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Child Welfare System Contact and Voting

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SAMMANFATTNING

Hur påverkar kontakt med barnavårdsmyndigheten Child Protective Services (CPS) – som årligen utreder miljontals familjer i USA – om vuxna registrerar sig för att rösta eller deltar i val? Statsvetenskaplig forskning har hittills bortsett från hur interaktioner med CPS påverkar politiskt beteende. CPS är särskilt vanligt förekommande i låginkomstområden och i samhällen som domineras av etniska minoriteter. Insatserna uppfattas ofta som påträngande eller bestraffande.

Forskarna har i denna studie använt ett nytt dataset där CPS register kopplas samman med information om röstregistrering och valdeltagande i Allegheny County, Pennsylvania (USA).

Författarna fann att vuxna i hushåll som besökts av CPS röstar i lägre grad än andra. Till exempel röstade bara en tredjedel av dessa vuxna i valet 2016, jämfört med två tredjedelar av befolkningen i stort. Detta innebär att denna utsatta grupps politiska intressen är kraftigt underrepresenterade.

Trots att dessa familjer i högre grad står utanför politiken, verkar det inte bero på själva kontakten med CPS. Med hjälp av en avancerad metod kunde forskarna undersöka om kontakten med CPS minskar det politiska deltagandet, utan att finna några tecken på en sådan effekt. Varken beslut om att öppna ett ärende eller att omhänderta ett barn påverkade röstregistrering eller valdeltagande.

Det här resultatet går på tvärs mot politisk teori om att bestraffande kontakter med myndigheter avskräcker människor från att delta politiskt – i alla fall i kontexter där denna institution är CPS.

Till skillnad från andra statliga instanser i USA fungerar CPS både som en kontrollerande aktör och som förmedlare av en tjänst, vilket kan göra de politiska reaktionerna mindre tydliga.

Att förstå hur olika typer av statlig intervention påverkar människors politiska beteende är avgörande, särskilt när dessa insatser berör miljontals familjer. Studien pekar därför på behovet av en mer grundlig undersökning av hur system som CPS påverkar inte bara vem som röstar, utan även hur människor uppfattar och interagerar med det offentliga i stort.

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Child Welfare System Contact and Voting

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Abstract

Scholars have increasingly recognized how marginalized communities experience government through criminal legal system encounters, potentially shaping political engagement. A parallel state institution, Child Protective Services (CPS), intervenes with millions of U.S. families annually. The CPS case provides a chance to assess theoretical expectations about how punitive state systems affect political life. Analyzing administrative and voting data from a large Pennsylvania system, we first present descriptive estimates of adult voting in families with CPS contact, highlighting how low participation in this large fraction of the population matters for the electorate and overall participation. Then, using the nearly-random assignment of CPS caseworkers, we estimate the causal effect of having a case opened or a child removed on registration and voting. We do not find evidence of substantial participatory effects from these types of CPS contact. We consider interpretations of these null findings and suggest additional research on this vast, understudied government system.

Keywords: Voting, Families, Child Protective Services, Child Welfare, Political Participation

Supplementary material for this article is available in the appendix in the online edition. This study was reviewed by MIT's Committee on the Use of Humans as Experimental Subjects (protocol 1803282514) and the Swedish Ethical Review Authority (Dnr 2022-00759-02). Replication code is available in the JOP Data Archive on Dataverse (https://dataverse.harvard.edu/dataverse/jop).

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1 Introduction

Contact with the child protection system (CPS) is widespread in the United States. The system's reach and concentration mirrors the criminal legal system: About 37% of U.S. children will experience a CPS investigation by age 18 (Kim et al., 2017), comparable to the cumulative risk of arrest by age 24 (Tolliver et al., 2023). Both risks are dramatically unequal by race: Black children face a 53% likelihood of CPS investigation, compared with a 28% risk for white children (Kim et al., 2017), and young Black men face a 49% likelihood of arrest by age 23, compared with a 38% risk for young white men (Brame et al., 2014). Both types of state contact can also prompt ongoing, invasive state involvement in one's life: Parents under investigation by CPS can expect multiple home visits, long interviews about personal topics, and requests to communicate with children's teachers and pediatricians; for some, CPS investigations lead to years-long court proceedings, ongoing state monitoring, and even child removal. CPS can also be a conduit to social services, bridging the welfare and penal states; nevertheless, parents—especially those drawn more deeply into the system—experience CPS intervention as punitive and controlling (Fong, 2023; Lee, 2016).

Soss and Weaver (2017) point out that political science has traditionally neglected the political lives of race-class subjugated communities. Following their field-defining piece, a recent wave of research has examined the political impact of criminal legal system contact (Weaver, Prowse and Piston, 2019; Anoll and Israel-Trummel, 2019; Laniyonu, 2019), with some scholars identifying politically demobilizing effects of experiences such as police stops and jail (White, 2019; McDonough, Enamorado and Mendelberg, 2022; Ben-Menachem and Morris, 2023). However, although Soss and Weaver observe that CPS is also part of what they call "the state's more controlling 'second face,'" to our knowledge scholars have not yet analyzed the analogous political effects of CPS involvement. As Soss and Weaver point out, a focus on policing and incarceration, which disproportionately affect men, may overlook issues that specifically affect poor women of color, such as CPS intervention.

Measuring the possible effects of CPS contact on political behavior is challenging because the surveys and administrative data that are typically used to measure CPS contact (the National

Longitudinal Survey of Youth and county-level CPS administrative data) do not typically contain information about the political behavior of parents subject to CPS intervention. In this study, we use a unique data set from Allegheny County, Pennsylvania (Pittsburgh and suburbs) that links data across a variety of human services agencies to other publicly available individual-level information, including the Pennsylvania voter file. These data allow us to analyze the voting and turnout patterns of voting-eligible parents or guardians who were subject to CPS investigations in the presidential and midterm elections from 2014 to 2020. They come from a large and diverse county that is in many ways representative of the country and the population of families encountering CPS nationally, and from a bureaucracy that resembles national averages in terms of referral types and outcomes (US Department of Health & Human Services, 2021).

We begin with a descriptive look at voting participation by people who experience CPS contact, which we view as one of the main contributions of this paper. We find substantially lower voter registration and turnout rates for adults subject to CPS referrals. In the 2016 general election, the turnout rate for CPS-referred adults, even at the lowest level of system contact, was approximately half the county average. Those with CPS referrals are also less connected to formal political parties, as measured by party registration. These patterns are similar among those who experience more intensive system contact, including those with CPS cases open beyond the investigation and those with children removed from home. Given the scale of CPS contact and its concentration among low-income communities and families of color, these patterns translate into notably lower rates of voter turnout in the county (than would be seen if CPS-involved parents voted at the same rates as other residents) and have potential implications for the ideological composition of the electorate.

Next, we turn to causal estimates of CPS contact on political participation. A key challenge in estimating the effect of CPS contact on political behavior is that families with more CPS contact are adversely selected. Thus, comparing the political participation of families with and without CPS contact would produce biased estimates. We address this challenge using an identification strategy developed by Grimon (2023), which addresses the many other differences between families that do and do not encounter CPS. This design, based on nearly-random caseworker assignment, compares

how voting changes after cases are assigned to workers more or less likely to recommend a CPS intervention, relative to prior participation. Despite some theoretical expectations that encounters with the "second face" of the state (Soss and Weaver, 2017) are politically demobilizing, we do not find substantial effects of CPS case opening or child removal on either voter registration or voter turnout, either on average or within any theoretically relevant subgroups (gender, race, or age). Because of the large sample size, well-identified causal design, and a variety of robustness checks, we are able to rule out the possibility that this is a noisily estimated null effect; on the contrary, it is a precisely estimated zero effect. Further, we can largely rule out the possibility that the zero average effect is a function of demobilizing effects among some subgroups and mobilizing effects among other subgroups. This finding holds implications for political scientists' understanding of the types of state contact that mobilize or demobilize voters, as well as the theorized mechanisms underlying demobilization effects in other contexts.

This paper proceeds as follows. First, we provide a brief overview of CPS, specifying how various features of the system may align it (or not) with prior theories on the effect of government interactions on political behavior. Second, we describe our unique data from Allegheny County. Third, we analyze this data and present our core findings. Finally, we discuss the political implications of CPS contact as a fruitful area for future research and highlight CPS as a case that can yield broader understandings of how state contact shapes political participation. We suggest that political scientists examining this system attend to whether and how CPS contact differs from criminal legal contact, and how it may constitute a "bundled treatment" that mixes both mobilizing and demobilizing experiences.

2 CPS and Political Life

Tasked with providing rehabilitative services as well as empowered to separate families, the child welfare system spans the welfare and penal states, incorporating both supportive and coercive capacities of government. Scholars have documented a disciplinary, punitive U.S. welfare state

alongside a penal state that serves as a major provider of social and healthcare services (Soss, Fording and Schram, 2011; Sufrin, 2017). As such, the dual institutional missions of child welfare exemplify the contemporary fusion of penal and welfare techniques in governing marginalized groups (Wacquant, 2009).

Moreover, like the welfare and criminal legal systems, the child welfare system primarily intervenes with marginalized groups. More than two decades ago, legal scholar Dorothy Roberts (2002) highlighted the scale and the political nature of CPS-initiated family disruptions. Contact with the child welfare system represents a massive and stratified state intervention into race-class subjugated communities, one that reflects and reinforces societal power dynamics. Federal, state, and local spending on the child welfare system totals $33 billion annually (Service, 2022). Point-in-time foster care rates for U.S. children, at 563 per 100,000, exceed imprisonment rates for U.S. adults, at 539 per 100,000 (Carson, 2020; US Department of Health & Human Services, 2020). One in three children experiences a CPS investigation during childhood (Kim et al., 2017) and one in nineteen is placed in foster care (Yi, Edwards and Wildeman, 2020); in race-class subjugated communities, these rates are even higher (Fong, 2019; Putnam-Hornstein, Prindle and Rebbe, 2022). Among Black children, for instance, fully half experience a CPS investigation (Kim et al., 2017) and one in eleven is placed in foster care (Yi, Edwards and Wildeman, 2020). As most CPS cases involve family adversity, typically borne of conditions of racialized poverty, the child welfare system serves as a central institution of poverty governance and racialized state supervision (Fong, 2023; Woodward, 2021). The system is highly gendered as well, primarily intervening with women (US Department of Health & Human Services, 2021). State intervention with marginalized men through the criminal legal system thus runs parallel to state intervention with marginalized women through the child welfare system.

Although CPS interventions are widespread and consequential points of contact between governmental authorities and those they govern, political scientists have paid scant attention to the experiences of people interacting with this system. As Soss and Weaver (2017) argue, political scientists must attend to the governmental "supervision, interference, and predation" central to po-

litical life in race-class subjugated communities; in this paper, we extend their point to consider the implications of contact with the child welfare system.

Social science scholarship on other state functions, such as welfare and the criminal legal system, can help us imagine potential political consequences of CPS intervention. Survey analyses find that associations between public assistance and voting vary based on characteristics such as the extent to which programs are designed around paternalism (Bruch, Ferree and Soss, 2010) and the individual's attention to political issues (Sugie and Conner, 2022). With respect to the criminal legal system, some studies analyzing survey data suggest demobilization following system contact (Weaver and Lerman, 2010), while others suggest political mobilization (Walker, 2020), depending on the specific experiences and samples considered. Studies with a causal identification strategy have found that criminal legal system contacts ranging from police stops to jail have net negative effects on voting, especially concentrated among Black residents (White, 2019; McDonough, Enamorado and Mendelberg, 2022; Ben-Menachem and Morris, 2023).

However, these well-identified studies struggle to parse the different mechanisms through which state contact could affect voting. The case of CPS aligns with—and diverges from—other state systems in ways that may be illuminating. Specifically, CPS brings together components reminiscent of the criminal legal system—intense surveillance and the possibility of coercive intervention—with features of the welfare system such as access to social service programs (though, as noted by Bruch, Ferree and Soss (2010) and others, welfare programs can also include surveillance and coercion). What does this combination of program features mean for the political behavior of people who interact with CPS?

Drawing on the concept of policy feedback (Campbell, 2002, 2012), scholars have proposed two main ways by which policies may affect individual political participation (Bruch, Ferree and Soss, 2010; White, 2022): "interpretive" and "resource" mechanisms (Pierson, 1993). We take these two types of mechanisms in turn and consider how they might operate in the CPS context. First, scholars suggest an interpretive, often also called a learning or socialization, channel, in which people's everyday interactions with state systems give them firsthand lessons about the

government—how it operates and how state actors treat people like them—that then affect their inclination to participate in the political process. This conception helps us understand how programs designed around paternalism, rather than those encouraging client participation and voice, may depress political engagement (Bruch, Ferree and Soss, 2010). When people encounter state programs (including welfare programs) oriented around threatening, supervising, and disciplining them, they withdraw from formal political participation (Soss, 1999; Watson, 2015). Those entangled in the criminal legal system, for instance, experience a state that treats them with suspicion, as second-class citizens; they learn "that government does not understand or respond to their needs," leaving them alienated from political life (Lerman and Weaver, 2014, 233). In highly policed communities, involuntary police encounters give residents detailed knowledge about government practices; these everyday experiences of injustice prompt residents to "distance themselves from the antidemocratic face of the state" (Weaver, Prowse and Piston, 2020, 1153).

Sociological and social work scholarship shows how CPS shares many of the potentially alienating features of paternalistic welfare systems and punitive criminal legal systems, particularly for parents whose cases progress beyond investigation. Like those highly exposed to police and prisons, parents with open CPS cases contend with an adversarial state that they see mistreating, disrespecting, and unfairly judging them (Fong, 2023; Merritt, 2021). CPS's social service provision and referrals could potentially give impacted parents positive experiences with government that might increase their inclination to participate. In practice, however, CPS offers assistance in a disciplinary, paternalistic manner, akin to the Temporary Assistance for Needy Families welfare program that scholars have linked to lower voting rates (Bruch, Ferree and Soss, 2010). Parents with open CPS cases are tightly managed and supervised by state caseworkers who meet regularly with them to ensure they meet the requirements of their assigned "service plans." The state doles out rewards and sanctions based on parents' compliance and directs all manner of parental behaviors, from parents' daily activities to their romantic partnerships (Fong, 2023; Lee, 2016; Reich, 2005). Like defendants in criminal court (Clair, 2020), for many CPS-impacted parents, court is perceived as a space of racism and disempowerment rather than a venue for justice (Lee, 2016).

Collectively, these experiences give people in highly impacted communities extensive knowledge about CPS, knowledge that—like knowledge about policing—prompts distrust of and disengagement from state systems and adjacent social service providers (Fong, 2023).

Theory developed in prior work suggests that CPS learning experiences would reduce participation in formal electoral politics (Lerman and Weaver, 2014; Soss, 1999; Weaver, Prowse and Piston, 2020; Watson, 2015). Notably, Walker (2020) challenges the notion that negative experiences with the criminal legal system are uniformly demobilizing, pointing to widespread narratives of injustice around this system to explain why such contacts might increase nonvoting political engagement. Social movements like Black Lives Matter have drawn public awareness to, and driven political action around, racialized injustices in policing. However, despite burgeoning efforts to call attention to injustice and racism in CPS (Roberts, 2022), social movement activity around CPS has not yet become mainstream. In the absence of a widespread injustice narrative, we do not expect that CPS would mobilize political participation through an interpretive channel.

Second, scholars suggest a resource pathway through which system involvement may affect political participation. For instance, public assistance provides economic resources that may alleviate some material hardship and facilitate political engagement. Conversely, a wealth of research traces the negative collateral consequences of incarceration and other forms of criminal legal system involvement, where the resulting job market or housing problems may make it hard for people to vote (Pager, Bonikowski and Western, 2009; White, 2022).

Unlike the criminal legal system, CPS cannot formally incapacitate or disenfranchise clients from political participation, and CPS involvement is not a formal barrier to employment (although some employers do discriminate in hiring after consulting child maltreatment registries, potentially cutting CPS-impacted people off from a key sector of the labor market (Henry and Lens, 2021)). Thus, it is not clear that CPS involvement reduces citizen resources in the same politically-demobilizing ways as the criminal legal system. Indeed, some existing research suggests potentially varied or countervailing resource effects of CPS (Hook et al., 2016). In recent years, U.S. child welfare policy and practice have embraced CPS as a vehicle for service provision (Berrick,

2011). Assignment to CPS caseworkers inclined to initiate more intensive CPS involvement increases mothers' enrollment in mental health and substance use services in the short run, and there is some evidence that improved mental health is associated with greater political engagement (Grimon, 2023; Burden et al., 2017). By supporting parents, greater social service take-up could ultimately facilitate political participation.

Alternatively, participation in CPS-facilitated services may strain parents' resources, as these services are often time-consuming and burdensome. Meeting an often long list of CPS requirements requires substantial effort and attention (Edwards et al., 2023). Further, when parents lose custody of children, they may become ineligible for certain public benefits. Research shows that mothers assigned to removal-inclined CPS caseworkers are less likely to receive cash assistance benefits in the short and longer term, and less likely to receive housing assistance in the long term (although these impacts do not correspond to shifts in earnings or employment) (Grimon, 2023).

Prior theory and research thus suggest that with respect to the political learning mechanism, we would expect more intensive CPS contact to reduce political participation in the form of voter registration and turnout. With respect to the resource mechanism, theoretical expectations are mixed. To our knowledge, no study has yet examined the relationship between CPS involvement and voting, either descriptively or with a strategy to identify potential causal effects. Analyzing CPS involvement brings an understudied state system into scholarship on political life and may offer insight into broader questions about political participation in the wake of government contact.

3 Data

We use data on referrals to CPS between 2009 and 2018 collected from the Allegheny County Data Warehouse.[1] Recording residents' encounters with local government, this database is uniquely detailed and granular in two key ways. First, it has unusually good coverage of the caregivers of children referred to CPS (not limited to perpetrators). In particular, 94% of referrals include

[1]Data request instructions can be found on the Allegheny County Data Analytics webpage.

information on a mother, and 87% on at least one father (Grimon, 2023). Second, we are not aware of any other CPS dataset that includes information on parents' political participation.

The county's CPS agency operates similarly to its counterparts nationwide. Like all other counties and states, Allegheny County operates a hotline that anyone can call to report suspected child abuse or neglect (together referred to as child maltreatment); most reports come from professionals legally mandated to report suspected maltreatment, such as doctors, police officers, and teachers (US Department of Health & Human Services, 2021). CPS then investigates the allegations over a certain time period (60 days in Allegheny County). After investigation, the agency may close the case, sometimes referring the family to voluntary services. Alternatively, CPS may decide to open the case for ongoing oversight to monitor parents' participation in services, either while children remain at home or, if the agency deems children unsafe at home, with children removed from home through the authority of the juvenile court. Upon taking custody of children, CPS arranges for their care with relative caregivers, with foster families, or in group care settings, until reunification with parents, adoption, or another permanent living arrangement. In some cases, parental rights are permanently terminated, among the most extreme actions a government can take.

Our dataset covers all reports of child neglect in 2009-2018 and all reports of child abuse in 2017-2018.[2] Over this period, CPS received over one hundred thousand calls naming over twice as many individuals (some as alleged perpetrators, others simply as adults in the household). Because of our focus on political participation, we restrict this dataset to focus on reports where an adult in the household could have been observed registering and voting in Pennsylvania during the period covered by our voting records, omitting children as well as parents/guardians too young for us to observe these political outcomes. This step, along with several smaller sample restrictions described in Appendix Table A1, leave us with a sample of nearly 2.5 million year-specific individual observations, for over 91,000 unique individuals.

Table 1 presents descriptive statistics about the people included in this sample. Consistent

[2]Child abuse reports are a quarter of calls in any given year, but reports for child abuse prior to 2017 are partly or fully expunged due to legal requirements.

with national data (US Department of Health & Human Services, 2021), about half of referrals in Allegheny County during our study period are investigated, a small minority lead to child removal in the period following the referral, most referrals allege child neglect (rather than child abuse), and Black families are disproportionately referred to CPS. Information on the race of parents and children comes from the reporter (e.g., a teacher), and if that is unavailable, CPS will attempt to link the family to race information that might have been recorded in previous CPS involvement. Recent work from other jurisdictions suggests any racial misclassification leads to *underreporting* of racial disparities in CPS involvement (Baron et al., 2024).

Women comprise approximately half of the overall sample, though they are especially central to CPS cases: 67% of alleged perpetrators in the sample are women. This is the main margin along which alleged perpetrators differ from the rest of the sample, though alleged perpetrators are also ten percentage points less likely to be Black and have marginally higher voting and registration rates than other adults listed on referrals. Given these differences, we analyze subgroups based on gender and on alleged perpetrator status in the appendix (and race later in the paper).

To learn about the political participation of adults in these cases, we rely on snapshots of voter registration and turnout data from Pennsylvania's Secretary of State. To join these records to the dataset of CPS reports, we rely on record linkage done by the Allegheny County Department of Human Services (DHS) data warehouse. After initial pre-processing of voter file data to remove invalid or missing records, voting records are matched to existing observations in the data warehouse using first name, last name, and date of birth. Exact name/date of birth matches are prioritized, followed by those with "soundex" matches on name, and duplicate matches where a voter matches to multiple client records have ties broken by the number of systems the client record appears in (the record with more DHS program observations takes precedence). More details on this matching approach, including a series of permutation tests and other means of validating the merge to estimate false positive and negative match rates, appear in Chizeck et al. (2024, SI pg. A11). Our analyses of voter registration include the 2012, 2016, 2018, and 2020 elections, while our main analyses of turnout include the 2014, 2016, 2018, and 2020 elections. We describe the specific

snapshots and elections used in more detail in SI section A.1.

Table 1: Descriptive Statistics on Referred Families

Panel A: Child Welfare Referrals	
Investigated (%)	49
Opened a Child Welfare Case (%)	20
Child Removed within 3 months (%)	6
Allegation of Physical or Sexual Abuse (%)	17
Number of Referrals per Parent	2.5
Number of Referrals	96589
Panel B: Individual Characteristics	
Age	36.5
Black American (%)	40
Race Missing (%)	12
Women (%)	48
Ever Registered (%)	76
Ever Voted (%)	45
Observations	228701

Notes: Unless noted as a percentage or number of observations, this table reports means. Panel A includes all referrals made to child welfare from May 2009 to December 2018 which are in our sample. Panel B includes all individuals referred to CPS in our sample. Individuals referred multiple times will be included once for each referral.

This process leaves us with a dataset that includes nearly the full universe of voting-aged individuals who experienced CPS reports over multiple years, combined with information on their political participation. As shown in Table 1, 76% of adults in this dataset were registered to vote in Pennsylvania at some point from 2012 to 2020, and slightly less than half were observed voting at some point from 2014 to 2020. Again, this dataset includes all referrals, regardless of whether CPS ultimately investigated the allegations, opened a formal case, or removed a child from the home.

This combination of records allows us to undertake several analyses of political participation among these families that have not previously been possible. We begin with a simple descriptive look at voting among this set of people having a particularly invasive set of interactions with government, and ask whether this group is underrepresented in the electorate relative to their share

of the population. Then, we turn to measuring the causal effects of specific types of CPS contact—case opening and child removal—on adults' political participation.

4 Analysis

4.1 Descriptive Patterns

We begin by describing electoral participation among adults in families with CPS referrals and comparing it to the rest of Allegheny County. This analysis does not seek to measure causal effects of CPS contact, as families called in to CPS may differ from other residents along many dimensions that could matter for political participation. Families with CPS contact are disproportionately drawn from race-class subjugated communities (Fong, 2019), and may have other past life experiences or material circumstances that make them unlikely to vote. We seek here to describe their participation in order to consider how their voices may or may not be heard as part of the broader electorate, in ways that could affect legislative representation and policymaking.

Table 2 presents summary statistics of 2016 registration and voting among adults in the CPS-contact sample described above. We focus on 2016 participation because it falls squarely within the period for which we have CPS records, allowing us to observe participation in a high-salience presidential election that is close in time to the CPS referrals experienced by these families (ensuring that we examine participation among people who largely still live in Allegheny County).

Nearly two-thirds of adults in the CPS-referrals sample were registered to vote as of 2016, and only about one in three (34%) actually voted in the general election that year.[3] Adults with CPS referrals are not completely disconnected from political participation—across the entire sample, nearly half have voted and three-quarters have been registered at some point (Panel B of Table

[3]We see equivalent patterns of underrepresentation even if focusing on individuals with CPS referrals in 2016 only, indicating that the patterns we see are not being driven by CPS-involved individuals being more likely to move out of state (and thus not be visible in the Pennsylvania voter file) than other county residents.

1)—but these are still substantially lower registration and turnout rates than the national, state, and county averages for a given presidential election year. In Allegheny County overall, over two-thirds of voting-eligible residents turned out in 2016.[4] While we note that the calculated registration rate of 96% for all county residents (87% if restricting to active voters) is likely an overestimate due to outdated or duplicate registration records remaining on the file after people move or expire, the turnout rate reflects actual votes cast (Ansolabehere and Hersh, 2010). Furthermore, a two-thirds registration rate among CPS-referred adults is likely much lower than the general rate, even if that true rate is below 96%.

Table 2: 2016 Political Participation of Adults in CPS-Referred Families

	(1) CPS Individuals	(2) All Allegheny
Registered	0.62	0.96
Registered as Republican	0.11	0.27
Registered as Democrat	0.42	0.56
Voted	0.34	0.68
Observations	91752	961595

Notes: The first column reports 2016 registration and voting behavior of people referred to CPS in our time period, with one observation per individual regardless of the number of referrals. The second column provides the comparison for all of Allegheny County.

Adults in families with CPS contact were also less likely to register with a political party: a total of 53% of this group (unconditional on registration) were registered Republicans or Democrats,

[4]We calculate county-level turnout by dividing the total number of 2016 presidential votes cast (reported by the Pennsylvania Secretary of State) by the Census Bureau's estimate of the Citizen Voting-Age Population (CVAP) count of eligible voters in the county (5-year estimates from the American Community Survey): 650114/961595 = .68. Similarly, we calculate county registration rates by dividing the number of registered voters in the county as of the 2016 election (reported by PA SOS) by the county CVAP estimate: 924631/961595 = .96. If we focus on active registered voters rather than inactive ones, the registration-rate calculation is: 837297/961595 = .87.

compared to 83% of county residents. (Individuals not registered as Republican/Democrat can be registered with a non-mainstream party, or most often with no party.) These patterns suggest that families with CPS referrals have less connection to formal electoral politics than their neighbors.

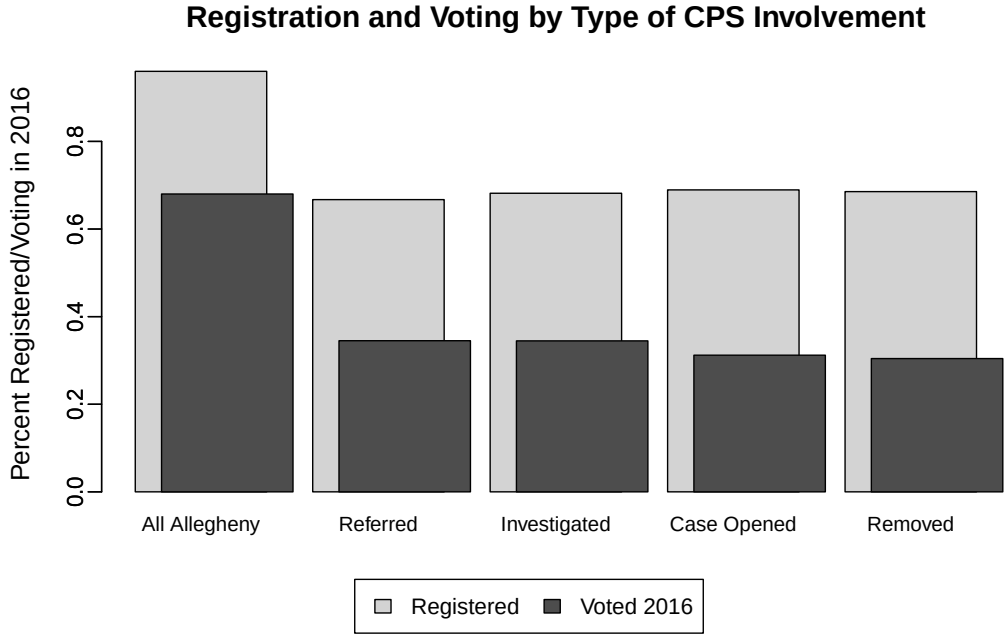


Figure 1: 2016 Registration and Voting by Type of CPS Involvement. Adults in our sample may be (1) referred with no investigation, (2) referred and investigated with no further CPS involvement, (3) investigated with a case opened, or (4) have a case opened with a child removal.

This phenomenon is not limited to people with extensive CPS contact: Figure 1 breaks out the sample into subgroups with various forms of CPS contact and finds similarly low rates of registration and turnout among those with CPS contact across the board, whether considering people with only a call and no investigation, or those with a child removed from home. Light gray bars show the registration rates for each group, which range from 66.7% to 68.9%, while the darker bars show 2016 voter turnout rates ranging from 30.4% to 34.5%. The leftmost bars plot the equivalent values for the county as a whole (including CPS-involved individuals), illustrating the relative underrepresentation of these families within the broader electorate. Some of the differences between groups of CPS-involved families are statistically significant (registration rates for those with only referrals are significantly lower than those with investigations or open cases, and turnout rates are

significantly lower for those with open cases or removals than for those with only investigations or referrals), while others are not. But in all cases, the small differences between CPS-involved groups—all below 5 percentage points—are swamped by the differences between these families and the participation rates of the rest of the county. The relative stability of participation rates across different levels of CPS involvement may be surprising, but it is broadly consistent with the causal estimates we present in the next section; the conclusion of the paper discusses possible reasons for these null results.

We view these descriptive patterns as politically meaningful and one of the key contributions of this paper. Given the scale of adult exposure to CPS referrals in the county (close to one in ten adults in the county over our study period), the vastly lower rates of participation among CPS-referred adults translate into visibly lower rates of county participation than we would expect to see if CPS-involved families participated at the same rates as others. A simple back-of-the-envelope calculation illustrates this point: if adults with CPS referrals had voted in the 2016 election at the same rates as other county residents, we would have seen over 31,000 additional votes cast in the county, or nearly a 5% increase in total votes cast county-wide.[5] This is a substantial counterfactual increase in turnout, comparable to some of the largest "get-out-the-vote" effects observed in the mobilization literature.

Further, these missing voters are demographically different from the county's electorate as a whole. Figure 2 breaks out the sample by various personal characteristics and again displays registration and voting rates (for case-related characteristics, like number of referrals, see Figure A1 in the SI.) .[6] Within the set of CPS-involved families, we see limited variation in participation. Both white and Black adults in CPS-involved families vote at relatively low rates, and though older

[5]The calculation here is 91752 affected individuals, with turnout increasing from .34 to .68, so 91752*.34 = 31,195.

[6]Figures 1, 2, and A1 are all produced with referral-level data (rather than individual-level as in Table 1). This means that a person could conceivably move between some categories across referrals (if, for example, they were under age 26 on one referral and over 26 on another).

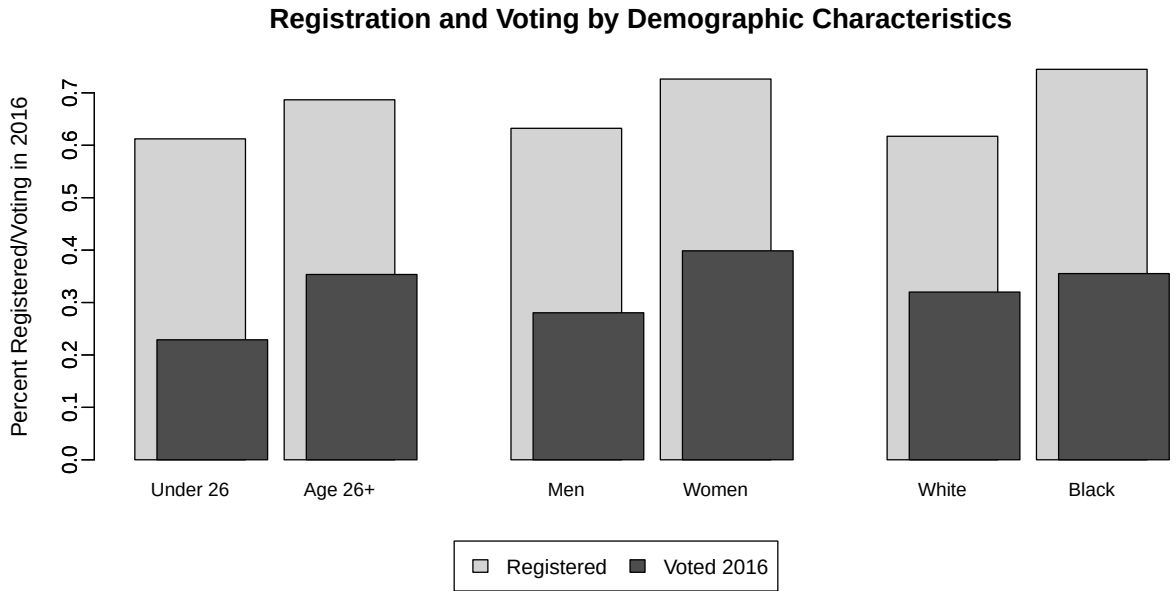


Figure 2: 2016 Participation by Demographic Characteristics (Adults in CPS-involved Families)

people in the sample vote more than younger (under age 26) ones, for no subgroup did 2016 voter participation exceed 40%. Given the overrepresentation of Black and young residents in the CPS-contacted group (as shown in Table 1), the low overall participation rates of CPS-involved families translates into an electorate that is older and whiter than it would be if these families voted at rates comparable to the rest of the county. For example, if all CPS-involved families voted at the same rate as we see in the county as a whole (68% in 2016), the 2016 general election would have seen over 10,000 additional Black voters go to the polls along with nearly 20,000 voters from other groups.[7] These additional voters would have shifted the county's electorate from less than 13% Black to about 14% Black. That such a shift would be visible in overall voting statistics for the county underscores both how broad the reach of the CPS system is and how large the participatory gaps facing this group are. The demographics of these missing voters indicate that they likely hold different policy preferences than the average voter in the county, such that their absence from

[7]For this calculation, we use the most conservative possible coding of race, counting only people who are listed as Black in the county records and assuming everyone with missing values for race are not Black. We describe this calculation in more detail in SI Section A.5.

the electorate could matter for politicians' incentives. Separate from demographic differences, the low political participation rates of CPS-involved individuals means that voters and non-voters have meaningful differences in the ways they have experienced the state, skewing the electorate towards those with limited knowledge of CPS and its interventions.

4.2 Causal Effects of CPS Contact

In the prior section, we showed that families with CPS involvement have different voting rates than those without CPS contact. We do not interpret these differences as causal effects of CPS contact because this association is likely confounded, with CPS-involved families differing in important ways even from demographically similar families without CPS involvement. Since CPS contact is not amenable to randomized experimentation, we instead look for natural variation that can let us learn about the causal effects of CPS on political participation. We do so using a design that combines random-judge-assignment study methods with event-study approaches. This design relies on variation in how otherwise-similar cases are handled by different caseworkers once they are "screened in" for further CPS investigation. Thus, we are able to compare otherwise-similar families who face higher or lower chances of case opening or case removal, simply based on the caseworker who happened to be assigned to their case.

As described previously, CPS referrals in Allegheny County go through a central screening office, where staff determine whether a call warrants further investigation. If further investigation is deemed necessary, a caseworker called an "investigator" is assigned to the referral.[8] This investigator visits the family and decides whether a CPS case should be opened, and may also recommend that a child be removed from the household.[9] In 20% of Allegheny County referrals,

[8]The assignment is typically done based on a quasi-random rotation system within the appropriate regional CPS office. However, supervisors commonly make exceptions to this rule such as taking into account caseloads, travel distances or language.

[9]Caseworkers' decisions to remove children are reviewed by a judge (either before or after removal, depending on circumstances), but judges show great deference to investigator recom-

CPS will decide to open a case (Panel A of Table 1). Cases are opened to monitor the situation, to provide services to household members, and/or—in the most severe circumstances—to remove a child. About 6% of households will have a child removed within three months of the call. Key to our design is that individual investigators differ in how often they recommend opening a case or removing a child, even when faced with similar household situations.

This variation in case handling is important because some other common approaches to causal inference are not appropriate to measuring the effects of CPS contact. As noted above, we cannot simply compare families with and without CPS referrals, since these families differ on many politically-important background characteristics. Similarly, we cannot compare families with CPS contact to their own prior history of political involvement (a "before-after" or "event study" approach), because CPS referrals often occur at moments of extreme crisis for families. If adults become less likely to vote after a CPS referral, that shift could be due to the CPS contact itself, or it could be due to the family events (domestic violence, substance use, job loss, or another crisis) that prompted the CPS referral. Thus, we cannot rely simply on cross-sectional or over-time comparisons to learn about the effects of CPS contact. Instead, we look at how the trajectories of families who face different CPS treatment, due to their caseworker assignment, diverge over time. This variation in case handling by different caseworkers lets us separate out the effect of CPS contact itself from the events that precipitated the contact.

4.2.1 Combining event-study and caseworker-assignment designs

Our approach, following Grimon (2023), combines the intuition of an event-study design and that of "tendency approaches" such as random-judge-assignment studies (Mueller-Smith, 2015; White, 2019). Different investigators' tendencies to open cases or pursue removal means that otherwise-similar families can face quite different "treatments" by chance depending on their caseworker assignment, allowing us to estimate the causal effects of these experiences. But because caseworker assignment is not perfectly random (due to considerations like caseloads, language, or travel time),

mendations so investigator tendency is quite predictive of outcomes (see SI Table A3).

we cannot use a typical instrumental-variables setup that expects a randomly-assigned instrument. Thus, we incorporate elements of event-study designs to relax this requirement, relying instead on over-time data with a parallel-trends assumption. Essentially, we assume that adults whose cases are assigned to different investigators (those more or less prone to open a case or recommend removal) would have moved in parallel had they not received different interventions recommended by their investigators. People may have different baseline levels of political participation, but the design requires that had they been assigned to the same investigator, their political participation would have responded equally over time to factors such as electoral salience or competitiveness.

We investigate this parallel-trends assumption in Appendix Table A2. Essentially, we run analyses analogous to the main specification, but we look for "effects" of being assigned to a case investigator with high case-opening or removal rates in the years *prior* to a person's referral to CPS. Finding such logically-impossible effects of CPS contact before that contact ever happened would indicate violations of the parallel trends assumption, since it would imply that people with different caseworker assignments were moving on different paths even before they came into contact with CPS. We see no such concerning patterns, so we proceed with the specification as described.

Relying on this assumption of parallel trends in political participation, for outcome Y observed in year t of adult i listed on an investigated referral r that is referred in year y_r, and assigned to investigator j, we estimate:

$$Y_{i,r,j,t} = c + \sum_{d=-k}^{m} \theta_d \mathbb{1}_{(t=y_r+d)} + \sum_{d=0}^{m} \phi_d \mathbb{1}_{(t=y_r+d)} \cdot TO^*_{r,j} + \mu_{i,r} + \lambda_y + \epsilon_{i,r,j,t} \tag{1}$$

where our parameters of interest are the ϕ_d's which we plot in graphical format. ϕ_d estimates the effect of being assigned to an investigator with a tendency to open a case $TO^*_{r,j}$. For example, being assigned to an investigator with a tendency to open a case $TO^*_{r,j}$ of 0.1–someone 10 percentage points more likely to open a case than the average caseworker–increases Y d years after the referral by $0.1\phi_d$. Conversely, being assigned to an investigator 10 percentage points less likely to open a case than average decreases Y by $-0.1\phi_d$. Across investigators, there is substantial variation in

the tendency to open a case and to remove a child, as shown in Appendix Figure A3.[10] The θ_d's are event study dummy variables, to account for the fact that families being investigated may be on different trends prior to the referral relative to families who were simply reported. We include year fixed effects (λ_y) to control for time trends and election-year specific shocks. In Appendix Tables A3 and A4, we also provide estimates of a simplified specification which pools all years before and after (ϕ_{after}). We do not include the main effect for the tendency variable because we include individual-referral fixed effects $\mu_{i,r}$ which are referral-specific since parents can be listed on a CPS referral more than once in 2009-2018. The individual-referral fixed effects control for the fact that the baseline turnout and registration rates may be different for individuals over time across multiple referrals. Following recommendations from Chyn, Frandsen and Leslie (2024), we cluster at the individual level.

We present reduced form estimates throughout as there is no methodological literature on how to estimate confidence intervals for Two-Stage Least Squares (2SLS) in our setting. Thus, the estimates shown represent the effect of having one's case assigned to an investigator with higher-than-average tendency to open a case or recommend removal. The first stage estimates are presented in Column 1 of SI Table A3 and can be used to get a sense of the IV point estimates in combination with the reduced form estimates presented in Columns 2 and 3 of Appendix Table A3. Table A3 shows that investigator assignment is important for case outcomes: for example, moving from a hypothetical investigator who never recommends removal to one that always does (though these extreme values are not seen in the data) corresponds to a predicted 37% difference in actual removals from the home. Despite an active research front on the methodologies used in this

[10]The tendency to open and to remove are constructed using the standard "leave-out" procedure: for each person in the dataset in turn, we calculate the share of their investigator's referrals that lead to a case opening or child removal, leaving out this person's own case from the calculation. The star indicates that they are then residualized on regional office-year fixed effects to only use variation in potential investigators to which a family could have been assigned given the family's location and year when referred.

paper, our appendix highlights that we are well above the most stringent recommended F-statistic thresholds (Lee et al., 2022), suggesting that we can be reasonably confident in these estimates.

We use this approach to study two types of contact with CPS: case opening and child removal from the home. We cannot learn about earlier stages of CPS contact (referral or any investigation) via this design, because these steps are not based on assignment to caseworkers: caseworkers are assigned only after a family has been referred and "screened in" for further investigation, as occurs in about half of the referrals we observe in our data. So when we estimate the effect of being assigned to a caseworker with a high case-opening rate, we are comparing otherwise-similar families who have been referred to CPS and screened in for further investigation, but for whom different caseworkers would disagree about the need for opening a case.

The estimates we present are local average treatment effect (LATE) estimates for families where different caseworkers would disagree about how to handle their cases. In the language of instrumental variables estimation, our analysis relies on "complier" families who might plausibly have a case opened (or a child removed), or not, based on caseworker assignment, not on families whose circumstances would always or never yield an open case or a child removal. We note, however, that especially with respect to case opening, the complier population in this analysis is quite large and appears broadly similar to the full sample on background characteristics such as race, age, and gender. As child removal is a less common outcome and one where compliers make up a smaller share of the sample, we should be more cautious about presuming that the LATE of child removal among this group can be extrapolated more broadly. Section A.3 of the Appendix describes our analysis of compliers in more detail.

4.2.2 Estimated Causal Effects of Case Opening and Removal

Figure 3 presents over-time estimates of the effect of CPS intervention on adults' voter registration and turnout. For ease of interpretability, we scale the point estimates from equation (1), assuming linearity, so that they represent the effect on adult political participation of being assigned to an investigator at the 75^{th} percentile of the distribution of the tendency to intervene relative to an

investigator at the 25^{th} percentile of this distribution (unscaled estimates are provided in SI Figure A4). The dashed vertical line on each panel indicates the timing of the family's referral to CPS, with years to the left showing the pre-referral period and those to the right the post-period.[11] Thus, the points to the left of the dashed line allow for a check of the parallel trends assumption: we should not see an "effect" of CPS contact before the referral even takes place, and if we did we would worry about the validity of the design. These pre-trend coefficients do not differ from zero in ways that would prompt concern, so we proceed with interpretation of the main estimates.

The top row of plots uses investigators' tendency to open a CPS case to learn about how case opening shapes participation in the years that follow, and the bottom row of plots focuses on child removals. In both cases, we see null effects of CPS contact on political participation: the exact point estimates vary year-to-year, and there is some estimation noise, but we do not see any systematic evidence of mobilization or demobilization in the wake of these types of CPS contact. Point estimates indicate that being assigned to an investigator in the top quartile vs bottom quartile of the tendency to open a case (approximately fifteen percentage points more likely to open cases) for their assigned families impacts registration from -0.5% to +0.5% depending on the year (Figure 3a). Unscaled estimates pooling all years after the referral together are presented in Columns 2 and 3 of Appendix Table A3 and show similar patterns. For example, the estimated turnout effects for investigators' case-opening and child-removal tendencies are respectively -1.31% and +1.15%, both indistinguishable from zero and confirming with more precision the patterns shown here.

Thus, we find no significant effect of CPS case opening or removal on adults' voter registration or turnout in the years that follow. This is perhaps unsurprising given the descriptive patterns outlined in the previous section: Although voter registration and voting rates are much lower among CPS-referred individuals (compared to county residents overall), those who have experienced an

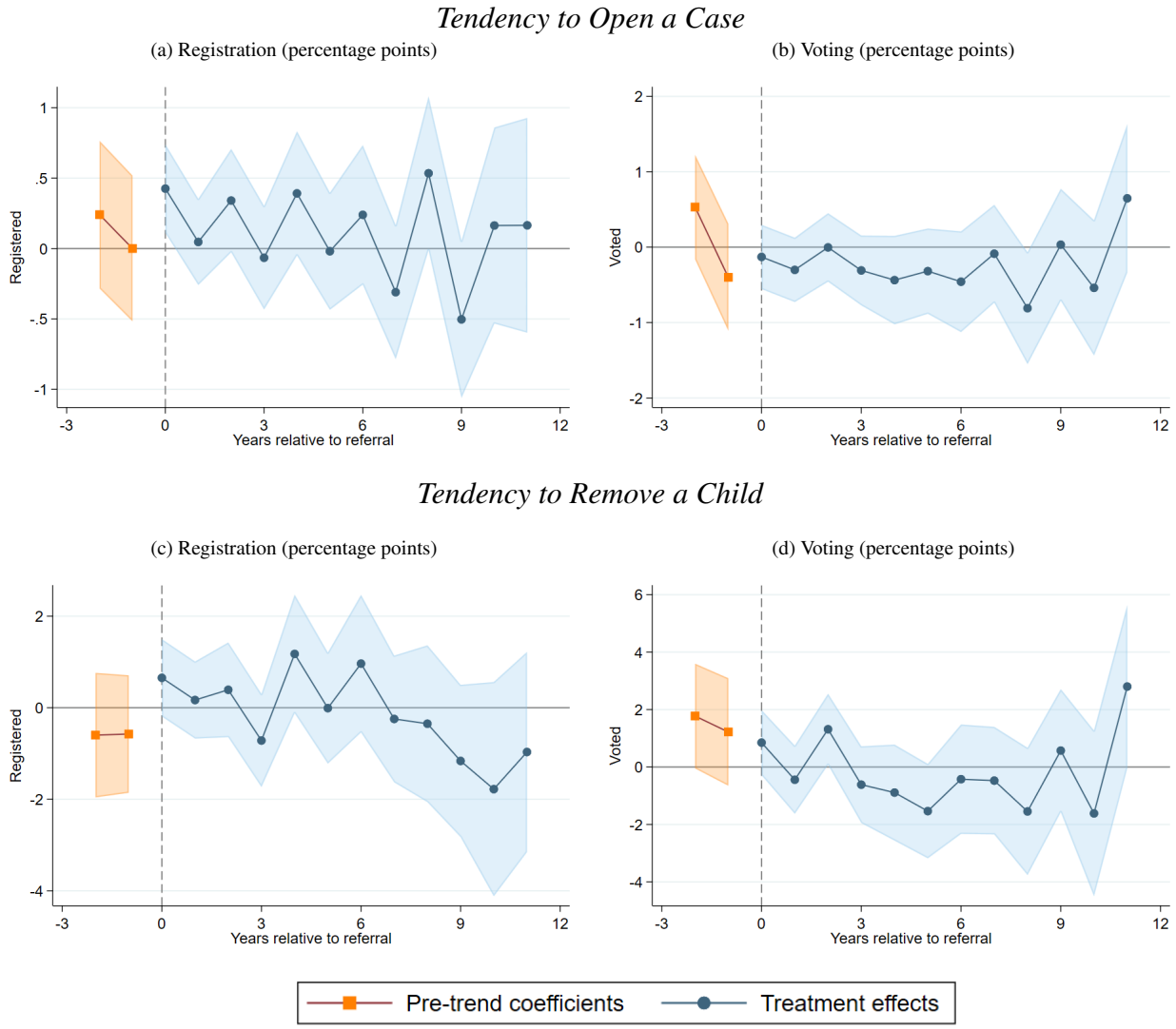
[11] Because referrals occur continuously, we are able to estimate effects for 1, 2, 3, etc. years out from a referral even though elections only occur every two years. This approach means that the people included in the estimation sample shifts year-to-year (even versus odd years), so we include individual fixed effects to capture potential differences in their baseline behavior.

open case or removal of a child register and vote at similar rates as those who have only experienced a CPS investigation, as well as those whose referrals CPS did not investigate. These descriptive findings, depicted in Figure 1, support our estimate of a null effect; if CPS intervention impacted political participation, we would expect to see gradually lower participation among individuals with more intensive CPS intervention.

The causal estimates rely on a design that makes specific assumptions about what would have happened absent CPS intervention, and that relies on specific margins of intervention (that is, we learn about the effects of CPS intervention from the outcomes of families who are treated differently by different caseworkers, not those that would always or never have a child removed from the household regardless of caseworker assignment). However, we do not think our estimates are driven by violations of these assumptions or by the use of a narrow pool of families: as noted above, "compliers" represent a large fraction of the sample and are broadly similar to the rest of the sample, particularly when examining the process of opening a case. It is also not the case that these estimates merely appear null due to a lack of statistical power to detect substantively-important effects. Our confidence intervals, especially those for the pooled estimates shown in Table A3, exclude effects smaller than those seen in previous papers on the criminal legal system and political participation. Section A4 of the Appendix further discusses statistical power.

Finally, Appendix Table A5 shows that the overall findings of zero effect are not driven by an averaging of positive and negative causal effects across different subgroups of the overall sample. For most theoretically relevant subgroups—including mothers only, those referred for the first time, and those alleged as perpetrators of abuse or neglect—there is a statistically insignificant zero effect on both registration and voting within the subgroup for both case opening and child removal. (These results are necessarily less precisely estimated than the overall results, since the subgroups are smaller than the whole sample.) For fathers, there is a statistically significant ($p < 0.05$) 3 percentage-point positive effect on registration for a case opening, for Black adults, there are statistically significant ($p < 0.05$) 3 percentage-point positive effects on registration and 4 percentage-point negative effects on voting for a case opening, and for young adults (under age

Figure 3: Effects of CPS Intervention on Political Participation
—Scaled to Interquartile Range—



Notes: These figures report the effects of being assigned to an investigator who opens more cases (panels a and b), or to an investigator who removes more children (panels c and d). For ease of interpretation, they report the effects of being assigned to an investigator at the 75^{th} percentile of the distribution relative to an investigator at the 25^{th} percentile of the distribution (interquartile range $IQR = 0.15$). 90% confidence intervals are reported throughout, with clustering at the individual level. The dashed vertical line is the year of the referral (year 0), so orange points show pre-trends tests and blue points the effects of investigators after the referral.

26) there is a 4 percentage-point positive effect on voting for a case opening ($0.1 > p > 0.05$), but we are hesitant to over-interpret these four significant results among 32 hypothesis tests, especially since there are null effects for registration or voting for child removal in every single subgroup. Effects among Black adults are not consistent in sign across the two types of political participation examined. Appendix Figure A2, furthermore, shows that the heterogeneous effects for Black adults may be driven by significant pre-trends in that group. As such, we encourage future work into potential heterogeneity, but do not read our analyses as evidence for any such patterns.

We also consider whether effects could differ with election type or case timing. Might CPS contact matter more in lower-turnout midterm election years? Table A4 in the SI presents effects separately by midterm and presidential years and does not find substantially different (or significant) effects across these two types of elections. Similarly, one might wonder whether CPS contact that falls just before an election matters more: perhaps there are short-term effects that are not visible when considering all cases together. To address this possibility, Figure A6 in the SI estimates registration/turnout effects of caseworker assignment only for cases that fall just before an election (1 month, 2 months, 3 months, or 6 months prior), focusing only on the short-run effects of case opening on the very next election. The point estimates grow larger and more negative (for both registration and voting, though none of the estimates are statistically distinguishable from zero) as we focus on narrower windows before the election. These estimates could suggest short-run demobilizing effects for families who are in the process of navigating a CPS case just as an election takes place, though we urge caution in interpreting them given the statistical-power concerns raised by discarding so much of the sample.

5 Discussion and Conclusion

In this article, we bring detailed data on child welfare system contact into the scholarly literature on political participation. The child welfare system is a major government bureaucracy that intervenes with a considerable share of U.S. families, particularly Black, Native American, and low-income

families. Scholars have previously analyzed how individuals' various contacts with the state—from receiving public benefits to being incarcerated—shape political participation. Like the criminal legal system, the child welfare system holds immense coercive power; after investigating parents, the system can forcibly separate families, sometimes permanently. As such, it is a central system of social control over marginalized groups (Roberts, 2002).

Drawing on linked administrative data in Allegheny County, PA, we first find that parents who have been referred to CPS—and had contact at any level—register to vote and vote at much lower rates than other county residents. This descriptive pattern is important in itself, reflecting the relative absence of these important stakeholders from the voting public. Elected officials oversee the operation of CPS, yet people with direct, personal experience with the system, even at the lowest level, are starkly underrepresented in the electorate. Given observable differences between CPS-impacted and non-impacted groups, we would expect that increasing voting among CPS-impacted residents would shift the average policy preferences of the electorate. For instance, CPS-impacted people are more likely to have low incomes, identify as Black, and, at least in Allegheny County, register with the Democratic Party. Further, their experiences with CPS may themselves carry implications for their political preferences beyond partisanship (Bruch, Ferree and Soss, 2010). Voter outreach efforts might consider additional outreach with CPS-impacted people to move towards a more representative electorate, analogous to the reasons that scholars have experimented with outreach to voting-eligible individuals with felony criminal records (Gerber et al., 2015).

Second, we employ a research design combining random-caseworker-assignment study methods with event-study approaches to examine whether the association between CPS contact and political participation is causal. Using these methods, we find precisely-estimated null effects of having a CPS case opened (or a child removed) on voter registration and turnout. These findings are informative, especially in light of prior theory and research that point to the demobilizing effect of encounters with other disciplinary state systems. We observe this null effect at multiple levels of system contact: child removal as well as the slightly lighter-touch (although still intensive) case opening stage. Additionally, we do not think these null results emerge from "saturation"

by previous CPS experiences, as we observe null effects even within subgroups of first-time CPS contacts and young adults. (Nevertheless, CPS-impacted people may have had other punitive state encounters, including in adolescence and young adulthood, that we are unable to observe.)

Our study uses data from a single county, so it is important to consider how these results generalize to other places. Though outstanding in its collection of data, the county is representative of many places in the U.S. As we note in our discussion of Table 1, descriptive patterns observed in Allegheny County mirror those observed in national CPS data. The "treatment" of interacting with CPS here is not notably different from the same experience in many other jurisdictions. And although the county does not perfectly match overall U.S. demographics, our supplemental analyses indicate that the null findings observed here are not driven by those specific features of the county's population. For example, Allegheny's population is whiter than the country as a whole, and the county had somewhat higher-than-average voter turnout in 2016 (68%, compared to 61% nationally). But we do not think that the racial composition of the county is masking underlying CPS effects that would be seen elsewhere, since we do not observe any clear effects among individual racial groups. Pennsylvania's status as a battleground state in presidential elections may mean that Allegheny County receives more campaign attention in these years, boosting turnout. However, given that we observe null results across election types, presidential-year campaign efforts are likely not masking or offsetting a negative CPS effect. We hope that future studies (including qualitative studies) can cover more places, but we see Allegheny County as a useful site for learning about CPS effects on participation.

Our null results are surprising in light of the two main mechanisms by which involuntary encounters with punitive state institutions are thought to affect participation. Parents with open CPS cases and/or children removed from home experience an adversarial, controlling state (Fong, 2023), and so such events could decrease political participation through an interpretive mechanism. Having a CPS case opened or a child removed from the home can also be a considerable drain on a family's time and financial resources as they attend hearings and comply with court conditions (Edwards et al., 2023; Fong, 2023), and so could also affect participation through a resource mech-

anism. Thus, our results suggest that these prior theories of political participation among those who have involuntary interactions with the state are not fully capturing the reasons why marginalized people may or may not register or turn out to vote. Our causal identification strategy also enables us to address concerns about selection bias inherent in much research in this area, as families with and without CPS involvement likely differ in unobservable ways. Our cross-sectional, correlational analyses show a negative relationship between CPS involvement and voting, yet our null causal effects suggest that this association is likely driven by factors other than CPS intervention.

Of course, it is difficult to substantively interpret and assign a definitive mechanism to a null effect. One possibility is that, because CPS involvement involves both punitive elements and social service elements, its function as a site of political learning mixes messages of state marginalization and state support, with neither clearly dominating the other as a political mobilizer or demobilizer. Another possibility, not inconsistent with the first, is that CPS involvement can both provide resources to families (in the form of various types of social services assistance) and be resource-costly to families (because of changes to benefits eligibility as well as the taxing process of engaging with CPS), and so CPS's function as a type of tax or benefit to families' resources mixes elements that might both mobilize and demobilize. Although our data do not allow us to assess potential explanations for these findings, we hope that future quantitative and qualitative research can test some of these proposed mechanisms, as a means of refining theories of how and under what conditions government contacts affect political participation.[12]

It is possible that interpretive and/or resource effects are operating on non-voting political behaviors we cannot observe here. Voting and voter registration are just one type of political par-

[12]One might contemplate research into CPS' effects on voluntary contacts with government, such as use of particular programs or services (under the theory that CPS-impacted people might be trying to stay out of sight). We urge caution in interpreting such designs, as CPS cases often require parents to take up certain social services. Thus, shifts in program use following CPS intervention may not indicate political or attitudinal changes so much as compliance with demands imposed by CPS itself.

ticipation, which does not necessarily align with other forms of civic and political life such as attending protests, attending political meetings, donating to a political cause, or expressing confidence in one's political capacities (Soss, 1999; Owens and Walker, 2018; Walker, 2020; Christiani and Shoub, 2022). These types of political engagement are essential to study as well, as CPS contact might shift these behaviors in either direction. Qualitative work finds that some parents, worn out by their experiences with CPS, express disinterest in political activity broadly, feeling their efforts will be for naught; others, galvanized by experiences of injustice, respond by advocating for CPS reform (Fong, 2023). There might be additional dimensions of heterogeneity in non-voting outcomes, such as by race, class, or group identity (Michener, 2019; Christiani and Shoub, 2022; Anoll and Engelhardt, 2023; Garcia-Rios et al., 2023). Surveys of political engagement should include detailed questions on CPS involvement to enable further research along these lines. More broadly, the child welfare system's political import may extend beyond any individual-level participatory effects. As Roberts (2002) has argued, CPS intervention—and its concentration among Black families in the U.S.—constitutes a "group-based harm" that helps to maintain the marginalized status of Black Americans by reinforcing negative stereotypes and weakening Black communities. Studies of individual political participation such as ours do not examine these wide-reaching and consequential implications, which might be a fruitful direction for future research.

This paper focuses on political participation among adults involved in CPS cases. Of course, children are also impacted by CPS involvement. For children, having an open CPS case and/or being removed from home are experiences that may socialize them politically. Placing children in foster care is also a pivotal life course event that may shift their trajectories (Bald et al., 2022; Baron and Gross, 2022; Doyle Jr, 2007) in ways that facilitate or hinder political participation. Our study is unable to estimate the effect of CPS intervention on children experiencing this intervention. With our study design–comparing within-person political participation trends before and after CPS intervention for those assigned to different caseworkers–we cannot observe children's political participation before CPS intervention because CPS intervention happens before they reach voting age. Our findings do not speak to the impact of CPS contact on the political behavior of people

subject to CPS intervention as children, and we recommend future research take up this question.

Qualitative work has provided rich insight into how punitive state systems shape the political lives of race-class subjugated communities (Weaver, Prowse and Piston, 2020, 2019; Lerman and Weaver, 2014). Data from existing qualitative research suggest varying ways that CPS intersects with political engagement (Fong, 2023). However, because CPS has primarily been studied by sociologists and social work scholars, the system's political consequences have not been the primary focus of extant research. Further qualitative research focused specifically on this dimension of the system may be illuminating in itself and may also point to additional outcome measures and potential mechanisms to explore.

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A Supporting Information for 'Child Welfare System Contact and Voting'

Contents

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A.1 More detail on Registration/Voting Data

As noted in the paper, we rely on registration and voting records drawn from secretary of state data that has been merged into the county data warehouse at various points by ACDHS. Specifically, ACDHS recorded voter registration record matches for each person in their data warehouse using copies of the state's voter registration data from 11/19/2012, 12/26/2016, 12/24/2018, 9/14/2020, and 1/11/2021. For each of these dates, we have information about whether each person in the data warehouse appeared in the voter registration records at that point in time, and what their state voter identification number was if they matched. These state voter ID's allow us to link registrants back to snapshots[13] of the state's voter history data, in order to also see whether registrants voted in a given election.

In order to record whether a person was registered to vote for a particular election, we rely on the voter-registration snapshot closest in time to that election: for the 2012 election, we use the November 2012 registration snapshot, for the November 2016 election the December 2016

[13]To observe voter turnout, we rely on the same set of snapshots listed in our description of the registration merge, plus some additional snapshots collected to ensure good coverage of vote history: the dates of these additional snapshots are 2/6/2017, 2/11/2019, 2/8/2021, 1/17/2022.

snapshot, for the November 2018 election the December 2018 snapshot, and for the November 2020 election we rely on a combination of the September 2020 and January 2021 snapshots (as some people could have registered after September and prior to the November election). We do not have a snapshot close in time to the 2014 election, so we do not calculate voter registration for that election. The descriptive analysis in the paper focuses on 2016 turnout both because of our confidence in the completeness of the available voter-registration and vote-history data for that election, and because of the timing of the CPS records used in the paper (the 2016 election falls squarely within the period where we observe CPS cases and thus limits concerns about out-of-state moves).

In order to determine whether a given registered voter[14] cast a ballot in a particular election, we rely largely on snapshots that fall a few months after the election in question, though we include vote history from all subsequent snapshots after that election, to allow for the possibility of slowly-updating or initially-misrecorded vote history records. For example, we mark voters as having turned out in the November 2018 midterm elections if they are recorded as having voted in 2018 in any of the following voter-file snapshots: 2/11/2019, 2/8/2021, or 1/17/2022. When processing the voter data in this way, we confirm that the number of voters observed for a given election (statewide, using the whole voter-file snapshot rather than just those merged to CPS records) closely matches the number of voters reported by the Secretary of State for each election. We calculate and merge in voter-turnout data from the November elections in 2008, 2010, 2012, 2014, 2016, 2018, and 2020; we are confident about having complete vote-history data beginning in 2014, as this is the year for which our aggregate vote counts from vote history data begin to closely match SOS counts (records from prior years may be incomplete as registered voters may have dropped off the file prior to the collection of the next snapshot, such that their vote history would not be included even if they cast a ballot).

Based on available snapshots and our checks for record completeness, we include the 2014, 2016, 2018, and 2020 elections in our analyses of voter turnout, and the 2012, 2016, 2018, and

[14] Anyone not recorded as registered is treated as a non-voter (0) in turnout calculations.

2020 elections in our analyses of voter registration.

A.2 Additional Figures and Tables

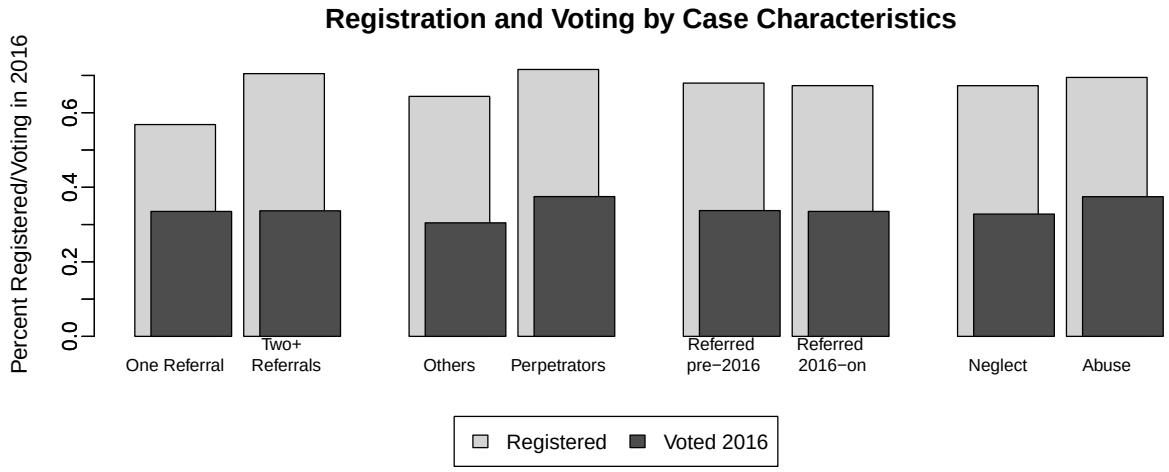
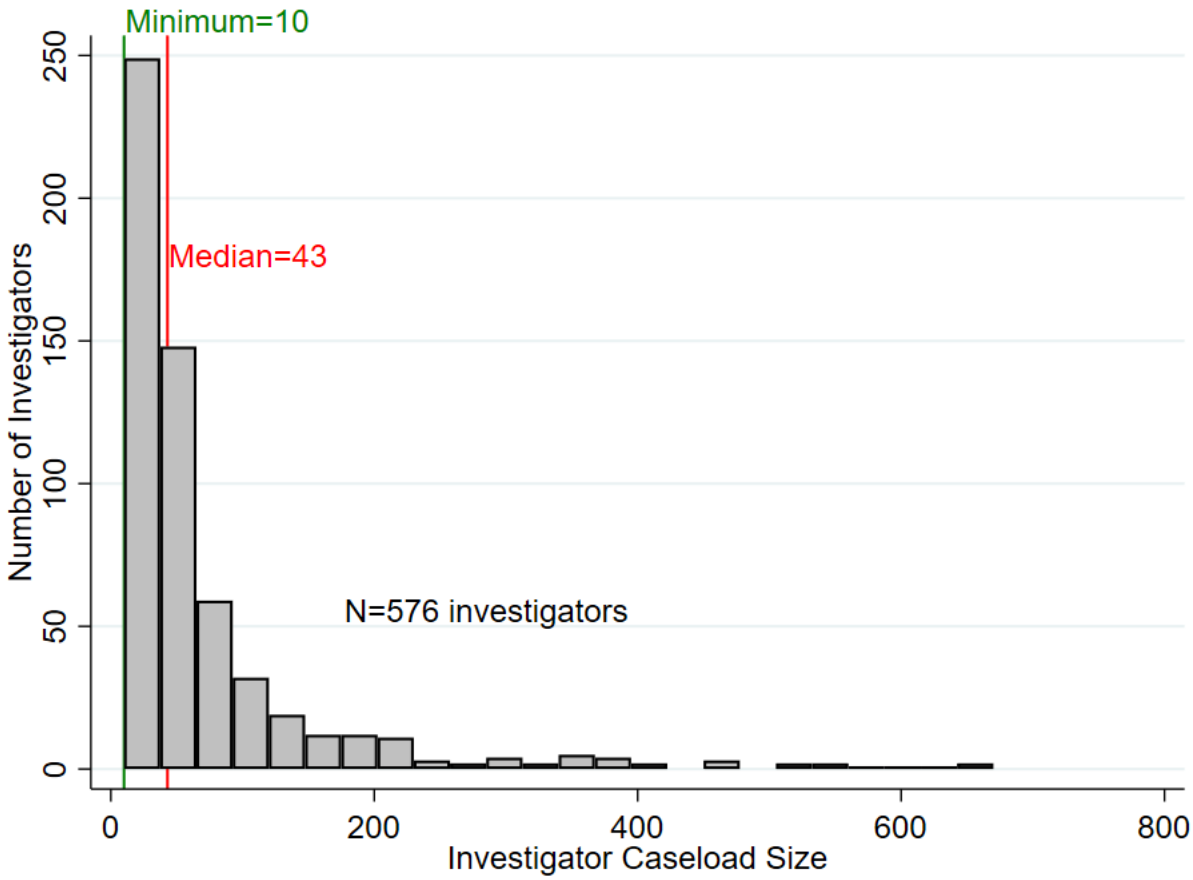


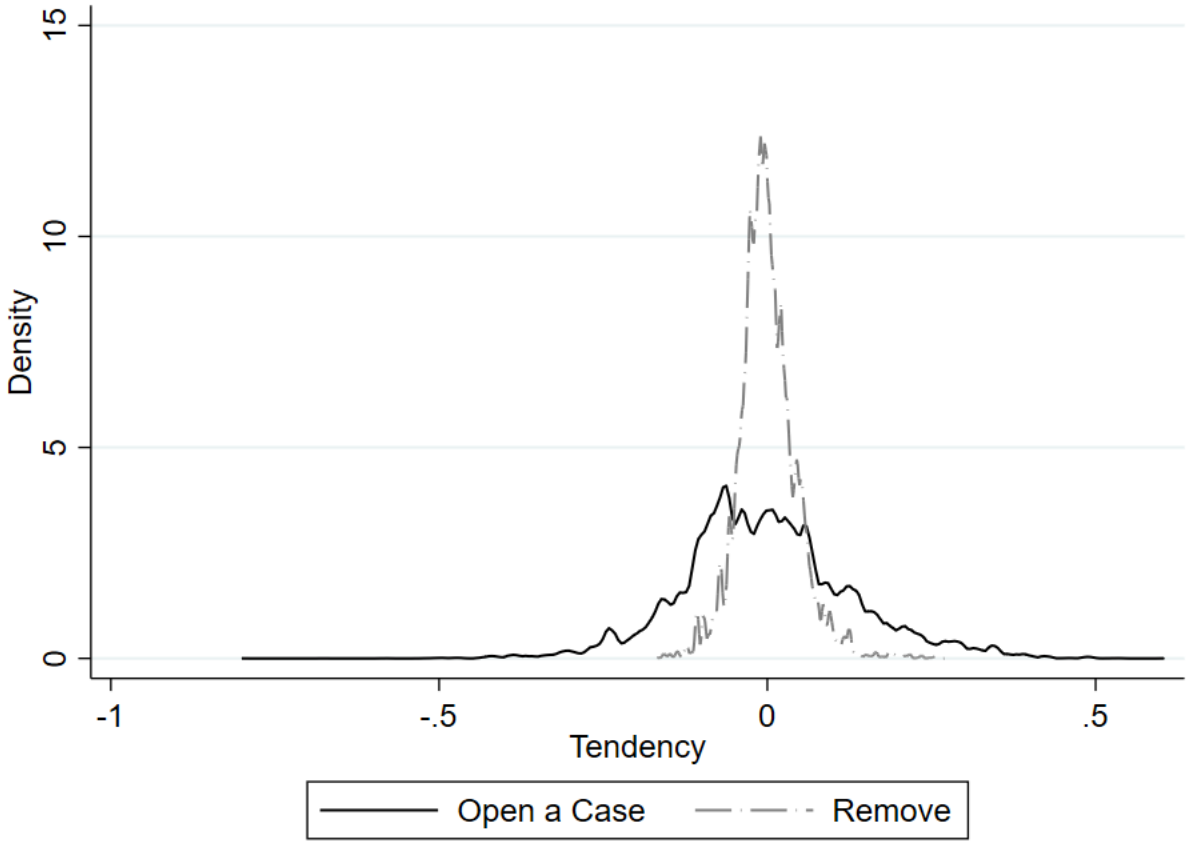
Figure A1: Political Participation in 2016 by Case Characteristics (Adults in CPS-involved Families)

Figure A2: Investigator caseloads



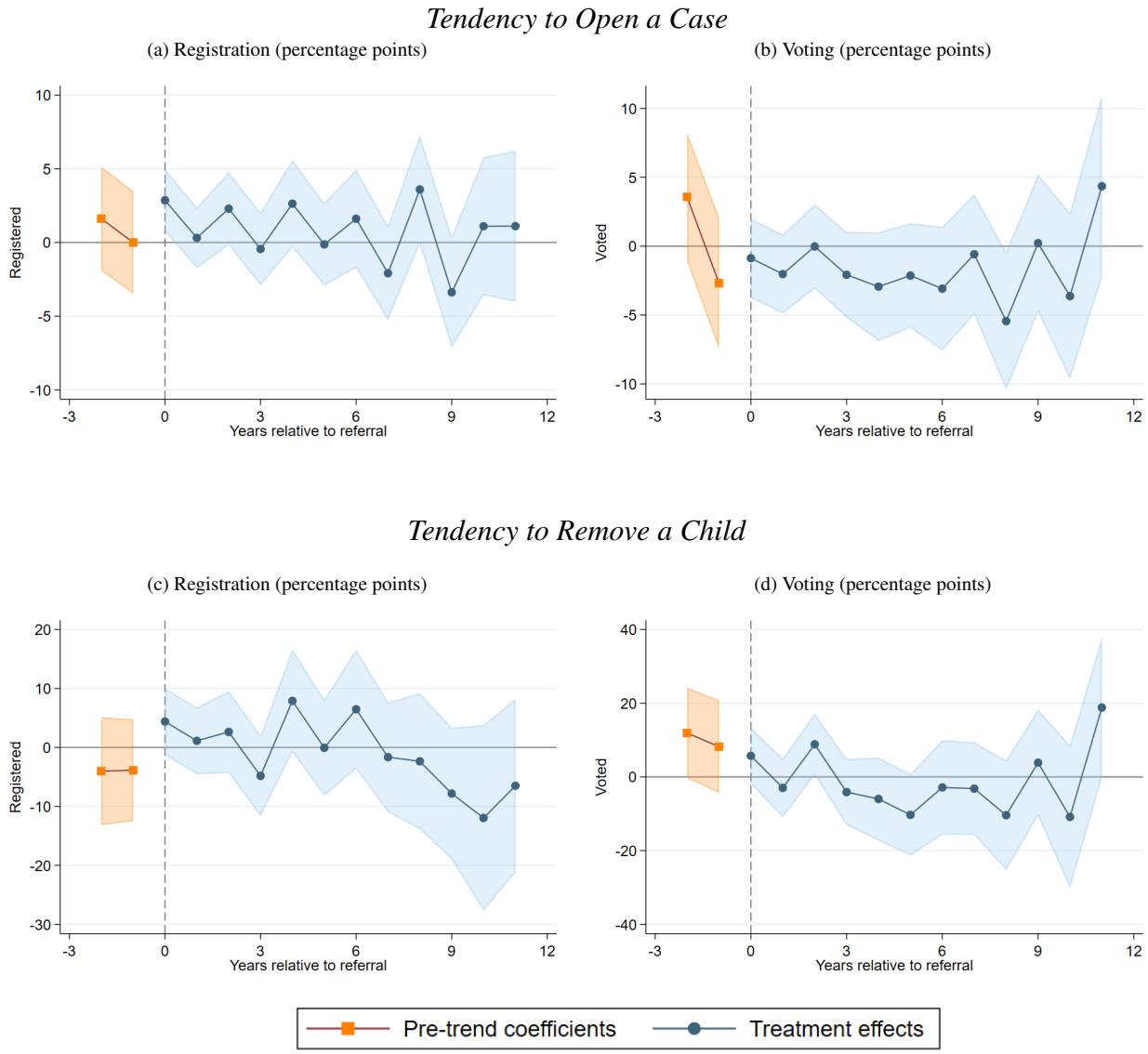
Notes: This graph presents the distribution of caseloads across investigators.

Figure A3: Variation in Tendency Measures



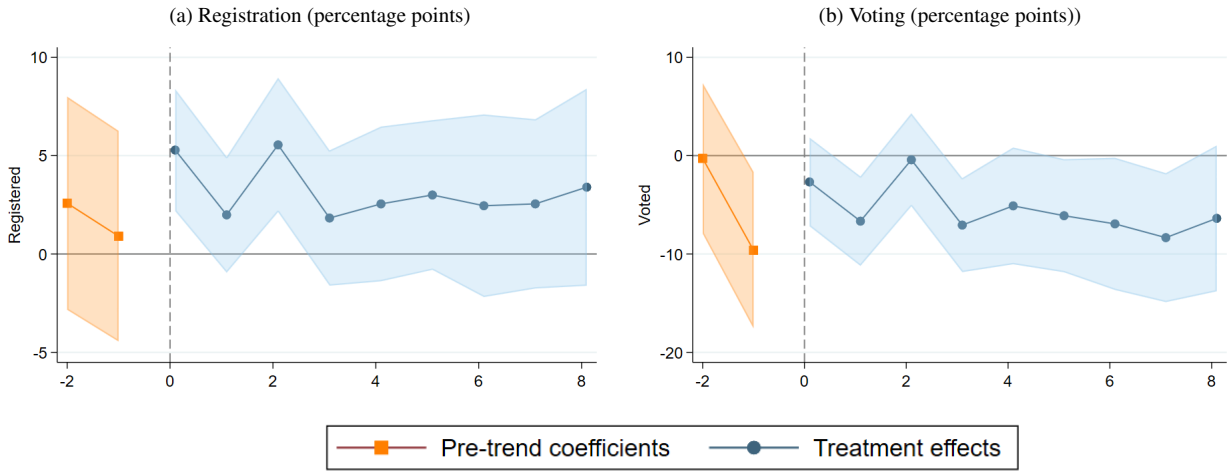
Notes: This graph presents the distributions of the tendency measures to open a case and remove after residulization on office-year fixed effects. Before residualization, the tendency measures only take values between 0 and 1. After residualization, the tendency measures indicate the variation in the tendency to open a case and to remove relative to the average in a given office-year. So for example, some investigators have a tendency to open a case that is -0.3 which indicates that they are 30 percentage points less likely to open a case than the average investigator in that office-year.

Figure A4: Effects of CPS Intervention on Political Participation



Notes: These figures report the effects of being assigned to an investigator who opens more cases (panels a and b), and of being assigned to an investigator who removes more children (panels c and d). This graph reports the unscaled estimates of the ϕ_ds from equation (1). In Figure A4a, for example, the estimate of approximately four in year 0 indicates that being assigned to an investigator with a tendency to open a case 10 percentage points more likely to open a case than the average caseworker increases registration in the year of the referral by $0.1 \cdot 4 = 0.4$ percentage points. 90% confidence intervals are reported throughout, with clustering at the individual level. The dashed vertical line is the year of the referral (year 0), so orange points show pre-trends tests and blue points the effects of investigators after the referral.

Figure A5: Effects of Opening a Case on Political Participation of Black Americans



Notes: These figures report the effects of being assigned to an investigator who opens more cases for the sample of Black Americans. 90% confidence intervals are reported throughout with clustering at the individual level. The dashed vertical line is the year of the referral (year 0). In orange are the pre-trends tests, and in blue the effects of investigators after the referral. The outcomes are reported in percentage points.

Table A1: Number of Individuals with CPS Interaction, May 2009 - Dec 2018

Restrictions	**Year Observations**	**Unique Individuals**	**Referrals**
1. On Referrals	5944851	217701	108569
2. Demographics for Voter Matching	5468298	192248	107457
3. Home address in PA	5380023	189256	107138
4. Adult to vote at least once	3095999	126199	105335
5. Political outcomes before referral	2654141	95980	102409
6. No CPS record expungement	2478945	91816	96593
7. No Termination of Parental Rights	2478365	91795	96589

Notes: This table provides more detail on sample size restrictions. The first row reports the total number of observations across years, unique individuals and referrals listed between May 2009 and December 2018. Each subsequent row adds an additional restriction. The second row restricts individuals for whom the name, gender and date of birth were not missing which were used for matching to voting registration records. The third row restricts to the subset that had a home address listed in Pennsylvania when they were called into CPS. The fourth row to those that had reached the age of 18 to be able to vote at least once in the years where we have voting registration records. In addition to being of age to register vote, our identification strategy requires availability of some political participation data prior to the referral. The fifth row thus restricts to adults for whom we could observe at least some political outcomes data prior their CPS referral: this excludes individuals who had just barely turned 18 and individuals referred too early in our sample period to have political participation prior to their referral. A subset of referrals for sexual and physical abuse prior to 2017 were expunged from CPS records due to legal requirements. So as not to introduce selection into our sample, in the sixth row we remove the referrals that were not expunged from this subset even though they could have been (these referrals were not expunged because they had met sufficiently high severity levels relative to the expunged referrals). In the seventh row, we remove a small subset of individuals who had a termination of parental rights as their records may not be traceable over time.

Table A2: Pre-trends F-tests

	Opened a Case	Opened w/o Removal	Child Removed	N
Registered	0.751	0.583	0.597	495951
Registered as Democrat	0.227	0.033	0.244	943624
Registered as Republican	0.906	0.682	0.388	943624
Voted	0.267	0.219	0.176	499566

Notes: This table reports the p-values of the F-tests for pre-trends in the two years prior to a referral using years at least three years before as a reference. Each column reports tests for detectable differences of a different CPS intervention margin. For example, the first column tests whether the families assigned to investigators with a higher tendency to open a case had different pre-trends in the two years prior to the referral. Subsequent columns report the equivalent for investigators who open more cases without removing a child and for investigators who open more cases with a child removal. The last column reports the sample sizes for these tests.

Table A3: Effects of CPS Interventions on Political Participation

		Political Participation	
	(1) First Stage	(2) Registration	(3) Voting
Tendency to Open a Case	0.69***	1.01	-1.31
	[0.01]	[0.95]	[1.12]
Tendency to Open a Case w/out a Removal	0.67***	1.06	-1.78
	[0.01]	[1.05]	[1.25]
Tendency to Remove a Child	0.37***	1.24	1.15
	[0.02]	[2.57]	[3.10]
Number of Obserations	111711	873443	873443
Number of Clusters	61323	90518	90518
Prior Mean		67.21	18.97

Notes: * p<0.1, ** p<0.05, *** p<0.01 Each cell reports the regression estimates of the outcomes listed in the columns on the tendency listed on each row. Observations are at the individual-referral level in column (1) and at the year-individual-referral level with individual-referral and year fixed effects included in columns (2) and (3). Standard errors are clustered at the individual level. Reduced form estimates in columns (2) and (3) are reported in percentage points (%). The effect of the tendency to remove a child is always holding constant the tendency to open a case without a removal: it is to be interpreted as the effect of removing more children without opening more cases without a child removal. We report investigated families' political participation prior to the referral at the bottom of the table. F-statistics for the regressions reported in column (1) are $F = 3972$ for the first row, $F = 1697$ for the second row and $F = 150$ for the third row highlighting that the tendency measures are strong predictors.

Heterogeneity Table A4 reports versions of our main analyses that subset the data to focus on specific demographic groups, years, or types of cases.

Columns 1 and 2 examine effects of CPS on registration, whereas columns 3 and 4 are for voting. Columns 1 and 3 report the effect of opening a case, and column 2 and 4 the effect of removing a child while controlling for the tendency to open a case without a removal. For comparison, the first row reports the effects for the full sample of adults presented in Table A3. The following rows report the estimates for: mothers (row 2), fathers (row 3), the first referral in 2009-2018 for each adult (row 4), alleged perpetrators of the abuse or neglect (row 5), adults listed as Black American (row 6), adults listed as White American (row 7), young adults below the age of 26 (row 8), midterm election years (row 9), presidential election years (row 10), and cases with allegations of neglect (row 11). We cannot estimate registration effects for midterm years alone because we do not have enough years of outcome data for this outcome (see SI Section A.1 for full discussion of registration data). We cannot estimate effects for abuse cases alone because we do not have enough years of these cases given legal requirements to purge records over time.

Table A4: Effects by Subgroup and CPS Intervention

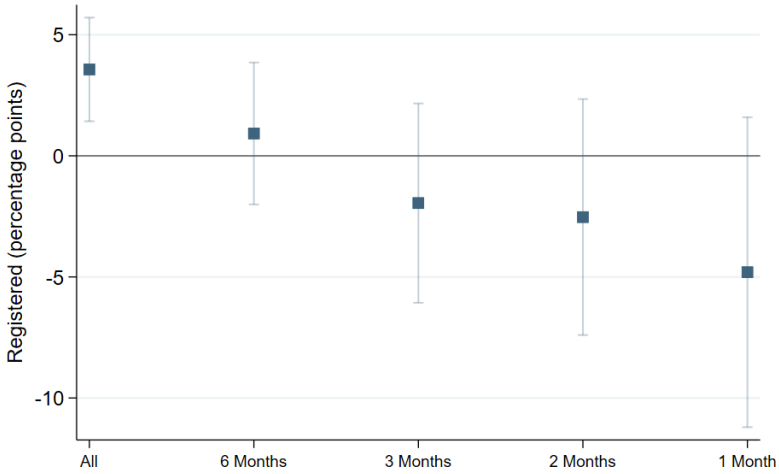
	Registered		Voted	
	(1) Opened a Case	(2) Removed	(3) Opened a Case	(4) Removed
1. All Adults	1.01	1.24	-1.31	1.15
	[0.95]	[2.57]	[1.12]	[3.10]
	(870450)	(870450)	(873443)	(873443)
2. Mothers	1.48	-1.23	-2.78	-2.98
	[1.56]	[4.13]	[1.95]	[5.45]
	(336622)	(336622)	(338047)	(338047)
3. Fathers	2.82**	4.91	1.19	1.91
	[1.41]	[3.81]	[1.53]	[4.23]
	(398720)	(398720)	(399483)	(399483)
4. First Referral in Sample	1.22	0.30	-2.73	7.43
	[1.50]	[4.16]	[1.81]	[4.86]
	(363913)	(363913)	(365121)	(365121)
5. Alleged Perpetrators	0.92	0.66	-1.41	-0.87
	[1.33]	[3.59]	[1.68]	[4.51]
	(395937)	(395937)	(397546)	(397546)
6. Black American	3.02**	-0.06	-4.38**	-1.00
	[1.34]	[3.63]	[1.79]	[4.61]
	(345359)	(345359)	(346644)	(346644)
7. White American	-0.30	1.10	1.08	-2.07
	[1.53]	[4.22]	[1.62]	[4.67]
	(418580)	(418580)	(419748)	(419748)
8. Young Adults	3.77	2.36	4.43*	-3.14
	[3.01]	[8.06]	[2.62]	[7.01]
	(115726)	(115726)	(118719)	(118719)
9. Midterm Years			-2.26*	2.02
	[]	[]	[1.33]	[3.61]
	()	()	(434368)	(434368)
10. Presidential Years	1.00	2.21	-1.01	1.69
	[1.10]	[3.06]	[2.12]	[5.68]
	(651521)	(651521)	(437330)	(437330)
11. Neglect	0.83	2.39	-0.48	-0.59
	[1.00]	[2.72]	[1.17]	[3.19]
	(814226)	(814226)	(816839)	(816839)

Notes: * p<0.1, ** p<0.05, *** p<0.01 Observations are at the year-individual level with individual-referral and year fixed effects throughout. Standard errors are clustered at the individual level and reported in square brackets. Sample sizes are reported in parentheses below the standard errors.

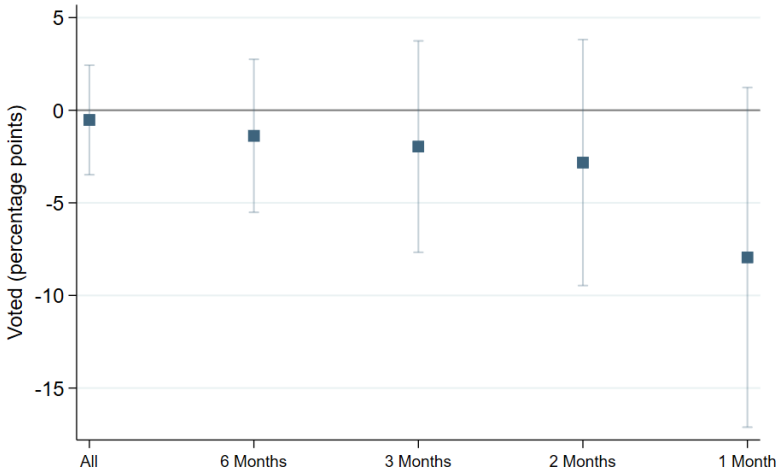
Short-run effects We also consider whether there might be transitory effects for people who are dealing with CPS cases just as an election occurs. In Figure A6, we present effects of being assigned to a caseworker with a high case-opening rate on registration or voter turnout, as in our main estimates in the paper. But here, we focus only on the year-0 coefficient (the effect on the very next election after the referral), and we subset to smaller and smaller parts of the sample. The leftmost estimate includes our whole sample, as in the main analyses; next, we subset to people with referrals falling within one year before the election, and then to people whose referrals are in the three-month, two-month, or one-month window just prior to the election. As we narrow our focus to people with referrals immediately before the election, we see larger and more negative point estimates, though none are statistically distinguishable from zero. This pattern could suggest very short-run demobilizing effects for people with referrals quite near the next election, though we urge caution in interpreting these estimates this way given how much of the sample is discarded (the one-month window retains only about 5% of the overall sample).

Figure A6: Short-Run Effects of Being Assigned to Investigators more likely to Open a Case

(a) Registration (percentage points)



(b) Voting (percentage points)



Notes: These graphs report the effects in the first year after contact with CPS (year 0 of Figure A4), with 90% confidence intervals, for individuals referred within differing time-spans prior to an election. We report the base sample on the left, and then progressively restrict the sample to shorter-time windows as we move to the right: base specification (families referred in the year of an election), families referred within 6, 3, 2 and 1 months of an election date. The top panel reports effects on voter registration and the bottom panel on voter turnout. Graphs only report the effects of being being assigned to caseworkers who open more cases, as our instrument for child removal is too noisy to restrict our sample so much.

A.3 Compliers

The estimates presented in the paper of the causal effect of CPS case opening or child removal are based on an instrumental-variables approach that returns a LATE, a local average treatment effect, among compliers. In this section, we consider how much of the sample is made up of compliers and what their characteristics are, in order to get a sense of whether we are attempting to extrapolate from a small and unrepresentative set of families. The compliers cannot be individually identified, but they can be characterized, as explained in Angrist and Pischke (2009, p.171). Briefly, it appears that the complier population makes up a fairly large share of our sample (especially when considering case opening) and is broadly representative of the set of families encountering CPS.

Our estimates of the share of compliers—of how many families in the sample could plausibly have seen a case opened, or not, based simply on their caseworker assignment—depends on estimates of caseworker tendencies to open cases or remove children. The share of always takers can be estimated from the share of cases that the lowest intervening caseworker decides to intervene in, and the share of never takers from the highest tendency to intervene. Together, these provide an estimation of the share of compliers, under the assumption that there are no defiers. These tendencies are noisily estimated for caseworkers that handle few cases in our dataset, so we repeat this calculation with several different thresholds of number of cases handled and find that it yields fairly similar estimates of the share of compliers. When limiting to caseworkers who handled at least ten cases, as in our analyses, we calculate a complier share of 65% across the various CPS offices and years in the sample when considering case opening, and 21% when considering child removal. These may be overestimates of the complier shares as the variance in tendency measures may be overestimated due to noise. If we more conservatively limit to caseworkers that handled at least 70 cases during the period of our dataset, we reach similar conclusions: 44% compliers for opening a case and 13% for child removal. These indicate that a substantial fraction of our sample are compliers, especially when focusing on the decision to open a case.

Next, we consider the characteristics of compliers: how are the families that drive our estimates different from or similar to the full set of families encountering CPS? Taking the ratio of the first

stage coefficient is in the subgroup relative to the full sample indeed indicates how much more or less likely the group was to be affected by this disagreement across workers (Angrist and Pischke, 2009, p.171). Results of this exercise are presented in Table A5 which reports how much more or less likely each background characteristic is in the set of compliers relative to the rest of the sample. Particularly on the margin of case-opening, where compliers represent a large fraction of the sample, compliers are broadly quite similar to the rest of the sample on dimensions such as race, age, and gender. The largest differences emerge around the types of allegations included in referrals: compliers are much more likely to have allegations of drug abuse, and less likely to have allegations of physical abuse, than the rest of the sample (suggesting that drug cases have substantially more room for caseworker disagreement, while physical abuse cases are ones where caseworkers generally tend to agree on a course of action). Somewhat larger differences emerge when considering child removal, a rarer outcome and one where there appears to be less caseworker disagreement overall (compliers represent a smaller share of the sample). Here, compliers are more likely to be Black, less likely to be women, and more likely to have allegations of a parent struggling or drug abuse or physical abuse, compared to the sample as a whole. Thus, we may wish to be more careful about extrapolating the LATE of child removal among this group to the broader set of families with CPS contact, compared to the estimates for case opening.

Table A5: Complier Characteristics at each CPS Margin (%)

	Opened a Case	Child Removed	N
Black American	-5	14	49136
Missing Race	-2	2	13819
Woman	0	-7	52940
Young Adult	6	8	15942
More than One Referral	1	9	90129
Allegation: Parent Struggling	0	41	30532
Allegation: Parent Drug Abuse	14	12	20102
Allegation: Failure to Protect Child	-6	-3	18664
Allegation: Physical Abuse	-23	30	18817
Ever Registered Prior to Referral	-2	2	70783
Ever Voted Prior to Referral	-4	-5	25205

Notes: This table reports how different the compliers are for the two margins of intervention (opening a case and removing a child) relative to the full sample. Characteristics are listed in rows, and coefficients are to be interpreted as the percent increase/decrease in likelihood of having a given characteristic. In the last column we report the number of observations in each group. So, for example, individuals where investigators would disagree about whether to open a case are 5% less likely to be Black American, but those where investigators would disagree to remove a child are 14% more likely to be Black American. We have $N = 49,136$ Black Americans in our sample. Young adult refers to adults below the age of 26 at the time of the referral.

A.4 Statistical Power

In this section, we consider the statistical power of our design. Table A6 first presents a fairly straightforward approach based on considering a simple composite summary of our overall study findings: an F-test of the null hypothesis that *neither* margin of CPS intervention (opening a case without a removal or removing a child) has any effect on either of our outcome measures (registration or voting). This is a particularly well-powered approach even relative to some of those shown in the paper, as it pools all years of data to look for *any* change in participation on *either* margin. The first row of the table presents the p-value of this joint F-test, establishing that consistent with the estimates shown in the paper, we do not reject the null. Then row 2 considers the statistical power of a slightly simplified version of that F-test, using the analytical formula for the power of an F-test and ignoring (in column 2) the individually-clustered nature of the data we use. This approach gives an approximate sense of power (and indicates that our design is extremely well-powered), but is limited in that it does not incorporate information about clustering because there is no obvious way to do so given the formula. As seen in a comparison of the p-value on the F-test across Columns 1 (accounting for clustering) and 2 (not considering clustering), our estimates are slightly noisier when considering clustering, in ways that are not captured by this power calculation. Nevertheless, we read Table A.4 as evidence that the null results presented throughout the paper represent relatively precise and well-powered zeroes rather than simply noisy estimates obscuring substantively-important effects.

Table A6: Power Calculations

	(1) With Clustering	(2) No Clustering
F-test pvalue	0.43	0.29
Test's Statiscal Power		1.00
Number of Observations	1743893	1743893
Number of Clusters	90518	

Notes: This first row in this table reports the pvalue of the F test that neither margin of CPS intervention (opening a case without a removal and opening a case with a removal) has any effect on either registration or voting, and the second row the corresponding power of this test. The first column includes clustering at the individual level, while the second column does not.

A.5 Hypothetical Electorate

The descriptive section of the paper includes a back-of-the-envelope calculation of how different the Allegheny County electorate would have looked if CPS-affected adults had voted at the county's average rate in 2016. Here we briefly describe that calculation.

We begin with a snapshot of the Pennsylvania voter file from spring 2017, limiting it to 653,446 voters with Allegheny County addresses who voted in the 2016 election.[15] We then use the wru() package to impute race for these voters, using surname and county Census data, and aggregate up these predictions to reach an estimate of 84,238 Black 2016 voters on the file (12.9% of all voters in 2016).

Then, we use information about the CPS-involved sample to calculate how many additional voters would have turned out at the higher turnout rate in 2016. The sample is approximately 35% Black[16] and Black turnout in the sample (shown in Figure 2) was 35.5% in 2016. The overall sample includes 91,752 individuals. So if we imagine moving turnout from .355 to .68, that implies 91752 * .35 *(.68-.355) = 10,437 additional Black voters. We can construct an analogous estimate for voters from other groups (the remaining 65% of the CPS sample, with turnout of 32%), which indicates an additional 19,818 voters.

Finally, we add both these groups of hypothetical additional voters to the actual voting population of the county, yielding a total of 683,701 voters, 94,674 of whom (13.8%) are Black.

[15]This number will not precisely match the reported vote count from the Secretary of State's election results due to small amounts of voter file churn, but we need to use the voter file records so that we can impute race based on individual characteristics.

[16]As noted in Table 2, 12% of the sample has missingness on the race variable. To make this calculation conservative, we assume no one in that group was Black; assuming instead that the missingness was random would make the demographic shift we describe look even more extreme.