X_i, and state fixed effects, $\beta_{s(i)}$. The predicted value of AR is then used in place of observed AR track placement in the second stage equation:

$$\Pr(Y_i) = \gamma_1 + \gamma_2 \widehat{AR}_i + \gamma_3 X_i + \gamma_{s(i)} + \epsilon_i \quad (3)$$

where, the coefficient γ_2 represents the exogenous effect of the AR track on the probability of re-reporting outcomes, Y_i, for report-child i. X_i is a vector of control variables for report-child i. State fixed-effects, $\gamma_{s(i)}$, account for time-invariant differences between states and, ϵ_i is the error term with standard properties.

5. Results

5.1 Difference-in-Difference: The Effect of Differential Response Policies on Re-reporting

First, I show the effect of DR reforms on re-reporting outcomes by leveraging the staggered treatment timing. In Panel A, the TWFE models cluster standard errors by state and use wild-cluster bootstrapped t-statistics to create confidence intervals and determine significance (Cameron et al., 2008; MacKinnon at al., 2022; Roodman, 2022). Wild bootstrap is used because of the small number of treated state clusters. To assess the pre-trends, Figure 4 shows the corresponding event study with 5 periods before and 5 periods after the DR reform. As shown in Table 1, DR policies marginally increase the likelihood of a re-report at all three intervals. DR increases the likelihood of a r-report 2 percentage points, which is a relatively large effect compared to the average likelihood of a re-report within 6 months (0.21), 12 months (0.30) and 3 years (0.46). The significance of this increase is significant at the 5 percent level within 6 months and 12 months, and at the 1 percent level within 3 years. There is no effect DR on re-reporting with substantiation within any of the three bounded time intervals.

Heterogeneity by Risk Tercile

DR policies are intended to allow CPS to work with lower risk families by providing referrals to services via the AR track. Therefore, we might expect to find the effect of DR policies on re-

reporting to be concentrated for reports of lower to medium risk. Next, I look at effects by risk levels. To do this, a re-report propensity score was created in periods without DR and was assigned to observations in no-DR *and* DR periods to hold constant the "risk" of a maltreatment re-report (see Appendix Table B4). In periods with DR, this creates a reasonable counterfactual risk measure for what re-reporting would be in the absence of the DR reform. Using the propensity score, I partition report-child observations in each state into risk terciles: low-risk, medium-risk, and high-risk. Table 2 shows the heterogeneity in the TWFE estimates by risk tercile. The effects of DR reforms on re-reporting outcomes are significant for the middle-risk and high-risk terciles.

Heterogeneity by Race

Child welfare policies and practices merit attention for many reasons including because they can reduce or contribute to racially disparate processes. DR is a particularly interesting, existing policy because it has been recognized as a strategy that could potentially reduce disproportionality and disparity in foster care placements (Lemon et al., 2008; Martin & Connelly, 2015). One evaluation of a pilot DR program in 10 Ohio counties in 2006 showed a decrease in foster care placements across all races, including a reduction in the number of African American children (Kaplan & Rohm, 2010). However, a decline in the number of African American foster care placements does not necessarily lead to reductions in disparities or disproportionalities.

Table 3 shows the TWFE results disaggregated by the race of the reported child. DR policies are associated with higher likelihoods of re-reporting for White children and children of two or more races within 6 months, 12 months, and 3 years. On average, the re-reporting outcomes for Black and Asian or Pacific Islander children are unaffected by the policy. American Indian and Alaskan Native children in states with DR have marginally higher likelihoods of being re-reported within 6 months, but not necessarily 12 months or

3 years. Finally, when looking at re-reporting with substantiation, there are null effects of DR policies on children of all races across all three intervals of time. To definitively determine the overall efficacy of DR at reducing racial disproportionality and disparities in the child welfare system a stronger evidence base is needed (J. Fluke et al., 2010).

5.2 Instrumental Variables: The Effect of Alternative Response Utilization on Re-reporting

The effect of AR utilization on re-reporting is found using instrumental variables and the second stage of the 2SLS results are shown in Table 4. The covariates in this model include the vector of characteristics related to the child, the report, and the location in both stages of the 2SLS. Ceteris paribus, increasing the use of AR is associated with higher likelihoods of a re-report within 6 months, 12 months, and 3 years. Use of the AR track is associated with a 11-percentage point higher likelihood of a re-report within 6 months; a 16-percentage point higher likelihood of a re-report within 12 months; and a 20-percentage point higher likelihood of a re-report within 3 years. However, increasing the use of AR is associated with lower likelihoods of re-reporting with substantiations within 3 years.

6. Summary and Concluding Remarks

6.1 Summary and Implications

The investigative response to *all* reports has been met with pertinent concerns that CPS involvement, in the form of an investigation, can be adversarial for children and their caretakers, particularly because it may place undue scrutiny on families at the intersection of poverty and suspected child maltreatment. These concerns surrounding investigative protocol are a part of a larger movement in child welfare which emphasizes family-focused, strengths-based approaches to child welfare (National Quality Improvement Center, 2011).

Descriptive analysis allows me to establish who is getting placed on the AR track in states with differential response. I find that reports made by education, child care, substitute care personnel, and medical and mental health personnel are more likely to receive an AR, relative to reports made by social services personnel. Children who are flagged as prior victims in CPS and children who are reported due to physical abuse or sexual abuse are less likely to receive an AR. Ceteris paribus, age, race and ethnicity are also predictive of who receives an AR. This is seen for younger children (<5 years old) relative to children who are age 5, as well as Black, and Asian or Pacific Islander children, relative to White children, and Hispanic children relative to non-Hispanic children.

While previous studies predominately look at state-level outcomes such as re-reporting rates, I look at outcomes for individual children, and unlike previous studies, my models control for children's reported characteristics (e.g., race, age) to improve our understanding of how children are affected by the adoption of this popular reform. In my main empirical analyses, I find that on average DR policies marginally increase the likelihood of a re-report within 6 months and 12 months, but not within 3 years. Disaggregated results show that this is driven by children who would be considered medium-risk and high-risk in the absence of the policy based on propensity score weighting from the periods without DR. When disaggregated by race, the increase the likelihood of a re-report is driven by White children and children of two or more races. The results that are significant in the aggregate analysis and disaggregated analysis are nonnegligible relative to the average likelihood of re-reporting at each of these intervals. Yet, on average, there is no effect DR on re-reporting with substantiation within any of the three bounded time intervals. Disaggregating the results by risk tercile or race of the reported child affirms that DR policies do not on average increase re-reporting with substantiation for these risk and racial groups of interest.

Finally, using IV methods I find that increasing the use of AR is associated with a significantly higher likelihood of a re-report within 6 months, 12 months, and 3 years, but a lower likelihood of re-reporting with substantiations within 3 years.

6.2 Limitations

To study the effect of DR on child safety in a feasible context, I made numerous author-led decisions. Thus, the conclusions of this study should be understood relative to its limitations. The first limitation is related to the data. It is known that administrative CPS data underestimates child maltreatment (e.g., Hussey et al., 2006; Swahn et al., 2006), thus my outcomes (re-reporting and re-reporting with substantiation) will miss any subsequent maltreatment that went unreported. Missed ('undercounted' or 'underreported') incidences of chronic or acute maltreatment would exist for children who did not get referred to CPS, and for children whose incidences were referred to CPS but were screened out (i.e., they did not formally become reports). Data on referrals to CPS (i.e., allegations at intake) would marginally improve upon the data-bound limitation by including the screened-out referrals. Yet, identifying incidences that are not referred to CPS would be more difficult.

The second limitation is related to the exclusions made in the sample creation. In this study I limited my analyses to DR systems that only make the determination for AR *after a report has been screened in*. Thus, I cannot comment on DR systems that screen out to the AR track. Here, data on referrals to CPS (with response indicators for screened out AR) would drastically improve upon this limitation and make it possible to incorporate both determination timings. Furthermore, to have a consistent sample throughout this study, I excluded states with both implemented and discontinued DR over this period. I also excluded states that were always treated (e.g., Minnesota).

6.3 Job Market Paper Note: Other Forthcoming Work on Differential Response

Whereas all reports are traditionally investigated, a key premise of this study is that DR reforms introduce a new step in decision making for whether to place the report on the AR track or the IR track. Evaluating the effect of the IR track or the AR track on safety as measured by re-reporting outcomes is complicated due to selection. Notably, both the outcome (a re-report) and the child-level treatment are influenced by a child's risk and other possible confounders. The unobserved differences in characteristics of higher risk reports and lower risk reports should be critically considered. These unobservable confounders may not be directly known by the agency at the time of decision making, or the researcher in retrospective analysis. The seemingly simple decision-making scenario turns out to be surprisingly complex, with important child welfare and policy implications.

An empirical approach that models endogenous selection mechanisms and accounts for a reported child's characteristics is hereby needed. With these considerations, in a separate paper, I assess whether there is an association between CPS decision-making and use of AR and IR tracks and subsequent maltreatment outcomes. See Appendix D for a theoretical model for child welfare decision making.

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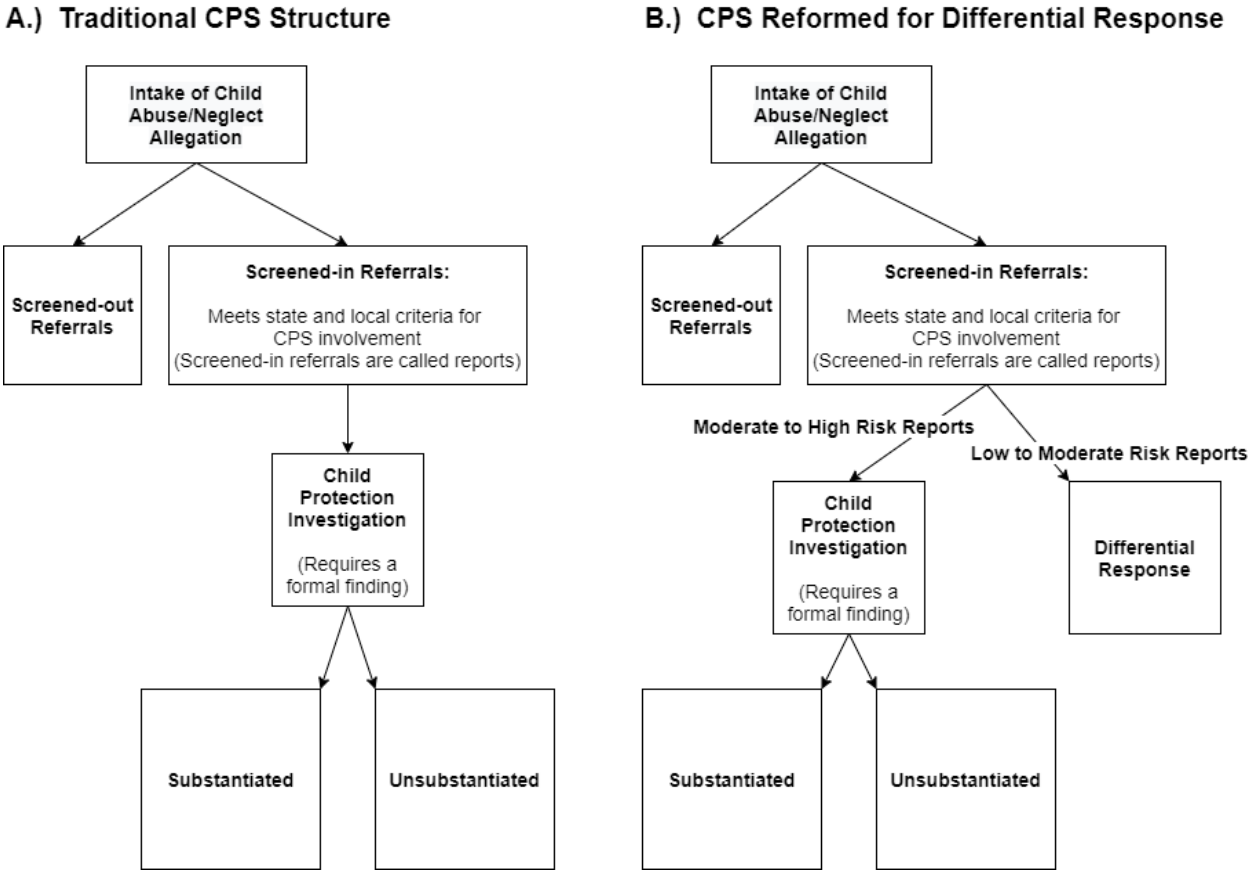
FIGURES AND TABLES

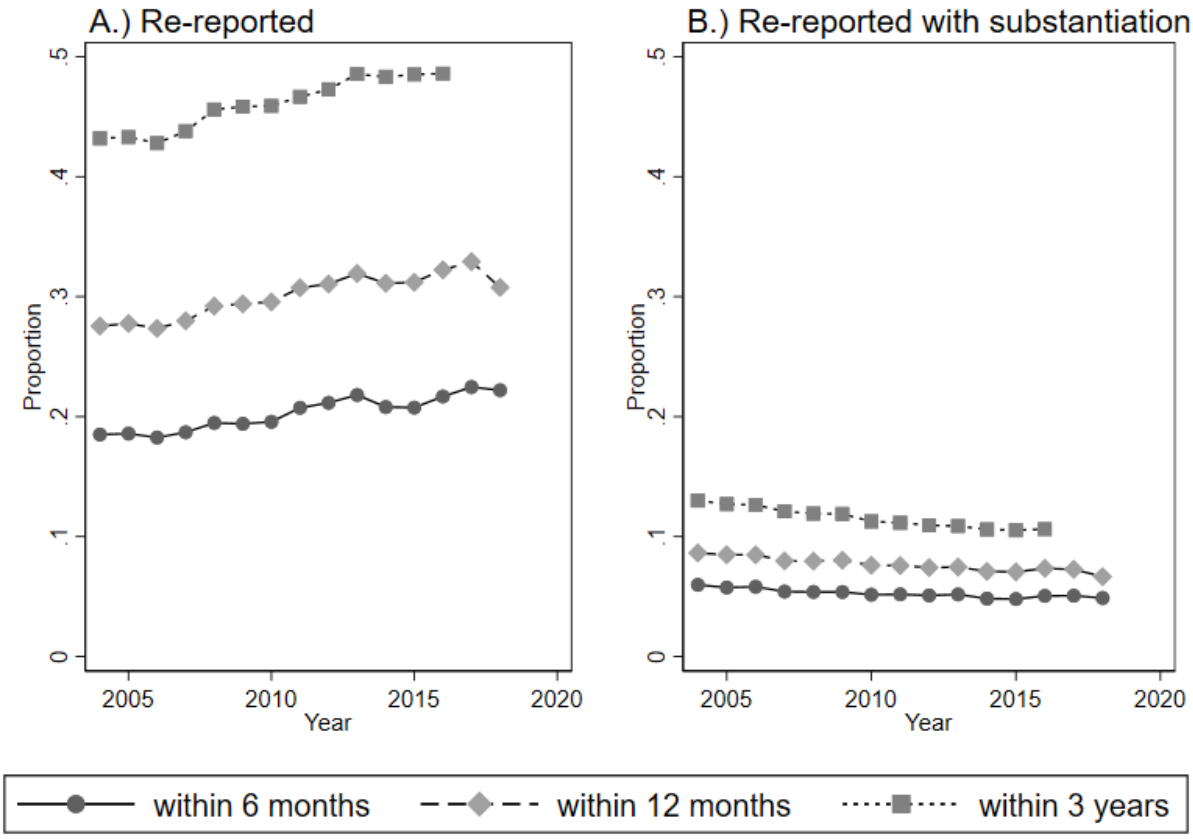
FIGURES

Figure 1. Types of Child Protection Services (CPS) structure



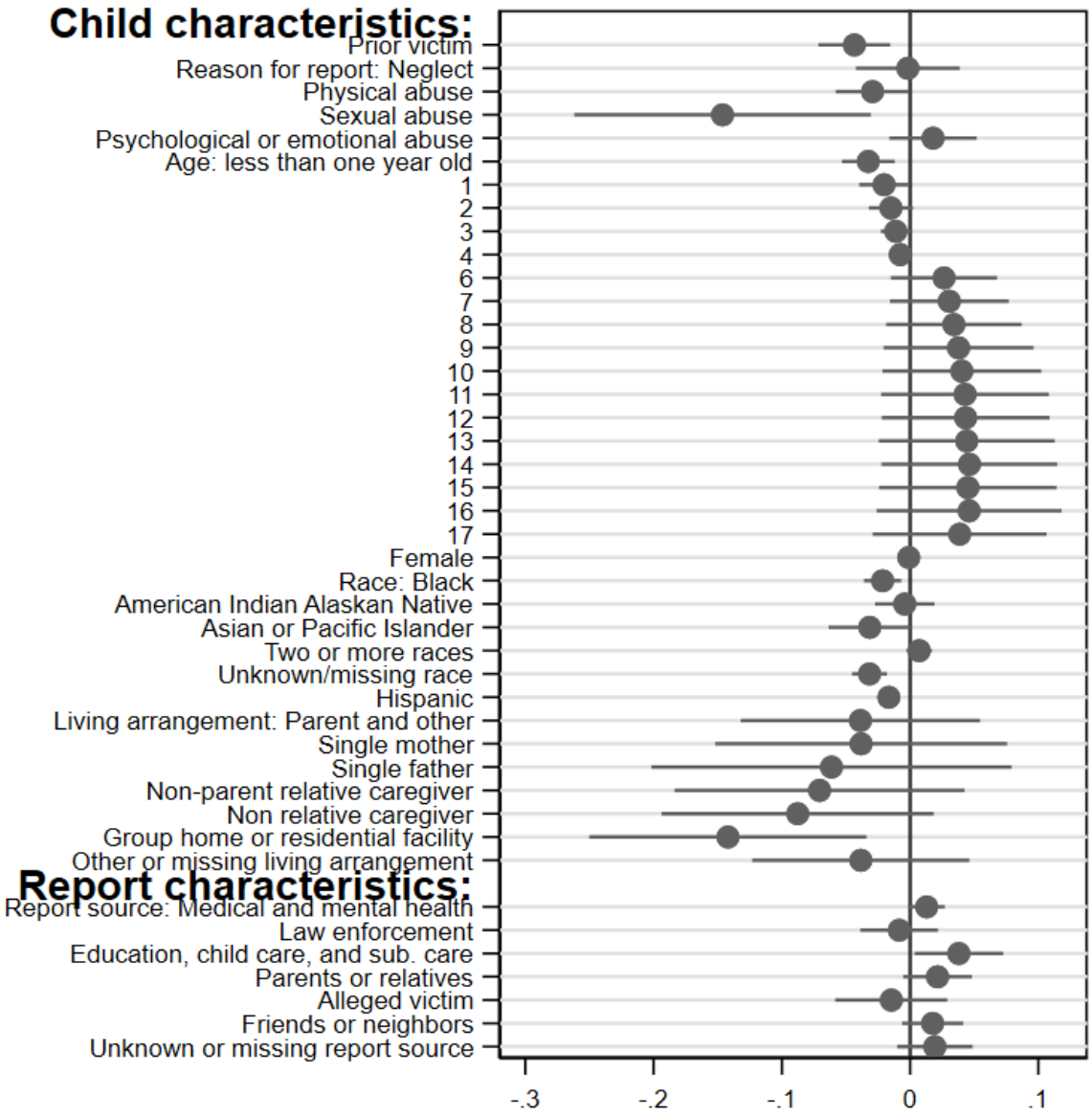
Notes. Author's creation. Panel A is a traditional one-track investigative CPS system. Panel B is a DR system that has two tracks, and *only* makes the track determination after a report is screened in.

Figure 2. Trends in re-reporting outcomes by year of the report



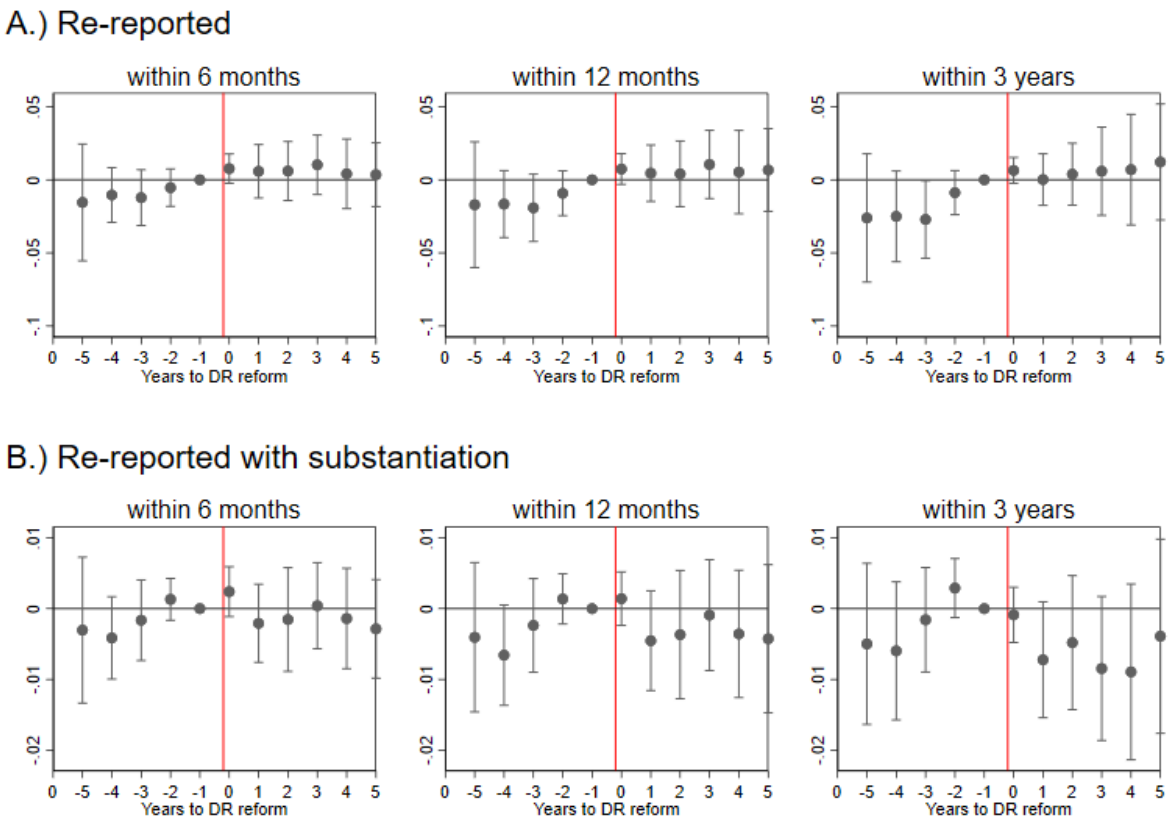
Notes. For my analytic sample of maltreatment reports, I show the re-reporting outcomes pooled across states by the calendar year in which the initial report was made. Panel A shows the proportion of report-child observations that are re-reported each year given different intervals of time. Panel B shows the proportion of report-child observations that are re-reported with substantiation each year given different intervals of time.

Figure 3. Predictors of an alternative response in periods with DR reform



Notes. In this coefficient plot, the circles indicate point estimates and bars indicate the 95 percent confidence intervals. Omitted reference groups which are not shown include: non-prior victim, no neglect, no physical abuse, no sexual abuse, no psychological or emotional abuse, age 5, race=White, non-Hispanic, living arrangement=both parents (married, unmarried, or unknown marital status), report source=social services personnel. The coefficients for months, local characteristics, and state and year fixed effects are not shown. The ordinary least squares regressions use cluster robust standard errors that are clustered by state. The full specification is shown in column 3 of Appendix Table B3.

Figure 4. Event studies estimating the effects of differential response reforms on re-reporting outcomes



Notes. The event studies control for child and report characteristics, local demographic and economic factors, and state and year fixed effects. Bars indicate the 95 percent confidence intervals.

TABLES

Table 1. Estimating the effects of differential response reforms on re-reporting outcomes

	DV = Re-reported			DV = Re-reported w. substantiation		
	within 6 mo.	within 12 mo.	within 3 yrs.	within 6 mo.	within 12 mo.	within 3 yrs.
	(1)	(2)	(3)	(4)	(5)	(6)
Mean outcome	0.21	0.30	0.46	0.05	0.08	0.11
Panel A. Two-way fixed effects						
Differential Response	0.02**	0.02**	0.02*	0.00	0.00	-0.00
	[0.001, 0.038]	[0.001, 0.046]	[-0.003, 0.051]	[-0.003, 0.008]	[-0.008, 0.009]	[-0.011, 0.010]
Adjusted R-squared	0.047	0.062	0.093	0.023	0.029	0.037
Observations	15,660,574	15,660,574	12,706,342	15,660,574	15,660,574	12,706,342

Notes. Regressions in Panel A control for child and report characteristics, administrative CPS outcomes, local demographic and economic factors, and state and year fixed effects. Standard errors are clustered by state using wild-cluster bootstrapped t-statistics to create confidence intervals and determine significance. Wild bootstrap is used because of the small number of treated state clusters. The 95% confidence intervals are in brackets. Significance levels: * 10 percent level, ** 5 percent level, *** 1 percent level.

Table 2. Estimating the effects of differential response reforms on re-reporting outcomes, by risk tercile

	DV = Re-reported			DV = Re-reported w. substantiation		
	within 6 mo.	within 12 mo.	within 3 yrs.	within 6 mo.	within 12 mo.	within 3 yrs.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Low risk tercile						
Differential Response	0.00	0.00	-0.00	-0.00	0.00	-0.01
	[-0.008 0.008]	[-0.008, 0.013]	[-0.008, 0.006]	[-0.013, 0.008]	[-0.011, 0.015]	[-0.014, 0.003]
Observations	5,220,204	5,220,215	4,202,890	5,220,236	5,220,202	4,231,177
Adjusted R-squared	0.023	0.031	0.057	0.006	0.010	0.016
Mean: outcome	0.15	0.23	0.35	0.03	0.05	0.07
Mean: AR | Low risk tercile, DR	0.11	0.11	0.11	0.12	0.12	0.11
Panel B. Middle risk tercile						
Differential Response	0.02	0.03*	0.02**	0.00	0.00	-0.00
	[-0.012, 0.047]	[-0.007, 0.064]	[0.006, 0.052]	[-0.004, 0.008]	[-0.004, 0.013]	[-0.006, 0.006]
Observations	5,220,201	5,220,183	4,239,644	5,220,180	5,220,222	4,190,751
Adjusted R-squared	0.032	0.039	0.052	0.016	0.022	0.030
Mean: outcome	0.20	0.31	0.47	0.05	0.07	0.11
Mean: AR | med. risk tercile, DR	0.13	0.13	0.13	0.12	0.12	0.13
Panel C. High risk tercile						
Differential Response	0.01	0.02**	0.02***	0.01	0.02**	0.02***
	[-0.002, 0.036]	[0.002, 0.044]	[0.005, 0.045]	[-0.005, 0.044]	[0.001, 0.051]	[0.008, 0.049]
Observations	5,220,169	5,220,176	4,263,808	5,220,158	5,220,150	4,284,414
Adjusted R-squared	0.054	0.066	0.076	0.029	0.037	0.044
Mean: outcome	0.28	0.41	0.60	0.08	0.12	0.17
Mean: AR | high risk tercile, DR	0.10	0.10	0.09	0.09	0.09	0.09

Notes. Risk terciles were constructed within state using weights from the periods without DR to assign a reasonable counterfactual risk measure for report-child observations in DR periods; the assigned "risk" of a re-report is based on what it would be in the absence of the DR reform. The ordinary least squares regressions control for child and report characteristics, administrative CPS outcomes, local demographic and economic factors, state and year fixed effects, and cluster standard errors by state and use wild-cluster bootstrapped t-statistics to create confidence intervals and determine significance. Wild bootstrap is used because of the small number of treated state clusters. The 95% confidence intervals are in brackets. Significance levels: * 10 percent level, ** 5 percent level, *** 1 percent level.

Table 3. Estimating the effects of differential response reforms on re-reporting outcomes, by race of the reported child

	DV = Re-reported			DV = Re-reported w. substantiation		
	within 6 mo.	within 12 mo.	within 3 yrs.	within 6 mo.	within 12 mo.	within 3 yrs.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. White						
Differential Response	0.02**	0.02*	0.02*	0.00	0.00	-0.00
	[-0.002, 0.041]	[-0.000, 0.048]	[-0.002, 0.051]	[-0.004, 0.008]	[-0.009, 0.009]	[-0.012, 0.009]
Observations	10,429,224	10,429,224	8,494,825	10,429,224	10,429,224	8,494,825
Adjusted R-squared	0.049	0.064	0.096	0.022	0.028	0.035
Mean: outcome	0.22	0.32	0.48	0.05	0.08	0.11
Mean: AR | White, DR	0.13	0.13	0.13	0.13	0.13	0.13
Panel B. Black						
Differential Response	0.01	0.02	0.02	0.00*	0.01	-0.00
	[-0.007, 0.034]	[-0.011, 0.048]	[-0.018, 0.052]	[-0.001, 0.011]	[-0.002, 0.017]	[-0.005, 0.016]
Observations	3,563,321	3,563,321	2,863,724	3,563,321	3,563,321	2,863,724
Adjusted R-squared	0.0341	0.0470	0.0781	0.0240	0.0334	0.0451
Mean: outcome	0.20	0.30	0.47	0.05	0.08	0.12
Mean: AR | Black, DR	0.10	0.10	0.10	0.10	0.10	0.10
Panel C. American Indian, Alaskan Native						
Differential Response	0.02	0.02	0.02	-0.00	-0.00	-0.00
	[-0.008, 0.045]	[-0.036, 0.058]	[-0.027, 0.072]	[-0.014, 0.017]	[-0.022, 0.023]	[-0.041, 0.021]
Observations	136,457	136,457	115,800	136,457	136,457	115,800
Adjusted R-squared	0.030	0.041	0.066	0.018	0.026	0.036
Mean: outcome	0.21	0.31	0.48	0.06	0.08	0.12
Mean: AR | AIAN, DR	0.09	0.09	0.09	0.09	0.09	0.09
Panel D. Asian or Pacific Islander						
Differential Response	0.00	0.01	0.01	-0.00	-0.00	-0.00
	[-0.008, 0.034]	[-0.003, 0.041]	[-0.011, 0.055]	[-0.005, 0.007]	[-0.008, 0.007]	[-0.012, 0.015]

Observations	119,250	119,250	94,091	119,250	119,250	94,091
Adjusted R-squared	0.034	0.048	0.074	0.024	0.031	0.042
Mean: outcome	0.12	0.18	0.27	0.03	0.05	0.07
Mean: AR | API, DR	0.08	0.08	0.08	0.08	0.08	0.08
Panel E. Two or more races						
Differential Response	0.03**	0.03**	0.03**	-0.00	-0.00	-0.00
	[0.002, 0.060]	[0.005, 0.080]	[0.007, 0.087]	[-0.007, 0.015]	[-0.010, 0.014]	[-0.014, 0.013]
Observations	766,054	766,054	581,336	766,054	766,054	581,336
Adjusted R-squared	0.041	0.054	0.081	0.020	0.026	0.032
Mean: outcome	0.26	0.38	0.56	0.06	0.09	0.13
Mean: AR | API, DR	0.16	0.16	0.16	0.16	0.16	0.16

Notes. The ordinary least squares regressions control for child and report characteristics, administrative CPS outcomes, local demographic and economic factors, state and year fixed effects, and cluster standard errors by state and use wild-cluster bootstrapped t-statistics to create confidence intervals and determine significance. Wild bootstrap is used because of the small number of treated state clusters. The 95% confidence intervals are in brackets. Significance levels: * 10 percent level, ** 5 percent level, *** 1 percent level.

Table 4. Estimating the effects of alternative response utilization on re-reporting outcomes

	DV = Re-reported			DV = Re-reported w. substantiation		
	within 6 mo.	within 12 mo.	within 3 yrs.	within 6 mo.	within 12 mo.	within 3 yrs.
	(1)	(2)	(3)	(4)	(5)	(6)
AR Track (second stage)	0.11*	0.16**	0.20**	-0.03**	-0.04***	-0.09***
	(0.06)	(0.07)	(0.09)	(0.01)	(0.01)	(0.02)
Adjusted R-squared	0.044	0.057	0.089	0.023	0.029	0.037
Observations	15,660,574	15,660,574	12,706,342	15,660,574	15,660,574	12,706,342

Notes. The key variable results for the two-stage least squares (2SLS) are shown for stage 2. In stage 1, DR policy was used to instrument AR track placement. The F-statistic in the first stage is 1,968.63 and the p-value for this F-statistic is 0.000. Regressions control for child, report, and local characteristics, as well as state fixed effects, and cluster standard errors by state. Cluster robust standard errors are shown in parentheses. Significance levels: * 10 percent level, ** 5 percent level, *** 1 percent level.

APPENDICES

Appendix A. Analytic Sample Construction of Reported Children in NCANDS

In my analytic sample construction for maltreatment reports, I first make systematic state-year exclusions, then I make child exclusions across states. Note that NCANDS is a voluntary system. Therefore, the data on reported children begins an unbalanced panel of state-years due to some the fact that some states did not submit data to NCANDS for some fiscal years.

- *State-year exclusion criterion*

Systematic exclusions of state-year pairs are made to fit a feasible context of the studying DR and re-reporting. While the analysis throughout this study refers to 'year' as the 'year of the report,' in creating the sample, state-years exclusions are based on NCANDS Child File fiscal years.

- To be able to identify maltreatment re-reporting, I need to be able to link children across reports. The first set of state-year exclusions are based on state-years that were not linkable by child ID. Breaks in the child ID links were determined by NDACAN statisticians and shared with the author in the form of a Child File linking grid Excel file. I drop state-years in which no reported children can be linked to reports in preceding years (i.e., 0% success rate for linking). Then, the state-year pairs that occur prior to a year with child ID linkage break for that state are dropped as well to ensure continuity.

- Second, I exclude all report-child observations from Alaska, Nevada, and New Jersey due to an incorrect use of alternative response codes or implementation of a program that was inconsistent with DR core components. This comes from Johnson-Motoyama et al. (2023), who had validated their characterization of DR using longitudinal information collected by the QIC-DR regarding actual DR program implementation and direct outreach to states. Given their reasoning for placing states in the control group (non-DR reforming) in their paper, I choose to exclude these states from my analyses.

- The third set of exclusions is for states that have DR systems that make determinations of track at the time of screening to screen out to AR. This is because NCANDS data do not provide the appropriate universe for analysis of those states; it would be more appropriate to use data on referrals to CPS, rather than data on CPS reports.

- The fourth set of exclusions is for states that are always treated. This exclusion is done to avoid issues that arise for comparisons between already-treated observations as control and later-treated observations as treatment. The comparison would specifically introduce bias to the TWFE estimate when the treatment effects are heterogeneous (Baker et al., 2022; Borusyak et al., 2023; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021)

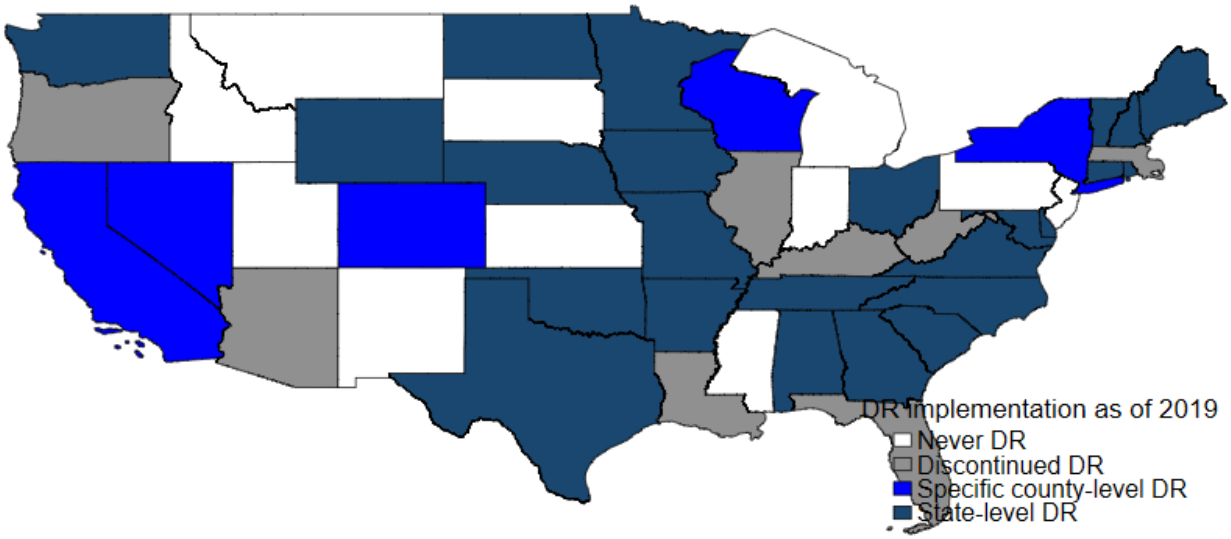
- The fifth set of exclusions is for states that discontinue the use of DR over this time frame.

- *Child exclusion criterion*

Then for child-level exclusions, I limit my sample to only consist of reported children ages 0 – 17 at the time of report. I also drop observations where the report disposition is categorized as 'other' or 'unknown/missing.' Based on the maltreatment allegation type, I also drop observations where 'neglect,' 'physical abuse,' 'emotional or psychological abuse,' or 'sexual abuse' were not at least one of the (up to four) documented reasons for the report. Again, these four types of child maltreatment are the most recognized and documented. The reports that were dropped had 'medical neglect,' 'no alleged maltreatment,' 'other,' or 'unknown or missing' documented as the reasons(s) for the report. Note, these reasons are not universally used across all jurisdictions.

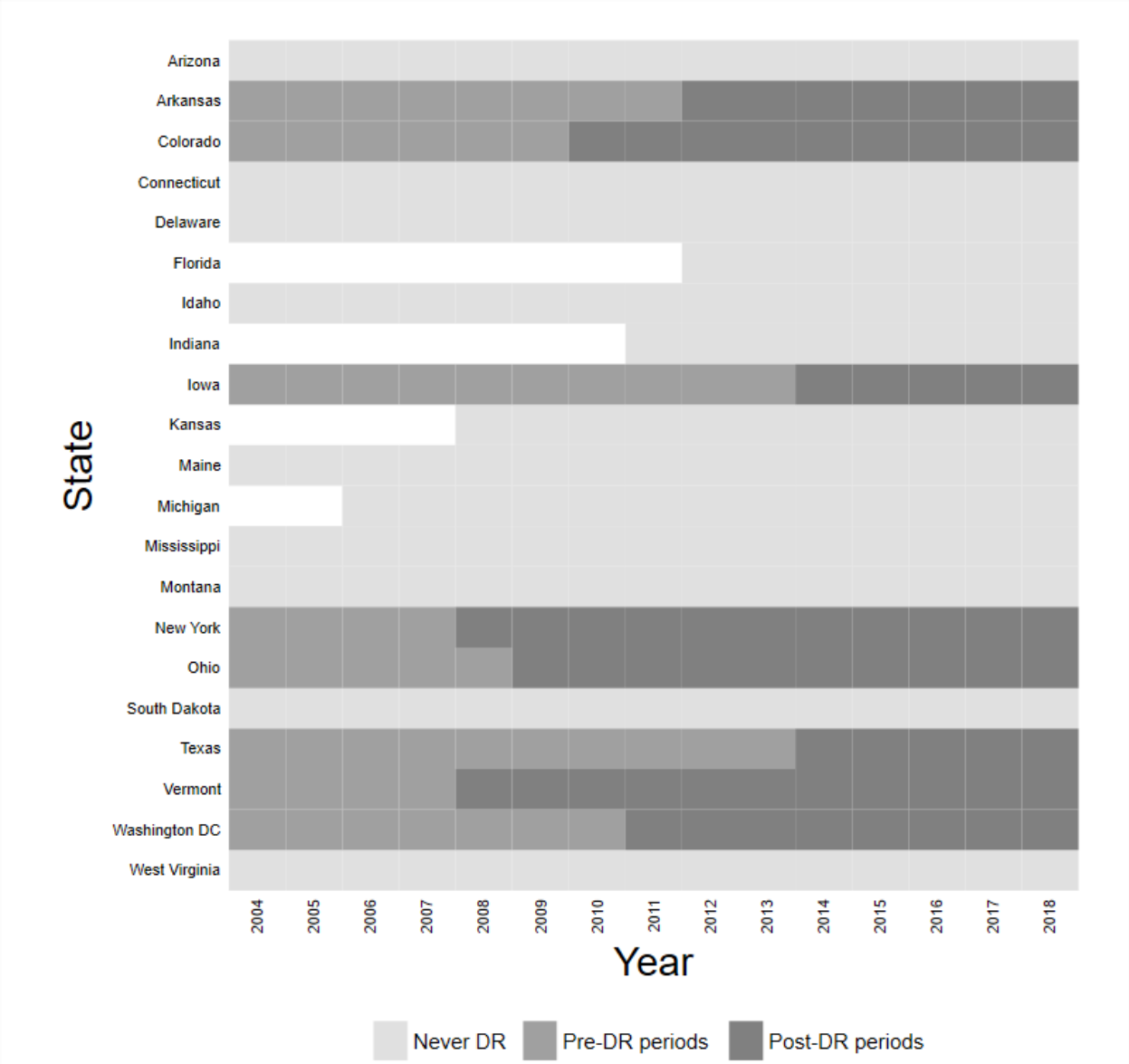
Appendix B. Supplementary Figures and Tables Noted in Text

Figure B1. Differential response implementation across U.S. states as of 2019



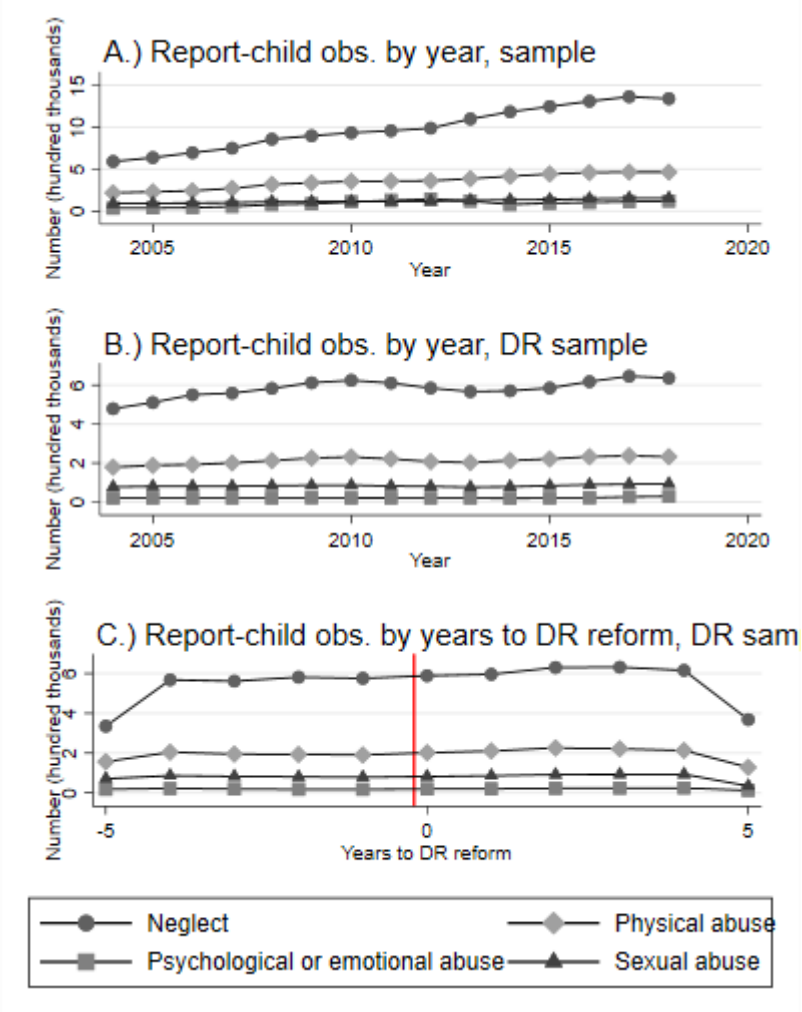
Notes. Differential response is a popular CPS reform. The states with statewide and specific county-level DR in 2019 were identified using the SCAN Policies Database 2019 (Weigensberg et al., 2022). The states that previously implemented DR but discontinued it were identified using other sources. By 2019, Alaska (not shown) had discontinued DR, Hawaii (not shown) had state-level DR, and the District of Columbia (not shown) had discontinued DR. Discontinuation occurs for numerous reasons including funding.

Figure B2. Differential response treatment status by year for states in my analytic sample



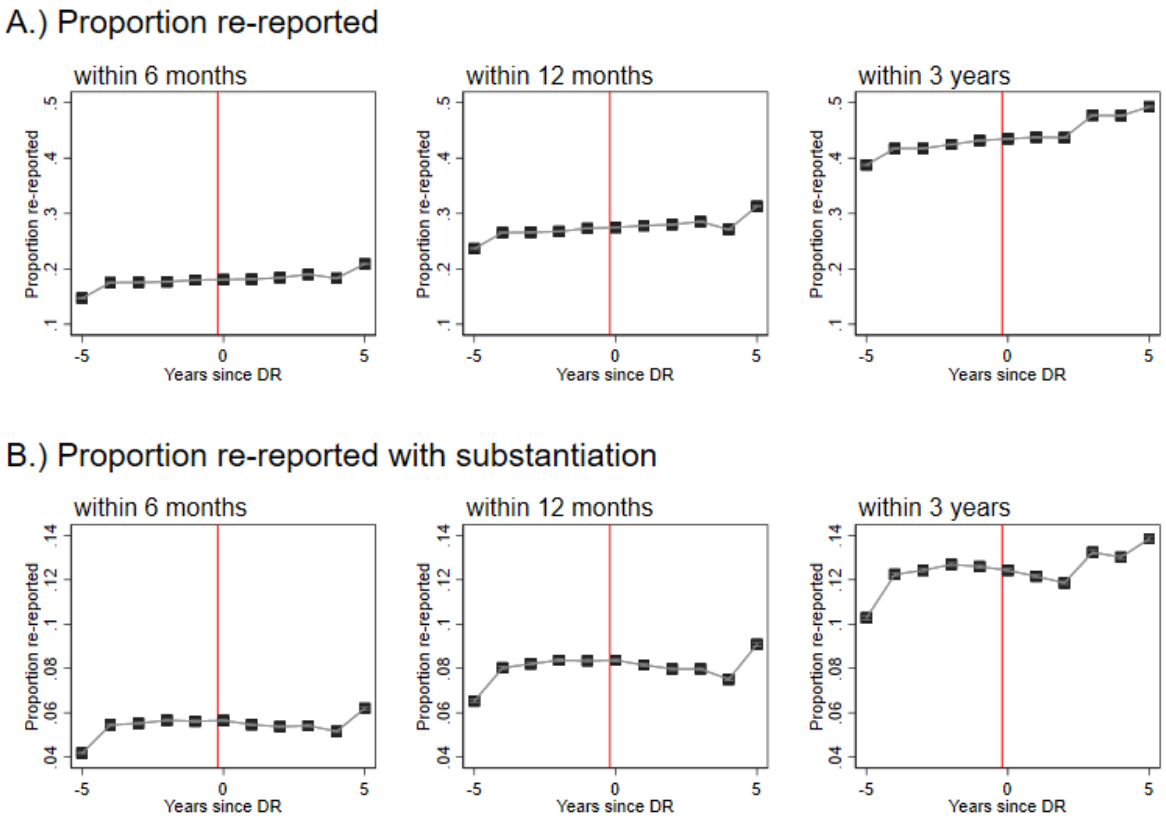
Notes. There are 21 states in the sample, and 8 of these states adopt DR between 2004 and 2019. These are specifically states that do not discontinue the use of DR before 2019, and states that make the track determination (AR or IR) after a report is screened in. The white cells indicate state-year pairs that are dropped from the sample (See Appendix A).

Figure B3. Number of annual reports-child observations by reasons for report



Notes. In panel A, the number of annual report-child observations are pooled across all sampled states (N=21 states) by the reason for the report. In panels B and C, the annual sums are pooled across the 8 states that adopt DR between 2004 and 2019 and do not discontinue in this time frame. In panel C, year 0 represents the relative year to DR reform.

Figure B4. Proportion of reports-child observations that are re-reported by year to DR reform



Notes. The average proportions of reports-child observations that are re-reported are pooled across the 9 states by the relative year to DR reform. These 9 states adopt DR between 2004 and 2019 and do not discontinue in this time frame.

Table B1. Characteristics of report-child observations in states that adopt DR policies between 2004 and 2019

	Pre-DR reform IR pathway (1)		**Post-DR reform** IR *or* AR pathway (2)		**Post-DR reform** IR pathway (3)		AR pathway (4)		**Difference: (2) - (1)** (5)			**Difference: (4) - (3)** (6)		
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Diff.*		*SE*	*Diff.*		*SE*
Child characteristics														
Prior victim	**0.24**	0.43	**0.30**	0.46	**0.31**	0.46	**0.22**	0.41	**0.06**	**	(0.02)	**-0.09**	**	(0.04)
Prior victim missing	**0.00**	0.06	**0.00**	0.07	**0.00**	0.04	**0.03**	0.17	**0.00**		(0.01)	**0.03**		(0.04)
Reason for Report														
Neglect	**0.75**	0.44	**0.79**	0.40	**0.81**	0.39	**0.70**	0.46	**0.05**		(0.05)	**-0.10**	*	(0.05)
Physical abuse	**0.30**	0.46	**0.27**	0.44	**0.26**	0.44	**0.36**	0.48	**-0.03**		(0.06)	**0.10**	*	(0.05)
Sexual abuse	**0.13**	0.34	**0.10**	0.29	**0.11**	0.31	**0.00**	0.06	**-0.03**	**	(0.01)	**-0.10**	**	(0.04)
Psychological/emotional abuse	**0.03**	0.17	**0.03**	0.17	**0.03**	0.16	**0.05**	0.22	**-0.00**		(0.00)	**0.02**	**	(0.01)
Age at report	**7.20**	4.99	**7.62**	5.07	**7.56**	5.08	**8.09**	4.92	**0.43**	*	(0.21)	**0.52**		(0.83)
Female	**0.51**	0.50	**0.50**	0.50	**0.50**	0.50	**0.48**	0.50	**-0.01**	**	(0.00)	**-0.02**	***	(0.00)
Female missing	**0.00**	0.06	**0.01**	0.07	**0.01**	0.07	**0.00**	0.06	**0.00**	**	(0.00)	**0.00**	*	(0.00)
Race														
White	**0.66**	0.47	**0.57**	0.50	**0.56**	0.50	**0.63**	0.48	**-0.09**	***	(0.03)	**0.07**		(0.06)
Black	**0.20**	0.40	**0.24**	0.43	**0.24**	0.43	**0.21**	0.41	**0.04**	**	(0.01)	**-0.04**		(0.02)
American Indian, Alaskan Native	**0.00**	0.05	**0.00**	0.06	**0.00**	0.06	**0.00**	0.05	**0.00**		(0.00)	**-0.00**		(0.00)
Asian or Pacific Islander	**0.01**	0.08	**0.01**	0.11	**0.01**	0.11	**0.01**	0.09	**0.00**	*	(0.00)	**-0.00**		(0.00)
Two or more races	**0.03**	0.17	**0.04**	0.21	**0.04**	0.20	**0.06**	0.24	**0.02**	*	(0.01)	**0.02**	*	(0.01)
Unknown/missing race	**0.10**	0.30	**0.13**	0.34	**0.14**	0.35	**0.09**	0.28	**0.04**		(0.02)	**-0.05**		(0.04)
Hispanic ethnicity	**0.32**	0.47	**0.27**	0.44	**0.29**	0.45	**0.17**	0.37	**-0.05**		(0.04)	**-0.12**	***	(0.03)
Hispanic ethnicity missing	**0.14**	0.35	**0.17**	0.37	**0.17**	0.37	**0.17**	0.38	**0.03**		(0.05)	**0.01**		(0.03)
Living arrangements														
Both parents	**0.12**	0.33	**0.29**	0.45	**0.29**	0.45	**0.29**	0.45	**0.16**	***	(0.03)	**0.00**		(0.08)
Parent and other	**0.04**	0.20	**0.10**	0.31	**0.10**	0.30	**0.15**	0.36	**0.06**		(0.04)	**0.06**		(0.04)
Single mother	**0.07**	0.26	**0.14**	0.34	**0.13**	0.34	**0.18**	0.39	**0.06**		(0.04)	**0.05**		(0.03)
Single father	**0.01**	0.10	**0.02**	0.13	**0.02**	0.13	**0.02**	0.15	**0.01**		(0.01)	**0.01**		(0.01)
Non-parent relative caregiver	**0.01**	0.11	**0.02**	0.15	**0.02**	0.14	**0.02**	0.15	**0.01**		(0.01)	**0.00**		(0.00)
Non-relative caregiver	**0.02**	0.12	**0.02**	0.12	**0.02**	0.13	**0.00**	0.07	**0.00**		(0.00)	**-0.01**	***	(0.00)
Group home or residential facility	**0.00**	0.06	**0.00**	0.05	**0.00**	0.06	**0.00**	0.02	**-0.00**		(0.00)	**0.00**	*	(0.00)

Other or missing living arrangements	**0.72**	0.45	**0.41**	0.49	**0.43**	0.49	**0.32**	0.47	**-0.31**	***	(0.08)	**-0.11**		(0.10)
Report characteristics														
Report source														
Social services personnel	**0.10**	0.30	**0.14**	0.34	**0.14**	0.35	**0.11**	0.31	**0.04**	***	(0.01)	**-0.03**	**	(0.01)
Medical and mental health personnel	**0.11**	0.31	**0.12**	0.32	**0.12**	0.32	**0.11**	0.31	**0.01**		(0.01)	**-0.01**		(0.01)
Law enforcement	**0.15**	0.36	**0.16**	0.36	**0.16**	0.37	**0.15**	0.36	**0.01**		(0.01)	**-0.01**		(0.01)
Education, child care, substitute care personnel	**0.16**	0.37	**0.17**	0.38	**0.17**	0.38	**0.22**	0.41	**0.01**	**	(0.01)	**0.05**	*	(0.02)
Parents or relative	**0.18**	0.38	**0.15**	0.36	**0.15**	0.35	**0.16**	0.37	**-0.03**		(0.02)	**0.02**		(0.01)
Alleged victim	**0.00**	0.05	**0.00**	0.04	**0.00**	0.04	**0.00**	0.05	**-0.00**		(0.00)	**0.00**		(0.00)
Friends or neighbors	**0.06**	0.24	**0.05**	0.22	**0.05**	0.22	**0.05**	0.22	**-0.01**		(0.01)	**0.00**		(0.00)
Other, unknown, or missing	**0.23**	0.42	**0.21**	0.41	**0.22**	0.41	**0.20**	0.40	**-0.02**		(0.02)	**-0.02**		(0.01)
Administrative CPS outcomes														
Substantiated	**0.30**	0.46	**0.27**	0.44	**0.31**	0.46	**0.00**	0.00	**-0.03**		(0.02)	**-0.31**	***	(0.02)
Foster care (FC) services	**0.16**	0.36	**0.17**	0.38	**0.18**	0.39	**0.11**	0.31	**0.02**		(0.02)	**-0.08**	***	(0.01)
Length of time in care | FC	**0.30**	1.07	**0.28**	0.94	**0.30**	0.99	**0.11**	0.47	**-0.02**		(0.05)	**-0.19**	***	(0.01)
Re-reported														
within 6 mo.	**0.16**	0.37	**0.20**	0.40	**0.20**	0.40	**0.18**	0.38	**0.04**	***	(0.01)	**-0.03**	**	(0.01)
within 12 mo.	**0.25**	0.43	**0.30**	0.46	**0.30**	0.46	**0.27**	0.44	**0.04**	***	(0.01)	**-0.03**	**	(0.01)
within 3 yrs.	**0.40**	0.49	**0.47**	0.50	**0.47**	0.50	**0.45**	0.50	**0.06**	***	(0.01)	**-0.02**		(0.01)
Re-reported w. sub.														
within 6 mo.	**0.05**	0.22	**0.06**	0.24	**0.06**	0.24	**0.03**	0.17	**0.01**		(0.01)	**-0.04**	***	(0.01)
within 12 mo.	**0.08**	0.27	**0.09**	0.28	**0.09**	0.29	**0.04**	0.20	**0.01**		(0.01)	**-0.05**	***	(0.01)
within 3 yrs.	**0.12**	0.32	**0.13**	0.34	**0.14**	0.35	**0.07**	0.25	**0.01**		(0.01)	**-0.07**	***	(0.01)
Observations	4,888,868		6,410,877		5,686,047		724,830		11,299,745			6,410,877		

Notes. Report-child observations are pooled from 2004 to 2019. In columns (5) and (6), the statistical significance of the difference between the two sets of mutually exclusive groups of observations comes from separate regressions of the group indicator (e.g., post-DR =1) on each characteristic. Cluster robust standard errors are clustered by state.

Table B2. Characteristics of report-child observations that are investigated in state-years with no DR reform (conventional investigative CPS structure) and state-years that are post-DR reform (CPS with DR reform)

	No DR reform: *Only IR* (Pre-DR *or* never DR reform) (1)		**Post-DR reform**: *Selected to be IR* (2)		**Difference: (2) - (1)** (3)		
	Mean	*SD*	*Mean*	*SD*	*Diff.*		*SE*
Child characteristics							
Prior victim	**0.28**	0.45	**0.31**	0.46	**0.03**		(0.05)
Prior victim missing	**0.02**	0.13	**0.00**	0.04	**-0.02**		(0.02)
Reason for Report							
Neglect	**0.76**	0.43	**0.81**	0.39	**0.04**		(0.08)
Physical abuse	**0.28**	0.45	**0.26**	0.44	**-0.02**		(0.05)
Sexual abuse	**0.09**	0.29	**0.11**	0.31	**0.02**		(0.03)
Psychological or emotional abuse	**0.09**	0.29	**0.03**	0.16	**-0.06**	**	(0.03)
Age at report	**7.42**	5.05	**7.56**	5.08	**0.15**		(0.19)
Female	**0.50**	0.50	**0.50**	0.50	**-0.00**		(0.00)
Female missing	**0.00**	0.07	**0.01**	0.07	**0.00**		(0.00)
Race							
White	**0.66**	0.47	**0.56**	0.50	**-0.10**	*	(0.05)
Black	**0.20**	0.40	**0.24**	0.43	**0.04**		(0.03)
American Indian, Alaskan Native	**0.01**	0.10	**0.00**	0.06	**-0.01**		(0.00)
Asian or Pacific Islander	**0.01**	0.07	**0.01**	0.11	**0.01**	*	(0.00)
Two or more races	**0.04**	0.21	**0.04**	0.20	**-0.00**		(0.01)
Unknown/missing race	**0.08**	0.26	**0.14**	0.35	**0.06**	*	(0.03)
Hispanic ethnicity	**0.22**	0.41	**0.29**	0.45	**0.07**		(0.04)
Hispanic ethnicity missing	**0.17**	0.38	**0.17**	0.37	**-0.00**		(0.04)
Living arrangements							
Both parents	**0.20**	0.40	**0.29**	0.45	**0.09**		(0.10)
Parent and other	**0.13**	0.33	**0.10**	0.30	**-0.03**		(0.04)
Single mother	**0.13**	0.34	**0.13**	0.34	**-0.00**		(0.04)

Single father	**0.02**	0.14	**0.02**	0.13	**-0.00**		(0.01)
Non-parent relative caregiver	**0.02**	0.15	**0.02**	0.14	**-0.00**		(0.01)
Non-relative caregiver	**0.01**	0.11	**0.02**	0.13	**0.00**	*	(0.00)
Group home or residential facility	**0.00**	0.06	**0.00**	0.06	**-0.00**		(0.00)
Other or missing living arrangements	**0.49**	0.50	**0.43**	0.49	**-0.06**		(0.13)
Report characteristics							
Report source							
Social services personnel	**0.11**	0.31	**0.14**	0.35	**0.03**		(0.03)
Medical and mental health personnel	**0.12**	0.32	**0.12**	0.32	**0.00**		(0.01)
Law enforcement	**0.16**	0.37	**0.16**	0.37	**-0.00**		(0.02)
Education, child care, substitute care personnel	**0.17**	0.38	**0.17**	0.38	**-0.00**		(0.01)
Parents or relative	**0.17**	0.38	**0.15**	0.35	**-0.03**	***	(0.01)
Alleged victim	**0.00**	0.06	**0.00**	0.04	**-0.00**		(0.00)
Friends or neighbors	**0.06**	0.24	**0.05**	0.22	**-0.01**	*	(0.01)
Other, unknown, or missing	**0.20**	0.40	**0.22**	0.41	**0.02**		(0.02)
Administrative CPS outcomes							
Substantiated	**0.22**	0.42	**0.31**	0.46	**0.08**	**	(0.03)
Foster care services	**0.20**	0.40	**0.18**	0.39	**-0.01**		(0.02)
Length of time in care | Foster care	**0.34**	1.02	**0.30**	0.99	**-0.04**		(0.03)
Re-reported							
within 6 mo.	**0.21**	0.41	**0.20**	0.40	**-0.01**		(0.02)
within 12 mo.	**0.31**	0.46	**0.30**	0.46	**-0.01**		(0.03)
within 3 yrs.	**0.46**	0.50	**0.47**	0.50	**0.00**		(0.03)
Re-reported w. sub.							
within 6 mo.	**0.05**	0.21	**0.06**	0.24	**0.02**		(0.01)
within 12 mo.	**0.07**	0.25	**0.09**	0.29	**0.02**		(0.02)
within 3 yrs.	**0.11**	0.31	**0.14**	0.35	**0.03**		(0.02)
Observations	12,772,738		5,686,047		18,458,785		

Notes. Report-child observations are pooled from 2004 to 2019. Column (1) shows average characteristics of reported children in state-years with no DR (i.e., cases from pre-DR state-years and never DR states). Column (2) shows average characteristics of reported children in state-years with DR currently in place (i.e., post-DR). In column (3), the statistical significance of the differences between the mutually exclusive groups of observations comes from separate regressions of the group indicator (e.g., post-DR =1) on each characteristic. Cluster robust standard errors are clustered by state.

Table B3. Descriptive regressions predicting an alternative response in periods with DR reform

	DV = Alternative Response Track		
	(1)	(2)	(3)
Child characteristics			
Prior victim	-0.04***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)
Reasons for report			
Neglect	-0.00	0.00	-0.00
	(0.02)	(0.02)	(0.02)
Physical abuse	-0.03*	-0.03*	-0.03**
	(0.01)	(0.01)	(0.01)
Sexual abuse	-0.15**	-0.14**	-0.15**
	(0.05)	(0.05)	(0.05)
Psychological/emotional abuse	0.02	0.02	0.02
	(0.01)	(0.02)	(0.01)
Child age at report (omit. age 5)			
Child age: 0	-0.04***	-0.03***	-0.03***
	(0.01)	(0.01)	(0.01)
Child age: 1	-0.02**	-0.02*	-0.02**
	(0.01)	(0.01)	(0.01)
Child age: 2	-0.02*	-0.01	-0.01*
	(0.01)	(0.01)	(0.01)
Child age: 3	-0.01*	-0.01*	-0.01*
	(0.01)	(0.00)	(0.00)
Child age: 4	-0.01*	-0.01*	-0.01*
	(0.00)	(0.00)	(0.00)
Child age: 6	0.03	0.03	0.03
	(0.02)	(0.02)	(0.02)
Child age: 7	0.03	0.03	0.03
	(0.02)	(0.02)	(0.02)
Child age: 8	0.03	0.03	0.03
	(0.02)	(0.02)	(0.02)
Child age: 9	0.04	0.04	0.04
	(0.02)	(0.02)	(0.02)
Child age: 10	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)
Child age: 11	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)
Child age: 12	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)
Child age: 13	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)
Child age: 14	0.05	0.05	0.05
	(0.03)	(0.03)	(0.03)
Child age: 15	0.04	0.04	0.04

	(0.03)	(0.03)	(0.03)
Child age: 16	0.05	0.05	0.05
	(0.03)	(0.03)	(0.03)
Child age: 17	0.04	0.04	0.04
	(0.03)	(0.03)	(0.03)
Sex (omit. male)			
Female	-0.00*	-0.00	-0.00*
	(0.00)	(0.00)	(0.00)
Race (omit. White)			
Black	-0.04**	-0.04**	-0.02**
	(0.02)	(0.01)	(0.01)
American Indian, Alaskan Native	-0.01	-0.01	-0.00
	(0.01)	(0.01)	(0.01)
Asian or Pacific Islander	-0.04**	-0.04**	-0.03*
	(0.01)	(0.01)	(0.01)
Two or more races	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)
Unknown/missing race	-0.04**	-0.04**	-0.03***
	(0.01)	(0.01)	(0.01)
Ethnicity (omit. non-Hispanic)			
Hispanic	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)
Living arrangements (omit. Both parents)			
Parent and other	-0.03	-0.03	-0.04
	(0.04)	(0.04)	(0.04)
Single mother	-0.04	-0.04	-0.04
	(0.05)	(0.05)	(0.05)
Single father	-0.06	-0.06	-0.06
	(0.06)	(0.06)	(0.06)
Non-parent relative caregiver	-0.07	-0.07	-0.07
	(0.05)	(0.05)	(0.05)
Non relative caregiver	-0.09*	-0.09*	-0.09*
	(0.05)	(0.04)	(0.04)
Group home or residential facility	-0.15**	-0.15**	-0.14**
	(0.05)	(0.05)	(0.05)
Other or missing living arrangements	-0.04	-0.04	-0.04
	(0.04)	(0.03)	(0.04)
Report characteristics			
Report source (omit. social services personnel)			
Medical and mental health personnel		0.01*	0.01*
		(0.01)	(0.01)
Law enforcement		-0.00	-0.01
		(0.01)	(0.01)
Education, child care, substitute care personnel		0.04**	0.04**
		(0.02)	(0.01)

Parents or relative		0.02	0.02
		(0.01)	(0.01)
Alleged victim		-0.02	-0.01
		(0.02)	(0.02)
Friends or neighbors		0.02	0.02
		(0.01)	(0.01)
Other, unknown, or missing		0.02	0.02
		(0.01)	(0.01)
Month of report (omit. January)			
February		0.00	0.00
		(0.00)	(0.00)
March		0.00	0.00
		(0.00)	(0.00)
April		0.00	0.01
		(0.00)	(0.00)
May		0.00	0.00
		(0.00)	(0.00)
June		0.00	0.00
		(0.00)	(0.00)
July		0.01	0.01
		(0.00)	(0.00)
August		0.01*	0.01*
		(0.00)	(0.01)
September		0.01*	0.01*
		(0.00)	(0.01)
October		0.01*	0.01*
		(0.01)	(0.01)
November		0.01	0.01*
		(0.01)	(0.01)
December		0.01	0.01
		(0.01)	(0.01)
Local demographic and economic factors	No	No	Yes
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	5,306,355	5,306,355	5,306,355
Adjusted R-squared	0.15	0.15	0.18

Notes. Report-child observations from periods with DR (i.e., post-DR) are used. Year and state fixed effects are binary indicators. The ordinary least squares regressions use cluster robust standard errors that are clustered by state. Standard errors are shown in parentheses. Significance levels: * 10 percent level, ** 5 percent level, *** 1 percent level.

Table B4. Descriptive regressions of a re-report in periods without DR reform

	DV = Re-reported						DV = Re-reported w. substantiation					
	within 6 mo.		within 12 mo.		within 3 yrs.		within 6 mo.		within 12 mo.		within 3 yrs.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Child characteristics												
Prior victim	0.11***	0.11***	0.15***	0.14***	0.18***	0.18***	0.05***	0.05***	0.06***	0.06***	0.07***	0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Reasons for report												
Neglect	0.03***	0.04***	0.05***	0.05***	0.06***	0.06***	0.01***	0.02***	0.02***	0.02***	0.02***	0.03***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Physical abuse	0.02***	0.01***	0.02***	0.01***	0.03***	0.02**	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Sexual abuse	0.00	0.00	0.00	-0.01	-0.01	-0.02**	0.00	0.00	-0.00**	0.00	-0.01***	-0.01**
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	-0.01	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Psychological/emotional abuse	0.02***	0.01	0.02***	0.01	0.02**	0.01	0.01***	0.00	0.01***	0.00	0.01***	0.00
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age at report (omit. age 5)												
Child age: 0	0.00	-0.01	-0.01	-0.01	0.00	0.00	0.02***	0.01***	0.02***	0.02***	0.03***	0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 1	0.01*	0.00	0.01**	0.01*	0.02***	0.02***	0.01***	0.01***	0.02***	0.02***	0.02***	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 2	0.00	0.00	0.00	0.00	0.01***	0.01***	0.01***	0.01***	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 3	0.00	0.00	0.00*	0.00	0.01***	0.01***	0.00***	0.00***	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 4	0.00	0.00	0.00	0.00	0.01***	0.01***	0.00***	0.00***	0.00***	0.00***	0.00***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 6	-0.00***	-0.00***	-0.00***	-0.00***	-0.01***	-0.00**	-0.00*	-0.00*	-0.00***	-0.00***	-0.00*	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 7	-0.01***	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***	-0.00***	-0.00***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 8	-0.02***	-0.01***	-0.02***	-0.02***	-0.03***	-0.03***	-0.00***	-0.00***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Child age: 9	-0.02***	-0.02***	-0.03***	-0.03***	-0.04***	-0.04***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 10	-0.03***	-0.03***	-0.04***	-0.04***	-0.05***	-0.04***	-0.01***	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 11	-0.03***	-0.03***	-0.04***	-0.04***	-0.05***	-0.04***	-0.01***	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 12	-0.03***	-0.02***	-0.04***	-0.03***	-0.05***	-0.04***	-0.01***	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 13	-0.03***	-0.02***	-0.03***	-0.03***	-0.05***	-0.04***	-0.01***	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 14	-0.03***	-0.02***	-0.04***	-0.03***	-0.06***	-0.06***	-0.01***	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 15	-0.04***	-0.03***	-0.06***	-0.05***	-0.13***	-0.12***	-0.01***	-0.01***	-0.02***	-0.02***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Child age: 16	-0.06***	-0.06***	-0.10***	-0.09***	-0.24***	-0.23***	-0.02***	-0.02***	-0.03***	-0.03***	-0.06***	-0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Child age: 17	-0.12***	-0.11***	-0.21***	-0.20***	-0.39***	-0.38***	-0.03***	-0.03***	-0.05***	-0.05***	-0.09***	-0.10***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Sex (omit. male)												
Female	0.00***	0.00***	0.01***	0.01***	0.01***	0.01***	0.00***	0.00***	0.00***	0.00***	0.01***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Race (omit. White)												
Black	-0.03***	-0.03***	-0.03***	-0.03***	-0.03***	-0.03**	-0.01***	-0.01***	-0.01***	-0.01***	0.00	-0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
American Indian, Alaskan Native	-0.01	-0.02**	-0.01	-0.02**	-0.01	-0.01	0.01**	0.00	0.01*	0.00	0.02**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Asian or Pacific Islander	-0.07***	-0.07***	-0.10***	-0.10***	-0.16***	-0.16***	-0.02***	-0.02***	-0.02***	-0.02***	-0.04***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Two or more races	0.02***	0.02***	0.03***	0.03***	0.04***	0.04***	0.01***	0.01***	0.01***	0.01***	0.01***	0.01***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Unknown/missing race	-0.06***	-0.06***	-0.08***	-0.08***	-0.11***	-0.11***	-0.02***	-0.02***	-0.03***	-0.02***	-0.03***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Ethnicity (omit. non-Hispanic)												
Hispanic	-0.02***	-0.02***	-0.03***	-0.03***	-0.03***	-0.04***	-0.00**	-0.00***	0.00	-0.00**	0.00	0.00

	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Living arrangements (omit. Both parents)												
Parent and other	0.03***	0.04***	0.04***	0.05***	0.03***	0.05***	0.00	0.00	0.00	0.00	0.00	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Single mother	0.02***	0.03***	0.03***	0.04***	0.03***	0.04***	0.00	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Single father	0.03***	0.04***	0.03***	0.04***	0.02**	0.04***	-0.00*	-0.01***	-0.01**	-0.01***	-0.01*	-0.01***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Non-parent relative caregiver	0.00	0.01	0.00	0.02*	0.00	0.02	-0.01**	-0.01***	-0.01***	-0.01***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Non relative caregiver	0.00	-0.01	0.00	-0.01	-0.01	-0.01	-0.02**	-0.02**	-0.02**	-0.02***	-0.03***	-0.03***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Group home or residential facility	0.05***	0.04	0.06**	0.05	0.08***	0.07***	-0.01	-0.01*	-0.01	-0.02*	-0.02**	-0.02**
	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Other or missing living arrangements	0.02	-0.01	0.02	-0.02	0.00	-0.02	0.00	0.00	0.00	0.00	-0.01*	0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
Report characteristics												
Report source (omit. SS personnel)												
Medical and mental health personnel	-0.01*	-0.01**	-0.01*	-0.01**	0.00	-0.01	-0.00***	-0.00**	-0.01***	-0.01**	-0.00**	0.00
	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Law enforcement	-0.02***	-0.03***	-0.03***	-0.03***	-0.02***	-0.03***	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Education, CC, sub. care	0.00	0.00	0.01*	0.01	0.01***	0.01*	-0.00***	-0.00**	-0.00*	-0.00*	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Parents or relative	0.01**	0.01	0.01***	0.01**	0.02***	0.02***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Alleged victim	0.03***	0.03***	0.04***	0.03***	0.03***	0.03***	-0.00*	-0.01***	-0.00**	-0.01***	-0.00*	-0.01***
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Friends or neighbors	0.00	0.00	0.01	0.01	0.01	0.01	-0.00***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Other, unknown, or missing	0.01	0.00	0.01*	0.01	0.01**	0.00	-0.00***	-0.00***	-0.01***	-0.00**	-0.01***	-0.00*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

Month of report (omit. January)												
February	-0.00***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
March	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.00**	-0.00**	-0.00*	-0.00*	-0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
April	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.00*	-0.00*	-0.00**	-0.00**	-0.00**	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
May	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.00**	-0.00**	-0.00**	-0.00**	-0.00**	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
June	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.00***	-0.00***	-0.00**	-0.00**	-0.00**	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
July	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.00***	-0.00***	-0.00***	-0.00***	-0.01***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
August	-0.01**	-0.01**	-0.00**	-0.00**	-0.01*	-0.01*	-0.00***	-0.00***	-0.00**	-0.00**	-0.00**	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
September	0.00	0.00	0.00	0.00	0.00	0.00	-0.00***	-0.00***	0.00	-0.00*	0.00	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
October	0.00	0.00	0.00	0.00	0.00	0.00	0.00	-0.00**	-0.00*	-0.00**	-0.00*	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
November	0.01***	0.01***	0.00	0.00	0.00	0.00	0.00	0.00	0.00	-0.00*	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
December	0.00**	0.00**	0.00	-0.01*	0.00	0.00	0.00	0.00	-0.00**	-0.00***	-0.00*	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Local demographic and economic factors	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Observations	10,354,219	10,354,219	10,354,219	10,354,219	8,874,866	8,874,866	10,354,219	10,354,219	10,354,219	10,354,219	8,874,866	8,874,866
Adjusted R-squared	0.048	0.041	0.062	0.054	0.094	0.082	0.018	0.016	0.023	0.019	0.031	0.025

Notes. Report-child observations from periods without DR are used. Periods without DR are periods in states that never adopt DR and pre-DR periods for states that eventually adopt DR between 2004 and 2019. Year and state fixed effects are binary indicators. The

ordinary least squares regressions use cluster robust standard errors that are clustered at the state level. Standard errors are shown in parentheses. Significance levels: * 10 percent level, ** 5 percent level, *** 1 percent level.

Table B5. Alternative response utilization rates by state-year

	Year of the report														
State	*04*	*05*	*06*	*07*	*08*	*09*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*
Arizona	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Arkansas	0	0	0	0	0	0	0	0	0	0.08	0.11	0.12	0.13	0.16	0.18
Colorado	0	0	0	0	0	0	0.01	0.06	0.11	0.17	0.19	0.19	0.17	0.2	0.19
Connecticut	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Delaware	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Washington DC	0	0	0	0	0	0	0	0.01	0.04	0.14	0.38	0.4	0.44	0.44	0.44
Florida									0	0	0	0	0	0	0
Idaho	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Indiana								0	0	0	0	0	0	0	0
Iowa	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Kansas					0	0	0	0	0	0	0	0	0	0	0
Maine	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Michigan			0	0	0	0	0	0	0	0	0	0	0	0	0
Mississippi	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Montana	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
New York	0	0	0	0	0	0.02	0.04	0.06	0.08	0.08	0.07	0.07	0.07	0.07	0.05
Ohio	0	0	0	0	0	0	0.05	0.16	0.19	0.26	0.35	0.41	0.42	0.43	0.41
South Dakota	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Tennessee								0.43	0.29	0.21	0.31	0.51	0.58	0.59	0.58
Texas	0	0	0	0	0	0	0	0	0	0	0	0.03	0.07	0.09	0.12
Vermont	0	0	0	0	0	0.19	0.29	0.28	0.29	0.32	0.36	0.37	0.35	0.32	0.28
West Virginia	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

Appendix C. Differential Response Implementation Differs Across Jurisdiction

While there are common features of DR systems across jurisdictions, DR is not a uniformly defined reform, and its implementation varies. Four main differences in implementation stand out. I describe these differences using information from the SCAN Policies Database 2019 (Weigensberg et al., 2022).

First, in some states, the determination for the appropriate track (IR or AR) for maltreatment referrals is made *at the time of screening to screen out to AR*, whereas in other states, a determination is made *after a report is screened in*. Among the 30 states with DR in 2019, 17 states make the track determination only after a report is screened in, six states make the determination only *at the time of screening to screen out to AR*, six states allow for both (determinations at screening *and* after a report is screened in), and one state uses another process.

Second, some jurisdictions restrict the types of child maltreatment that are eligible for AR. In 2019, among the 30 states with DR, 27 states restricted eligibility for AR based on maltreatment type. In 24 DR states, sexual abuse cases are ineligible for AR. In 18 DR states, physical abuse cases are ineligible for AR. In 9 DR states, neglect cases are ineligible. Other report types that may be ineligible include child fatality (25 states), substance-exposed newborns (4 states), abandoned infant cases (11 states), or cases based on some other pre-defined ineligibility (13 states).

Third, among the 30 DR states in 2019, 24 states have eligibility for AR that is determined using a formal risk determination process, whereas in 6 states eligibility is not informed by risk determination. However, the tools differ even across states that use risk determination. Some examples of tools include structured decision-making or a standard assessment tool.

Fourth, for families involved with CPS, the efficacy of DR in addressing the underlying reasons for the maltreatment report likely depends on the criterion for who receives referrals to community services. In some jurisdictions, all cases on the AR track receive referrals to services (i.e., universal referrals to services). In other jurisdictions, referrals are selectively made when families express interest (i.e., opt-in referrals to services), when there is a determination of risk (i.e., meet the criterion for referrals to services), or via some other process.

Appendix D. Differential Response and Modelling the New Step in Child Welfare Decision Making

CPS agencies respond to reports of child maltreatment based on some (formal or informal) assessment of risk. To provide a simple characterization of a reported child's risk, I denote 'true' maltreatment risk with the term σ, where $\sigma \geq 0$. Then, I define the 'perceived' risk from the maltreatment report that the CPS agency receives as:

$$\boldsymbol{s = \sigma + \mu}, \tag{1}$$

where μ represents idiosyncratic noise from the initial report. Intuitively, the perceived risk assessment is uncertain. When $\mu \neq 0$, the perceived risk (s) may be greater than or less than the true risk (σ) depending on the direction of μ.

Traditionally, the status quo is that *all* CPS reports are investigated regardless of perceived risk for future maltreatment. This is true in all periods without DR reforms.[12] However, in periods reformed for DR (henceforth referred to as a post-DR period), the jurisdiction has a new binary choice problem. Let D_{IR} denote a binary decision variable indicating whether a reported child is investigated. Specifically, I use a standard binary decision setup and I write D_{IR} as:

$$D_{IR} = \begin{cases} 1 & \text{the child is placed on the IR track, and} \\ 0 & \text{the child is placed on the AR track.} \end{cases}$$

Whereas it is always true that $D_{IR} = 1$ in periods without DR reforms, in post-DR reform periods, the perceived risk index (which is a function of true risk) drives the decision to investigate. I write the selection equation as:

$$\boldsymbol{D_{IR} = 1[s \geq 0]}. \tag{2}$$

In equation (2), agencies the post-DR period will generally choose to place the child on the IR track ($D_{IR} = 1$) if the perceived risk, s, is considered of moderate to high risk. Otherwise, the agency will choose to place the child on the AR track ($D_{IR} = 0$). Equation (2) models (self-) selection into the IR and AR tracks in a way that is inspired by a Roy model (Heckman & Honoré, 1990; Roy, 1951). Endogeneity exists in this context because the severity of a child's report and their presumed risk is expected to be related to both CPS decisions (e.g., track placement) and the possibility of subsequent re-reporting.

[12] Periods without DR reforms include all periods for jurisdictions that never reform for DR, and pre-DR periods for jurisdictions that eventually reform for DR.