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Poverty and parental discipline☆

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ABSTRACT

Across and within countries, there are large differences in how parents discipline their children, and frequently, 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 its introduction changes parental discipline practices. We find that in districts that begin to receive the program, the average level of reported physical punishment by mothers and fathers among the poor declines by at least 2.7 percentage points (11%) driven by reductions in slapping. Our findings suggest that program participation may have additional second-order benefits through the reduction of harsh physical forms of discipline practices.

1. Introduction

In 2021, the United Nations Children's Fund (UNICEF) reported that in many countries, more than two in three children are subjected to violent discipline by caregivers (United Nations Children's Fund, 2021). Physical or corporal punishment—generally defined as non-injurious hitting or slapping 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 methods of disciplining children, and intra-household violence more generally, differ across the world and are evolving over time, the general pattern suggests that across and within countries, poverty is associated with higher levels of physical punishment (Fréchette and Romano, 2015; Gershoff and Grogan-Kaylor, 2016; Paolucci and Violato, 2004). Poverty may influence the use of these harsher forms of discipline 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 use data from Peru to first show that parents in poor households are significantly more likely to use physical punishment as a disciplinary practice. We then leverage the roll-out of a large-scale conditional cash transfer (CCT) program (*Juntos*) across time and space to study its effects on child disciplining practices of parents, showing that it reduces the use of physical punishment by both mothers and fathers. *Juntos*, Peru's main poverty alleviation program, started in 2005 and was expanded yearly through 2017. It benefited almost 700,000 households annually during the latter years of our study period. We link yearly administrative data on the number of *Juntos* beneficiaries at the district level with ten years of cross-sectional survey data that includes information on how parents discipline their children. In our primary specifications, we use the difference-in-differences estimator proposed by Borusyak et al. (2024) (BJS) which is robust and efficient in the presence of heterogeneous and dynamic treatment effects when there is staggered roll-out. We find that the introduction of *Juntos* in a district results in lower rates of recent punishment of children of at least 8%. This decline is driven by reductions in average rates of slapping, once the district becomes eligible for *Juntos*, the use of slapping declines by 2 and 1.7 percentage points among mothers and fathers respectively.

Our results represent the overall effect of *Juntos* at the district level capturing both the direct effects on mothers receiving the cash transfer and mothers who do not receive the transfer within the district. Furthermore, we show results that differentiate between the poor and

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non-poor households in the district. First, we show heterogeneity by poverty status for our main specification. We find that the main reductions in slapping are driven by poorer households in districts that begin to receive *Juntos*. Second, we show triple difference-in-differences results that interact the onset of *Juntos* with poorer household status, again showing that this change is being driven by poorer households. Third, we scale our results to account for the share of mothers receiving the transfer and find that for every one standard deviation increase in the share of mothers treated (approximately 0.128), reported use of slapping by the mother declines by about one percentage point. We also conduct a series of robustness checks and show that our results do not change in a substantive way if we focus on different sub-samples or time periods, or if we use population weighting, or conduct individual-level analysis that includes an extensive set of individual and household controls.

Understanding which children are most affected can be valuable in identifying which policies are more effective in reducing physical punishment. At the extensive margin, we find that male children show a larger reduction in punishment particularly when there are only male children in the household. Among mothers, we find that the estimated reductions in slapping are only statistically significant for male children. Among fathers, we find larger reductions in slapping for male children when *Juntos* is introduced. Together, these results suggest gendered results for both children and parents.

Our findings suggest that the reduction in the use of physical punishment practices is driven by both reductions on the extensive and intensive margins; we find that any punishment in the last month is reduced and, conditional on reporting punishment in the last month, we find that poor parents who are likely targeted by *Juntos* reduce slapping. Our results capture the overall effect of *Juntos*, which may operate through the cash transfer component, conditionalities, or other changes induced by the cash transfer such as household composition or the presence of mothers within the home. We investigate these potential mechanisms through which *Juntos* receipt may result in lower rates of physical punishment. We do so through causal mediation analysis and find suggestive evidence that the reduction in the use of slapping can be partially explained through the main mechanisms which are increased economic resources in the household, and the health conditionality which led to an increase in health check-ups.

Our findings are consistent with theoretical models of parent–child interactions in which parents can use rewards to shape children's behavior (Becker, 1974, 1991; Hao et al., 2008). Economists have also sought to theoretically explain differences in parenting practices across different socio-economic environments. For example, 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 physical punishment.[1] Our findings support these models, specifically, in showing how relaxing income constraints helps parents reduce harsh physical forms of punishment.[2] Beyond these theoretical models, the closest empirical work related to ours shows that tax benefits to low-income households in the US reduce extreme outcomes such as child maltreatment proxied by referrals to child protective services and placement in foster care (Rittenhouse, 2023). Unlike this work, our results focus on more common discipline practices, and we show that a conditional cash transfer can move parents away from physical punishment.

Our results also 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 (Devries et al., 2013). Scholars have theorized that poverty-related stressors could increase IPV and thus poverty-alleviation programs that ease these stressors could decrease IPV (Ellsberg et al., 2015; Vyas and Watts, 2009; Fox et al., 2002; Sánchez et al., 2020). Recent studies and 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 et al., 2016; Buller et al., 2018; Gibbs et al., 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. Here, we explore whether cash transfers reduce physical punishment of children and find effect sizes that are approximately half to two-thirds of the average percent reductions reported in meta studies of the effect of cash transfers on IPV.

Researchers have also sought to understand how different home environments and parent–child interactions contribute to the development of non-cognitive skills (Spera, 2005), which are important determinants of a range of later-in-life outcomes (Heckman and Kautz, 2012; Heckman and Mosso, 2014). Doepke et al. (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 work by Carneiro et al. (2024) has sought to understand what types of parenting interventions matter and through which mechanisms. Some of this research highlights the importance of parental instruction interventions as effective ways to reduce parents' use of harsh methods such as physical punishment (Kliem et al., 2015; García and Heckman, 2023; Diaz et al., 2023). Some work attempts to investigate the long-term consequences of physical punishment, however, this is empirically challenging as it is fraught with endogeneity difficulties. For example, Currie and Tekin (2012) find important negative and long-term effects of abuse and neglect in the household, while (Bald et al., 2022) show heterogeneous but positive effects of early removal from these households. 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.

Our paper provides some of the first evidence on the effect of a conditional cash transfer program on parental use of physical punishment. Our findings complement existing work by showing that conditional cash transfer programs that are common around the world, can also play a role in facilitating changes to parental disciplinary practices. In our context, through the *Juntos* program, parents appear to reduce their use of harsh physical disciplinary practices. Given the long-term negative consequences of physical punishment documented in the literature, our results suggest an additional benefit and potential mechanism through which poverty alleviation programs affect children in the long run.

The rest of the paper is structured as follows: in Section 2, we describe the *Juntos* program and the data we use in this analysis. In Section 3, we discuss the econometric approaches 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 mothers, pregnant women, and households with children up to 19 years

[1] Doepke and Zilibotti (2017) set up a model where more authoritarian parenting, which includes the use of physical punishment, emerges as an equilibrium outcome when social and occupational mobility are low.

[2] An alternative approach to modeling parent–child interactions relies on incorporating an informational friction. For example, Akabayashi (2006) models how the inability of parents to perfectly observe a child's effort can lead to an equilibrium with child maltreatment.

of age. In addition to providing income support, the goal of the program is to increase human capital among children by conditioning on school enrollment (among children ages 6–18) and health center visits for growth check-ups (ages 0–5) (Díaz and Saldarriaga, 2019). Participation is voluntary but, based on administrative records, 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 et al., 2020; Silva Huerta and Stampini, 2018; Andersen et al., 2015). The transfer is typically paid to mothers and is conditional on children under 59 months receiving comprehensive health and nutrition care, school age children attending school, and having a national identity card (Sánchez et al., 2020). 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 et al., 2008; Silva Huerta and Stampini, 2018).[3]

Consistent with the targeting criteria, households in *Juntos* districts tend to be poorer and more likely to be rural. In Appendix B (Table B1) we examine the characteristics of mothers living 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 were never eligible during our study period. Districts selected earlier for receiving the *Juntos* program have a higher proportion of households in the lowest wealth quintile and the highest proportion of households in rural areas. Districts that become eligible later, are more likely to be rural and on average poorer than districts that remained ineligible by the end of our study period, but are better off than initially enrolled districts. This pattern is consistent with the geographical targeting described in more detail in Appendix A.

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). 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 find that the educational gains are more nuanced, as early life exposure to *Juntos*, particularly during the first four years of life, leads to cognitive and nutritional improvements for children (Sánchez et al., 2020). There are many ways *Juntos* can change how a parent disciplines their children. With some limitations due to data constraints, we investigate how *Juntos* affects these potential mechanisms in Section 4.3 of the paper.

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 an extension of the Peruvian continuous Demographic and Health Surveys (DHS) series. We use the annual surveys conducted from 2010 to 2019 in our analysis because they include questions on parental discipline 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 health information.

One woman in each household age 18 or older was selected to participate in the domestic violence module with a section on child discipline. Specifically, biological mothers with children 18 years or younger in the home are asked about 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.[4]

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 that the typical module used in the DHS to measure intimate partner violence (IPV) 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 subgroups. While IPV is distinct from physical punishment of a child, we might expect similar concerns with the under-reporting of IPV, thus this validation study also assuages concerns in our study.

In our main results, 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.[5] In addition, for our scaling measure, we use information from ENDES on household *Juntos* receipt. 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.

2.3. Descriptive statistics

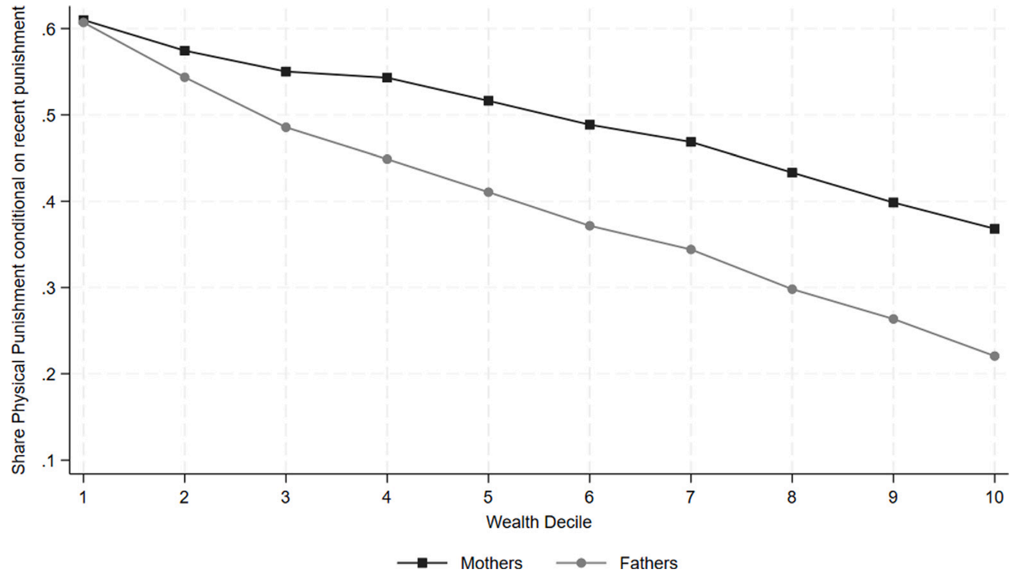
In Appendix Table B2 we show summary statistics for the full sample and our study sample. The study sample is restricted to respondents in district-years that are used in the analysis, which excludes always treated districts and districts for which the (Borusyak et al., 2024) estimator we use, does not have a comparison. In the full sample, two thirds of mothers live in urban areas and live in households that have on average 4.7 members. Mothers are just over 33 years old on average and have about 9 years of schooling. A majority of them are working (65%) and 11% are divorced or separated. The average age of their children is nearly 7 years.

Approximately 44% of mothers' report any punishment of their child(ren) in the last month (Panel E). We then list the physical forms of discipline, conditional on the children being punished in the last month, indicated by the mothers. The most common form of physical punishment, which is also the most violent, is *hitting* (15%), followed by *slapping* with 8%. About one-fifth of mothers use at least one of these forms of physical punishment. Physical forms of punishment used by fathers (as reported by mothers) show a similar pattern. 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

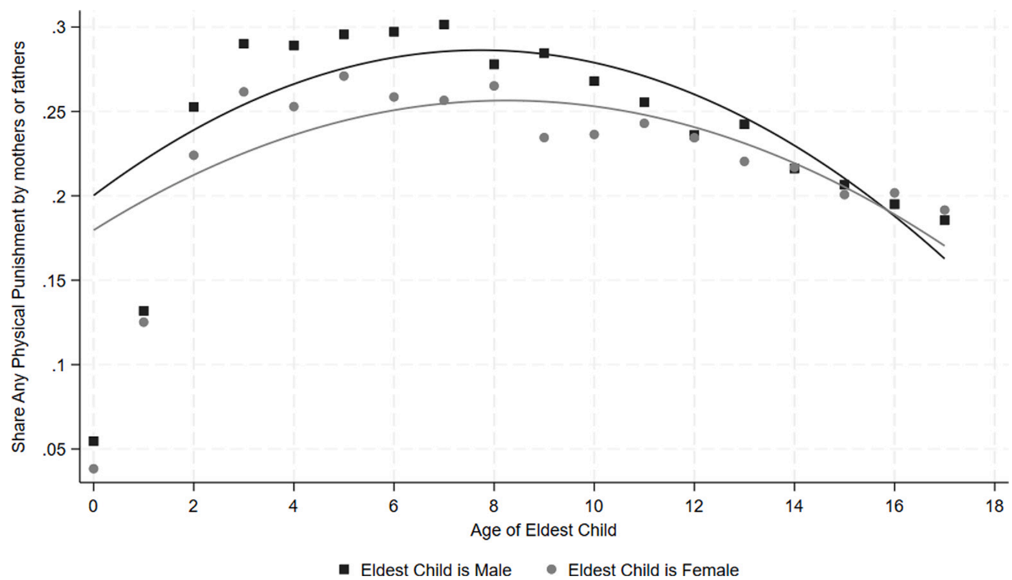
[3] We provide more detail for each of these in Appendix A.

[4] Slapping and hitting are the most common forms of physical discipline that the interviewers ask about. The exact wording in the questionnaire in Spanish for slapping is "palmadas" and for hitting is "con golpes o castigos fisicos". 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.

[5] Peru has 25 regions, formerly known as *Departamentos*, 196 provinces, and 1874 districts.



(a) Physical punishment by wealth among those reporting recent discipline.



(b) Recent physical punishment by age and sex of oldest child among mothers and fathers.

Fig. 1. The figure show reported physical punishment practices using the whole sample. Panel A shows that physical punishment (slapping or hitting) by wealth among parents who report recent discipline of children. In Panel B, we show physical punishment for mothers and fathers by age and sex of the eldest child.

that they do not physically punish their child, around 15% report that their child's father does; and among women reporting that they do physically punish their child around 37% report that their child's father does not.[6]

Finally, Panel F reports that in our full sample, about 17% of mothers indicate someone in their household receives *Juntos*; around 39% of mothers live in districts eligible for *Juntos* in the year they were interviewed; and about 48% live in districts that received *Juntos* at some point in the time period we study. The largest differences between the full and the study samples are related to *Juntos*, a natural consequence of the study sample excluding always treated districts.[7]

In Fig. 1(A), we show the incidence of using physical punishment in the last month for mothers and fathers by wealth decile. Parents in the bottom wealth deciles have the highest self-report of using any type of physical punishment. This incidence decreases for both parents as wealth increases and the difference in reported punishment widens between mothers and fathers. In Fig. 1(B) we show how physical punishment by either parent varies by the age and sex of the eldest child. Boys tend to have a higher incidence of physical punishment relative

[6] It is difficult to know whether these figures are over or under reported. 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.

[7] These samples are distinct as expected, all but one of the variables (the share of female children in the household) are statistically significantly different between the two samples at the 10% level or lower.

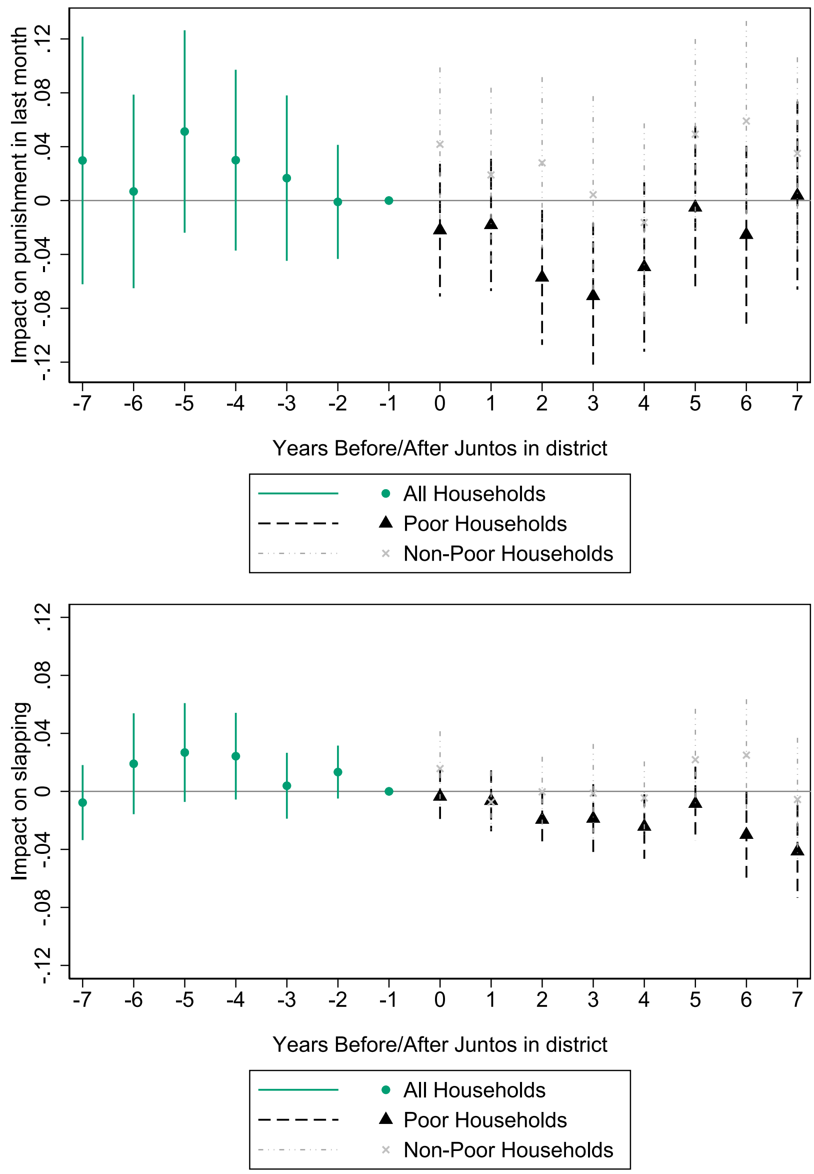


Fig. 2. These figures present the two-way fixed effects estimator event study for punished in the last month (PLM), and slap by wealth quintile (1–2 vs. 3–5).

to girls. The share of parents reporting using physical punishment as a form of discipline increases as children increase in age from birth to age four, then it is relatively constant at around 25%–30% until children are seven years of age, after which it declines.[8]

While understudied in economics, the physical punishment of children is well-studied in the social psychology literature. In general, this literature finds associations where children are more likely to experience physical punishment if they live in a single-parent household or with a non-relative caregiver; if they are poor; if their parents have a more traditional view of discipline; and if their 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 B3. In this table, we report OLS regression results for mother, child, and household characteristics correlated with the use of any physical punishment by the mothers. Column (1) shows raw differences across wealth where those in the richest two deciles are about 20 percentage points less likely to use physical punishment 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, meaningful differences across wealth persist at the extremes of the wealth distribution. Mothers in the top three deciles are between 2 to 4 percentage points less likely to physically punish their children relative to children in the lowest decile.

Estimating the long-term consequences of physical punishment is fraught with endogeneity concerns. Nonetheless, research findings suggest that physical punishment of children is strongly associated with

[8] Note that physical 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 do not change if we use the average age and majority sex of the children in the household.

negative short- and long-term physical, emotional, behavioral, and cognitive outcomes (Ferguson, 2013; Paolucci and Violato, 2004; Gershoff and Grogan-Kaylor, 2016; Larzelere and Kuhn, 2005).[9] Although imperfect, using our data, we examine long-term associations of mothers who were physically punished as children. These results are presented in Appendix Table B4, and indicate that these mothers attained fewer years of schooling, are more likely to approve of wife beating, are more likely to currently use physical punishment with their children, and agree that physical punishment is necessary to discipline children. These results show economically meaningful lifetime associations of exposure to physical punishment in childhood.

3. Estimation approach

To examine the impact of this conditional cash transfer program on parental punishment practices, we leverage the staggered geographical rollout of *Juntos* over time. *Juntos* is rolled out at the district level, thus, for our main analysis, we collapse our data at the district-year level weighting by the number of respondents within a district-year cell, and use district-year averages of our outcome variables.[10] We then estimate the following equation:

$$P_{dt} = \beta_0 + \beta_1 Juntos_{dt} + \delta_t + \gamma_d + \epsilon_{dt} \tag{1}$$

where P_{dt} is the average reported punishment by parents living in district d, in year t. Our main explanatory variable is $Juntos_{dt}$ which takes a value of one when the district is eligible for *Juntos* in year t. We include district fixed effects, γ_d, which controls for time-invariant district characteristics. For example, any fixed characteristics that would be correlated with higher or lower overall punishment practices, such as persistent weaker/ stronger prevailing norms about using physical punishment. We include country-wide year fixed effects, δ_t, to control for period specific effects common to the whole country. For example, during a recession all households may experience more income insecurity which could be correlated with overall levels of punishment strategies. In addition, these time fixed effects would adjust for any differences in survey implementation common to a particular survey year.[11] Finally, ϵ_{dt} is an unobserved error term. Standard errors are clustered at the district level (the unit of treatment).

We use OLS to estimate a two-way-fixed effect (TWFE) specification leveraging the staggered roll-out of *Juntos*. For our OLS estimator of β_1 to be unbiased, our set-up requires several assumptions. First, 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 in the absence of receiving *Juntos*. Although this assumption cannot be tested directly, we provide some reassurance that this assumption is likely to hold in Fig. 2, where pre-treatment data provides suggestive evidence of parallel trends prior to treatment. Furthermore, we discuss the sensitivity of this assumption in Section 4.4.1. 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 physical punishment behavior in prior waves in anticipation of treatment. Fig. 2 also shows no evidence of anticipatory changes in physical punishment behavior immediately prior to a district becoming eligible for *Juntos*.

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 et al., 2024; 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 et al., 2024). However, this cannot be interpreted as the proper weighted average because, as studies have shown, some weights can be negative (Goodman-Bacon, 2021).[12] 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).[13]

To address the potential for heterogeneous treatment effects in biasing our estimators we conduct a number of specification checks and use an estimator more appropriate for this type of setting. First, we follow (de Chaisemartin and d'Haultfoeuille, 2020) and calculate the weights and find that none of the (144) ATEs receive a negative weight in our sample. Second, even when the weights are non-negative, 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 et al. (2024) (hereafter BJS estimator). Intuitively, the estimator proposed by Borusyak et al. (2024) imputes counterfactuals for the treated units using only observations from units and time periods that are not yet-treated. Treatment effects are calculated for each treated group which are then used in a weighted average to get the target average treatment effect. This estimator is one of a series of estimators that have emerged in the last few years to address issues that arise in staggered roll-out designs.[14] (static setting), and Sun and Abraham (2021) (dynamic effects). We show results using Callaway and Sant'Anna (2021), Wooldridge (2025), and de Chaisemartin and d'Haultfoeuille (2020) for our main specifications in the Appendix Table B5. This is our preferred specification.

4. Results

In this section, we present our results that suggest that average levels of overall punishment and physical punishment are reduced in districts when *Juntos* is introduced. We show evidence that these are mainly driven by the poorer households in the district who are the likely recipients of *Juntos*. By examining the heterogeneity of the impacts by sex and age of the children showing stronger results for male children. We then investigate potential mechanisms through which *Juntos* can affect the physical punishment of children and, finally, we show that our main results are robust to different specifications and using alternative sub-samples.

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[9] A review by Gershoff (2002) of over 300 studies on physical punishment shows that physical 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 physical 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.

[10] Weighting by the number of respondents is important because there is considerable variation in the number of respondents within a district-year cell. Thus, by using frequency weighting we avoid down-weighting high population districts, and similarly up-weighting low population district-year observations in our analysis.

[11] We also show that our results are robust to including province-level time fixed effects. See Section 4.4.2.

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

[13] However, 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 et al., 2024). In our case, over half of the districts in our sample are never treated.

[14] Liu et al. (2021), Gardner (2022) and Wooldridge (2025) have put forth similar estimators. Different estimators are proposed by Callaway and Sant'Anna (2021), and de Chaisemartin and d'Haultfoeuille (2020).

Table 1
Juntos introduction on physical punishment practices in poor and non poor households.

Sample:	Double	Triple		PLM only	Double	Triple		PLM only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Punished in the last month								
District eligible for Juntos in current year	−0.022*		−0.035**					
	(0.012)		(0.014)					
Non poor households		−0.007						
		(0.013)						
Poor households		−0.034***						
		(0.013)						
Pre-treatment mean	0.381	0.385	0.385					
Pre-trend test: p-value	0.000	0.000	0.001					
	Mothers				Fathers			
Panel B: Slap								
District eligible for Juntos in current year	−0.020***		−0.025***	−0.031**	−0.018***		−0.019***	−0.031***
	(0.006)		(0.006)	(0.013)	(0.005)		(0.004)	(0.010)
Non poor households		−0.014*				−0.013**		
		(0.007)				(0.005)		
Poor households		−0.026***				−0.022***		
		(0.006)				(0.005)		
Pre-treatment mean	0.046	0.046	0.046	0.127	0.023	0.024	0.024	0.131
Pre-trend test: p-value	0.148	0.140	0.257	0.038	0.388	0.374	0.059	0.928
Panel C: Hit								
District eligible for Juntos in current year	0.004		−0.015	0.003	−0.001		−0.018	−0.016
	(0.007)		(0.012)	(0.015)	(0.008)		(0.013)	(0.018)
Non poor households		0.010				0.007		
		(0.008)				(0.009)		
Poor households		−0.004				−0.009		
		(0.009)				(0.009)		
Pre-treatment mean	0.211	0.216	0.216	0.552	0.186	0.188	0.188	0.549
Pre-trend test: p-value	0.152	0.145	0.083	0.706	0.019	0.013	0.329	0.050
Panel D: Physical punishment								
District eligible for Juntos in current year	−0.007		−0.027**	−0.009	−0.016*		−0.032**	−0.040**
	(0.008)		(0.013)	(0.014)	(0.009)		(0.013)	(0.019)
Non poor households		0.004				−0.005		
		(0.009)				(0.010)		
Poor households		−0.018*				−0.026***		
		(0.009)				(0.010)		
Pre-treatment mean	0.231	0.238	0.238	0.611	0.203	0.207	0.207	0.609
Pre-Trend test: p-value	0.072	0.073	0.135	0.122	0.017	0.013	0.123	0.174
District and Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	4545	7637	7569	4376	4508	7195	7120	4150
Unit of observation	DY	DYW	DYW	DY	DY	DYW	DYW	DY

Note: Standard errors clustered at the district (treatment) level in parentheses. Pre-treatment means are calculated the year prior to *Juntos* eligibility. The unit of observation is either district-year (DY) or district-year-wealth (DYW). DY or DYW cells are weighted by the number of respondents within a particular cell. The number of observations corresponds to Panel D, with the same or some variations for the other panels. All results in this table use the Borusyak et al. (2024) (BJS) estimator. PLM refers to "punished last month". *** p-value<0.01, ** p-value<0.05, * p-value<0.1

4.1. Main results

To examine the impact of *Juntos* on physical punishment, we estimate the change in average levels of punishment at the district-level when the district becomes eligible and households within the district begin to receive *Juntos*. To begin, we examine the impact of *Juntos* on punishment at the extensive margin by looking at any recent punishment within the household as an outcome. These results are presented in Panel A of Table 1.[15] When a district becomes eligible for *Juntos*, our result in Column (1) suggests that the district-level average incidence of any punishment in the last month goes down by 2.2 percentage points or approximately 6%. For this outcome, our results suggest a statistically significant pre-trend so we interpret these results with some caution.[16]

Next, we examine the impact of *Juntos* introduction on different types of recent physical discipline practices differentiating by mothers and fathers, the results of which are presented in Panels B through D.[17] In Panel B, we observe a clear reduction in average levels of reported slapping among both mothers and fathers in columns (1) and (5). At the district level, we find a 2 and 1.7 percentage point reduction in average district-level slapping rates among mothers and fathers, respectively. While the pre-trend test (proposed by Borusyak et al., 2024) for slapping does not suggest statistically significant violation of parallel trends, in Appendix Figure B3, we apply the approach of Rambachan and Roth (2023) to show how robust the estimated effect of *Juntos* on slapping among mothers is to violations of the parallel trends assumption. We discuss these results in the robustness Section 4.4.2.

We note that the *Juntos* program targets poor households; among the bottom two quintiles, self-reported receipt of *Juntos* is 51% compared to only 15% among the other three wealth quintiles. To examine whether the district-level effects we estimate are driven by the likely direct beneficiaries of *Juntos*, we collapse our data at the district-wealth-year

[15] Results in Table 1 are estimated using the BJS estimator. TWFE results are presented in Appendix Table B6.

[16] Specifically, we implement the Borusyak et al. (2024) pre-trend test using four periods prior to the treatment and present the associated p-value for the cluster-robust Wald test.

[17] We note that any punishment in the last month is reported at the household level whereas types of punishment are differentiated by mothers and fathers.

Table 2
Juntos introduction on physical punishment practices by the composition of children in the household.

	Study sample (1)	Only female (2)	Only male (3)	More than one child (4)	One child (5)	One child: Female (6)	One child: Male (7)
Panel A: Punished in the last month							
District eligible for Juntos in current year	−0.022*	0.014	−0.040**	−0.021	−0.016	0.017	−0.048**
	(0.012)	(0.017)	(0.020)	(0.013)	(0.019)	(0.023)	(0.023)
Pre-treatment mean	0.381	0.337	0.389	0.400	0.345	0.323	0.365
Mothers							
Panel B: Slap							
District eligible for Juntos in current year	−0.020***	−0.006	−0.019**	−0.026***	−0.008	−0.009	−0.007
	(0.006)	(0.010)	(0.010)	(0.007)	(0.010)	(0.014)	(0.011)
Pre-treatment mean	0.046	0.059	0.053	0.046	0.056	0.053	0.052
Fathers							
Panel C: Slap							
District eligible for Juntos in current year	−0.018***	−0.018*	−0.021***	−0.019***	−0.015*	−0.014	−0.023**
	(0.005)	(0.010)	(0.007)	(0.005)	(0.008)	(0.014)	(0.011)
Pre-treatment mean	0.023	0.037	0.033	0.019	0.044	0.040	0.036
Year and District FE	✓	✓	✓	✓	✓	✓	✓
Observations	4545	4119	4220	4512	4142	3477	3589

Note: Standard errors clustered at the district level in parentheses. The unit of observation is at the district-year (DY) level, and are weighted by the number of respondents within a particular cell. Pre-treatment means is calculated the year prior to *Juntos* eligibility. The number of observations corresponds to Panel D, with the same or some variations for the other panels. All results in this table use the Borusyak et al. (2024) estimator. *** p-value<0.01, ** p-value<0.05, * p-value<0.1

level, where we distinguish between poor and non-poor households and examine the differential impact by poverty status, shown in columns (2) and (6).[18] We see clearly that the average district-level reduction in slapping rates is driven by the poorer households in the sample.[19] The estimated reduction in slapping ranges from 2.3 to 2.6 percentage points among the poor.

Building on this differentiation by poverty status, we further estimate a triple difference model, where we use the non-poor within a treated district as an additional *less-likely-to-be* treated reference group. We present the treatment coefficients in columns (3) and (7). This triple-difference approach allows us to additionally compare poor to non-poor households within the same district. Thus, violations would require shocks that are not only district specific but district-poor household specific.[20] We see for both mothers and fathers that there is a statistically significant decrease in slapping rates in poor households who are the likely direct beneficiaries during district-level expansions of *Juntos*.

In general, we do not find any statistically significant changes in the use of hitting (Panel C). In Panel D of Table 1, we create a single physical punishment variable that takes on the value of one if a parent reports either slapping or hitting. We find that overall reductions in average rates of physical punishment are concentrated among the poor. Using the results from the triple difference approach (columns 3 and 7), we find that *Juntos* introduction led to an 11% decline in physical punishment among mothers and a 13% decline among fathers.

Our main results show both a reduction in recent punishment and in a child's exposure to more physical forms of punishment. Thus, the decline in exposure to physical punishment could be driven by parents who are reducing their overall reliance on punishment or parents who are reducing the frequency of punishment. To test for this, we examine whether there are changes in punishment practices in households where mothers (column 4) or fathers (column 8) reported that their children received any punishment in the last month.[21] The sample is endogenous, nonetheless, we find that in the sample of households who report recent punishment, the reductions in physical punishment are more pronounced. This pattern of results implies that parents are reducing their reliance on violent discipline even among those who are engaging in any discipline.

Scaling—Our estimated effects are an average among all parents in the district some of whom are receiving *Juntos* (direct beneficiaries) and others who are not.[22] In other words, the estimates are averages of changes among mothers affected directly through the receipt of *Juntos*, mothers potentially affected indirectly through behavioral spillovers, and mothers not affected. While we differentiate by poverty in Table 1, we also show results in Appendix Table B7 where the explanatory variable is an approximate share of mothers within a district receiving *Juntos* instead of an indicator for district eligibility. With caution, we use these coefficients to consider what happens when a larger share of mothers within a district receive *Juntos*. For example, the coefficient from Panel B suggests that a one standard deviation (0.128) increase in the share of mothers within a district receiving *Juntos* decreases average reported slapping by about one percentage point in the district.

Taken together, our results show that *Juntos* decreases overall punishment of children and in particular the use of *slapping*. Combining mothers and fathers together, our results suggest that when a district becomes eligible for *Juntos*, average levels of physical punishment are reduced by about 11%–13% among the poor. This effect is of similar magnitude to other forms of violence in the household. Díaz and Saldarriaga (2022) estimate that *Juntos* reduced intimate partner violence by 25%–30% ; while a meta-analysis of recent work on the link between cash transfers and intimate partner violence suggests an average of 18%–26% reduction in rates of IPV (Baranov et al., 2021; Buller et al., 2018).

[18] We categorize households in the lowest two wealth quintiles as poor while others not.

[19] We see a similar pattern in Panel A Column (2) for any recent punishment.

[20] We do not use the triple difference estimator as our main specification in the rest of the paper due to cell-sample size considerations. For any particular district-year cluster, the number of observations in the "Non-poor" and "Poor" subgroups we have defined varies from 0 to 268 households. As we make further sample restrictions, such as limiting the sample to women with only male or female children, or children of a certain age, for example, this further reduces the sample sizes within each cell. Thus, we are making trade-offs between cleaner identification and sample representativeness of the particular group in a particular district-year poor/non-poor cell.

[21] We restrict to mothers who report punishment in the last month before collapsing the data at the district-year level. The sample size varies because in some districts (particularly small ones) in some time periods there may not be a mother who reports recent punishment.

[22] As Appendix Figure B2B shows, on average about 40% of mothers within an eligible district receive *Juntos*.

4.2. Which children are affected?

Children are less exposed to physical forms of punishment when their district becomes eligible for *Juntos*. Understanding whether all children are equally impacted sheds light on potentially important differences across children. In this section, we explore whether there is heterogeneity in the reduction of physical punishment by sex and age of the children. In Section 2.3, we showed differences in the overall use of physical punishment for male and female children across all ages. Given that our effects are concentrated on slapping (among mothers), we explore whether *Juntos* contributes to a gendered effect of parental discipline practices.

Since physical punishment is measured at the parent level rather than the child level, and then averaged out at the district level, we are unable to directly differentiate by the sex of the child. Instead, to explore potentially gendered effects, in Table 2 we first show results among two sub-samples: punishment in households with only female children (column 2) and those with only male children (column 3).[23] These sub-samples include parents with one or more children. While most of the results do not differ in a statistically significant way from those in the main table, the results on punishment experienced in the last month exhibits a noticeably gendered pattern. On the extensive margin, we find that any recent punishment is reduced in households with male only children. This pattern also holds when restricting the sample to single child only households (see column 6 and 7).

We find similar results in households with multiple children whereby reductions in slapping by mothers are larger in households with only male children as compared to only female children. For fathers, we find statistically significant reductions in slapping in households with only 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 perhaps reduce the misbehavior of male children and thus change the severity of discipline by the parents.

Parents with multiple children may feel more subjected to stressors associated with poverty, and as such, we might also expect differential responses for those with more than one child. We find results that suggest that the effect of the introduction of *Juntos* on slapping is more pronounced among parents with more than one child.

Our descriptive findings from Fig. 1B show a lower but quite rapidly increasing use of punishment for the youngest children as they get older, pretty stable punishment practices for children in the middle age group, and declining punishment practices as children get older. Thus, we further investigate how punishment strategies vary by the age of the children. We split our sample into households with children in each of the following age-groupings: under 5, between 5 and 11, and between 12 and 18 to map the different portions of Fig. 1B. In the analysis, these groups are not mutually exclusive as a household can have two children, for example, ages 4 and 8. In this situation, the household would be included in the analysis of households with children younger than 5 and also in the sample of households with children ages 5–11.[24]

Table 3 columns (1) to (3), present the results for samples restricted to households with children in the specified age ranges.[25] The general pattern of results across the three samples and the types of physical punishment by mothers and fathers we consider, are broadly consistent with our main findings, but there are a few differences worth highlighting. In particular, we see a larger, and statistically significant, reduction in households with kids who are aged 5 to 11 for exposure to any form of punishment in the last month as well as the use of physical punishment among mothers. For fathers, the results are more pronounced among the younger two age groups.

In sum, our results show that with the introduction of *Juntos* recent punishment and specifically slapping declines. Male children appear to be the most impacted, seeing larger reductions. There are many potential drivers attributable to *Juntos* that we explore next.

Table 3
Juntos introduction on physical punishment practices by child age.

	Children under 5 (1)	Children 5 to 11 (2)	Children 12 to 18 (3)
Panel A: Punished in last month			
District eligible for Juntos in current year	−0.001	−0.027**	−0.011
	(0.014)	(0.013)	(0.017)
Pre-treatment mean	0.416	0.396	0.349
Mothers			
Panel B: Slap			
District eligible for Juntos in current year	−0.013	−0.018***	−0.027**
	(0.009)	(0.007)	(0.013)
Pre-treatment mean	0.062	0.040	0.032
Fathers			
Panel C: Slap			
District eligible for Juntos in current year	−0.020***	−0.017***	−0.007*
	(0.007)	(0.005)	(0.004)
Pre-treatment mean	0.032	0.016	0.012
Year and District FE	✓	✓	✓
Observations	4492	4514	4440

Note: Standard errors clustered at the district level in parentheses. The unit of observation is at the district-year (DY) level, and are weighted by the number of respondents within a particular cell. Pre-treatment means are calculated the year prior to *Juntos* eligibility. The number of observations corresponds to Panel C, with the same or some variations for the other panels. All results in this table use the Borusyak et al. (2024) estimator. *** p-value<0.01, ** p-value<0.05, * p-value<0.1

4.3. Potential mechanisms

Conditional cash transfers, such as *Juntos*, can affect the discipline practices of parents through many channels. To explore which of these channels may play a meaningful role in our setting, we begin by estimating the effect of *Juntos* introduction on variables that proxy for several of the potential mechanisms.[26] We draw extensively on the economics literature on income and IPV, as well as the social psychology literature on the physical punishment of children to identify potential mechanisms, and organize them into five different bins :

1. *Economic conditions*—Relaxing the income constraint may lead to lower stress (Baranov et al., 2021; Buller et al., 2016), higher mental bandwidth, and the ability to use pecuniary rewards (Weinberg, 2001); all of these offer potential pathways through which cash transfers may lower reliance on physical punishment practices. These are similar to mechanisms proposed for the link between cash transfers and intimate partner violence (Baranov et al., 2021; Hidrobo et al., 2016). In addition, it may be that more resources in the household results in better behaved children leading to a decrease in overall discipline

[23] We restrict to mothers with these characteristics before collapsing the data at the district-year level. The sample size varies because in some districts (particularly small ones) in some time periods there may not be a mother with that particular composition of children.

[24] In addition to the general punishment practices elicited in the ENDES data, the survey also asks mothers child-specific questions for children under the age of five. Reported punishment is relatively low in this group. In results not shown, similar to our results in column 2 of Table 3, we find a negative but insignificant coefficient on slapping.

[25] Again, here we restrict the samples before collapsing to the district-year level.

[26] There are certainly other potential mediators, however, we are limited by what data is available to us in the survey, but we think these capture the some of the main mechanisms through which *Juntos* can result in reduced slapping of children.

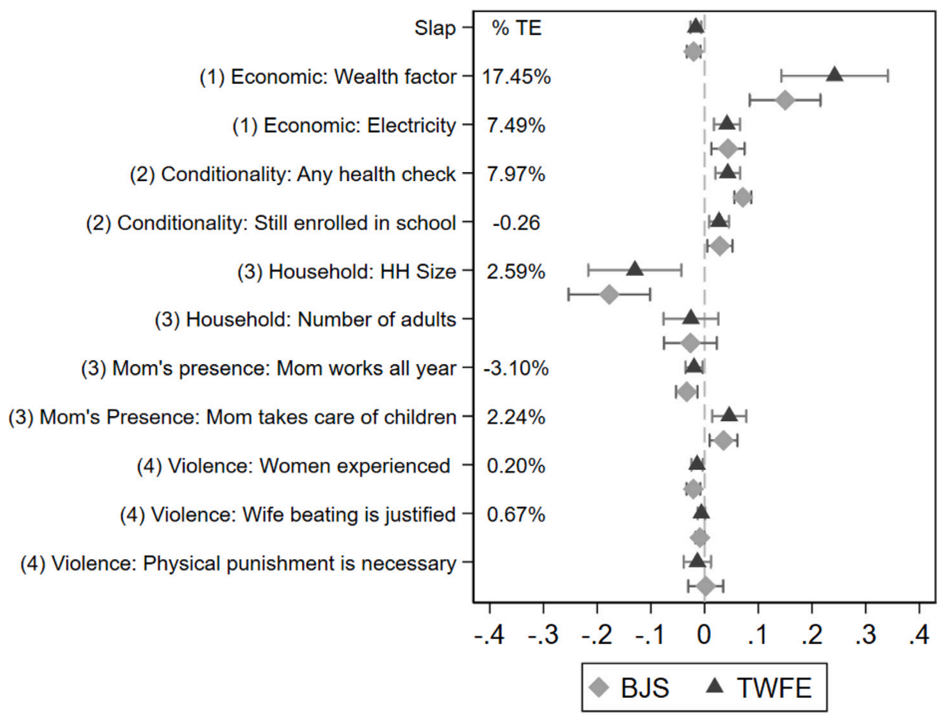

Fig. 3. Estimates presented use our main specification of equation (1) but with the mechanism as the outcome variable for both the two-way fixed effects estimator and Borusyak et al. (2024) estimators. We also conduct mediation analysis, and report the share of the total effect of a district becoming eligible for *Juntos* on slapping that could be explained by each of our hypothesized mechanisms that are directly impacted by *Juntos*. A negative share means that the mechanism attenuates the reduction on slapping. The number in parenthesis corresponds to the bin as outlined in Section 4.3.

and/or a reduction in the harshness of the discipline (Gennetian et al., 2016).

2. *Conditionalities*—The conditionalities for *Juntos* receipt are health check-ups for children under 6 years of age and regular school attendance for older children (Silva Huerta and Stampini, 2018), can also lead to meaningful changes in parental use of specific disciplinary practices. Parents may increase contact with health providers where they could gain information on non-violent disciplinary practices, or during check-ups, parents might acquire new information if their child has any visible signs of physical punishment and adjust behavior accordingly. In addition, healthier kids may simply behave better, reducing the need to punish the kids. Children spending more time at school away from parents may reduce the amount of interactions that lead to potential discipline. On the other hand, misbehavior in school may lead parents to engage in more discipline. Thus, *ex ante*, it is unclear what the sign of increased attendance in school would play in modifying punishment at home.
3. *Household composition*—Since the size and composition of the household could affect child–parent interactions, a change in household composition due to *Juntos* may be a mechanism through which the program affects discipline practices of parents. Some cash transfer programs have been shown to lead to changes in household composition (Posel et al., 2006), and researchers have hypothesized that the number and types of caregivers in the household is correlated with corporal punishment practices of children (Ember and Ember, 2005). Although existing evidence supporting this argument is weak, household compositional changes due to *Juntos* may explain some of the reduction in physical punishment.
4. *Mother's presence at the home*—There is some evidence that cash transfer programs may change the labor supply of the mother (Del Boca et al., 2021; Dona, 2023). Independent of income changes, the changing physical presence of the mother in the home could alter the duration and quality of interactions with the child (Cabrera-Hernández and Padilla-Romo, 2020). Because mothers tend to be the primary caregivers, more time at home may lead to an increase in overall discipline, but could also decrease the need for harsh punishment due to more consistent supervision. Furthermore, maternal employment may reduce the mother's presence in the house if the mother is working outside of the home, or may increase demands on her time if the mother is working from home.
5. *Violence and attitudes towards violence in the household*—Own experiences and perceptions of violence among household members can affect broader views on violent behavior within the household. In particular, previous literature has shown that cash transfers can lead to reductions in intimate partner violence experienced by mothers in Peru (Díaz and Saldarriaga, 2022; Perova, 2010). It may be that a reduction in experiencing physical violence reduces the likelihood that women themselves use physical punishment as a form of discipline (Kyegombe et al., 2015; Stern et al., 2022).

We present our mechanisms results in Fig. 3, and the equivalent regression results in Appendix Table B8. First, we demonstrate whether *Juntos* has an effect on our hypothesized mediators, and, in doing so, confirm that results from the broader cash transfer literature are applicable to Peru during our sample period. We then apply causal mediation analysis summarized by Nguyen et al. (2022) to estimate—for each proposed mediator—the direct and indirect effects of *Juntos* introduction. Using these results, we calculate what share of the total effect of *Juntos* on slapping (by mothers), could operate through each of our specified mediators impacted by *Juntos*, and report these shares in Fig. 3.

In Appendix C, we present a discussion of this approach to mediation analysis and the assumptions it requires. To summarize key ideas here, in addition to standard assumptions on the exogeneity of the treatment, mediation analysis requires that the mediators of interest are independent of potential outcomes conditional on treatment and observed pre-treatment covariates. This is a strong assumption: it essentially states that conditional on treatment and pre-treatment covariates, the mediators are *as if randomly assigned*. Additionally, well-identified mediation analyses need to satisfy the sequential unconfoundedness assumption whereby the treatment cannot affect other mediators that may then affect the mediator of interest as it would then again confound the relationship between the mediator and the outcome of interest. Given these strong assumptions required to properly identify the second step in the causal chain—the effect of the mediator on the outcome—we focus our analysis on the first step in the causal chain: the effect of the treatment (introduction of *Juntos*) on the mediators outlined above.

Starting with improved *economic conditions*, the ENDES data, as with DHS, does not have information on income, thus, we instead rely on a standardized wealth factor reported by ENDES, and on access to electricity in the household to proxy for the economic status of the household.[27] We find a statistically significant increase in both the wealth factor and the availability of electricity when the district becomes eligible for *Juntos*. The mediation analysis suggests that the wealth factor and electricity access can explain about 17% and 7% of the total effect on slapping, respectively. We again interpret these results with caution given the strong assumptions required; however, these mediation estimates suggest that economic conditions play a role.[28]

Moving on to the role of the program's *conditionalities*, we use information on health check-ups for younger children and whether the child is still enrolled in school for older children as measures of meeting these conditionalities. In Fig. 3 we replicate findings documented in the literature by Lagarde et al. (2009) and Garcia and Saavedra (2023) by showing increased health check-ups and school enrollment for children living in districts that received *Juntos*. These changes in education and health behaviors could further impact the way parents discipline their children. The mediation analysis suggests that almost 8% of the total effect on slapping can potentially be explained by the health check-up conditionality, thus, these health checks may play a meaningful role in why *Juntos* is reducing physical punishment. The education conditionality requires that children of school age attend school at least 80% of the time; this could also affect parental discipline if it leads to changes in attendance. While we do observe an increase in children still enrolled in school, our mediation analysis indicates that this plays a negligible role in explaining the impact on slapping.[29]

Considering our third bin of potential mechanisms, of *household composition*, we investigate channels that may alter the home environment in ways that could influence the discipline of children. Specifically, we use household size and number of adults in the household. We observe a statistically significant decline in average household size (1%–2%) but no statistically significant effect for the number of adults in the household after districts become eligible for *Juntos*. Given these small direct effect sizes, it is unsurprising that the mediation analysis suggests that this potential mechanism plays a modest role in explaining the reduction in physical punishment (around 3%).

In our fourth bin of *mother's presence at the home*, we use two measures: the first is if the mother works all year and, the second is if the mother is the primary caretaker of the children. We find a small decrease in the likelihood that the mother works all year, and an increase in the share of mothers who report taking care of the children. These results suggest that mothers have (limited) increased contact with their children, and correspondingly the mediation analysis shows these factors to play a modest role.

In our final bin of *violence and attitudes towards violence in the household*, we group the mother's experience of physical violence and her views towards physical punishment. We start by corroborating the results of Díaz and Saldarriaga (2022) by showing that when a district becomes eligible for *Juntos*, the share of women who report experiencing either physical or sexual domestic violence in the household declines. When it comes to reported beliefs related to intra-household violence, we see a reduction in women's view that wife beating is justified, when a district becomes eligible for *Juntos*. However, our results indicate no discernible change in mother's views on the need to use physical punishment to discipline children.[30] Overall, we find that these mediating variables play a negligible role in explaining the reduction in slapping (less than 1%).

Taken together, *Juntos* leads to changes in economic conditions, health checks and school enrollment, household composition, mothers' presence in the household, and domestic violence. Arguably, only the direct effects on economic conditions and health checks are economically meaningful. Depending on the causal effect that each of these mediators themselves have on how mother's discipline their children, these may be meaningful mechanisms through which *Juntos* acts. With caution, given the strong assumptions we outline above that allow us to estimate the causal effect of the mediators on our outcome of interest, we find that the most important mediators are economic conditions and the health check up conditionality. Thus, it is not only the cash, but the conditionality and (much) more modestly the modified home environment, that are important elements of the design of the *Juntos* program contributing to changes in how parents discipline their children. Our findings in this section are a first step in trying to disentangle these mechanisms. More research is needed in other contexts to further understand how transfer design features can be used effectively to reduce physical punishment.

4.4. Sensitivity and robustness

In this section we discuss sensitivity analyses and robustness checks we conduct to support our main results. First, we discuss analyses that use placebo treatment timing and show sensitivity to violations of parallel trends. Next, we show our that our main results are robust to alternative specifications, specific sub-samples, and using individual-level data with controls.

[27] Given the difficulty in collecting reliable income and expenditure data, ENDES and DHS construct a continuous relative wealth index that includes information on observable household characteristics. This index typically includes information on asset ownership, housing characteristics, and access to services.

[28] Complementary to the mediation analysis, we also examine the heterogeneity of effects on parental punishment by stratifying by baseline district characteristics. Note, we cannot control for individual baseline characteristics, as the data is a series of repeated cross-sections not a panel data set. We also do not observe all districts in 2010, the first year of our sample period. Thus, to do this, we consider the first year of data available for a district as that districts' "baseline" year. We then classify districts as above/below the median with respect to each of the most important identified mediators (those that explain 7% or more of the variation). These results are presented below in Appendix Table B9. These results demonstrate that households within poorer and less poor districts reduce their slapping of children, this is consistent with our mediation results, in that not all households are treated within a district, and the economic conditions are mechanically improved by receipt of the transfer.

[29] We are limited by the data, insofar as we only observe information for children and whether they are still enrolled in school. A more nuanced measure of attendance may yield different results.

[30] In results not shown due to limited data, we see a statistically significant decline in average PHQ-14 scores after the introduction of *Juntos* in the district. This means that negative emotions that are symptoms of depression and anxiety are reduced. See Spitzer et al. (1999) for more information on PHQ.

4.4.1. Sensitivity analysis

To give further credibility to our main results, we consider treatment effects obtained from the placebo timing of the rollout of *Juntos*. To do so, we estimate the treatment effect using the observed treatment timing and then generate a placebo distribution by re-estimating the model 1000 times with randomly assigned fake treatment start years. We compare the estimated treatment effect to this placebo distribution. In Appendix Figures B1 A and B, we plot the distribution of the placebo coefficients, and indicate the estimated treatment effect by the vertical line. Thus, we test whether the observed effect could plausibly arise under random treatment timing. In Appendix Figures B1 C and D, we plot the distribution of placebo coefficients matching the triple difference BJS estimation approach. For both specifications, the placebo distribution is quite unlikely to provide a larger negative coefficient, providing further support for our findings.

Next, we provide sensitivity analysis for our main results on slapping by implementing the tool for robust inference where parallel trends assumptions may be violated proposed by Rambachan and Roth (2023). We apply this to results using the (Borusyak et al., 2024) estimator and we use the relative magnitude bounds restrictions on the possible violations of trends, since it is unlikely that factors that affected parental discipline practices outside of *Juntos* followed a smooth trend. Instead we think it is more likely that there could have been unobserved district specific shocks that may have affected those receiving *Juntos* differently from those who did not received it, such as particular parental education programs adopted in some districts. We report the average effect for the first period (tau 0). We limit the sample to 4 pre-periods and 5 post periods, to ensure that there are at least a third of the districts within this time frame. Given that we are estimating a coefficient for each time period, longer time periods would fall below this minimum threshold. The results only marginally change when we extend the time frame to all periods available. Appendix Figure B3 shows the "breakdown" value at which we can no longer reject our null hypothesis of no effect for our main result of slapping. This break down value is when $\overline{M} = 0.6$. This result rules out substantial nonlinear pre-trends and it means that the observed reduction in slapping that we see due to *Juntos* remains statistically significant as long as the violation of the parallel trends assumption in the post-treatment period is no more than 60% larger than the maximum violation observed in the pre-treatment period. Given this, some caution should be applied in interpreting our results, as it is possible that violations larger than these of the parallel trend assumption would yield our findings no longer statistically significant.

4.4.2. Robustness

First, we show results in Appendix Table B5 where we use alternative estimators proposed for staggered difference-in-differences design. Namely we show that the estimators proposed by Callaway and Sant'Anna (2021), Wooldridge (2025), and de Chaisemartin and d'Haultfoeuille (2020) yield results similar to our main results in Table 1.

Next, we include results from several robustness checks in Table 4. Results from alternative specifications and different sub-samples are all qualitatively in line with our main estimates and suggest that our core findings are robust.

In column (1), we limit our estimating sample to districts that receive *Juntos* at some point in our sample period. These districts are more similar to one another than districts that never receive the program. We continue to observe a decline in slapping and physical punishment for both parents and the point estimates are very similar to those in Table 1.

Our results could also be influenced by individuals moving in response to the roll-out of *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 (2) of Table 4 showing that our main results are robust to controlling for migration responses.

The ENDES data is representative at higher levels of aggregation but it is not representative at the district level. In columns (3) and (4) we consider the implications of this for our analysis. In column (3), we test the sensitivity of our results to the use of the ENDES survey weights, and our results remain robust for slapping. We further show that our results are robust to restricting to districts that are selected into the ENDES in eight or more periods.[31] Thus, limiting our sample to districts that appear in almost all rounds is akin to restricting to larger districts. These results are presented in column (4). This restriction enables us to check if our results are driven by smaller districts more prone to outliers. We find similar results across all panels.[32]

Our main specification includes year-fixed effects which flexibly controls for the general national declining trend in physical punishment over time. However, there may exist province-specific shocks in any given year that may affect our estimated results. Thus in column (5), we also control for province-year fixed effects.[33] We find similar results, as well as stronger evidence of reductions in hitting.

Next, we run specifications with repeated cross-sections at the individual level instead of collapsing the data to the district-year level. We first report results without controls and obtain the same results as those reported in columns (1) and (5) of Table 1. Individual level data also allows us to test whether our results are robust to controlling for a host of individual, households, and child characteristics. For example, 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).[34] In examining the effect of these centers on violence against women, Sviatschi and Trako (2024) systematically document that the expansion of the WJCs was not correlated with the *Juntos* roll-out.[35] Therefore, this is unlikely a confounding factor in our analysis. Furthermore, our data are repeated-cross-sections, not panel data, thus we only observe the covariates for the household at the time of the survey, and so we cannot control for covariates prior to *Juntos* receipt. As such, many of the controls are endogenous and are likely poor controls as they too might be influenced by *Juntos* receipt. Nonetheless, to mitigate the remaining concerns on other forms of aid, we include controls for other social protection programs as well as individual, household and children controls in column (7).[36] Our results are qualitatively similar to our main results on reported discipline practices for mothers and fathers.

[31] Districts that are repeatedly observed tend to be the larger districts.

[32] As an additional check, we iteratively restrict our estimating sample to cells with X or more respondents (with children) in a district in a particular time period, and re-estimate our main results. Our results are quite robust regardless of the restriction, suggesting that the results are not driven by an aggregation issue stemming from small districts.

[33] This would account, for example, for an active non-government organization operating in some provinces in specific years and pursuing a specific agenda regarding violence in the family. In this case, it leverages variation only across districts within the same province. Thus, it identifies effects only in provinces that have districts receiving *Juntos* at different times.

[34] These 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.

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

[36] Other social programs identified in the survey include: food aid, childcare aid, scholarships, work aid, and old age pensions. Mother controls include: age, age squared, years of schooling, working status, divorce or separated indicator, language and ethnicity indicators, mom physically punished as a child. Household controls include: household size, number of children under

Table 4
Juntos introduction on disciplinary practices: robustness checks.

	Exclude never *Juntos* districts (1)	Never moved (2)	ENDES weighted (3)	Districts observed 8+ times (4)	Flexible time FE (5)	Indiv (6)	Indiv with controls (7)
Panel A: Punished in last month							
District eligible for Juntos in current year	−0.007	−0.042**	0.007	−0.022	−0.048**	−0.022*	−0.014
	(0.015)	(0.017)	(0.015)	(0.013)	(0.019)	(0.012)	(0.012)
Pre-treatment mean	0.381	0.409	0.374	0.379	0.379	0.398	0.402
Mothers							
Panel B: Slap							
District eligible for Juntos in current year	−0.026***	−0.022***	−0.019***	−0.023***	−0.043***	−0.020***	−0.016**
	(0.008)	(0.008)	(0.006)	(0.007)	(0.011)	(0.006)	(0.006)
Pre-treatment mean	0.046	0.046	0.044	0.053	0.053	0.054	0.054
Panel C: Hit							
District eligible for Juntos in current year	0.001	−0.005	0.012	0.004	−0.054***	0.004	0.008
	(0.008)	(0.011)	(0.009)	(0.008)	(0.013)	(0.007)	(0.007)
Pre-treatment mean	0.211	0.232	0.208	0.209	0.209	0.207	0.210
Panel D: Physical punishment							
District eligible for Juntos in current year	−0.015*	−0.020*	0.000	−0.009	−0.073***	−0.007	0.001
	(0.009)	(0.012)	(0.010)	(0.008)	(0.013)	(0.008)	(0.008)
Pre-treatment mean	0.231	0.255	0.227	0.234	0.234	0.235	0.238
Fathers							
Panel E: Slap							
District eligible for Juntos in current year	−0.023***	−0.012*	−0.019***	−0.018***	−0.021**	−0.018***	−0.015***
	(0.005)	(0.007)	(0.006)	(0.005)	(0.011)	(0.005)	(0.005)
Pre-treatment mean	0.023	0.023	0.022	0.025	0.025	0.032	0.032
Panel F: Hit							
District eligible for Juntos in current year	−0.001	−0.004	−0.003	0.002	−0.017	−0.001	0.005
	(0.009)	(0.011)	(0.009)	(0.009)	(0.017)	(0.008)	(0.008)
Pre-treatment mean	0.186	0.180	0.183	0.176	0.176	0.179	0.182
Panel G: Physical punishment							
District eligible for Juntos in current year	−0.021**	−0.016	−0.018*	−0.012	−0.036*	−0.016*	−0.007
	(0.010)	(0.014)	(0.010)	(0.009)	(0.021)	(0.009)	(0.008)
Pre-treatment mean	0.203	0.199	0.199	0.195	0.195	0.202	0.205
Year and District FE	✓	✓	✓	✓		✓	✓
Province-Year and District FE					✓		
Observations	3094	4292	4545	2846	2391	116,390	114,416
Unit of observation	DY	DY	DY	DY	DY	Ind	Ind

Note: Standard errors clustered at the district level in parentheses. The unit of observation is either at the district-year (DY) or the individual (Ind) level. DY averages are weighted by the number of respondents within a particular cell. Pre-treatment means are calculated the year prior to *Juntos* eligibility. The number of observations corresponds to Panel C, with the same or some variations for the other panels. Refer to Section 4.4.2 for an explanation of each column. In column (7) we include the following controls: a control for receipt of other social programs (food aid, childcare aid, scholarships, work aid, and old age pensions); mother controls (age, age squared, years of schooling, working status, divorce or separated indicator, language and ethnicity indicators, mom physically punished as a child; household controls (household size, number of children under 5, number of adult males and females, urban indicator, age of household head, education of household head, indicator for household head currently married or divorced or separated), and children controls (average age, proportion of children who are female, proportion of children in school). All results in this table use the Borusyak et al. (2024) estimator. *** p-value<0.01, ** p-value<0.05, * p-value<0.1

5. Conclusion

We study the effect of the conditional cash transfer program *Juntos* on the discipline practices of parents. Research in economics has focused on the effect of cash transfers on one form of intra-household violence—intimate partner violence. Many studies including meta-analyses of these studies suggest that easing the stresses of poverty leads to overall reductions in IPV (Buller et al., 2018; Gibbs et al., 2017; Baranov et al., 2021). In our study, we focus on the use of physical punishment to discipline children within a household. We find that when districts become eligible for *Juntos*, average rates of reported punishment in the last month decline, with larger reductions among poorer households. Moreover, we find that average reported use of slapping by both parents as a form of discipline also falls. While these results are robust to alternative estimators and a different specifications, using the methods of Rambachan and Roth (2023), we find them to be sensitive to violations of parallel trends larger than 60% of those in the pre-treatment period. Nonetheless, our preferred estimates suggest an 11% and 13% reduction in average recent physical punishment rates among mothers and fathers in the poorest households in the district, respectively.

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 including the conditionalities of *Juntos*. Keeping in mind that the assumptions needed for mediation analyses are strong, we find that suggestive evidence that improved economic resources, and the health check-up conditionality, can potentially explain some of the reductions in parents' reported use of slapping. Future research that can directly address the underlying mechanisms would be an important next step, as well as demonstrating the long-term impacts from these reductions in physical violence towards children. Moreover, we find evidence in a context where physical 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. Our results further find that health but not education conditionalities may be a factor, albeit not the only

5, number of adult males and females, urban indicator, age of household head, education of household head, indicator for household head currently married or divorced or separated. Children controls include: average age, proportion of children who are female, proportion of children in school.

one, but schemes without conditionalities may have 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.

CRediT authorship contribution statement

Mo Alloush: Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Conceptualization. **Emily Conover:** Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization. **Susan Godlonton:** Writing – review & editing, Writing – original draft, Methodology, Formal analysis, Data curation, Conceptualization.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2025.103694.

Data availability

The authors do not have permission to share data.

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