Income 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 cash transfer program in Peru to test whether changes in income affect 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 7-12%. Our findings imply that alleviating income constraints 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 the effect of easing poverty 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 TWFE results are robust to using the estimator proposed by Borusyak, Jaravel and Spiess (2021).

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 7-12% at the district level. Our results are robust to focusing on different sub-samples or time periods, population weighting, and adding a variety of controls. We use an event study type specification to explore whether the dynamics of the average effect and find an immediate response that persists. In some specifications, we also find suggestive evidence that parents substitute towards other less violent forms of discipline such as forbidding activities the children like and verbal admonishment. Moreover, we find that the strongest reductions in hitting happens in households with younger children. We also explore the effect of the program on fathers and find similar—yet less precise—results. If the negative effects of corporal punishment on children outweigh any corrective benefits from discipline, our findings imply that alleviating income constraints may have additional second-order benefits through the reduction of harsh physical forms of parenting discipline practices.

As far as we are aware, our paper is the first to study the causal effect of poverty alleviation on parental use of corporal punishment. Recent related work that is closest to what we study examines the effect of tax benefits on child maltreatment proxied by referrals to child protective services and placement of children in foster care (Rittenhouse, 2022). Economists have sought to theoretically explain difference in parenting practices across different socio-economic environments. Weinberg (2001) sets up an agency problem to models parent-child interactions where differences in child-rearing 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 incentives such as corporal punishment. More recently, Doepke and Zilibotti (2017) set up an overlapping generations model to try reconcile differences in parenting practices across time and countries. In their model, parents choose their preferences for their child as well as take (costly) actions that can either expand or restrict the set of economic decisions their child can make. The result is multiple equilibria that capture different parenting styles. More authoritarian parenting, which includes the use of corporal punishment, emerges as an equilibrium outcome when social and occupational mobility are low.

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, that we are aware of, 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., 2019). 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.

We additionally contribute to the literature by 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 (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. Finally, given the long-term negative consequences of corporal punishment, our results suggest an added benefit and potential mechanism through which poverty alleviation programs affect children in the long-run.

2 Background and Data

2.1 The *Juntos* Program

Peru's *Juntos* is a conditional cash transfer program for poor households with children under the age of 19 and for poor pregnant mothers. 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%. The monthly transfer during our period of study was around USD 60 (around 15%) of poor households' monthly consumption (Sánchez, Meléndez and Behrman, 2020; Andersen et al., 2015). The transfer is typically paid to mothers and the conditionality depends on children receiving comprehensive health and nutrition care, children's school enrollment and attendance rate of at least 85%, and having a national identity card. 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). We provide more detail for each of these in Appendix C.

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 standard 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, 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).[1]

ENDES also collects information on household participation in a range of social protection programs, including *Juntos*. For the first 3 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.[2]

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 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. *Ver-*

[1]The twelve categories in the order in which they appear are: slapping, verbal admonishment, forbidding things they like, 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.

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

bal admonishment is at the top of the list and is used by approximately 74% of mothers. This is followed by *forbidding things 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.

In Figure 1(A) we show how the most severe form of corporal punishment (hitting) 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.[3] In Figure 1(B), we show the incidence of the most common discipline practices by wealth 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 relation with wealth. Overall, mothers in the bottom wealth deciles have the highest self-report of any type of corporal punishment.

While understudied in economics, the corporal punishment of children is well-studied in the social psychology literature. In general, this literature finds that 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 Table A1 in the Appendix. 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 ex-

[3]Note 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.

planatory 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.

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:

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

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.[4] Finally, ϵ_{idpt} is an unobserved error term.

Our coefficient of interest in the 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*,[5] and thus this coefficient is the weighted average of within-district average effects which has some households receiving *Juntos* and others who are not.

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 B2, that pre-treatment data does not suggest violations of the

[4]For 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.

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

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).[6] 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 heterogenous and/or dynamic effects (De Chaisemartin and D'Haultfoeuille, 2022).[7]

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 show that none of the (98) LATEs receive a negative 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

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

[7] 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, Jaravel and Spiess, 2021). In our case, over half of the districts in our sample are never treated.

effects are then calculated for each treated group which are then used to 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.[8]

Instrumental Variable Strategy—The estimand of the TWFE model above captures an effect akin to intent-to-treat at the district level (Field, 2007), since, as seen in Figure B1, approximately 40% of mothers in the district receives *Juntos* when the district becomes eligible. 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*. The DID-IV approach has been used in studies over the years yet the econometric literature on this method is sparse.[9] 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 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. A more detailed discussion of this can be found in Appendix D.

4 Main Results

Our results 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 certain activities.

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.[10] Our aim here is to exclude "forbidden" comparisons that may bias our results in the presence of heterogeneous effects (De Chaisemartin and D'Haultfoeuille, 2022). In column (3), we restrict

[8] 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).

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

[10] This excludes districts that started receiving *Juntos* early, sometime between 2005-2010, which are likely poorer on average than the rest of the sample.

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. Finally, in column (4), we add mother, children, and household-level controls.[11]

Panel A presents results on the extensive margin, it examines whether relaxing the income constraint 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. The TWFE results with different samples suggest that there is a reduction in hitting (panel B). Our preferred specification with controls (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 activities and other forms of punishment 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). We similarly find estimates that suggest that hitting is reduced in districts that start receiving *Juntos*. In our preferred specification with controls (column 6), we find a point estimate that is larger (5.2 *pp*) than that of column (4). Introduction of *Juntos* in an district results in a 12 percent reduction in hitting on the district. Using this estimator gives consistently statistically significant positive estimates of changes in another, less violent, form of punishment—forbidding some activities.

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.[12] 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*.

These estimated effects are on average among all mothers in the district some of whom

[11] In 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.

[12] We 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 B2.

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.

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 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 the fact that 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). Estimating the IV results are nevertheless useful in providing an upper bound for the true effect.

4.1 Robustness

We consider several robustness specification 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. In all three cases, we continue to observe a decline in hitting. Moreover, applying the sample weights we find even stronger evidence in support of substitution of punishment from more severe methods such as hitting and slapping to less severe such as forbidding certain activities.

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 (4) of Table A3. We estimate a negative effect, but we do lose statistical significance in the smaller sample.

Another concern is related to small sample sizes within some treated districts which could be driving these results.[13] To address this, in Figure B3, 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 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 Figures B3 shows, the TWFE coefficient is similar across these varying sample restrictions which mitigates these potential concerns.

4.2 Additional Outcomes

Substitution— In panel A of Table 2 we find a reduction in punishment in the previous month in districts that become eligible for *Juntos*. This reduction, along the extensive margin, implies that our findings could be driven by parents who stop punishing the kids, or by a combination of these parents along with those who are reducing the most severe punishment strategies. In column (1) of Table 3, we limit the sample to parents who reported engaging in any punishment in the last month. We find very similar results among these parents who continue to use punishment, suggesting that the observed effect is both among these parents and those who switch out of punishment altogether.

Fathers— We also examine how fathers respond to the introduction of *Juntos*. Women answer the same series of questions regarding child disciplinary practices about 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 their child's 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 of their disciplinary practices; 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 the disciplinary

[13] ENDES data are not representative at the district level.

practices. We replicate Table 2 but for the father's corporal punishment practices. These results are presented in column (2) of Table 3. The pattern of results for father's is similar to the results observed among mothers. Changes to the most severe punishment, hitting, is aligned with our results for mothers, however, the coefficient is consistently smaller and is not statistically significant.

These results for fathers and the similar results among mothers who punished in the last month help mitigate concerns regarding response biases among women. If women in districts eligible for *Juntos* are more likely to under-report punishment strategies perhaps thinking *Juntos* monitoring may view this negatively, then our results could be driven by this treatment induced measurement error. However, ENDES is not connected to the *Juntos* program, and individuals are more likely to accurately self-report stigmatized actions of others. Also, those most likely to respond to this perceived incentive would likely report no punishment. Thus, our results in the restricted sample (column (1) of Table 3) that excludes these women is also reassuring.

Age of Children— We further investigate how punishment strategies vary by the age of the children. We split children into three age-groupings: under 7, between 7 and 14, and between 14 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.

Table 3 columns (3) to (5), 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 slightly 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 younger children where households seem to switch much more to restricting certain activities in these households.

5 Discussion and Conclusion

In this paper, we study the effect of a conditional cash transfer program on the discipline practices of parents. 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 an 7% reduction in average hitting rates in the district. This effect comes from a combination of some parents reducing overall punishment, and others reducing hitting. We further find some indication that mothers may be switching towards other less violent forms of punishment.

Studies in economics have focused on the effect of cash transfers on one form of intra-household violence—intimate partner violence. Meta-analyses of these studies suggest that the 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. While causality is difficult to establish especially when considering the long-term consequences of corporal punishment, 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 Grogan-Kaylor, 2016; Larzelere and Kuhn, 2005).[14]

Our results capture the overall effect of *Juntos*, which include both the cash transfer component and the conditionality. There are several pathways through which the estimated impacts could occur. Mothers may change their parenting practices due to the alleviated stress of financial pressures. Alternatively, the child could be changing their behavior resulting in less punishment by the parent. We view both of these as the effect of relaxing the income constraint. Moreover, *Juntos* requires child health check-ups and school attendance which offers other mechanisms. For example, it may be that higher school attendance rates reduce time with the parent leading to less punishment overall.[15] Regardless of the specific channel we view this as the overall effect of the program likely working through a poverty-related mechanism.

[14] A 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).

[15] In results not shown, we examine heterogeneity of our results leveraging variation in interview date and school holidays. We find that hitting declines with number of days at school, but we find no differential impact of *Juntos* when interacted with the number of days at school. This suggests the school mechanism seems unlikely to be driving the results.

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Tables and Figures

TABLE 1: Mother-level Descriptive Statistics

	Full Sample N=145,291		Study Sample N=84,237	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Panel A. HH Characteristics				
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
Panel B. HH Head Characteristics				
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
Panel C. Mother's Characteristics				
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
Panel D. Child Characteristics				
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
Panel E. Outcomes				
Verbal admonishment	0.737	0.440	0.716	0.451
Forbidding things 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
Panel F. Juntos Program				
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, thus the proportions reported for *Juntos* receipt, eligibility and affiliation are lower.

TABLE 2: Main Results

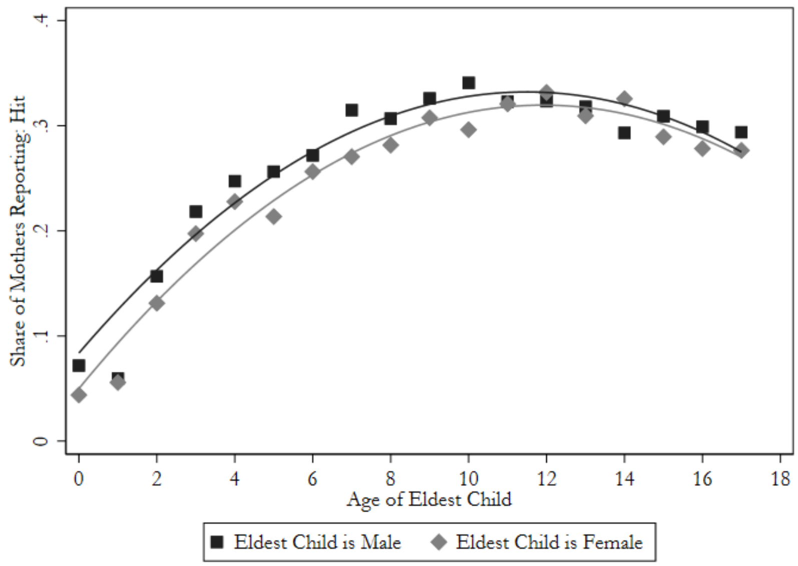
	TWFE				BJS Estimator	
	Full Sample (1)	Without Always Treated (2)	Study Sample (3)	Study Sample (4)	Study Sample (5)	Study Sample (6)
Panel A: Punished in the last month						
Ubigeo eligible for Juntos in current year	-0.020**	-0.033***	-0.035***	-0.024*	-0.047**	-0.031
	(0.010)	(0.011)	(0.012)	(0.013)	(0.023)	(0.023)
Pre-Treatment Mean	0.407	0.407	0.408	0.408	0.408	0.408
Panel B: Hit						
Ubigeo eligible for Juntos in current year	-0.029**	-0.033**	-0.045**	-0.031*	-0.065***	-0.052**
	(0.013)	(0.015)	(0.019)	(0.017)	(0.023)	(0.022)
Pre-Treatment Mean	0.418	0.418	0.413	0.413	0.413	0.413
Panel C: Slap						
Ubigeo eligible for Juntos in current year	-0.001	-0.012	-0.021*	-0.015	-0.033**	-0.025
	(0.007)	(0.008)	(0.011)	(0.011)	(0.016)	(0.016)
Pre-Treatment Mean	0.111	0.111	0.116	0.116	0.116	0.116
Panel D: Verbal admonishment						
Ubigeo eligible for Juntos in current year	-0.012	-0.014	-0.030*	-0.030*	-0.026	-0.027
	(0.011)	(0.012)	(0.017)	(0.017)	(0.019)	(0.019)
Pre-Treatment Mean	0.794	0.794	0.795	0.795	0.795	0.795
Panel E: Forbid some activities						
Ubigeo eligible for Juntos in current year	-0.005	-0.001	0.017	0.015	0.063***	0.063***
	(0.012)	(0.014)	(0.018)	(0.018)	(0.023)	(0.022)
Pre-Treatment Mean	0.347	0.347	0.361	0.361	0.361	0.361
Panel F: All other punishment						
Ubigeo eligible for Juntos in current year	0.004	0.002	-0.001	0.000	0.014	0.016*
	(0.006)	(0.006)	(0.008)	(0.008)	(0.009)	(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. *district Juntos* is defined as the district received *Juntos* in the current year. 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.

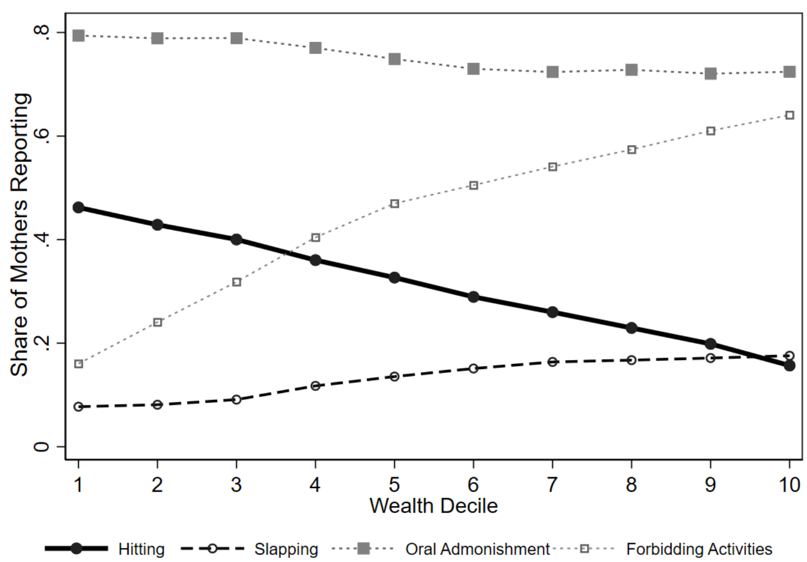
TABLE 3: Additional Outcomes

Estimator:	PLM Sample only TWFE (1)	Biological Father BJS (2)	Children Under 7 BJS (3)	Children 7 to 13 BJS (4)	Children 14 to 18 BJS (5)
Panel A: Punished in last month					
Ubigeo eligible for Juntos in current year		-0.031	-0.024	-0.026	-0.049
		(0.023)	(0.026)	(0.029)	(0.038)
Pre-Treatment Mean		0.408	0.427	0.401	0.360
Panel B: Hit					
Ubigeo eligible for Juntos in current year	-0.046**	-0.011	-0.055**	-0.055**	0.014
	(0.021)	(0.026)	(0.026)	(0.027)	(0.037)
Pre-Treatment Mean	0.509	0.372	0.423	0.455	0.407
Panel C: Slap					
Ubigeo eligible for Juntos in current year	-0.030*	-0.022*	-0.015	-0.044**	-0.094***
	(0.017)	(0.012)	(0.018)	(0.019)	(0.025)
Pre-Treatment Mean	0.140	0.068	0.131	0.088	0.084
Panel D: Verbal admonishment					
Ubigeo eligible for Juntos in current year	-0.052**	-0.025	-0.025	-0.045**	-0.079**
	(0.024)	(0.023)	(0.023)	(0.022)	(0.032)
Pre-Treatment Mean	0.789	0.804	0.795	0.797	0.816
Panel E: Forbid some activities					
Ubigeo eligible for Juntos in current year	0.043*	0.045*	0.072***	0.052*	-0.049
	(0.023)	(0.025)	(0.024)	(0.028)	(0.036)
Pre-Treatment Mean	0.393	0.304	0.351	0.367	0.335
Panel F: All other punishment					
Ubigeo eligible for Juntos in current year	0.001	0.005	-0.000	0.023*	0.042**
	(0.012)	(0.011)	(0.010)	(0.012)	(0.018)
Pre-Treatment Mean	0.078	0.059	0.071	0.071	0.075
District FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Region Province-Year FE	✓	✓	✓	✓	✓
Observations	39,130	51,398	66,590	52,689	20,885

Note: Borusyak estimator. Standard errors clustered by district in parentheses. *district Juntos* is defined as the district received *Juntos* in the current year. 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 1 refers to the sample of women who punished their children in the last month (PLM).

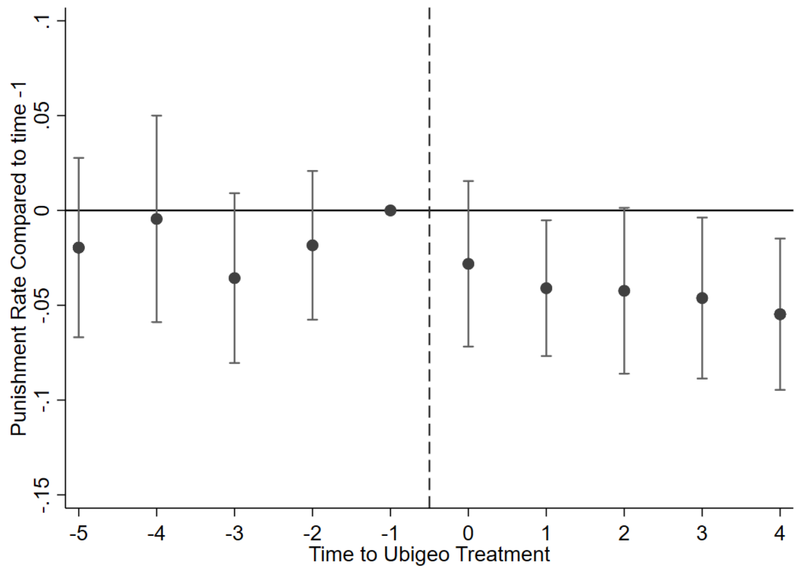


(A) Share of Mothers Reporting Hitting by Age and Sex of Eldest Child.

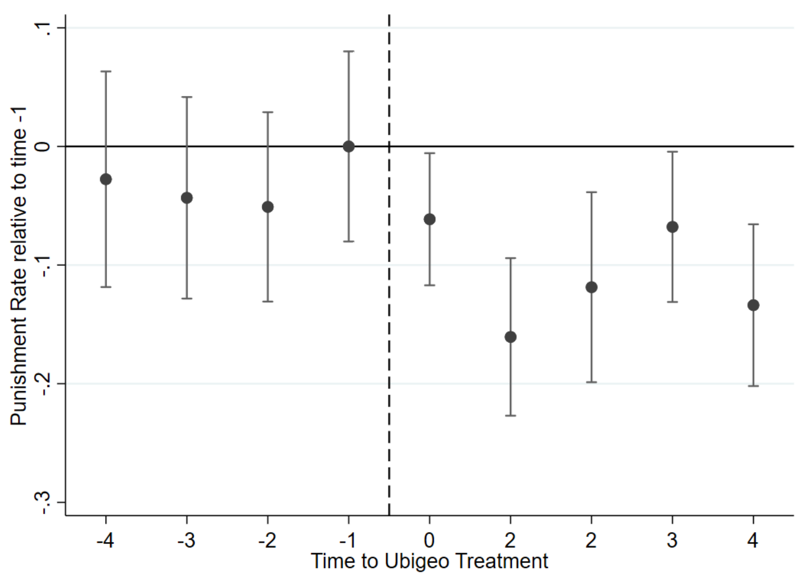


(B) Discipline Practice by Wealth.

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 and we clearly see that 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.

Appendix

A. Appendix Tables

TABLE A1: Predictors of Hitting as a Form of Punishment

	(1)	(2)	(3)	(4)	(5)
Wealth decile=2	-0.030***	-0.019**	-0.010	-0.010	-0.002
	(0.010)	(0.009)	(0.009)	(0.008)	(0.008)
Wealth decile=3	-0.065***	-0.040***	-0.025***	-0.023**	-0.003
	(0.011)	(0.009)	(0.009)	(0.009)	(0.008)
Wealth decile=4	-0.108***	-0.074***	-0.055***	-0.049***	-0.016*
	(0.011)	(0.010)	(0.010)	(0.010)	(0.009)
Wealth decile=5	-0.135***	-0.092***	-0.070***	-0.061***	-0.020**
	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
Wealth decile=6	-0.171***	-0.115***	-0.090***	-0.081***	-0.035***
	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
Wealth decile=7	-0.185***	-0.127***	-0.100***	-0.092***	-0.037***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)
Wealth decile=8	-0.216***	-0.149***	-0.119***	-0.111***	-0.050***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)
Wealth decile=9	-0.250***	-0.170***	-0.137***	-0.131***	-0.066***
	(0.012)	(0.013)	(0.013)	(0.014)	(0.013)
Wealth decile=10	-0.287***	-0.190***	-0.154***	-0.151***	-0.078***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
Mom: age		0.028***	0.016***	0.017***	0.017***
		(0.002)	(0.002)	(0.002)	(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***	-0.006***	-0.005***	-0.004***
		(0.001)	(0.001)	(0.001)	(0.001)
Mom: currently working		0.016***	0.019***	0.016***	0.014***
		(0.003)	(0.003)	(0.003)	(0.003)
Mom: divorced or separated		-0.021***	-0.013*	-0.010	-0.009
		(0.006)	(0.007)	(0.007)	(0.007)
Mom: wife beating justified		0.062***	0.064***	0.047***	0.044***
		(0.010)	(0.010)	(0.010)	(0.010)
Mom: physically punished as child		0.144***	0.143***	0.142***	0.125***
		(0.004)	(0.004)	(0.004)	(0.004)
CP necessary to educate child		0.255***	0.249***	0.249***	0.235***
		(0.008)	(0.008)	(0.008)	(0.008)
Household size			0.044***	0.044***	0.045***
			(0.002)	(0.002)	(0.002)
Number adult females			-0.057***	-0.056***	-0.058***
			(0.003)	(0.003)	(0.003)
Number adult males			-0.048***	-0.048***	-0.049***
			(0.003)	(0.003)	(0.003)
Number of children under 5			-0.026***	-0.031***	-0.030***
			(0.004)	(0.004)	(0.004)
Mean age of children in the HH			-0.004***	-0.004***	-0.005***
			(0.001)	(0.001)	(0.001)
Share female children			-0.024***	-0.023***	-0.024***
			(0.004)	(0.004)	(0.004)
Urban				-0.025***	-0.006
				(0.007)	(0.008)
Constant	0.449***	-0.153***	0.002	0.061*	-0.009
	(0.009)	(0.030)	(0.031)	(0.032)	(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)	Without Always Treated (2)	Study Sample (3)
Panel A: Punished in the last month			
Juntos affiliation	-0.042	-0.091	-0.107
	(0.054)	(0.067)	(0.091)
First Stage F-stat	149.126	95.764	47.885
Panel B: Hit			
Juntos affiliation	-0.103	-0.102	-0.240*
	(0.071)	(0.090)	(0.137)
First Stage F-stat	148.922	95.610	47.793
Panel C: Slap			
Juntos affiliation	0.009	-0.035	-0.040
	(0.040)	(0.054)	(0.090)
First Stage F-stat	148.922	95.610	47.793
Panel D: Verbal admonishment			
Juntos affiliation	-0.065	-0.054	-0.164
	(0.061)	(0.077)	(0.124)
First Stage F-stat	148.922	95.610	47.793
Panel E: Forbid some activities			
Juntos affiliation	-0.042	-0.047	-0.016
	(0.064)	(0.086)	(0.134)
First Stage F-stat	148.922	95.610	47.793
Panel F: All other punishment			
Juntos affiliation	0.004	-0.014	-0.070
	(0.035)	(0.044)	(0.066)
First Stage F-stat	148.922	95.610	47.793
District FE	✓	✓	✓
Region Province-Year FE	✓	✓	✓
Mother, child, and household controls		✓	✓
Observations	129,355	106,666	73,856

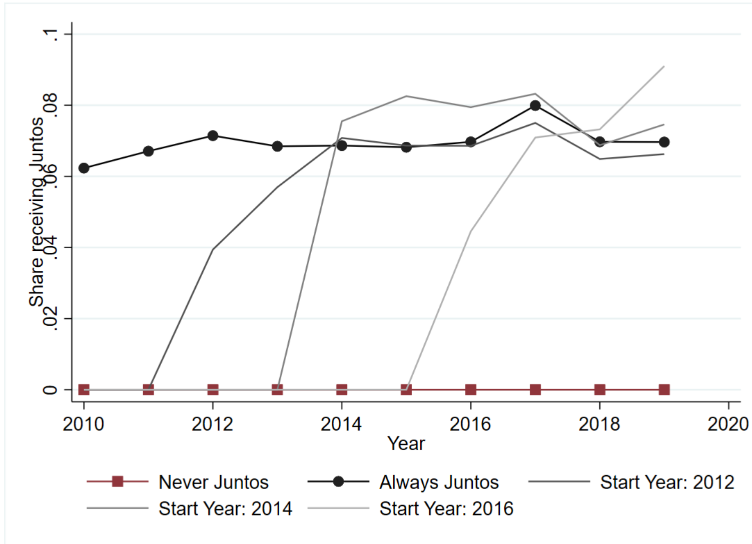
Note: Standard errors clustered by district in parentheses. *district Juntos* is defined as the district received *Juntos* in the current year. Pre-treatment mean refers to the mean in the estimating sample in district's in the year prior to *Juntos* eligibility. Controls included are as listed in the notes to Table 2. The sample in this regression is smaller than in 2 because *Juntos* participation was only asked among women with children under 5 years of age for the period 2010-2012.

TABLE A3: Robustness Checks

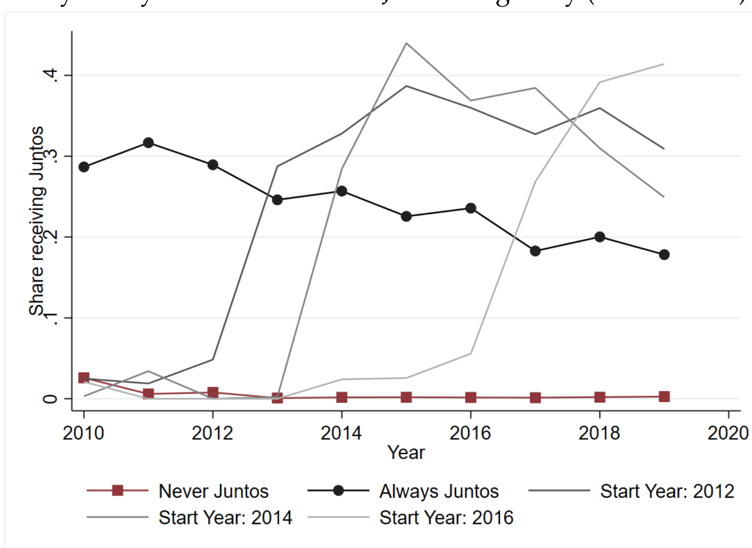
	Exclude Never Juntos districts	Population Weighted	Includes Poverty control	Never Moved
Estimator:	BJS	BJS	BJS	TWFE
	(1)	(2)	(3)	(4)
Panel A: Punished in last month				
Ubigeo eligible for Juntos in current year	-0.035	-0.057**	-0.030	-0.013
	(0.034)	(0.025)	(0.023)	(0.015)
Pre-Treatment Mean	0.409	0.409	0.409	0.410
Panel B: Hit				
Ubigeo eligible for Juntos in current year	-0.071**	-0.043*	-0.052**	-0.018
	(0.030)	(0.025)	(0.022)	(0.022)
Pre-Treatment Mean	0.412	0.412	0.412	0.428
Panel C: Slap				
Ubigeo eligible for Juntos in current year	-0.031	-0.043**	-0.025	-0.024**
	(0.024)	(0.018)	(0.016)	(0.012)
Pre-Treatment Mean	0.116	0.116	0.116	0.117
Panel D: Verbal admonishment				
Ubigeo eligible for Juntos in current year	-0.040*	-0.063***	-0.028	-0.055**
	(0.023)	(0.023)	(0.019)	(0.023)
Pre-Treatment Mean	0.794	0.794	0.794	0.794
Panel E: Forbid some activities				
Ubigeo eligible for Juntos in current year	0.043	0.068***	0.063***	0.002
	(0.033)	(0.025)	(0.023)	(0.018)
Pre-Treatment Mean	0.363	0.363	0.363	0.368
Panel F: All other punishment				
Ubigeo eligible for Juntos in current year	0.017	0.024**	0.018*	0.001
	(0.012)	(0.010)	(0.009)	(0.009)
Pre-Treatment Mean	0.068	0.068	0.068	0.071
District FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Region Province-Year FE	✓	✓	✓	✓
Observations	45,295	76,362	84,237	41,821

Note: Standard errors clustered by district in parentheses. *district Juntos* is defined as the district received *Juntos* in the current year. 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 3 includes a control for the proportion of the district in the poorest wealth quintile. Column 4 keeps only mothers always lived in their current residence.

B. Appendix Figures

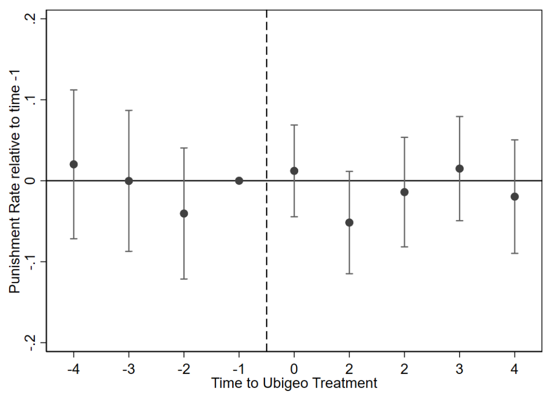


(A) Average Share (within district) of Individuals Receiving Juntos by start year of district-level Juntos Eligibility (Census data).

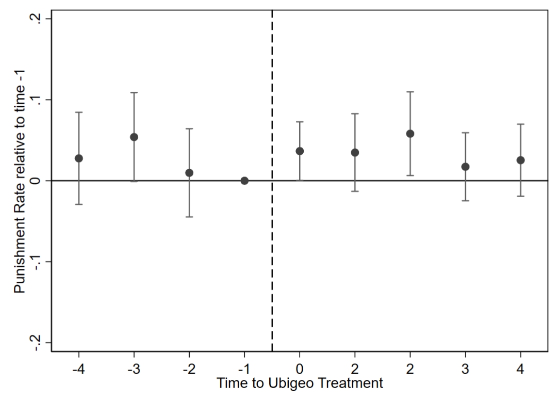


(B) Average Share (within district) of Mothers Reporting Receiving *Juntos* by start year of district-level Juntos Eligibility.

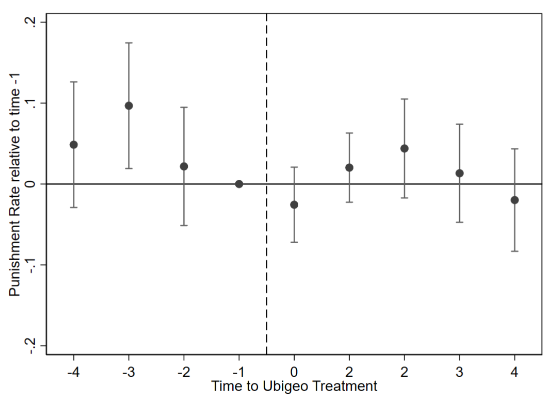
FIGURE B1: These figures show the average share of poor mothers with children who are eligible for *Juntos* within each district based on the onset year for *Juntos* at the district. Figure (A) uses census district population counts; Figure (B) uses our survey data on mothers reporting receiving *Juntos*. 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.



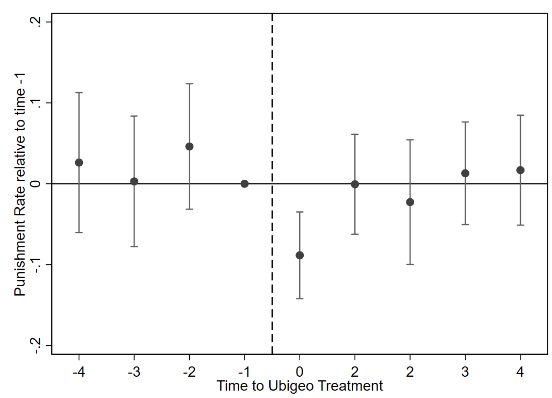
(A) PLM



(B) Slap



(C) Oral



(D) Forbidden

FIGURE B2: Event Study Figures using Borusyak, Jaravel and Spiess (2021) estimator for other measures of punishment.

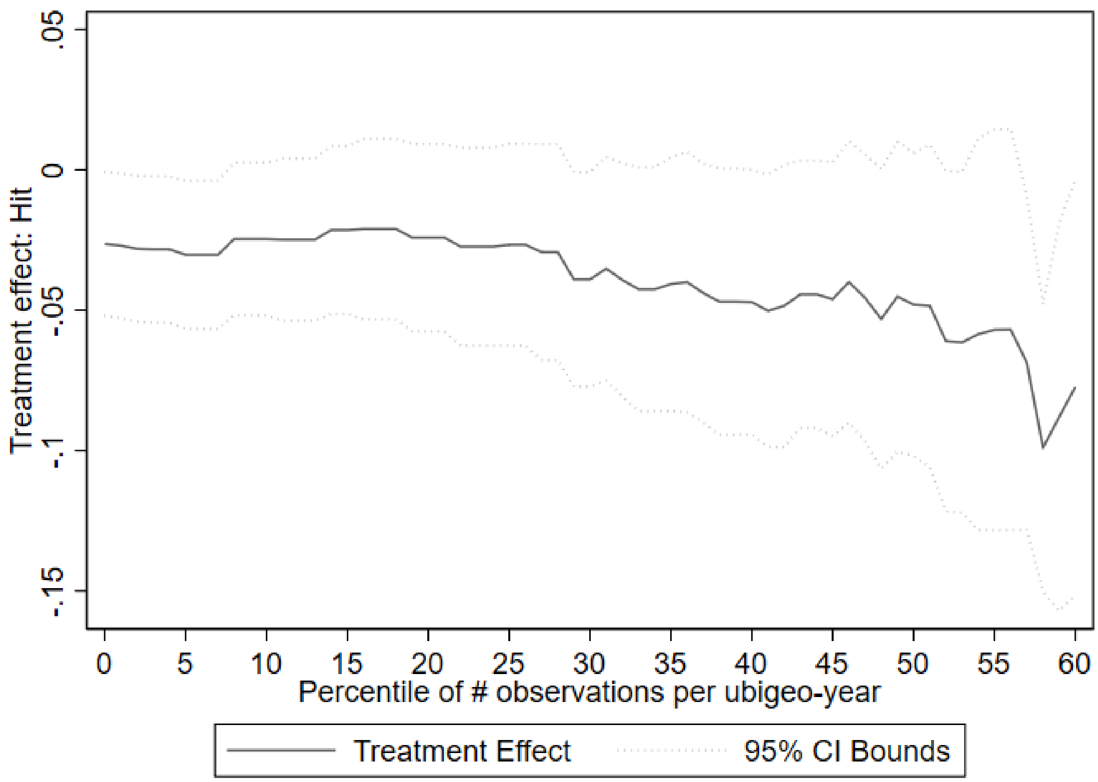


FIGURE B3: Treatment effect using TWFE estimator restricting the sample to all districts observed above the threshold percentile cutoff.

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).

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. 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 household 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 2017, *Juntos* covered 693,000 families in 1,305 of Peru's 1,943 districts (Carpio et al., 2019; Jones, Vargas and Villar, 2008).

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*).[16] 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 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 enrollment.[17]

[16] https://www.gob.pe/9242

[17] Source: https://dds.cepal.org/bpsnc/programme?id=29

D. Appendix: Instrumental Variables Approach

The estimand of the TWFE model above captures an effect akin to intent-to-treat at the district level (Field, 2007), since, as seen in Figure B1, approximately 40% of mothers in the district receives *Juntos* when the district becomes eligible. 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.[18]

The DDIV coefficient comes from the following instrumental variable setup:

$$P_{idpt} = \delta_0 + \delta_1 Jun\hat{t}os_{idpt} + X'_{idpt}\Theta + \gamma_d + \sigma_{pt} + e_{idpt} \tag{2}$$

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

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*.[19] 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*.[20] 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 (Abdulkadiroğlu 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.

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

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

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

Our empirical approach requires additional assumptions: (1) *Exclusion*—We assume 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.[21] (4) Finally, we need to assume that the treatment effect are stable over time.[22]

[21] This assumption implies that Assumptions 1,2, and 3 in De Chaisemartin and d'Haultfoeuille (2018) hold.

[22] In 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.