

CONDITIONAL CASH TRANSFERS AND CORPORAL PUNISHMENT*

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Abstract

Across and within countries there are large differences in how parents discipline their children and, in most contexts, poverty is associated with higher levels of physical punishment. We leverage the roll-out of a conditional cash transfer program in Peru to test whether program eligibility affects parental disciplining of their children—especially physical punishment. We find that when parents in a district receive the cash transfer, the average level of hitting is reduced by 7-13%. Our findings suggest that program participation may have additional second-order benefits through the reduction of harsh physical forms of parenting discipline practices.

Keywords: Corporal Punishment, Children, Cash-transfer, Poverty, Peru.

JEL Codes: I38, I15

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1 Introduction

In 2021, the United Nations Children’s Fund (UNICEF) reported that in most countries, more than 2 in 3 children are subjected to violent discipline by caregivers ([United Nations Children’s Fund, 2021](#)). Corporal punishment—generally defined as non-injurious hitting of children to inflict pain in response to misbehavior or to modify behavior—is the most common form of violence against children ([Gershoff and Grogan-Kaylor, 2016](#)). While social norms on the physical punishment of children differ across the world and are evolving over time ([Fréchette and Romano, 2015](#); [Gershoff and Grogan-Kaylor, 2016](#); [Paolucci and Violato, 2004](#)), the general pattern suggests that across and within countries, poverty is associated with higher levels of physical punishment. Poverty may influence the use of corporal punishment of children if parents become less patient due to increased stress; if economic conditions cause the child to behave in ways that the parent deems worthy of punishment; or if caregivers have fewer options such as (costly) incentives to encourage preferred child-behaviors.

In this paper we leverage the roll out of a large-scale conditional cash transfer program (*Juntos*) across time and space to study its effects on the corporal punishment of children in Peru. *Juntos* started in 2005 and was expanded yearly throughout 2017. It benefited almost 700,000 households annually during the latter years of our study period. We link administrative data on the number of *Juntos* beneficiaries at the district level each year, with ten years of cross-sectional survey data that includes information on parental disciplining practices. We use a difference-in-differences approach with staggered roll out while controlling for district fixed effects, province-year fixed effects, and a series of demographic characteristics at the mother, children, and household level. We show that the results are in line with estimates calculated using methods robust to treatment effect heterogeneity.

We find that the program results in lower rates of hitting of children by mothers—the more severe form of corporal punishment. These effects are non-trivial and indicate a reduction of 7-13% at the district level. Our results are robust to focusing on different subsamples or time periods, population weighting, and adding a variety of controls. We use an event study type specification to explore the dynamics of the average effect and find an immediate response that persists. We also find suggestive evidence that parents substitute towards other less violent forms of discipline such as forbidding something the children like. Moreover, we find that the strongest reduction in hitting happens in households with children ages 5 to 11 and male children. We also explore the effect of the program on the discipline practices of fathers and estimate coefficients of the same sign that are less precise. Our results capture the overall effect of *Juntos*, which include both the cash

transfer component and the conditionalities. We investigate potential mechanisms through which *Juntos* receipt may result in lower rates of physical punishment. We find suggestive evidence that changes in mothers' attitudes towards physical punishment can explain a large share, while the conditionalities and increased resource channels likely explain a smaller share of the overall observed effect.

Models of parent-child interactions have been proposed in the literature where parents can use pecuniary rewards to shape children's behavior (Becker, 1974, 1991; Hao, Hotz and Jin, 2008). Other models by Akabayashi (2006) suggest the inability of parents to perfectly observe a child's effort can lead to an equilibrium with child maltreatment. Economists have sought to theoretically explain differences in parenting practices across different socio-economic environments. Weinberg (2001) sets up an agency problem to model parent-child interactions where differences in parenting practices can arise endogenously as lower income parents are less able to rely on pecuniary incentives (e.g. financial rewards for good grades) and thus rely more heavily on alternative practices such as corporal punishment. More recently, Doepke and Zilibotti (2017) set up a model where more authoritarian parenting, which includes the use of corporal punishment, emerges as an equilibrium outcome when social and occupational mobility are low. Recent related empirical work that is closest to what we study shows that tax benefits reduce child maltreatment proxied by referrals to child protective services and placement of children in foster care (Rittenhouse, 2022).

Related work investigates the long-term consequences of corporal punishment. However, empirically studying the relationship between corporal punishment and future economic outcomes is fraught with endogeneity difficulties. The few studies in economics that have done this have found mixed results. Petrova, Rao and Wheaton (2020) use historical data and a difference-in-differences approach and find that exposure to corporal punishment in schools increases trust in institutions and tolerance for free speech. Their results suggest that it increases educational attainment and reduces later-life crime. They note that very few children experienced punishment and that the average positive outcomes were likely due to most students benefiting from the restraining of disruptive students. Other studies, that look at abuse and neglect in the household, find important negative and long-term effects (Currie and Tekin, 2012) and heterogeneous but positive effects of early removal from these households (Bald et al., 2022). Using rich panel data from China, Kim and Wang (2022) find that parents are more likely to use harsher punishment practices on later born children—especially in rural and low-income households—suggesting a likely mechanism for the negative correlation between cognition (and academic achievement) and birth order in China.

Moreover, non-cognitive skills are important determinants of a range of later-in-life outcomes (Heckman and Kautz, 2012; Heckman and Mosso, 2014), and researchers have sought to understand how different home environments and parent-child interactions contribute to such development (Spera, 2005). Doepke, Sorrenti and Zilibotti (2019) provide a recent review of this literature within economics particularly as they relate to changing macroeconomic conditions across countries and over time. Parenting styles are shown to be a critical input to the development of such skills (Fiorini and Keane, 2014), and recent work has sought to understand what types of parenting interventions matter and through which mechanisms (Carneiro et al., 2024). Some of this research highlights the importance of parental instruction interventions as effective ways to reduce parents' use of harsh punishment such as corporal punishment (Kliem, Foran and Hahlweg, 2015; García and Heckman, 2023; Diaz et al., 2023).

As far as we are aware, our paper is the first to study the causal effect of a conditional cash transfer program on parental use of corporal punishment. Our findings complement existing work by showing that conditional cash transfer programs that are common around the world, also play a role in facilitating changes to parental disciplinary practices. In our context, through the *Juntos* program, parents appear to shift away from harsher punishments towards alternative disciplinary practices. Given the long-term negative consequences of corporal punishment documented in the literature, our results suggest an added benefit and potential mechanism through which poverty alleviation programs affect children in the long-run.

We additionally contribute to the literature studying violence in the household. Most of the work in this area focuses on intimate partner violence (IPV) which is prevalent around the world, with reports indicating that one in three women have experienced some form of IPV (Devries et al., 2013). Scholars have theorized that poverty-related stressors could increase IPV and thus programs that ease these stressors could decrease IPV (Ellsberg et al., 2015; Vyas and Watts, 2009; Fox et al., 2002). Recent meta-analyses that try to isolate the effect of cash transfers on IPV find that most evidence suggests that cash transfers reduce IPV rates (Hidrobo, Peterman and Heise, 2016; Buller et al., 2018; Gibbs, Jacobson and Kerr Wilson, 2017). Another meta-analysis by Baranov et al. (2021) suggests that on average, cash transfer programs reduce physical and emotional violence towards partners consistent with household resource and stress theory perhaps dominating other bargaining theories. We explore whether cash transfers reduce physical violence towards children, other vulnerable members of the household.

The rest of the paper is structured as follows: In section 2, we highlight the *Juntos* program and the data we use in this analysis. In section 3, we discuss the econometric ap-

proaches we use to show the effect of *Juntos* on discipline practices of parents. In section 4, we present our main results, show their robustness, and explore the mechanisms through which *Juntos* likely acts. Finally, in section 5 we conclude.

2 Background and Data

2.1 The *Juntos* Program

Peru's *Juntos* is a conditional cash transfer program for poor pregnant mothers and poor households with children up to 19 years. In addition to providing income support, the goal of the program is to increase school enrollment and preventive health checks. Participation is voluntary but take up is high at 93%. For families that met the conditionalities, the monthly transfer during our period of study was 100 Peruvian Soles (around \$30 USD in 2019 exchange rate), which was approximately 15% of poor households' monthly consumption (Sánchez, Meléndez and Behrman, 2020; Silva Huerta and Stampini, 2018; Andersen et al., 2015). The transfer is typically paid to mothers and the conditionality depends on children under 59 months receiving comprehensive health and nutrition care, school age children attending school, and having a national identity card (Sánchez, Meléndez and Behrman, 2020). Beneficiaries are issued with ID cards, which they need to take to the National Bank to receive their payments. Identification of beneficiaries and targeting occurs in three stages: first geographical targeting identifying eligible districts; then household targeting using a poverty index score; and lastly community validation of potential beneficiaries (Jones, Vargas and Villar, 2008; Silva Huerta and Stampini, 2018).¹

Consistent with the targeting criteria, households in *Juntos* districts tend to be poorer and more likely to be rural. In Appendix C (Table C1) we examine the characteristics of mothers resident in three types of districts in our sample: districts that became eligible for *Juntos* prior to 2011, districts that became eligible for *Juntos* during our sample period of 2011 to 2019, and districts that are never eligible within our period of study. Districts selected earlier for receiving the *Juntos* program are much poorer and more rural. Districts that become eligible later, are more likely to be rural and on average poorer than districts that remain ineligible, but are better off than initially enrolled districts. This pattern is consistent with geographical targeting described in more detail in Appendix C.

In general, researchers find that *Juntos* resulted in increased use of health facilities, school enrollment and attendance, a moderate reduction in poverty and increased household consumption (Díaz and Saldarriaga, 2019; Gaentzsch, 2020; Perova and Vakis, 2009).

¹We provide more detail for each of these in Appendix C.

Some scholars find that despite the increase in school enrollment, there were limited cognitive gains in children (Andersen et al., 2015; Gaentzsch, 2020; Escobal and Benites, 2012), and they point to supply side problems. More recent studies found that the educational gains are more nuanced, as early life exposure to *Juntos*, particularly during the first 4 years of life, leads to cognitive and nutritional improvements for children (Sánchez, Meléndez and Behrman, 2020).

2.2 Data

We use *Encuesta Demográfica y de Salud Familiar* (ENDES) data conducted by *Instituto Nacional de Estadística e Informática* (INEI), the Peruvian government statistical agency. These data are very similar to the Demographic and Health Surveys (DHS). We use the surveys conducted from 2010 to 2019 in our analysis because they include questions on parental corporal punishment practices. Sampling follows the standard DHS approach of selecting households with women ages 15-49. Data collected include demographic characteristics, information on household assets and living conditions, and extensive health information.

One woman in each household age 18 or older was selected to participate in the domestic violence module with an extensive section regarding child discipline. Specifically, biological mothers with children 18 years or younger in the home are asked about twelve specific child discipline strategies and whether they themselves, their child(ren)'s biological father, and/or another household member has used each method to punish their child(ren). Gage and Silvestre (2010) indicate that interviewers probe to determine whether more than one form of punishment was used by the person disciplining the children.²

These discipline data are self-reported by mothers and there could be concerns about under-reporting being correlated with *Juntos* receipt. However, there are several factors about our setting that alleviate these concerns: first, the program benefits were not linked to discipline practices and as opposed to data collection that happens as part of targeted program or policy evaluations, ENDES is not directly connected to *Juntos*. Second, Gage and Silvestre (2010) indicate that Berger (2005) and Tang (2006) report that women in two-parent households are more likely than men to report physical violence against children, and the data here is reported by the mother. Finally, Arguero and Frisancho (2022) show

²The twelve categories in the order in which they appear are: slapping, verbal admonishment, forbidding something the child likes, depriving them of food, hitting or physical punishment, leaving them locked up, ignoring them, giving them more work, leaving them outside the house, throwing water at them, taking away their clothes/belongings, and taking away monetary support. The exact wording in the questionnaire in Spanish for slapping is “palmadas” and for hitting is “con golpes o castigos físicos”. Gage and Silvestre (2010) who also use these data, translate slapping as “slapping or spanking” and hitting as “beating”, and they indicate it corresponds to “hitting/striking or physical punishment”. They also provide more details on the origin of the survey questions, which were adapted from questions developed in Colombia.

that the typical module used in the DHS to measure intimate partner violence yields similar results to that measured using indirect methods such as a list experiment. They document this in Peru and find that this seems to hold for multiple different sub-groups. While IPV is distinct from corporal punishment, we might expect similar concerns with the under-reporting of IPV, as such this validation study also assuages concerns.

ENDES also collects information on household participation in a range of social protection programs, including *Juntos*. For the first three years of our analytical sample (2010-2012), *Juntos* participation was only asked among women with children under 5 years of age. However, from 2013 information is available for all households. We link the ENDES data with administrative data from the *Ministry of Development and Social Inclusion*. This dataset provides information on how many households were deemed eligible to receive *Juntos*, as well as the number of households receiving it in each district from 2005 to 2020.³

2.3 Descriptive Statistics

In Table 1 we show summary statistics at the mother-level both for the full and study samples. In the full sample, three quarters of the mothers live in urban areas and live in households that have on average 4.6 members. Mothers are just over 34 years old on average and have about 10 years of schooling. A majority of them are working (67%) and 14% are divorced or separated. The mothers in the sample have on average 2.1 children. The average age of the children is nearly 8 years, and most of them are in school.

In Panel E we list the most common forms of discipline indicated by the mothers. *Verbal admonishment* is at the top of the list and is used by approximately 74% of mothers. This is followed by *forbidding something a child likes* (49%). The most common form of physical punishment, which is also the most violent, is *hitting or physical punishment* (28%), followed by *slapping* with 13%. Only 6% of mothers indicated that they use any of the remaining eight forms of discipline. Panel F reports that in our full sample, about 13% of mothers indicate someone in their household receives *Juntos*; around 29% of mothers live in districts eligible for *Juntos* in the year they were interviewed; and about 77% live in districts that received *Juntos* at some point in the time period we study. The main differences between the full and working sample appear here. Our study sample excludes always treated districts and districts we observe 7 or fewer times across the ten year period, thus the proportions reported for *Juntos* receipt, eligibility and affiliation are lower.⁴

In Figure 1(A) we show how the most severe form of corporal punishment (hitting)

³Peru has 25 regions, formerly known as *Departamentos*, 196 provinces, and 1,874 districts.

⁴As explained in section 4, we impose this restriction to be comparable to the [Borusyak, Jaravel and Spiess \(2021\)](#) estimating sample.

varies by the age and sex of the eldest child. The share of mothers reporting using hitting as a form of discipline is highest when the age of the eldest child is between ten and twelve. Throughout the age distribution male children have a higher proportion of mothers that report hitting.⁵ In Figure 1(B), we show the incidence of the most common discipline practices by wealth decile of the household. Compared to other forms of punishment verbal admonishment (scolding) is high and relatively stable across wealth deciles. Forbidding something the child likes, as well as slapping increases with wealth. This contrasts with the most severe form of corporal punishment, hitting, which shows a strong negative relationship with wealth. Overall, mothers in the bottom wealth deciles have the highest self-report of any type of corporal punishment. Finally, in the Appendix Figure B2 we document the trend in hitting across time in our sample split by rural and urban districts and in the lower panel, by child sex. This shows a slow but decreasing trend in the share of mothers reporting hitting over time in both urban and rural areas, thus the decline in violence towards children in Peru over this time period is not particularly restricted to rural regions where *Juntos* is most prevalent. Similarly, in the lower panel we show that the trend in hitting steadily decreases in households with all male and all female children.

While understudied in economics, the corporal punishment of children is well-studied in the social psychology literature. In general, this literature finds associations where children are more likely to experience corporal punishment if they live in single parent households or with a non-relative caregiver; if they are poor; if the parents have a more traditional view of discipline; and if the caregivers were physically punished as children (Rohner, 1986; Ember and Ember, 2005; Douglas and Straus, 2006; Gershoff and Grogan-Kaylor, 2016). These patterns largely hold in our data as shown in Appendix Table A1. In this Table, we report OLS results for mother, child, and household characteristics correlated with hitting. Column (1) shows raw differences across wealth where those in the richest two deciles are almost 25 percentage points less likely to use hitting as a form of discipline. The explanatory power of wealth goes down as we add more controls. In column (5), despite controlling for year and district fixed-effects and a host of mother, child, and household-level controls, differences across wealth persist. Mothers in the richest decile are about 8 percentage points less likely to hit their children.

Estimating the long-term consequences of corporal punishment is fraught with endogeneity concerns. Nonetheless, research findings suggest that physical punishment of children is strongly associated with negative short- and long-term physical, emotional, behavioral, and cognitive outcomes (Ferguson, 2013; Paolucci and Violato, 2004; Gershoff and

⁵Note that corporal punishment is not reported for every child, but rather whether the parent/caregiver uses it as a form of punishment. Nevertheless, the qualitative patterns in the figure does not change if we use the average age and majority sex of the children in the household.

Grogan-Kaylor, 2016; Larzelere and Kuhn, 2005).⁶ Although imperfect, using our data we examine long term associations of mothers who were physically punished as children. These results are presented in Table B1, and indicate that these mothers attained fewer years of schooling, are more likely to approve of wife beating, are more likely to currently use corporal punishment with their children and agree that it is necessary to discipline children. Thus, suggesting economically meaningful lifetime consequences of exposure to corporal punishment in childhood.

3 Estimation Approach

In our main approach we leverage the staggered geographical roll out of *Juntos* across districts over time. Using ordinary least squares (OLS), we estimate the following two-way-fixed effects (TWFE) specification:

$$P_{idpt} = \beta_0 + \beta_1 UbiJuntos_{dpt} + X'_{idpt} \Theta + \gamma_d + \sigma_{pt} + \epsilon_{idpt} \quad (1)$$

where P_{idpt} is reported punishment by mother i , living in district d , in province p , in year t . $UbiJuntos_{dpt}$ is our main explanatory variable and takes a value of one when the mother lives in an district eligible for *Juntos* in year t . X_{idpt} is a vector of household, household head, mother and child characteristics. To control for time-invariant differences across districts we include district fixed effects (γ_d). We include σ_{pt} —a province-year fixed effect—which accounts for province specific shocks in any given year. This controls, in a flexible way, for potentially different time trends in corporal punishment in each province.⁷ Finally, ϵ_{idpt} is an unobserved error term.

Our coefficient of interest in this specification is β_1 . Under the assumptions outlined below, this coefficient captures the average treatment effect on corporal punishment among mothers when their district of residence becomes eligible for (and in most cases is receiving) *Juntos*. Not everyone in the district receives *Juntos*,⁸ and thus this coefficient is the weighted average of within-district average effects which has some households receiving *Juntos* and others who are not. Moreover, the estimated coefficient captures the overall

⁶A review by Gershoff (2002) of over 300 studies on corporal punishment shows that corporal punishment is associated with aggression, anti-social and delinquent behavior in youth; and with aggression, criminal activity, poorer health, and anti-social behavior in adulthood. Other studies show that corporal punishment (and its frequency and severity) are associated with abusive acts towards spouses and children later in life (Zolotor et al., 2008; Douglas and Straus, 2006), and it is associated with lower levels of cognitive development (Berlin et al., 2009; Straus and Paschall, 2009). However, this literature is not causal.

⁷For example, this would account for an active non-government organization starting in a particular year in some provinces and pursuing a specific agenda regarding violence in the family.

⁸As Figure B1 shows, on average about 40% of mothers within an eligible district receive *Juntos*.

effect of *Juntos*, including the impact of the cash, or the conditionalities associated with the cash, i.e. kids spending more time in school and getting medical check-ups. We revisit this in the mechanisms section (Section 4.3).

This TWFE approach with staggered roll out requires several assumptions in order for our OLS estimator of β_1 to be unbiased. First, it requires parallel trends in the absence of the program: that is, average outcomes within treated and untreated districts would have followed a parallel path over time. Although this assumption cannot be tested directly, we provide some reassurance that this assumption is likely to hold as shown in Figure 2 and in Appendix Figure B3, that pre-treatment data does not suggest violations of the parallel trends prior to treatment. Second, TWFE assumes there are no anticipation effects. That is, we will assume that mothers residing in districts that become eligible for *Juntos* in year t do not change their corporal punishment behavior in prior waves in anticipation of treatment. In our data, there does not appear to be changes in corporal punishment behavior prior to treatment data.

The third assumption is treatment effect homogeneity. Given our staggered roll out, the consistency of the OLS estimator for β_1 in a TWFE specification requires that the treatment effect is constant between groups (in different districts) and over time (Borusyak, Jaravel and Spiess, 2021; Callaway and Sant’Anna, 2021; Sun and Abraham, 2021; De Chaisemartin and d’Haultfoeuille, 2020; Goodman-Bacon, 2021). This assumption is particularly strong. Using OLS in a TWFE specification, the $\hat{\beta}_1$ is a weighted average of potentially heterogeneous treatment effects (Borusyak, Jaravel and Spiess, 2021). However, this cannot be interpreted as the proper weighted average because, as studies have shown, some weights can be negative (Goodman-Bacon, 2021).⁹ This problem occurs when those in our data that are always treated (districts receiving *Juntos* before 2011) are used to identify period fixed effects. While this comparison leads to increased efficiency when the effect is homogeneous, it can create significant bias when there are heterogeneous and/or dynamic effects (De Chaisemartin and D’Haultfoeuille, 2022).¹⁰

To address the potential for heterogeneous treatment effects in biasing our estimators we do the following: (1) We show TWFE results without districts that are always treated in our data. Since our repeated cross-sections begin in 2010, we remove all districts that began receiving *Juntos* prior to 2011. (2) We follow De Chaisemartin and d’Haultfoeuille (2020) and calculate the weights and find that none of the (98) ATEs receive a negative

⁹Illustrative examples of why and when this negative weighting occurs can be found in Jakiela (2021) and Goodman-Bacon (2021) among others.

¹⁰However, with a large number of never-treated units or a large number of periods before any unit is treated, these negative weights will disappear (Jakiela, 2021; Borusyak, Jaravel and Spiess, 2021). In our case, over half of the districts in our sample are never treated.

weight in our sample. (3) Even when the weights are non-negative however, they may diverge from the estimand that we are interested in, so to address this we use the robust and efficient estimator in the presence of heterogeneous and dynamic treatment effects proposed by [Borusyak, Jaravel and Spiess \(2021\)](#).

The estimator proposed by [Borusyak, Jaravel and Spiess \(2021\)](#) can include covariates, district fixed effects, and a fixed effect for every province-time in a repeated cross-section setting such as ours. Intuitively, their method imputes counterfactuals for the treated units using only observations from units and time periods that are not yet-treated. Treatment effects are then calculated for each treated group which are then used in a weighted average to get the target average treatment effect. This proposed estimator is one of a series of estimators that have emerged in the last few years to address issues that arise due to heterogeneous treatment effects in staggered roll-out designs when using TWFE.¹¹

Scaling up and Instrumental Variable Approach—The estimand of the TWFE model above captures an effect at the district level, since, as seen in Figure B1(B), approximately 40% of mothers in the district receive *Juntos* when the district becomes eligible. We take two different approaches to show a more direct effect of *Juntos*. First, we use administrative data on the approximate share of mothers in each district who are receiving *Juntos*—0 for none and 1 if all mothers are receiving it.¹² We use this as an alternative explanatory variable in our TWFE specifications. Additionally, to estimate the effect of *Juntos* on mothers’ corporal punishment practices, we use a TWFE instrumental variable approach where we scale the TWFE estimands through the mediating variable of receiving *Juntos*. The DID-IV approach has been used in studies over the years yet the econometric literature on this method is sparse.¹³ Recent work by [De Chaisemartin and d’Haultfoeuille \(2018\)](#) provides a thorough discussion on the assumptions required when the treatment is fuzzy. Ours is a special case whereby the probability of receiving *Juntos* prior to the treatment and among those in the untreated group is effectively zero. In this case, [De Chaisemartin and d’Haultfoeuille \(2018\)](#) suggest that the assumptions required for identification are the same as those required for the standard DID case we discussed in the previous section.¹⁴

¹¹[Liu, Wang and Xu \(2021\)](#); [Gardner \(2022\)](#); [Wooldridge \(2021\)](#) have proposed similar estimators. Different estimators are proposed by [Callaway and Sant’Anna \(2021\)](#), and [De Chaisemartin and D’Haultfoeuille \(2022\)](#) (static setting), and [Sun and Abraham \(2021\)](#) (dynamic effects). We show results using [Callaway and Sant’Anna \(2021\)](#) and [De Chaisemartin and D’Haultfoeuille \(2022\)](#) for our main specifications in the Appendix Table A4

¹²Our census data does not identify mothers, however it identifies age groups. We use half the population count of 15-49 year olds in a district to approximate the number of mothers, since not all women are mothers, this very likely underestimates the share of mothers receiving *Juntos*.

¹³Examples include [Duflo \(2001\)](#), [Abdulkadiroğlu et al. \(2016\)](#), [Duflo \(2001\)](#), [Field \(2007\)](#), [Bleakley and Chin \(2004\)](#), and [Evans and Ringel \(1999\)](#).

¹⁴This IV approach has fewer observations (receipt of *Juntos* is not asked of all mothers in 2010-2013 of our data) and requires additional assumptions. A more detailed discussion of this can be found in Appendix D.

Akin to work in cluster randomized trials with imperfect compliance within the cluster, an assumption of no spillover effects is necessary for the IV to lead to causal estimates of the treatment. In the presence of spillovers, the estimand incorporates direct effects and spillover effects. In this case, and assuming non-positive spillovers, we view this estimate as an upper bound for the direct effect of *Juntos* (Keele and Kang, 2022).

4 Main Results

The results we present in this section suggest that cash transfer programs decrease overall punishment of children and particularly the harshest form, hitting or other physical punishment. We find some evidence that mothers may be substituting towards less violent forms of punishment such as forbidding something the child likes.

Columns (1)-(4) of Table 2 present our main results from a TWFE specification. In column (1), we show results using the entire sample. In column (2), we exclude individuals in districts that were always treated in our repeated cross-sectional panel.¹⁵ In column (3), we restrict our sample to our main study sample, which excludes always treated and districts that are observed seven or fewer times in the ENDES data across the ten-year period.¹⁶ Finally, in column (4), we add mother, children, and household-level controls.¹⁷

Panel A presents results on the extensive margin, it examines whether *Juntos* reduces any form of punishment in the last month. Across specifications, we find a reduction of approximately 2 to 3.5 percentage points on average when a district is eligible for *Juntos*. Panels B through F examine the impact on different types of punishment strategies, irrespective of if the punishment practice was implemented in the last month. The TWFE results suggest that there is a reduction in hitting (panel B). Including controls in the TWFE specification, in column (4), suggests that when a district becomes eligible for *Juntos*, there is a 3.1 percentage point reduction in reported hitting of children on average. In treated districts right before treatment, 41.3 percent of mothers report hitting their child in the last month, thus the 3.1 *pp* reduction translates to a 7% reduction. The results for other forms of punishment are less clear and differ between samples, although for slapping and verbal admonishment the estimated coefficients are negative with some statistically significant. On the other hand, forbidding something the child likes and other forms of punishment

¹⁵Our aim here is to exclude "forbidden" comparisons that may bias our results in the presence of heterogeneous effects (De Chaisemartin and D'Haultfoeuille, 2022). This excludes districts that started receiving *Juntos* early, sometime between 2005-2010, which are likely poorer on average than the rest of the sample.

¹⁶We impose this restriction to be comparable to the BJS estimating sample.

¹⁷In results not shown, to address concerns of possible correlation in the errors across our equations, we ran our TWFE specifications jointly for the dependent variables as Seemingly Unrelated Regressions and the standard errors were very similar to those in Table 2.

do not change on average.

In columns (5) and (6) of Table 2, we show results estimated using the estimator proposed by [Borusyak, Jaravel and Spiess \(2021\)](#), our preferred approach. We similarly find estimates that suggest that hitting is reduced in districts that start receiving *Juntos*. In our main specification with controls in column (6), we find a point estimate that is larger (5.2 *pp*) than that of column (4). Introduction of *Juntos* in a district results in a 13 percent reduction in hitting on average. Using this estimator gives consistently statistically significant positive estimates of changes in another, less violent, form of discipline—forbidding something the child likes. This fits with the pattern observed in Figure 1 which shows that hitting as a form of discipline declines with wealth whereas forbidding things the child likes goes up.

To explore whether the effect observed on hitting persists, and as an additional robustness check, we conduct a reduced form event study and show results in Figure 2.¹⁸ We find in both panels that prior to the onset of *Juntos* the estimated coefficients are close to zero. Once *Juntos* switches on in the district we see a decline in the probability of hitting, consistent with our regression results. The decline in the probability of hitting persists several years after the district becomes eligible for *Juntos*.

Understanding which children are impacted helps shed light on the ways in which *Juntos* affects punishment practices. Since our analysis is at the mother level, we are unable to directly differentiate by the sex of the child. To explore potentially gendered effects, in Table 3 we first show results among two subsamples: mothers with only female children (column 2) and mothers with only male children (column 3). These subsamples include mothers with one or more children. For the most severe form of punishment, hitting, the coefficients in both samples are negative, but, the coefficient estimated using the male children only subsample is larger and statistically significant. Mothers with multiple children may feel more subjected to stressors associated with poverty, and as such, we might also expect differential responses for those mothers with more than one child. We find results are similar for mothers with a single child relative to those with multiple children (columns 4 and 5), and then show results for mothers with one child by the sex of the child and again see that the estimate of the effect of *Juntos* on hitting is larger and statistically significant for subsamples of mothers with male children. These gendered results fit with others such as [Bertrand and Pan \(2013\)](#) who show that male children’s disruptive behavior is more affected by home environments; a change in the home environment due to *Juntos* can per-

¹⁸We exclude districts always treated, and our reference point is the year prior to the onset of *Juntos*. We estimate the coefficients in Panel A using a TWFE specification and those in Panel B using the estimator of [Borusyak, Jaravel and Spiess \(2021\)](#). Event study figures for other types of punishment are shown in Appendix Figure B3.

haps reduce the misbehavior of male children and thus change the severity of discipline by the parent.

These estimated effects are on average among all mothers in the district some of whom are receiving *Juntos* (direct beneficiaries) and others who are not. In other words, the estimates are averages of changes among mothers affected directly through the receipt of *Juntos*, mothers potentially affected indirectly through behavioral spill-overs, and mothers not affected. Results in columns 1 and 2 of Table A2 show estimates of the coefficient when our explanatory variable is an approximate share of mothers within a district receiving *Juntos* instead of an indicator for district eligibility. The results are noisier, however, we can use the coefficients to consider what happens when a larger share of mothers within a district are receiving *Juntos*. For example, the coefficient from Panel B in column 1 suggests that a 0.2 increase in the share of mothers within a district receiving *Juntos* decreases reported hitting by 1.7 percentage points.

The IV specification aims to capture the effect of treatment on the treated—to estimate the effect of *Juntos* receipt while accounting for potential endogeneity of *Juntos* take-up. Results are reported in columns 3 and 4 of Table A2. Similar to our main results, we find a statistically significant coefficient for hitting and that other forms of punishment do not seem to change in a statistically significant way. The estimated effect of *Juntos* receipt on hitting is around a 24 percentage point reduction. Assuming *Juntos* recipients are from the bottom quintile of wealth distribution, this is a 45% reduction in the likelihood of a mother reporting hitting her children as a form of discipline. This estimated effect is large. This magnitude could be due to spillover effects within the district and because our binary indicator variable of *Juntos* receipt could have misclassification errors, and thus the IV estimators will be biased upwards (Black, Berger and Scott, 2000). These IV results are nevertheless useful in providing an upper bound for the true effect.

4.1 Robustness

We consider several other specifications to show the robustness of our main results in Table A3. In column (1), we limit our estimating sample to districts that at some point receive *Juntos* in our sample period as these districts are more similar to one another than districts that never receive the program. In column (2), we estimate our main specifications with population weighting to take into account the variation in sampling over the ten year period of ENDES. In column (3), we add a control for the proportion of people in that district who are in the poorest income quintile, to account for poverty levels. In all cases, we continue to observe a decline in hitting and in several cases find even stronger evidence in support of substitution of punishment from more harsh methods such as hitting and

slapping to less physical discipline practices such as forbidding something the child likes.

A potential threat to identification would be any large-scale programs or policies that were rolled out at the same time as the *Juntos* expansion targeting the same households. One notable program targeting gender-based violence coincided with the *Juntos* roll-out is the expansion of state-led Women Justice Centers (WJC).¹⁹ In examining the effect of these centers on violence against women, [Sviatschi and Trako \(2023\)](#) systematically document that the expansion of the WJCs was not correlated with the *Juntos* roll-out.²⁰ Therefore, this is unlikely a confounding factor in our analysis. Nonetheless, to mitigate remaining concerns on other forms of aid, we include a control for other social protection programs as a specification check in column (4).²¹ Controlling for these additional programs effectively leaves our results unchanged.

In a series of checks we control for additional characteristics that are used by the government in targeting the *Juntos* program. In Column (5) we include a set of dummy variables for different roofing, flooring, and wall materials as well as for different drinking water source types. In column (6) we include the DHS asset index, a proxy for wealth, and in column (7) we restrict analysis to the bottom three quintiles. Our results are robust to all three specification checks.

Our results could also be influenced by individuals moving in response to *Juntos*. For instance, more vulnerable households could relocate from districts not yet eligible for *Juntos* to those eligible. This migratory behavior seems unlikely due to the community validation step in identifying eligible households. Nevertheless, we estimate our results using only women who have always lived in their current residence, a fairly restrictive constraint. Results are presented in column (8) of Table [A3](#). We estimate a negative effect on hitting, but we do lose statistical significance in the smaller sample.

Another potential concern relates to small sample sizes within some treated districts which could be driving these results.²² To address this, in Figure [B4](#), we explore the sensitivity of our results to different sample restrictions based on the number of observations per district. We first rank districts by the number of observations per year collected. Excluding always treated districts, on the left side of the figure we report the estimated treatment effect for hitting using all households. As we move to the right along the x-axis, we drop district-years with fewer observations than the indicated percentile, up to the 60th

¹⁹These centers have an explicit goal of reducing gender-based violence by providing women with access to a suite of services including legal and medical support.

²⁰The authors show instead that the placement of these centers was primarily driven by targeting urban areas with high population density.

²¹Other social programs identified in the survey include: food aid, childcare aid, scholarships, work aid, and old age pensions.

²²ENDES data are not representative at the district level.

percentile. Thus, the estimated coefficient at left of the figure includes districts with a small number of households in a year, while the right of the figure includes district-years with at least 47 households. As Figure B4 shows, the TWFE coefficient is similar across these varying sample restrictions which mitigates this concern.

4.2 Additional Results

Fathers—We also examine how fathers respond to the introduction of *Juntos*. Mothers answer the same series of questions regarding child disciplinary practices of their child(ren)’s biological father(s). While there is some correlation in behavior between biological parents, there is not a direct mapping between what women report about their own behavior and that of the father. For example, among women reporting that they do not hit their child, around 11% report that their child’s father does; and among women reporting that they do hit their child around 35% report that their child’s father does not. On the one hand, the mother might not perfectly observe the father’s disciplinary practices which might result in an underestimate; on the other hand, relative to own reporting, women may feel more comfortable reporting that someone else engages in these practices even if they under-report their own usage of harsh disciplinary practices. We estimate the effect of *Juntos* on the father’s reported disciplinary practices. These results are presented in column (1) of Table 4. Changes to the most severe punishment, hitting, is negative—aligned with our results for mothers, however, the coefficient is smaller and is not statistically significant. We do however observe a statistically significant reduction in fathers slapping children and an increase in forbidding something the child likes. We do not observe any change among fathers using other punishment practices as we did for mothers.

Age of Children—We further investigate how punishment strategies vary by the age of the children. We split our sample into mothers with children in each of the following age-groupings: under 5, between 5 and 11, and between 12 and 18. This grouping is informed by Figure 1 where we observe lower but quite rapidly increasing use of punishment for younger kids as they get older, pretty stable punishment practices for kids in the middle group, and declining punishment practices as kids get older. These groups are not mutually exclusive as a mother can have two children, for example, ages 4 and 8. In this situation, the mother would be included in the analysis of mothers with children younger than 5 and also in the sample of mothers with children ages 5-11.²³

²³ In addition to the general punishment practices elicited in the ENDES data, the survey also asks mothers child-specific questions for children under 5. Reported punishment is relatively low in this group. In results not shown, similar to our results in column 2 of Table 4, we find a negative but insignificant reduction in hitting.

Table 4 columns (2) to (4), presents the results using the [Borusyak, Jaravel and Spiess \(2021\)](#) estimator for samples restricted to households with children in the specified age ranges. The general pattern of results across the three samples and the five different types of punishment strategies we consider are broadly consistent with our main findings, but there are a few differences worth highlighting. We see a larger reduction in households with kids who are older for slapping and verbal admonishment and a greater increase in using other punishment techniques; but for the most severe punishment (hitting) we observe the largest reductions in households with children aged 5 to 11 (among whom corporal punishment is highest to begin with) where households seem to switch to forbidding things the child likes and other forms of discipline in these households.

4.3 Potential Mechanisms

There are many ways a conditional cash transfer program such as *Juntos* can change how a parent disciplines their children. A relaxation of the income constraint can relieve poverty stressors and reduce violence in the household ([Baranov et al., 2021](#); [Buller et al., 2016](#)). This reduction in stress, may give a parent the mental bandwidth to resort to different types of punishment which can require more effort to effectively change a child's behavior. Moreover, in as much as this transfer leads to better economic conditions in the household, a child who is less subject to conditions of poverty such as hunger may behave better leading to less overall or less harsh discipline by the parent ([Gennetian et al., 2016](#)). This fits with the gendered results we show in Table 3 which suggest that male children's misbehavior is more sensitive to household environments ([Bertrand and Pan, 2013](#)). Furthermore, a common way to discipline a child is with pecuniary rewards (or to restrict access to them) and better economic conditions means that a parent is more likely to be able to resort to this type of disciplinary practice ([Weinberg, 2001](#)).

There are other likely, less direct, mechanisms. A cash transfer program may change the labor supply of the mother ([Del Boca, Pronzato and Sorrenti, 2021](#); [Dona, 2023](#)). Even if the overall income level does not change, the mother being physically present in the household more or less could change the duration and quality of interactions with the child ([Cabrera-Hernández and Padilla-Romo, 2020](#)). How this would change parental discipline practices is ambiguous; a parent working less may have less external stress or could provide more consistent supervision, both of which may decrease the need for harsh punishment. On the other hand, more time in the household with the child may lead to an increase in overall discipline. Additionally, changing household characteristics may also lead to altered discipline practices: household composition either through a change of the number of adults in the household or the children (a change in fertility) ([Straus and](#)

Paschall, 2009); children’s primary care givers (for example a grandparent) (Ember and Ember, 2005); and mother’s experiences with IPV in the household (Gage and Silvestre, 2010; Li, Zhao and Yu, 2019; Skafida, Morrison and Devaney, 2022), to name a few.

Other changes may be directly related to the conditionality in *Juntos*. The two conditionalities in question are health checkups for children under 6 and regular school attendance for older children (Silva Huerta and Stampini, 2018). Changes in either of these can lead to changes in discipline practices. Parents may feel indirectly observed when their children are regularly visiting a health professional and this may make their discipline practices less physical or harsh to avoid leaving any visible marks on the child. If a child is going to school more regularly, this both increases the child’s awareness of structure and limits the amount of time that a parent and child are interacting in the household and thus may lower the amount and severity of punishment that the child receives. However, more time at school may also lead a child to learn behaviors from other children that a parent might not find acceptable.

These mechanisms are clearly endogenous in our empirical model making it difficult to conduct proper mediation analyses. However, to partially investigate whether these channels can play a meaningful role in our setting, we test whether the introduction of *Juntos* affects relevant variables in a way that suggests an important first step in the causal pathway. Specifically, we estimate the effect of *Juntos* introduction on variables that proxy several of the mechanisms we discuss above. Figure 3 presents these results.²⁴

Our results show a reduction in the child being punished, particularly using the harshest form of punishment, hitting. A child being punished is both a function of parental and child behavior, and our data does not allow us to observe the child’s behavior. We can however, explore whether *Juntos* receipt had an impact on a mother’s beliefs that “physical punishment is necessary to educate the children”. Results included in Figure 3 indicate a significant reduction in this variable after the introduction of *Juntos*. This suggests that the reductions in the use of hitting as a punishment strategy is not coming solely from changes in children’s behavior, but also from parental attitudes towards hitting.

The cash transfer could also be directly affecting the child’s behavior. If this is the case, parents may not feel the need to discipline their children anymore. Consistent with this possibility, in Panel A of Table 2 we observe a reduction in mothers’ reporting that the child received any form of punishment in the last month. This reduction, along the extensive margin, implies that our findings could be driven by parents who stop punishing their kids, or by a combination of these parents along with those who are reducing the most severe punishment strategies.

²⁴Table C2 in the Appendix displays these results in a more detailed manner.

Next, we examine whether there are changes in punishment practices in households where mothers reported that their children received any punishment in the last month. Restricting the sample to only these households, in column (5) of Table 4, we see that there is still a reduction in hitting, slapping, and verbal admonishment and an increase in forbidding something the child likes. Overall, this suggests that the cash transfer is changing the type of punishment being used by parents to correct behavior.

In Figure 3, we see a statistically significant change in mothers reporting physical punishment as necessary for the discipline of children. We also see that *Juntos* increased health checkups for younger children and school enrollment for school age children—the two variables in our data that best proxy the conditionalities of the program. Additionally, we see statistically significant changes in the availability of electricity. This suggests some improvement in economic conditions that may allow parents to substitute towards less harsh parenting when they are more likely to have things in the household they can give or take away from children. This is consistent with the increasing incidence we observe when we explore “Forbidding something the child likes” as an outcome variable (Table 2, Panel E, column 6). Outside of these three intermediate outcomes, our results are not statistically significant when using the BJS estimator. However, there are suggestive results. For example, upon the introduction of *Juntos* into a district, the share of mothers who work all year declines.

Apart from being affected by *Juntos*, potential mediating variables need to also have a direct effect on corporal punishment. The conditionality, while significant, is still small: for it to explain a large part of our observed total effect of *Juntos* on hitting, the direct effect of the conditionality on hitting needs to be a 1 to 1 reduction. In our data, a regression with many controls suggests that the conditionality is associated with a 0.028 reduction in using hitting as a form of punishment. Using methods proposed by Oster (2019), we estimate an upper magnitude bound of -0.067 on the effect of the conditionality on corporal punishment.²⁵ These numbers suggest that the conditionality can account for 5-6% of our overall estimated effect of *Juntos* on hitting.²⁶ We get a similarly small number when we consider electricity—our proxy for the change in resources, which can explain approximately 7% of

²⁵Using methods proposed by Oster (2019) and a conservative R_{max} of $2 \times \bar{R}$, we get upper magnitude bounds -0.067 on the true coefficient if unobservable selection is of equal value importance as observable selection ($|\delta| = 1$). We use this upper bound to conduct back of the envelope calculations on how much of our total *Juntos* effect these mechanisms could potentially explain.

²⁶We calculate this by multiplying 0.041 (estimated effect of *Juntos* on the conditionality—Table C2) by -0.067 (the upper bound we estimate using Oster (2019) methods of the direct effect of the conditionality to use of hitting to discipline) and dividing this by -0.052 (the overall estimated effect of *Juntos* on hitting—Table 2). Even if we assume attenuation due to measurement error in the conditionality variable, doubling its direct relation to hitting from the estimated upper bound, it would still mean it can explain only 10-11% of the overall effect.

the overall effect of *Juntos* on hitting.²⁷ However, the mediator which captures a mother’s perception that physical punishment is necessary in the education of a child can potentially explain a significant portion of the overall effect. Figure 3 shows that *Juntos* results in a 0.044 reduction in viewing physical punishment as necessary. A regression with controls shows that this mediator is strongly associated with a reduction in hitting of 0.26. Using an upper bound of 0.35 (estimated with Oster (2019) methods), this mediator can potentially explain up to 30% of the overall effect of *Juntos* on hitting.

5 Discussion and Conclusion

In this paper, we study the effect of the conditional cash transfer program *Juntos* on the discipline practices of parents. Studies in economics have focused on the effect of cash transfers on one form of intra-household violence—intimate partner violence (IPV). Meta-analyses of these studies suggest that easing the stresses of poverty leads to overall reductions in IPV. In our study, we focus on the physical punishment of other household members, children. We find that when districts become eligible for *Juntos*, average reported hitting declines, and this effect persists several years later. Our most conservative estimates suggest a 7% reduction in average hitting rates in the district. We further find that mothers may be switching towards other less violent forms of disciplinary practices, and that the results are strongest in households with male children.

Our results capture the overall effect of *Juntos*, which include both the cash transfer component and the conditionality. We explore several pathways through which the estimated impacts could occur. The strongest being a change in parental views about the need to use punishment to educate the children. Conditionality of the program, which included school enrollment and health check-ups, also matters but its importance is not as strong as the change in beliefs. Lastly, we posit that an increase in resources in the household, proxied by increased access to electricity, could be a third potential mechanism.

Future research that can directly address the underlying mechanisms would be an important next step in the literature. Moreover, we find evidence in a context where corporal punishment is quite high, and during the latter stages of the roll-out of a conditional cash transfer program where the cash was given to mothers. These impacts may vary depending on the underlying level of corporal punishment, the stage of the CCT, and the recipient in the household of the transfer. Our results further find that the conditionalities are a factor, albeit not the most important one, but schemes without conditionalities may have

²⁷This is similarly calculated by multiplying an upper magnitude bound of 0.173 reduction (direct effect of electricity on hitting—the estimated coefficient with controls is -0.014) by 0.021 (estimated effect of *Juntos* on electricity in the household) and dividing it by -0.052.

different impacts. Nevertheless, we help advance the literature in showing that outside of the documented effects of CCTs, there are additional child welfare benefits regarding the reduction of violence towards children.

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Tables and Figures

TABLE 1: Mother-level Descriptive Statistics

	Full Sample N=145,291		Study Sample N=84,237	
	Mean (1)	SD (2)	Mean (3)	SD (4)
<i>Panel A. HH Characteristics</i>				
Household size	4.639	1.626	4.608	1.622
Number of children under 5	0.604	0.695	0.577	0.683
Number adult females	1.326	0.623	1.372	0.663
Number adult males	1.170	0.755	1.207	0.791
Urban	0.746	0.435	0.924	0.265
<i>Panel B. HH Head Characteristics</i>				
HH head: age	41.915	12.285	42.568	12.473
HH head: years of schooling	9.717	4.199	10.576	3.920
HH head: married	0.833	0.373	0.818	0.386
HH head: divorced or separated	0.104	0.305	0.118	0.322
<i>Panel C. Mother's Characteristics</i>				
Mom: age	34.405	7.508	34.658	7.396
Mom: years of schooling	9.725	4.187	10.780	3.717
Mom: currently working	0.669	0.471	0.664	0.473
Mom: divorced or separated	0.136	0.342	0.151	0.358
Mom: physically punished as child	0.681	0.466	0.666	0.472
<i>Panel D. Child Characteristics</i>				
Mean age of children in the HH	7.740	3.685	7.753	3.754
Share female children	0.476	0.386	0.476	0.393
Share of children in school	0.734	0.355	0.734	0.360
<i>Panel E. Outcomes</i>				
Verbal admonishment	0.737	0.440	0.716	0.451
Forbidding something the child likes	0.489	0.500	0.562	0.496
Hitting or physical punishment	0.283	0.451	0.245	0.430
Slapping	0.132	0.339	0.149	0.356
All other punishment	0.065	0.247	0.066	0.248
<i>Panel F. Juntos Program</i>				
Juntos affiliation	0.135	0.342	0.072	0.259
Ubigeo eligible for Juntos in current year	0.291	0.454	0.190	0.393
Ubigeo that at some point had Juntos	0.766	0.423	0.737	0.440

Note: Our study sample excludes always treated districts and districts we observe 7 or fewer times across the ten year period to be comparable to the [Borusyak, Jaravel and Spiess \(2021\)](#) sample, thus the proportions reported for *Juntos* receipt, eligibility and affiliation are lower.

TABLE 2: Main Results

	TWFE			BJS Estimator		
	Full Sample (1)	Without Always Treated (2)	Study Sample (3)	Study Sample (4)	Study Sample (5)	Study Sample (6)
Panel A: Punished in the last month						
District eligible for Juntos in current year	-0.020** (0.010)	-0.033*** (0.011)	-0.035*** (0.012)	-0.024* (0.013)	-0.047** (0.023)	-0.031 (0.023)
Pre-treatment Mean	0.407	0.407	0.408	0.408	0.408	0.408
Panel B: Hit						
District eligible for Juntos in current year	-0.029** (0.013)	-0.033** (0.015)	-0.045** (0.019)	-0.031* (0.017)	-0.065*** (0.023)	-0.052** (0.022)
Pre-treatment Mean	0.418	0.418	0.413	0.413	0.413	0.413
Panel C: Slap						
District eligible for Juntos in current year	-0.001 (0.007)	-0.012 (0.008)	-0.021* (0.011)	-0.015 (0.011)	-0.033** (0.016)	-0.025 (0.016)
Pre-treatment Mean	0.111	0.111	0.116	0.116	0.116	0.116
Panel D: Verbal admonishment						
District eligible for Juntos in current year	-0.012 (0.011)	-0.014 (0.012)	-0.030* (0.017)	-0.030* (0.017)	-0.026 (0.019)	-0.027 (0.019)
Pre-treatment Mean	0.794	0.794	0.795	0.795	0.795	0.795
Panel E: Forbidding something the child likes						
District eligible for Juntos in current year	-0.005 (0.012)	-0.001 (0.014)	0.017 (0.018)	0.015 (0.018)	0.063*** (0.023)	0.063*** (0.022)
Pre-treatment Mean	0.347	0.347	0.361	0.361	0.361	0.361
Panel F: All other punishment						
District eligible for Juntos in current year	0.004 (0.006)	0.002 (0.006)	-0.001 (0.008)	0.000 (0.008)	0.014 (0.009)	0.016* (0.010)
Pre-treatment mean	0.066	0.066	0.068	0.068	0.068	0.068
District FE	✓	✓	✓	✓	✓	✓
Region Province-Year FE	✓	✓	✓	✓	✓	✓
Mother, child, and household controls						
Observations	145,291	119,739	84,237	84,237	84,237	84,237

Note: Standard errors clustered by district in parentheses. Pre-treatment mean refers to the mean in the estimating sample in district's in the year prior to *Juntos* eligibility. Mother controls include those listed in panel C of Table 1 in addition to age squared and a set of dummy variables for language and ethnicity. Household controls include those listed in panels A and B of Table 1. Child controls include those listed in panel D of Table 1. The number of observations correspond to Panel B-F. For Panel A, there is a slight reduction in the sample size at 145,201; 119,670; and 84,191 for Column 1, 2, and 3-6, respectively. TWFE refers to two-way fixed effects estimator, and BJS to the Borusyak, Jaravel and Spiess (2021) estimator.

TABLE 3: Child characteristics

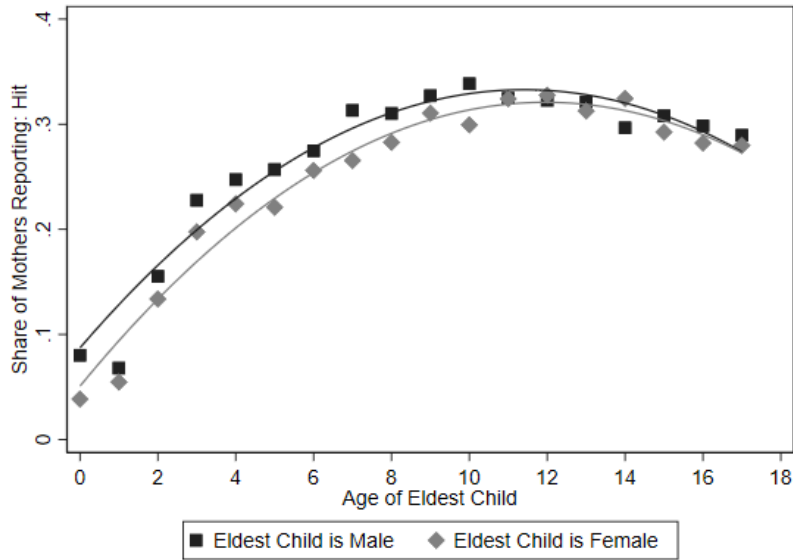
	Study Sample (1)	Only		More than one Child (4)	One Child (5)	One Child	
		Female (2)	Male (3)			Female (6)	Male (7)
Panel A: Punished in the last month							
District eligible for Juntos in current year	-0.031 (0.023)	-0.033 (0.045)	-0.056*** (0.016)	-0.021 (0.028)	-0.057 (0.040)	-0.037 (0.055)	-0.077*** (0.022)
Pre-treatment Mean	0.408	0.367	0.434	0.418	0.377	0.351	0.401
Panel B: Hit							
District eligible for Juntos in current year	-0.052** (0.022)	-0.036 (0.038)	-0.070*** (0.016)	-0.053* (0.027)	-0.054 (0.037)	0.001 (0.049)	-0.067*** (0.022)
Pre-treatment Mean	0.413	0.340	0.387	0.453	0.289	0.283	0.295
Panel C: Slap							
District eligible for Juntos in current year	-0.025 (0.016)	-0.007 (0.028)	-0.014 (0.011)	-0.051** (0.020)	0.015 (0.026)	-0.001 (0.039)	0.000 (0.014)
Pre-treatment Mean	0.116	0.141	0.119	0.107	0.142	0.154	0.131
Panel D: Verbal admonishment							
District eligible for Juntos in current year	-0.027 (0.019)	-0.074* (0.041)	-0.018 (0.013)	-0.031 (0.022)	-0.035 (0.034)	-0.070 (0.048)	-0.032* (0.018)
Pre-treatment Mean	0.795	0.800	0.795	0.792	0.802	0.802	0.801
Panel E: Forbidding something the child likes							
District eligible for Juntos in current year	0.063*** (0.022)	0.130*** (0.039)	0.105*** (0.015)	0.058** (0.028)	0.055 (0.038)	0.083 (0.053)	0.074*** (0.020)
Pre-treatment Mean	0.361	0.385	0.377	0.355	0.381	0.373	0.388
Panel F: All other punishment							
District eligible for Juntos in current year	0.016* (0.010)	0.012 (0.017)	0.014** (0.006)	0.015 (0.012)	0.010 (0.014)	0.014 (0.021)	0.027*** (0.008)
Pre-treatment Mean	0.068	0.060	0.064	0.073	0.055	0.057	0.054
District FE	✓	✓	✓	✓	✓	✓	✓
Region Province-Year FE	✓	✓	✓	✓	✓	✓	✓
Mother, child, and household controls	✓	✓	✓	✓	✓	✓	✓
Observations	84,237	21,782	28,971	59,311	24,867	11,842	14,806

Note: All results in this table use the [Borusyak, Jaravel and Spiess \(2021\)](#) estimator. Standard errors clustered by district in parentheses. Pre-treatment mean refers to the mean in the estimating sample in district's in the year prior to *Juntos* eligibility. Mother controls include those listed in panel C of Table 1 in addition to age squared and a set of dummy variables for language and ethnicity. Household controls include those listed in panels A and B of Table 1. Child controls include those listed in panel D of Table 1. All results in this table use the BJS estimator. The number of observations correspond to Panel B-F. For Panel A, there is a slight reduction in the sample size.

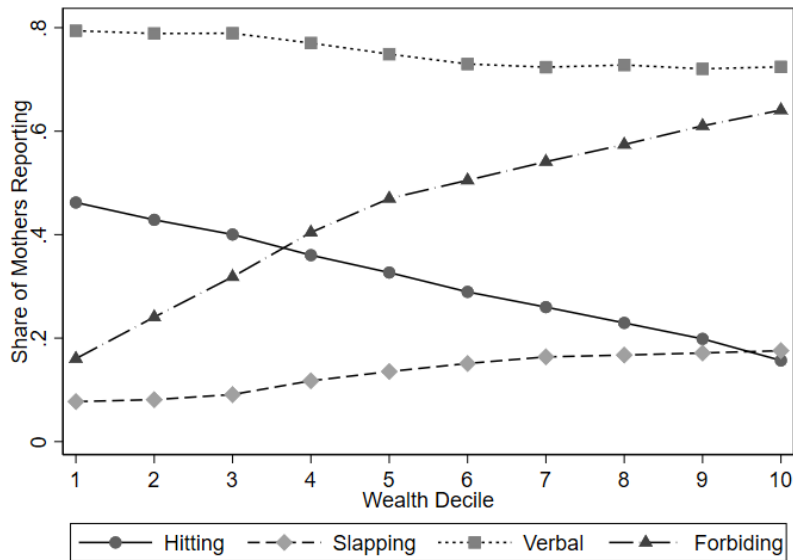
TABLE 4: Additional Results

	Biological Father BJS (1)	Children Under 5 BJS (2)	Children 5 to 11 BJS (3)	Children 12 to 18 BJS (4)	PLM Sample only TWFE (5)
<i>Estimator:</i>					
Panel A: Punished in last month					
District eligible for Juntos in current year			-0.036 (0.027)	-0.025 (0.028)	0.021 (0.027)
Pre-treatment Mean			0.416	0.370	0.432
Panel B: Hit					
District eligible for Juntos in current year	-0.011 (0.026)	-0.032 (0.027)	-0.071*** (0.027)	-0.038 (0.028)	-0.046** (0.021)
Pre-treatment Mean	0.372	0.416	0.461	0.434	0.509
Panel C: Slap					
District eligible for Juntos in current year	-0.022* (0.012)	-0.018 (0.020)	-0.045*** (0.017)	-0.075*** (0.017)	-0.030* (0.017)
Pre-treatment Mean	0.068	0.137	0.097	0.082	0.140
Panel D: Verbal admonishment					
District eligible for Juntos in current year	-0.025 (0.023)	-0.031 (0.024)	-0.020 (0.023)	-0.043* (0.025)	-0.052** (0.024)
Pre-treatment Mean	0.804	0.796	0.787	0.809	0.789
Panel E: Forbidding something the child likes					
District eligible for Juntos in current year	0.045* (0.025)	0.080*** (0.025)	0.069** (0.027)	-0.014 (0.027)	0.043* (0.023)
Pre-treatment Mean	0.304	0.351	0.365	0.346	0.393
Panel F: All other punishment					
District eligible for Juntos in current year	0.005 (0.011)	-0.009 (0.011)	0.017 (0.012)	0.027** (0.012)	0.001 (0.012)
Pre-treatment Mean	0.059	0.070	0.073	0.069	0.078
District FE	✓	✓	✓	✓	✓
Region Province-Year FE	✓	✓	✓	✓	✓
Mother, child, and household controls	✓	✓	✓	✓	✓
Observations	51,615	57,886	55,707	37,904	42,194

Note: Standard errors clustered by district in parentheses. Pre-treatment mean refers to the mean in the estimating sample in district's in the year prior to *Juntos* eligibility. Controls included are as listed in the notes to Table 2. Column 5 refers to the sample of women who punished their children in the last month (PLM). The number of observations correspond to Panel B-F. For Panel A, there is a slight reduction in the sample size.

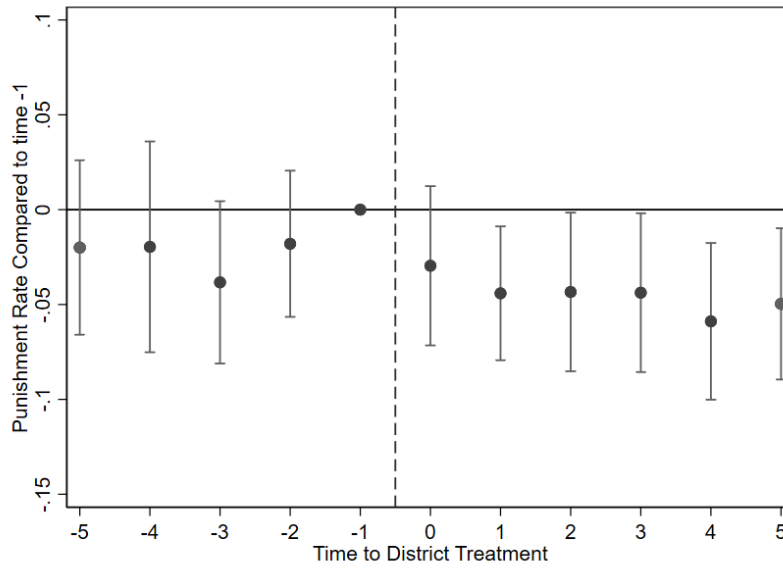


(A) Share of Mothers Reporting Hitting by Age and Sex of Eldest Child.

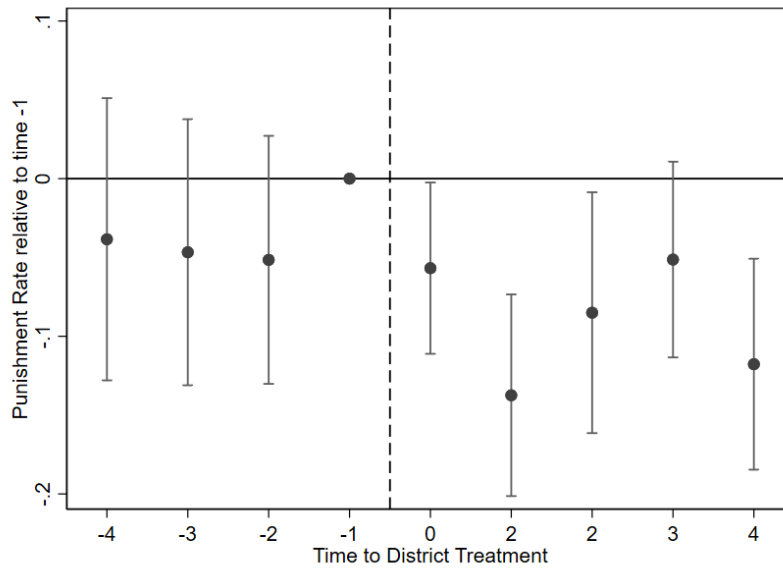


(B) Discipline Practice by Wealth.

FIGURE 1: The Figures show mothers' reported discipline practices in the whole Sample. Panel A shows that hitting peaks at age 10 and is higher among male children. Here we show by age and sex of the eldest child; similar patterns emerge when using average or median ages and/or bigger share of male female (male if equal). Panel B shows various discipline practices by wealth decile. The share of mothers reporting hitting decreases with wealth.



(A) Event Study (TWFE) for Hitting using the entire sample (excluding Always Treated).



(B) Event Study (Borusyak, Jaravel and Spiess (2021) estimator) for Hitting for Study Sample.

FIGURE 2: Event Study figures for the entire and study sample using two different econometric approaches. We can see that the effect for hitting persists.

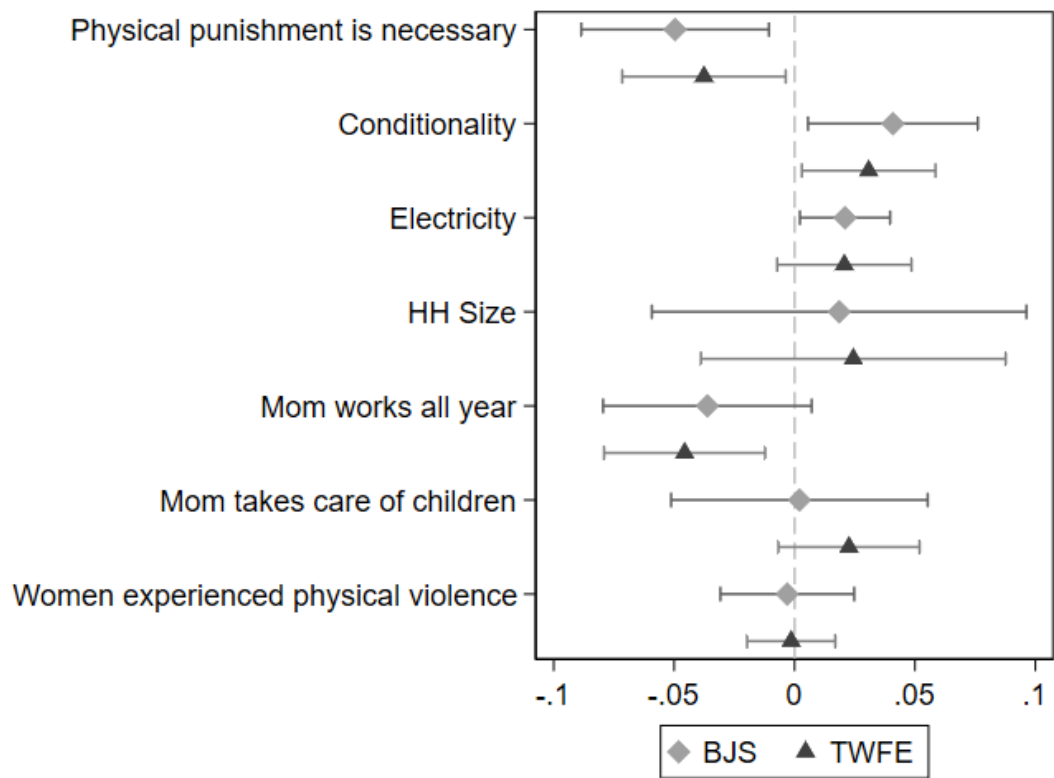


FIGURE 3: Mechanisms. Estimates presented use our main specification of equation (1) but with the mechanism as the outcome variable for both the TWFE and BJS estimators.

Appendix

A. Appendix Tables

TABLE A1: Predictors of Hitting as a Form of Punishment

	(1)	(2)	(3)	(4)	(5)
Wealth decile=2	-0.030*** (0.010)	-0.019** (0.009)	-0.010 (0.009)	-0.010 (0.008)	-0.002 (0.008)
Wealth decile=3	-0.065*** (0.011)	-0.040*** (0.009)	-0.025*** (0.009)	-0.023** (0.009)	-0.003 (0.008)
Wealth decile=4	-0.108*** (0.011)	-0.074*** (0.010)	-0.055*** (0.010)	-0.049*** (0.010)	-0.016* (0.009)
Wealth decile=5	-0.135*** (0.011)	-0.092*** (0.010)	-0.070*** (0.010)	-0.061*** (0.011)	-0.020** (0.010)
Wealth decile=6	-0.171*** (0.011)	-0.115*** (0.010)	-0.090*** (0.010)	-0.081*** (0.011)	-0.035*** (0.010)
Wealth decile=7	-0.185*** (0.011)	-0.127*** (0.011)	-0.100*** (0.011)	-0.092*** (0.012)	-0.037*** (0.011)
Wealth decile=8	-0.216*** (0.011)	-0.149*** (0.011)	-0.119*** (0.011)	-0.111*** (0.012)	-0.050*** (0.011)
Wealth decile=9	-0.250*** (0.012)	-0.170*** (0.013)	-0.137*** (0.013)	-0.131*** (0.014)	-0.066*** (0.013)
Wealth decile=10	-0.287*** (0.011)	-0.190*** (0.011)	-0.154*** (0.011)	-0.151*** (0.012)	-0.078*** (0.012)
Mom: age		0.028*** (0.002)	0.016*** (0.002)	0.017*** (0.002)	0.017*** (0.002)
Mom: age squared		-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Mom: years of schooling		-0.009*** (0.001)	-0.006*** (0.001)	-0.005*** (0.001)	-0.004*** (0.001)
Mom: currently working		0.016*** (0.003)	0.019*** (0.003)	0.016*** (0.003)	0.014*** (0.003)
Mom: divorced or separated		-0.021*** (0.006)	-0.013* (0.007)	-0.010 (0.007)	-0.009 (0.007)
Mom: wife beating justified		0.062*** (0.010)	0.064*** (0.010)	0.047*** (0.010)	0.044*** (0.010)
Mom: physically punished as child		0.144*** (0.004)	0.143*** (0.004)	0.142*** (0.004)	0.125*** (0.004)
CP necessary to educate child		0.255*** (0.008)	0.249*** (0.008)	0.249*** (0.008)	0.235*** (0.008)
Household size			0.044*** (0.002)	0.044*** (0.002)	0.045*** (0.002)
Number adult females			-0.057*** (0.003)	-0.056*** (0.003)	-0.058*** (0.003)
Number adult males			-0.048*** (0.003)	-0.048*** (0.003)	-0.049*** (0.003)
Number of children under 5			-0.026*** (0.004)	-0.031*** (0.004)	-0.030*** (0.004)
Mean age of children in the HH			-0.004*** (0.001)	-0.004*** (0.001)	-0.005*** (0.001)
Share female children			-0.024*** (0.004)	-0.023*** (0.004)	-0.024*** (0.004)
Urban				-0.025*** (0.007)	-0.006 (0.008)
Constant	0.449*** (0.009)	-0.153*** (0.030)	0.002 (0.031)	0.061* (0.032)	-0.009 (0.031)
Observations	143,388	143,388	143,388	143,388	143,387
R-squared	0.04	0.14	0.14	0.16	0.19

Note: Column 5 includes district and year fixed effects.

TABLE A2: Instrumental Variable Results

	Full Sample (1)	Study Sample (2)	Full Sample (3)	Study Sample (4)
<i>Panel A: Punished in the last month</i>				
Share of Mothers in District Receiving Juntos	-0.043 (0.033)	-0.153*** (0.056)		
Mother Receiving Juntos			-0.042 (0.054)	-0.109 (0.091)
<i>Panel B: Hit</i>				
Share of Mothers in District Receiving Juntos	-0.087** (0.036)	-0.039 (0.068)		
Mother Receiving Juntos			-0.103 (0.071)	-0.240* (0.137)
<i>Panel C: Slap</i>				
Share of Mothers in District Receiving Juntos	-0.003 (0.023)	-0.064 (0.061)		
Mother Receiving Juntos			0.009 (0.040)	-0.040 (0.089)
<i>Panel D: Verbal admonishment</i>				
Share of Mothers in District Receiving Juntos	0.005 (0.034)	-0.087 (0.097)		
Mother Receiving Juntos			-0.065 (0.061)	-0.164 (0.124)
<i>Panel E: Forbidding something the child likes</i>				
Share of Mothers in District Receiving Juntos	0.049 (0.033)	0.124 (0.086)		
Mother Receiving Juntos			-0.042 (0.064)	-0.016 (0.134)
<i>Panel F: All other punishment</i>				
Share of Mothers in District Receiving Juntos	0.003 (0.018)	0.000 (0.038)		
Mother Receiving Juntos			0.004 (0.035)	-0.070 (0.066)
First Stage F-stat			148.931	47.793
<i>District FE</i>	✓	✓	✓	✓
<i>Region Province-Year FE</i>	✓	✓	✓	✓
<i>Mother, child, and household controls</i>	✓	✓	✓	✓
Observations	144,881	83,920	129,355	73,856

Note: Standard errors clustered by district in parentheses. Controls included are as listed in the notes to Table 2. There is a reduction in the sample size because *Juntos* participation was only asked among women with children under 5 years of age for the period 2010-2012. The number of observations correspond to Panel B-F. For Panel A, there is a slight reduction in the sample size.

TABLE A3: Robustness Checks

<i>Estimator:</i>	Exclude	Never	Population	Includes	Includes	Includes	Includes	Restricted to	Never
	<i>Juntos</i>	districts	Weighted	Poverty control	Other aid	Dwelling controls	Asset index	Poor HHs	Moved
	BJS	BJS	BJS	BJS	BJS	BJS	BJS	BJS	TWFE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Panel A: Punished in last month									
District eligible for Juntos in current year	-0.035 (0.034)	-0.057** (0.025)	-0.030 (0.023)	-0.031 (0.023)	-0.027 (0.023)	-0.035 (0.023)	-0.041 (0.026)	-0.011 (0.015)	
Pre-treatment mean	0.408	0.408	0.408	0.408	0.407	0.408	0.403	0.409	
Panel B: Hit									
District eligible for Juntos in current year	-0.068** (0.030)	-0.042* (0.025)	-0.052** (0.022)	-0.052** (0.022)	-0.051** (0.022)	-0.051** (0.022)	-0.043* (0.026)	-0.017 (0.023)	
Pre-treatment mean	0.413	0.413	0.413	0.413	0.413	0.411	0.432	0.429	
Panel C: Slap									
District eligible for Juntos in current year	-0.032 (0.024)	-0.043** (0.018)	-0.025 (0.016)	-0.025 (0.016)	-0.022 (0.016)	-0.024 (0.016)	-0.040** (0.019)	-0.025** (0.012)	
Pre-treatment mean	0.116	0.116	0.116	0.116	0.115	0.117	0.114	0.117	
Panel D: Verbal admonishment									
District eligible for Juntos in current year	-0.039* (0.023)	-0.064*** (0.023)	-0.027 (0.019)	-0.027 (0.019)	-0.026 (0.019)	-0.031 (0.019)	-0.026 (0.022)	-0.057** (0.022)	
Pre-treatment mean	0.795	0.795	0.795	0.795	0.796	0.795	0.800	0.795	
Panel E: Forbidding something the child likes									
District eligible for Juntos in current year	0.044 (0.033)	0.070*** (0.025)	0.065*** (0.022)	0.063*** (0.022)	0.064*** (0.023)	0.065*** (0.022)	0.051** (0.025)	0.002 (0.018)	
Pre-treatment mean	0.361	0.361	0.361	0.361	0.361	0.364	0.327	0.366	
Panel F: All other punishment									
District eligible for Juntos in current year	0.018 (0.012)	0.023** (0.010)	0.015 (0.010)	0.016* (0.010)	0.016 (0.010)	0.018* (0.010)	0.009 (0.010)	0.000 (0.009)	
Pre-treatment mean	0.068	0.068	0.068	0.068	0.068	0.069	0.069	0.071	
District FE	✓	✓	✓	✓	✓	✓	✓	✓	
Region Province-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	
Mother, child, and household controls	✓	✓	✓	✓	✓	✓	✓	✓	
Observations	50,978	84,237	84,237	84,237	81,623	83,901	50,773	46,747	

Note: Standard errors clustered by district in parentheses. Pre-treatment mean refers to the mean in the estimating sample in district's in the year prior to *Juntos* eligibility. Controls included are as listed in the notes to Table 2. Refer to section 4.1 for an explanation of the specific additional controls included in each column. The number of observations correspond to Panel B-F. For Panel A, there is a slight reduction in the sample size.

TABLE A4: Other Estimators

<i>Estimator</i>	Panel Collapsed		D&D (3)
	BJS (1)	C&S (2)	
<i>Panel A: Punished in last month</i>			
District eligible for <i>Juntos</i> in current year	-0.005 (0.021)	-0.040 (0.047)	0.010 (0.036)
<i>Panel B: Hit</i>			
District eligible for <i>Juntos</i> in current year	-0.108*** (0.024)	-0.050 (0.038)	-0.077** (0.036)
<i>Panel C: Slap</i>			
District eligible for <i>Juntos</i> in current year	-0.031* (0.017)	-0.038 (0.027)	-0.011 (0.015)
<i>Panel D: Verbal admonishment</i>			
District eligible for <i>Juntos</i> in current year	-0.067*** (0.014)	-0.068** (0.032)	-0.012 (0.022)
<i>Panel E: Forbidding something the child likes</i>			
District eligible for <i>Juntos</i> in current year	0.110*** (0.033)	-0.003 (0.042)	0.032 (0.035)
<i>Panel F: All other punishment</i>			
District eligible for <i>Juntos</i> in current year	0.015** (0.007)	0.017 (0.013)	0.034 (0.015)
<i>District FE</i>	✓	✓	✓
<i>Region Province-Year FE</i>	✓	✓	✓
Observations	1,360	1,360	53,344

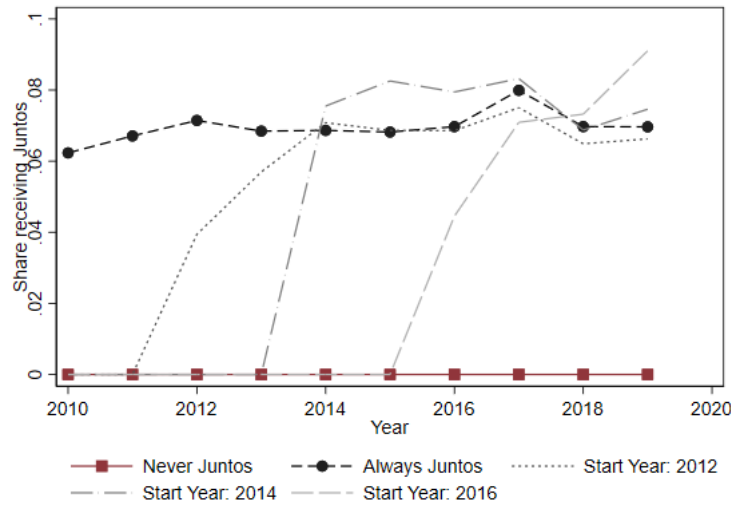
Note: Estimator in column 2 corresponds to that developed by Callaway and Sant’Anna (C&S) and in column 3 by DeChaisemartin and D’Haltfoeuille (D&D). The number of observations correspond to Panel B-F. For Panel A, there is a slight reduction in the sample size.

B. Appendix Figures

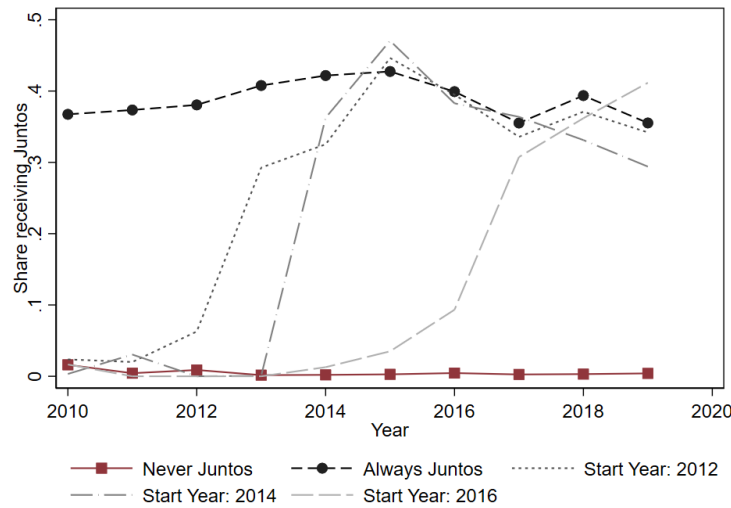
TABLE B1: Long-term Associations with Corporal Punishment: Mother

Dependent Variable:	Education	Wife beating justified	Currently hit kids	CP necessary
	(1)	(2)	(3)	(4)
Mom: physically punished as child	-0.357*** (0.028)	0.002** (0.001)	0.197*** (0.003)	0.104*** (0.004)
Pre-treatment mean	8.438	0.048	0.561	0.365
<i>Age & Age Squared</i>	✓	✓	✓	✓
<i>Language & Ethnicity FE</i>	✓	✓	✓	✓
<i>District FE</i>	✓	✓	✓	✓
<i>Region Province-Year FE</i>	✓	✓	✓	✓
Observations	145,291	145,291	145,291	145,291

Note: Corporal punishment among mothers when they were children is associated with fewer years of attained education, more likely to approve of wife beating, more likely to currently use corporal punishment and agree that it is necessary to discipline children.

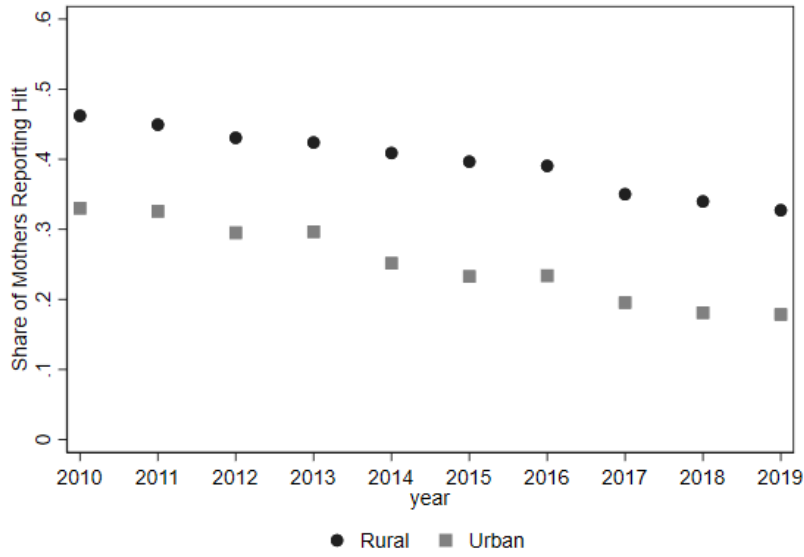


(A) Average Share (within district) of Individuals Receiving *Juntos* by start year of district-level *Juntos* Eligibility (Census data).

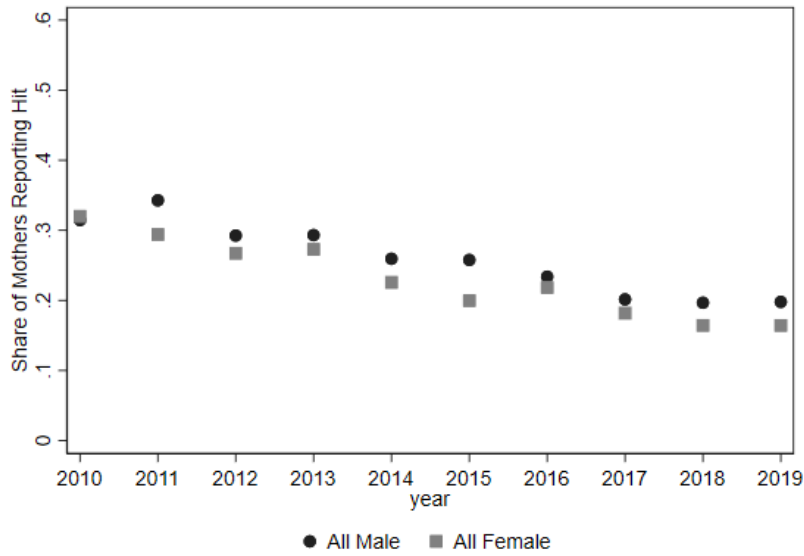


(B) Average Share (within district) of Mothers Reporting Receiving *Juntos* by start year of district-level *Juntos* Eligibility.

FIGURE B1: These figures show the average share of poor mothers with children who are eligible for *Juntos* within each district based on the onset year for *Juntos* at the district. Figure (A) uses census district population counts; Figure (B) uses our survey data on mothers reporting receiving *Juntos*. The difference in the shares is because in (A) we divide the number of eligible mothers by the entire population of the district, whereas in (B) we divide by number of mothers with children in our survey. Non-zero *Juntos* shares prior to the eligibility of the district seen in (B) could be explained by mothers who have recently moved. Slower increases on the onset year seen in (B) could be due to the survey interviews taking place throughout the calendar year.



(A) Urban/Rural.



(B) Male/Female

FIGURE B2: There is a steady decreasing trend in hitting over time in both urban and rural areas and across child sex.

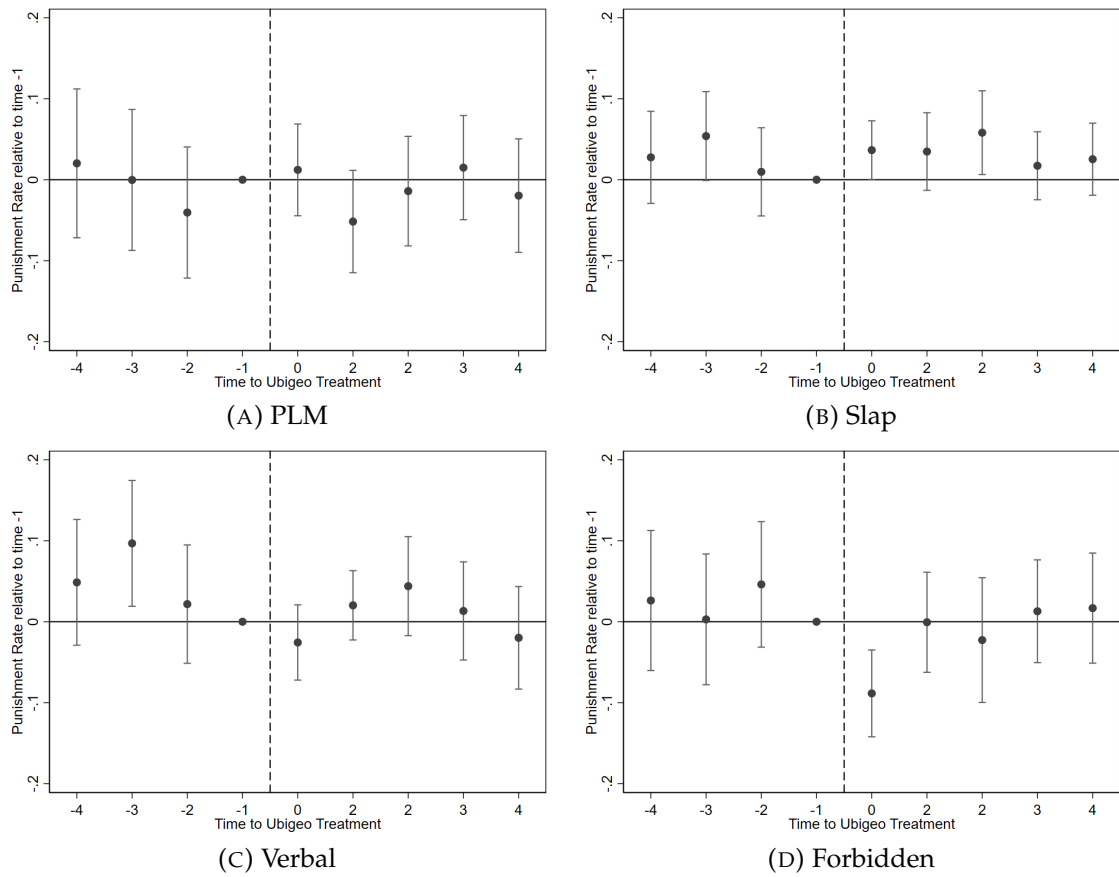


FIGURE B3: Event Study Figures using [Borusyak, Jaravel and Spiess \(2021\)](#) estimator for “punished last month” (PLM) and other measures of punishment.

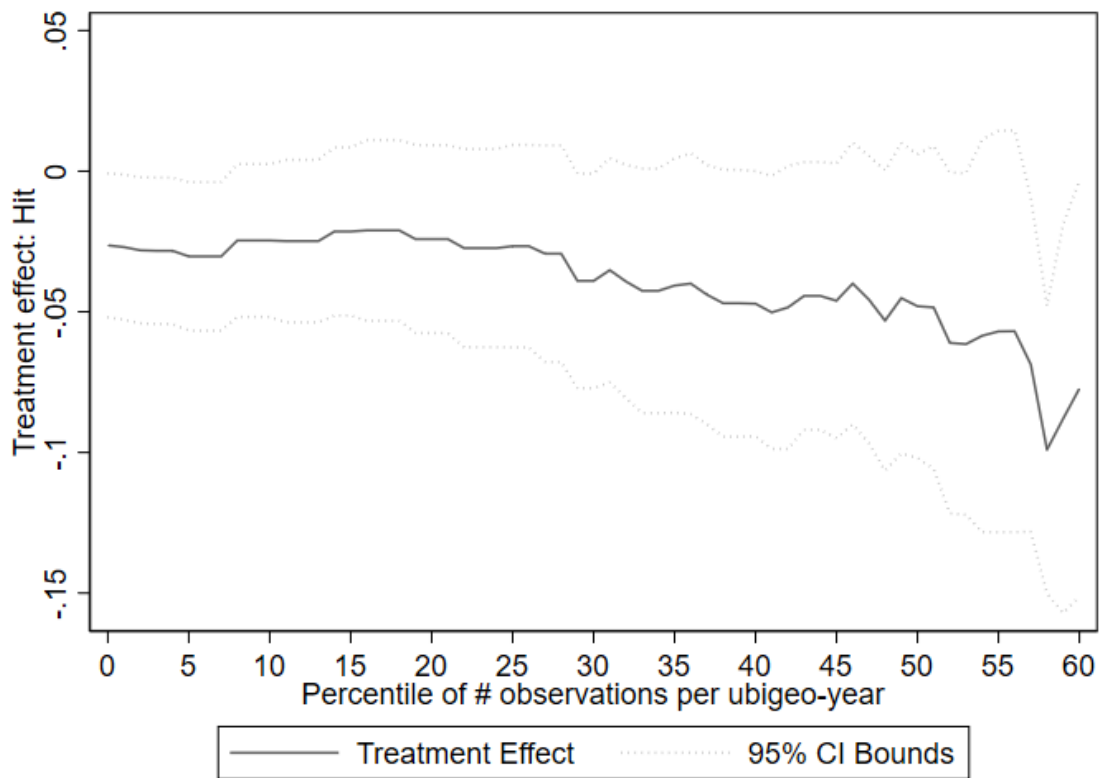


FIGURE B4: Treatment effect using TWFE estimator restricting the sample to all districts observed above the threshold percentile cutoff.

C. Appendix: *Juntos* Identification and Targeting

Juntos identification of beneficiaries and targeting occurs in three stages: first geographical targeting identifying eligible districts; then household targeting using a poverty index score; and lastly community validation of potential beneficiaries (Jones, Vargas and Villar, 2008; Silva Huerta and Stampini, 2018).

The geographical targeting of districts uses a formula that considers different district measures of poverty, chronic malnutrition of 6-9 year-old children, and the percentage of the population affected by political violence (Carpio et al., 2019). District-level targeting was used, but at times due to isolation of some very poor areas, regions with a high concentration of poor districts were prioritized and regions with fewer than 7 qualifying districts selected were excluded.²⁸ Once a district enters the program, it remains eligible (Carpio et al., 2019). The selection criteria for the districts was expanded over time to include more measures of poverty. At each expansion point, the poorest districts not previously enrolled were selected (Carpio et al., 2019). In 2011 the district eligibility criteria were revised to include identification of health and education establishments in the district (Carpio et al., 2019). From 2012 to 2014, the selection rule was changed again and households in rural areas and the jungle were prioritized. After 2015, there was no prioritization since most regions were covered by *Juntos* (Carpio et al., 2019). By 2019, *Juntos* covered over 747,000 households in 1,325 of Peru's 1,943 districts (Carpio et al., 2019; Jones, Vargas and Villar, 2008; MIDIS, 2019).

In Table C1 whether these targeting criteria are borne out by our data. We examine the characteristics of mothers resident in three types of districts: districts that became eligible for *Juntos* prior to 2011, districts that became eligible for *Juntos* during our sample period of 2011 to 2019, and districts that are never eligible within our period of study. Districts selected earlier on for receiving the *Juntos* program are much poorer and more rural. Districts that become eligible later are more likely to be rural and on average poorer than districts that remain ineligible, but are better off than initially enrolled districts. This pattern is consistent with the geographical targeting.

Once eligible districts have been identified, household identification was achieved by using a poverty index score constructed with information from a socio-demographic questionnaire known as SISFOH (*Sistema de Focalización de Hogares*).²⁹ This index has changed over time, in 2012 a new poverty score was adopted (Díaz and Saldarriaga, 2021). Households with pregnant women or eligible children and a score above a threshold value

²⁸Perova and Vakis (2012) indicate that the initial plan of the government was to roll-out the program from the most needy to the less disadvantaged districts, but that "random events –such as adverse weather conditions–" prevented them from adhering to this planned order (page 56).

²⁹<https://www.gob.pe/9242>

are eligible to participate in the program. Eligibility is valid for 3 years (Silva Huerta and Stampini, 2018). Program guidelines indicate that compliance is checked every two months. Households could no longer receive the program if they frequently do not comply with the guidelines, no longer meet the eligibility conditions, or decide to dropout (Díaz and Saldarriaga, 2021).

Lastly, a communal validation assembly is in charge of validating in public consultation that the household has at least one member of the target population, with a valid ID card, and that the members have lived in the district for more than six months at the time of enrollment³⁰. Further changes on the eligibility criteria were made after our period of study, for a detailed description of the Program see Silva Huerta and Stampini (2018).

³⁰Source: <https://dds.cepal.org/bpsnc/programme?id=29>

TABLE C1: Mother-level Descriptive Statistics by Type of District

	Always <i>Juntos</i> Districts N=25,550		Sometimes <i>Juntos</i> Districts N=7,558		Never <i>Juntos</i> Districts N=76,679	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)
<i>Panel A. HH Characteristics</i>						
Household size	4.787	1.637	4.762	1.707	4.599	1.616
Number of children under 5	0.686	0.734	0.641	0.715	0.573	0.681
Number adult females	1.195	0.473	1.307	0.599	1.376	0.667
Number adult males	1.047	0.620	1.187	0.750	1.208	0.794
Urban	0.168	0.374	0.703	0.457	0.938	0.241
Share district in quintile 1	0.693	0.297	0.276	0.237	0.042	0.110
<i>Panel B. HH Head Characteristics</i>						
HH head: age	39.909	11.291	41.299	12.013	42.646	12.497
HH head: years of schooling	6.837	4.060	8.722	4.116	10.690	3.879
HH head: married	0.873	0.333	0.846	0.361	0.816	0.387
HH head: divorced or separated	0.061	0.240	0.100	0.300	0.119	0.324
<i>Panel C. Mother's Characteristics</i>						
Mom: age	33.926	7.742	33.675	7.607	34.718	7.378
Mom: years of schooling	6.029	4.026	8.605	4.061	10.914	3.653
Mom: currently working	0.703	0.457	0.626	0.484	0.666	0.472
Mom: divorced or separated	0.089	0.285	0.123	0.328	0.153	0.360
Mom: wife beating justified	0.057	0.232	0.025	0.157	0.021	0.143
Mom: physically punished as child	0.720	0.449	0.689	0.463	0.664	0.472
<i>Panel D. Child Characteristics</i>						
Mean age of children in the HH	7.793	3.447	7.692	3.585	7.756	3.764
Share female children	0.482	0.361	0.462	0.378	0.477	0.394
Share of children in school	0.744	0.333	0.701	0.367	0.736	0.359
<i>Panel E. Outcomes</i>						
Verbal admonishment	0.784	0.411	0.723	0.448	0.716	0.451
Forbidding something the child likes	0.227	0.419	0.440	0.496	0.570	0.495
Hitting or physical punishment	0.416	0.493	0.314	0.464	0.240	0.427
Slapping	0.076	0.265	0.118	0.322	0.151	0.358
All other punishment	0.062	0.242	0.072	0.259	0.065	0.247

Note: Always *Juntos* district includes all mothers in all years for districts that became eligible for *Juntos* prior to 2011. Sometimes *Juntos* district includes all mothers in all years for districts that become eligible for *Juntos* during our period of observation, 2011 - 2019. Never *Juntos* district includes all mothers in all years for districts that are never eligible for *Juntos* district during our period of study.

TABLE C2: Potential Mechanisms

	Full Sample (1)	Study Sample (2)	BJS (3)
<i>Panel A: Hit</i>			
District eligible for Juntos in current year	-0.022* (0.012)	-0.031* (0.017)	-0.052** (0.022)
Pre-treatment Mean	0.418	0.413	0.413
<i>Panel B: Physical Punishment is necessary</i>			
District eligible for Juntos in current year	-0.020 (0.012)	-0.033* (0.017)	-0.044** (0.020)
Pre-treatment Mean	0.365	0.377	0.377
<i>Panel C: Conditionality</i>			
District eligible for Juntos in current year	0.029*** (0.011)	0.031** (0.014)	0.041** (0.018)
Pre-treatment Mean	0.693	0.683	0.683
<i>Panel D: Electricity</i>			
District eligible for Juntos in current year	-0.007 (0.015)	0.021 (0.014)	0.021** (0.010)
Pre-treatment Mean	0.849	0.858	0.858
<i>Panel E: Household size</i>			
District eligible for Juntos in current year	-0.068* (0.035)	-0.099** (0.049)	-0.061 (0.064)
Pre-treatment Mean	4.961	4.968	4.968
<i>Panel F: Mom works all year</i>			
District eligible for Juntos in current year	-0.020 (0.012)	-0.046*** (0.017)	-0.036 (0.022)
Pre-treatment Mean	0.473	0.475	0.475
<i>Panel G: Mom takes care of children</i>			
District eligible for Juntos in current year	-0.007 (0.012)	0.023 (0.015)	0.002 (0.027)
Pre-treatment Mean	0.404	0.411	0.411
<i>Panel H: Women experienced physical violence</i>			
District eligible for Juntos in current year	-0.004 (0.010)	-0.001 (0.014)	-0.008 (0.022)
Pre-treatment Mean	0.300	0.304	0.304
<i>District FE</i>	✓	✓	✓
<i>Region Province-Year FE</i>	✓	✓	✓
<i>Mother, child, and household controls</i>	✓	✓	✓
Observations	145,291	84,237	84,237

Note: Standard errors clustered by district in parentheses. Pre-treatment mean refers to the mean in the estimating sample in district's in the year prior to *Juntos* eligibility. Controls included vary by outcome. Outcome variables are listed in the panel name. Sample sizes vary depending on the outcome variable and controls included.

D. Appendix: Instrumental Variables Approach

The estimand of the TWFE model above captures the average treatment effect on corporal punishment among mothers when their district of residence becomes eligible for *Juntos*. To obtain the effect of *Juntos* on mothers' corporal punishment practices, we use a TWFE instrumental variable approach where we scale the TWFE estimands through the mediating variable of receiving *Juntos* at the mother level.³¹

The DDIV coefficient comes from the following instrumental variable setup:

$$P_{idpt} = \delta_0 + \delta_1 \hat{Juntos}_{idpt} + X'_{idpt} \Theta + \gamma_d + \sigma_{pt} + e_{idpt} \quad (2)$$

$$Juntos_{idpt} = \alpha_0 + \alpha_1 UbiJuntos_{dpt} + X'_{idpt} \Phi + \eta_d + \theta_{pt} + u_{idpt} \quad (3)$$

Our instrumental variable setup corresponds closely to equation (1), however we introduce $Juntos_{idpt}$ here as an indicator variable if the mother is receiving *Juntos*.³² We use the district's eligibility for *Juntos* as an instrument for *Juntos* receipt at the mother level, as noted in the first stage equation (3) above. Our parameter of interest is the Local Average Treatment Effect of treatment group (residents of districts receiving *Juntos*) switchers, i.e. those in treated districts who go from non-treatment to treatment when the district becomes eligible for *Juntos*.³³ With caveats, this LATE is captured by δ_1 .

The DID-IV approach has been used in different papers over the years with both two-period difference-in-differences and TWFE with multiple periods and groups (Abdulka-diroğlu et al., 2016; Duflo, 2001; Field, 2007; Bleakley and Chin, 2004; Evans and Ringel, 1999). The literature on this method is sparse but recent work by De Chaisemartin and d'Haultfoeuille (2018) provides a thorough discussion on the assumptions required when the treatment is fuzzy—in our case being in the eligible district results in a sizable increase in the probability of receiving *Juntos*. Ours is a special case whereby the probability of receiving *Juntos* prior to the treatment is zero, in addition, the probability of *Juntos* receipt in the untreated group is effectively zero. As seen in Figure B1, the probability of *Juntos* receipt among individuals in treated districts is close to zero prior to the district becoming eligible, and it sharply increases to around 40% after. In this special case, De Chaisemartin and d'Haultfoeuille (2018) suggest that the assumptions required for identification are the same as those required to standard DID we discuss in the previous section.

Our empirical approach requires additional assumptions: (1) *Exclusion*—We assume

³¹A canonical example of this approach is (Duflo, 2001) measuring the impact of schooling on adult labor market outcomes through school construction.

³²As indicated in Section 3, this question was only asked of women with children under 5 before 2013.

³³Our data are repeated cross-sections, thus we do not observe the same households before and after switching.

the only way the instrument (district eligibility for *Juntos*) affects child punishment outcomes is through the receipt of *Juntos*. This rules out spillover effects of behavioral changes. (2) *Common trends*—This is a standard assumption in difference in difference empirical designs. Conditional potential paths of outcomes and treatment are independent of instrument assignment. In our setting, mothers are not treated prior to district eligibility and thus the pre-treatment paths of the treatment are the same. We show evidence of parallel trends in our event study application in Section 4.1. (3) *Monotonicity*—this assumption constrains the effect of the instrument on treatment to be monotone and in our case positive. This condition is satisfied in our setting as the probability of receiving *Juntos* increases when the district becomes eligible while the probability of receiving *Juntos* in non-eligible districts is near zero and remains constant over time.³⁴ (4) Finally, we need to assume that the treatment effects are stable over time.³⁵

³⁴This assumption implies that Assumptions 1,2, and 3 in De Chaisemartin and d’Haultfoeuille (2018) hold.

³⁵In De Chaisemartin and d’Haultfoeuille (2018), one additional assumption is proposed: homogeneous treatment effect; switchers in both groups have same LATE conditional on our controls. However, this assumption is not required if the control group has a stable percentage of treated units.