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Abstract
This paper studies the effects of remittances from the U.S. on child labor and school attendance in recipient Mexican households. We identify these effects using the impact of the 2008-2009 U.S. recession on remittance receipts. The methodology employed is a differences-in-differences strategy that compares households that were remittance recipients before the crisis with never-recipient households. To avoid possible selection problems, we instrument for membership in the remittance recipient group. We find that the negative shock on remittance receipts caused a significant increase in child labor and a significant reduction of school attendance.
Keywords: Child labor, International migration, Remittances, Mexico.
JEL Classification: J43; J81; O15.

Resumen
En este documento se analizan los efectos de las remesas que los hogares mexicanos reciben de Estados Unidos sobre el trabajo infantil y la asistencia escolar. Para la identificación de estos efectos se utiliza el impacto de la recesión estadounidense de 2008-2009 sobre las remesas recibidas. Se emplea una metodología de diferencias-en-diferencias que compara hogares que recibían remesas antes de la crisis con hogares no receptores. Para evitar posibles problemas de selección, se instrumenta la variable de pertenencia al grupo receptor de remesas. Los resultados indican que el choque negativo sobre las remesas causó un incremento significativo del trabajo infantil y una reducción significativa de la asistencia escolar.
Palabras Clave: Trabajo infantil, Migración internacional, Remesas, México.

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1. Introduction

In this paper we analyze the direct effects of international remittances on child labor and schooling decisions of Mexican migrant households. We exploit the impact that the U.S. recession during 2008-2009 had on unemployment rates of Mexican immigrants and, thus, on remittance receipts of an important number of Mexican households, to identify whether these households responded to the decrease in remittances by taking their children out of school and sending them to work.

Studying the impact of migration and of remittances on child labor and educational outcomes is challenging, as a consequence of the complexity of the relationships involved and of the estimation difficulties usually present in the analysis. Indeed, migration involves different effects on household decisions that may go in opposite directions. In particular, remittance receipts from family members working abroad could lead to higher school retention and lower child labor, especially if they relax liquidity constraints (Taylor, 1992; Taylor and Wyatt, 1996). This effect, however, may be offset by low perceived returns to education, given the job that children are expected to find in the U.S. if they are to migrate themselves in the future, or by the fact that migration may disrupt family life and, thus, have negative effects on school attendance (Kandel and Kao, 2001; McKenzie and Rapoport, 2010; McKenzie and Sasin, 2007). Furthermore, if by relaxing credit constraints remittances lead households to start new family businesses, an increase of child labor may also be induced. From an econometric point of view, the endogeneity of migration decisions complicates the analysis further. Indeed, both migration and child labor/schooling decisions could be simultaneously driven by community or household-level factors that may be unobservable to the researcher (see e.g. Durand and Massey, 1992; Durand et al., 1996; Taylor et al., 1996).

Several papers have studied the effect of migration and remittances on child labor and schooling in developing countries. There is evidence from Pakistan, El Salvador, and other Latin American economies that suggests that migration tends to reduce the incidence of child labor and to promote school retention (e.g. Cox Edwards and Ureta, 2003; Acosta, 2006; Mansuri, 2006; Acosta, Fajnzylber and López, 2007). Most of these studies use cross-section data and analyze long-run effects of migration or remittances. In contrast, Yang (2008), the most closely related paper to this one, studies the direct impact of

remittances using a household panel data from the Philippines. The identification approach in that paper comes from the heterogeneous exchange rate shocks suffered by diverse countries hosting Philippine immigrants during the 1997 Asian crisis. As in the other studies, the main finding is that the probability of school attendance rises and child labor decreases as the value of remittances increases.

In Mexico, the existing evidence on the relationship between migration and schooling is generally inconclusive and, in some cases, seems to contradict the results from other countries. For instance, Hanson and Woodruff (2003) show that, after controlling for observable characteristics and instrumenting for migration, the effect of migration on child schooling in rural communities is positive only for girls in households with relatively uneducated mothers. For boys, as well as for girls in households with more educated mothers, belonging to a migrant household does not seem to have an effect on schooling. Borraz (2005) suggests that the effects found for girls in households with relatively uneducated mothers in the previously cited paper are only present in very small communities (population below 2,500). In contrast, he finds no significant effects in larger villages. Using a different sample, Boucher, Stark, and Taylor (2005) are also unable to identify a significant effect of migration on human capital formation in rural Mexico. Other studies tend to suggest the presence of *negative* effects of migration on schooling in Mexico's rural communities (López-Córdoba, 2005; McKenzie and Rapoport, 2010). None of these studies, however, looks for evidence concerning a possible effect of migration or remittances on child labor decisions.

As opposed to the papers cited above, our estimation focuses only on the short term direct effect of remittances on household decisions, and not on the overall longer-run effect that migration could have on these choices. In particular, our contribution is related to the question of whether households that face a negative shock on their remittance flows sort out, in the short term, the reduction in income by increasing child labor or taking their children out of school.

Our identification strategy relies on the fact that Mexican immigrants were strongly hit by the latest downturn of the U.S. economy. As may be seen in Figure 1, Mexican immigrant unemployment rates increased significantly after September 2008, when the recession started to become more pronounced and widespread across sectors. This shock

caused some immigrants to stop sending remittances to their families in Mexico or to reduce the dollar amount sent. Indeed, the U.S. dollar value of remittances received by Mexico decreased by a sharp 20% from the second quarter of 2008 to the first quarter of 2009 (see Figure 2). In this same period, the percentage of Mexican households receiving remittances fell from 4.3% to 3.4%.[1] In terms of their income level, a sudden stop in remittance receipts should translate into a significant negative shock for migrant households, since on average 38% of their total income comes from international family transfers.[2] The large magnitude of this shock suggests that affected families may have reacted strongly in terms of child labor and school attendance decisions.

Formally, we use a differences-in-differences estimation approach, where the treatment group is composed of 12 to 16 year old children as of 2008-II in households that received remittances in that quarter. The control group is formed by children of the same ages, but that belong to households that did not receive remittances in 2008-II (before the strongest effect of the crisis on immigrant unemployment rates) nor in 2009-I (close to the lowest point of the recession). To control for the possible endogeneity of the migration decision, we use distance to the U.S. border along the 1920 rail network as an instrument for the membership in the remittance-recipient group. The use of this instrument relies on the fact that the location of the 1920 rail lines determined the early sources of migrants in the Mexico-U.S. migration history. Mainly as a consequence of the formation of migrant networks, these locations continue to be relevant sources of migrants at present (Woodruff, 2007). To eliminate biases that could arise from correlation between the 1920 rail lines and the current level of regional economic development, we control for education, health, and income levels at the municipality level in our estimations.

As a brief preview of our results, we find that the shock on remittance-recipient households caused an increase in the probability that a child works of 9.8 percentage points, from a baseline level of 15.7 percent. Moreover, it caused a decrease in school attendance

[1] These figures come from the Mexican National Occupation and Employment Survey (ENOE) conducted by INEGI in a representative sample of Mexican households. This is the database we use in this paper.
[2] This figure is computed using data from the 2008 INEGI's Income and Expenditures Household Survey (Instituto Nacional de Estadística y Geografía, 2008).

of 15.6 percentage points, from a baseline of 82.2 percent. We find that these effects are fundamentally driven by the behavior of migrant households in rural communities.[3]

Our results could shed light on the role that remittances may have to relax credit constraints. Giuliano and Ruiz-Arranz (2009), in an empirical study for 100 countries, found evidence that remittances are an alternative means to finance investment and help overcoming liquidity constraints. Hanson and Woodruff (2003) and Woodruff and Zenteno (2007) have also emphasized the role that remittances may have in this sense. Here, we show that households that experiment a disruption in remittance flows seem to be forced to take their children out of school to work.

The rest of the paper is organized as follows. Section 2 describes the data we use for the analysis. Section 3 presents some descriptive statistics. Section 4 describes the identification strategy and the results. Section 5 concludes.

2. Data

The data we use comes from the Mexican National Occupation and Employment Survey for 2008 and 2009 (Encuesta Nacional de Ocupación y Empleo; ENOE) conducted by Mexico's National Statistics Institute INEGI (Instituto Nacional de Estadística y Geografía). This is a quarterly household survey with a rotating panel structure. Every quarter, one fifth of the sample is dropped and a new fifth added, so that each household is followed for five consecutive quarters. The purpose of the survey is to collect data on the employment situation of Mexicans 12 years of age or older in rural and urban areas. The survey has a basic and an extended questionnaire. The extended questionnaire is usually applied only once a year. This extended questionnaire contains a series of additional questions over the basic version, including whether the household receives international remittances. Unfortunately, no information on the value of resources received from this source is collected.

The extended questionnaire is normally applied in the second quarter of each year. However, in the period we study INEGI chose an unusual timing to apply it. In particular,

[3] Another adjustment mechanism that households could have when faced with a recession such as the one observed in 2008-2009 could be the return of the immigrant to Mexico. Passel and Cohn (2009) and Cornelius et al. (2009), however, find that the recent crisis did not cause an increase in return migration of Mexican immigrants in the U.S. This may reflect the presence of migration costs and may also suggest that migrants may have perceived the 2008-2009 shock as temporary in terms of its effects on immigrant unemployment.

while in 2008 the extended questionnaire was indeed applied in the second quarter, this questionnaire was again applied in the first quarter of 2009. This was especially helpful for our analysis for two reasons. First, we obtained the relevant information we needed for each household for both 2008-II, a point before the large increase in Mexican immigrant unemployment rates was observed and when remittances reached a maximum, and for 2009-I, when the deepest phase of the recession was taking place, immigrant unemployment was already high and remittances were at their lowest point (see Figures 1 and 2). Second, the early application of the extended questionnaire in 2009 allowed us to obtain information for that year on two fifths of the 2008-II sample (instead of on only one fifth). This effectively increased by two the sample size we could use for the analysis.

In relation to the sample we use, the 2008-II ENOE reports information for 315,876 persons 12 years of age or older living in 106,170 households. We restrict our sample to those households in their first and second interview in 2008-II, since these are the ones we can observe again in 2009-I (given the five quarters panel structure of the survey). We focus on children aged 12 to 16 in the 2008-II wave. We are able to identify 658 children in remittance recipient households in 2008-II that we can follow across the two survey waves.[4] For the control group, we identify 13,950 children in households that did not receive remittances in any of the two periods.

A possible concern with the use of this survey, as is the case with most panel data surveys, is attrition. Our results could be biased if attrition is correlated with some variables that also affect our outcome variables. Although we cannot directly test for this, we find that attrition among children belonging to households receiving remittances in 2008-II is around 13 percent, which may be considered to be within a reasonable range and turned out not to be statistically different from attrition of children in non-remittance recipient households. Attrition among all respondents is 16%, and attrition at the household level is around 12%. However, we do observe that attrition is higher for children who work (16%, as compared to 13% for those who do not work). If it is the case that children in households that were affected by a decrease in remittances are more likely to work and leave the sample for this reason, then our estimates could be downward biased.

[4] A household is considered to be receiving remittances if at least one member (12 years of age or older) reported receiving economic aid from someone abroad in 2008-II.

3. Descriptive Statistics

In Table 1 we present the main characteristics of children aged 12 to 16 in 2008-II and of their corresponding households. We show results for two groups: Children in households that did not receive remittances in 2008-II nor in 2009-I (column a) and children in all households that received remittances in 2008-II (column b). The left panel contains information for the overall sample. We observe that children in the never-recipient group are equally likely to work than those belonging to recipient households, but are significantly more likely to attend school. There are no significant differences between these groups in terms of their mean age or gender. However, there are important differences related to household characteristics across groups. First, non-recipient households have a higher labor income.[5] Furthermore, in this group the household head is less likely to be female, is on average more educated, younger, and more likely to be married. Moreover, remittance recipient households, when compared to non-recipients, are more likely to be located in small rural villages (under 2,500 inhabitants) and to live in a municipality with lower health, education, and income indexes.[6]

The right panel of Table 1 presents results restricting the sample to rural households, defined as those located in towns with population under 15,000. Note that rural children are more likely to work, when compared to the overall sample. In the rural sample, never-recipients are also more likely to attend school than remittance recipients. The differences on household characteristics mentioned for the whole sample go in the same direction for the rural sample.

4. Estimation of the effects of a negative shock on remittances

In this section we implement a differences-in-differences (DiD) approach to explore the effect of a negative shock on remittance receipts on child labor and school attendance.[7] With this purpose, we define a treatment group composed of children aged 12 to 16 whose households received remittances in 2008-II and a control group formed by children from

[5] A higher labor income does not necessarily mean that these households are richer. The survey we use does not capture other types of non-labor income nor the value of household assets.
[6] These indexes are used to construct the human development index published by the UNDP.
[7] A person is considered to be working if in the week previous to the survey he or she participated in some type of economic activity (production of goods or services) for at least one hour, with or without pay; or if he or she has a job but worked for zero hours for being in a vacation or sickness period.

households that did not receive remittances in 2008-II nor in 2009-I (never-recipients). The DiD estimator will capture the differential effect of the crisis on children from remittance recipient households relative to children in non-recipient households. While we do not observe the value of remittances received by each household, we do observe that 63% of the children in our sample in households receiving remittances in 2008-II saw this source of income totally interrupted by 2009-I. Furthermore, given the negative impact of the U.S. recession on employment levels of Mexican immigrants (Figure 1), it seems natural to expect that some households still receiving remittances by 2009-I may have faced a reduction in their value.[8]

The simple DiD procedure yields an unbiased estimate of the change in child labor and school attendance due to a negative shock on remittance receipts if both treatment and control groups reacted to the crisis in the same way, except for the behavior associated to the change in remittances. We acknowledge, however, that this assumption may fail if recipients are different from non-recipients on some unobservable variables; that is, if households are selected into migration. Therefore, as will be explained below, we implement an instrumental variables estimation to address this concern.

4.1. Differences-in-differences estimates

Table 2 presents the mean values of the outcome variables (child labor and school attendance) for both the control and treatment groups in the 2008-II and 2009-I waves. The DiD estimator is equal to the difference across waves of the difference between treatment and control groups. Taking the difference between the two waves for the treatment group gives us an estimate of the effect of remittances on the outcome variables plus the effect of any other seasonal or non-seasonal shock that affects the outcomes of both groups, such as the economic crisis or a time trend. The difference between waves for the control group provides an estimate of such additional non-remittance related factors. Therefore, differencing the estimate across waves for the treatment group with that of the control group should offer an estimate of the effect of remittances.

[8] Some remittance recipient households may have benefitted from the peso depreciation during the crisis. This could have increased the value of their remittance receipts in pesos, even if they decreased in dollar terms. Clearly, this effect could bias our results towards zero.

At baseline, 14.6% of children in the control group were working. The figure for the treatment group is 15.7%. Between the two waves, both groups increased their level of child labor: The control group by 1.6 percentage points and the treatment group by 3.5 percentage points. The simple DiD estimator is equal to 1.9 percentage points (not statistically significant). This would seem to suggest that on average there was no reaction in terms of child labor to the effect of the crisis on remittances. School attendance for the treatment and control groups, and the associated DiD estimation, are also presented in Table 2. There was a reduction in this indicator for both groups, and the decrease was larger for the treatment group. However, the DiD estimate again suggests that the additional reduction due to the change in remittances is not statistically significant.

We next include child and household characteristics in the estimation, in order to control for observable variables that could affect our outcomes of interest. We therefore estimate the following equation:

$$y_{it} = \alpha + \beta\, Remit_i + \gamma\, Crisis_t + \delta\, Remit_i \cdot Crisis_t + \varphi\, X_i + \varepsilon_{it} \qquad (1)$$

where *Remit* is a dummy variable that takes the value of one if the child belongs to a household that received remittances in 2008-II (treatment group) and zero if the child belongs to a household that did not receive remittances in 2008-II nor in 2009-I (control group); *Crisis* is a dummy that takes the value of one for 2009-I and zero for 2008-II; and *Remit·Crisis* is the interaction of the previous two dummies. In this context, the coefficient δ yields the DiD estimator. X_i is a series of control variables related to child and household level characteristics measured at 2008-II. The controls we include are the gender and age of the child, a dummy for villages with population under 2,500 persons, the number of household members, the number of household members under 18 years of age and under 5 years of age, and several characteristics of the household head (gender, years of schooling, age, and a dummy to identify whether he/she is married). For some specifications we also include total household labor income at baseline, excluding the child's if she works.[9]

[9] Household labor income may be an important determinant of whether a child is sent to work or taken out of school when facing a negative shock in remittance receipts, and therefore could be an important control. However, although we consider the value at baseline (not affected by the crisis), including it in the regression may cause some endogeneity problems. We therefore present estimates with and without this control.

As before, we study two outcome variables, labeled as y_{it} in Equation (1): Child labor and school attendance. We have two observations (t=2008-II and t=2009-I) for each child i (aged 12 to 16). Note that, since child labor and school attendance are measured as dummy variables, Equation (1) corresponds to a Linear Probability Model for these two outcomes.

The results of estimating Equation (1) with OLS are presented in Table 3. We propose three different specifications, each of which includes additional controls with respect to the previous. On the left hand side panel we present the DiD estimates of the effect of remittances on the probability that the child works. The first column corresponds to the case without controls and, therefore, the coefficient is the same as the one reported in Table 2.[10] We obtain a coefficient of 0.02, not statistically significant under any of the specifications. Likewise, we do not find a significant effect on school attendance (columns 4 to 6).[11]

4.2. Instrumental variables specification

As mentioned above, our previous identification strategy relies on the assumption that the control and treatment groups behaved similarly in response to common shocks that took place between 2008-I and 2009-II. It is likely that there is selection into migration, which implies that families with and without international migrants are not alike in terms of unobservables and may have therefore reacted differently to non-remittance related shocks during the period we analyze. If migrants, for example, care more about keeping children in school (given a school attainment objective) or if they have a higher discount factor, they will have a lower propensity to take their children out of school in case of a reduction in labor income, and our estimates would be biased downward. Another situation in which the linear probability estimates are downward biased arises if migrants particularly dislike child labor. To deal with biases from the possible endogeneity of migration, we instrument

[10] Standard errors in the estimation of Equation (1) are clustered at the household level and may therefore differ from those in Table 2.

[11] The coefficients on some of the control variables show patterns that seem to be consistent with prior expectations. For example, children are more likely to start working during the period of reference if they are male. Also, the probability of working increases with age and is higher for small villages (population under 2,500). Gender, age, and being located in a small village also enter significantly in the schooling regressions, but with negative signs. Additionally, the economic crisis seems to have had an overall negative effect on school attendance.

for the membership to the remittance recipient group, which is represented by *Remit* in Equation (1).

Our instrumental variables approach relies on the facts that early migration flows were closely associated to the then existent rail lines, and that current sources of migration are highly correlated with the original ones. At the early stages of the Mexico-U.S. migration history, during the first half of the Twentieth Century, Mexicans were recruited to work in the U.S., and the recruitment process and journey to the north took place along the rail line (see Massey et al., 2002; Woodruff and Zenteno, 2007). Two temporary guest worker programs played an especially important role in reinforcing this route of migration and in promoting the creation of migrant networks. The first *Bracero* program started in 1917 and the second in 1942, both with the aim of alleviating the shortage of workers in the U.S. as a consequence of World Wars I and II, respectively (Martin, 1998). As a result, the 1920 rail network determined the location of the original sources of migration. In this context, it turns out that regional migration rates have shown highly persistent patterns. Indeed, locations that were important sources of Mexican migrants at the early stages of the Mexico-U.S. migration history continue to be so at present (Woodruff, 2007). Migrant networks may play an important role in this persistence, by lowering the migration costs of individuals located in regions with high historical emigration rates (Munshi, 2003; McKenzie and Rapoport, 2007). As a consequence of the above, variables measuring past migration rates or the determinants of historical migration flows may serve as good instruments for current migration flows or remittance receipts.

Several authors in the Mexico-U.S. migration literature have exploited these insights to construct relevant instrumental variables to control for the possible endogeneity of migration. For example, McKenzie and Rapoport (2010), Hanson and Woodruff (2003) and Borraz (2005) use the historic state-level migration rates as an instrument for current migration. Other authors, such as Woodruff and Zenteno (2007) and Demirgüç -Kunt et al. (2010), have exploited the link between current migration and the placement of the 1920 rail lines. We implement the latter strategy, following closely Demirgüç -Kunt et al. (2010). In particular, to construct our instrument we use the distance from each municipality to the rail network as it existed in 1920 plus the distance along the railroad from that point to the U.S. border, adjusted by relative travel costs between rail and land

transportation. Following Coatsworth (1972), these authors argue that costs of rail travel were one-third to one-sixth as much as those for land transportation and, therefore, they estimate that the distance from the municipality to the rail should be multiplied by five before being added to the distance along the rail network to the border, to obtain a total distance from a municipality to the U.S. For migrants near the border and far from the railroad the direct distance from their municipality to the U.S. may be the relevant one. Given these arguments, the instrumental variable we use is the minimum between (1) five times the distance from the municipality to the rail network as it existed in 1920 plus the distance from that point to the border with the U.S. and (2) the direct distance from the municipality to the border. The variable is labeled *Distance* and is expressed in hundreds of kilometers.

Apart from being correlated with remittance receipts, the instrument should meet the exclusion restriction. This is, it should not be correlated with child labor or school attendance, except through its effect on remittances. A possible concern is correlation between distance in 1920 (as defined above) and the current level of development of the municipality. Indeed, municipalities close to the rail network could have developed faster, and current development levels in turn could have an impact on the incidence of child labor or on school attendance. To address this possible concern, we control for recent levels of development at the municipality level using the education, health, and income indexes that compose the human development index estimated by the UNDP for 2005 (see PNUD, 2009). These indicators have the advantage of reflecting long term development and therefore are less likely to affect short run decisions on child labor and schooling. Moreover, they are not contemporaneous to the period we are focusing on.

For the econometric estimation of Equation (1) we take into account the fact that the endogenous variable *Remit* is binary. Therefore, the procedure we implement has the following stages: We first estimate a probit model of the endogenous variable *Remit*, on the *Distance* variable and on the control variables (X). We then obtain the fitted probabilities, which we call *Remit-hat*. Finally, we estimate Equation (1) by 2SLS using *Remit-hat* as an instrument for *Remit* and the interaction *Remit-hat·Crisis* as an instrument for the interaction term *Remit·Crisis*. This yields a just-identified system. This procedure does not

require the probit stage to be correctly specified and the usual 2SLS standard errors and test statistics are asymptotically valid (see Procedure 18.1 in Wooldridge, 2001).

The first panel of Table 4 presents the results of the probit model for different choices of additional controls. As expected, the coefficient of *Distance* suggests a negative and significant relationship between this variable and *Remit*, indicating that the further away from the U.S. border along the 1920 railway, the lower the probability of receiving remittances. The table also presents the first stage results from the 2SLS estimation. Our equation includes two right hand side endogenous variables (*Remit* and the interaction *Remit·Crisis*). We therefore report results for the first stage associated to the endogenous variable *Remit* as well as for the first stage of the interaction *Remit·Crisis* in the second and third panels of the table, respectively. The coefficients of *Remit-hat* from the first stage of *Remit* and of *Remit-hat·Crisis* from the first stage of *Remit·Crisis* are statistically significant and have the correct sign in both cases. The F statistics of the first stage regressions seem to be sufficiently high (above 10) and the Kleibergen-Paap F statistic for weak identification exceeds the Stock and Yogo critical values, so we reject the null that the instrument is weak.[12]

The results from the instrumental variables estimation are presented in Table 5. We find a large and significant effect of remittances on the incidence of child labor. When we do not control for household labor income (excluding the child's if she works) nor development indexes, the increase in the probability of child labor as a response to a decrease in remittances is of 11.4 percentage points (column 1). This estimate decreases to 9.8 percentage points when all the additional controls are included (column 4).[13] With respect to school attendance, we find a large and significant reduction in the probability that the child goes to school as a consequence of the remittances shortage. Without household income and development measures as controls, the effect of the shortage of remittances is

[12] Kleibergen-Paap F statistics are estimated using the ivreg29 routine for Stata (Baum et al., 2010), which also reports the Stock and Yogo critical values.

[13] Results do not show important differences if only the education and health controls are included and not the income index. Additionally, including only per capita income (ppp adjusted, also published by UNDP) does not affect the results.

estimated to be of 15.4 percentage points. The results remain mostly unchanged once those controls are included.[14]

Given that in Mexico child labor seems to be especially prevalent in rural communities, where migration rates to the U.S. also tend to be high, and that an important number of studies about migration has focused on rural areas, it may be useful to assess to what extent our results are driven by the behavior of rural households. With that purpose, we performed the instrumental variables estimation procedure for two subsamples: i) children living in villages with a population under 15,000 persons (rural subsample); and ii) children in towns with a population of 15,000 persons or more (urban subsample).[15] Results from the probit and the first stage regressions indicate that the instrument is still valid.[16] As may be noted in Table 6, the effect of remittances on child labor we found before is indeed driven by households in rural communities. In particular, the estimates suggest that, in the rural environment, the interruption of remittances had a significant effect on child labor (12.3 percentage points in the estimation with all controls). In contrast, the estimates for child labor in urban environments turned out to be statistically insignificant. As for the impact on school attendance, we find a significant effect for the rural sample, and a large but not statistically significant effect for the urban sample. Possible reasons why these effects may be more apparent in the rural environment are: i) rural households may find it easier to send children to work, since productive land is generally accessible nearby; or, ii) rural households are more credit constrained than urban households. This last possibility is consistent with alternative evidence. Indeed, according to data from the Mexican Family

[14] These IV coefficients are substantially larger than those from the OLS estimation. The IV estimation corrects not only for omitted variable bias, but also for a possible measurement error problem in the right hand side endogenous variable, which would lead to an attenuation bias in OLS estimates (Angrist and Krueger, 1999).

[15] The number of observations is the following. For the full sample, 13,950 in the control group and 658 in the treatment group. For the rural sample, 4,651 in the control group and 377 in the treatment group. For the urban sample, 9,299 in the control group and 281 in the treatment group. Each child appears twice in each regression, once for each survey wave.

[16] For the rural sample with all controls (including household labor income not considering the child's if she works and the development index components), the coefficient on *Distance* from the probit estimation is -0.0098*** (0.002). From the first stage of *Remit*, the coefficient on *Remit-hat* is 1.39** (0.144), and the F statistic is 46.6. From the first stage of *Remit·Crisis*, the coefficient on *Remit-hat·Crisis* is 1.1*** (0.079), and the F statistic is 98.41. The Kleibergen-Paap F statistic for weak identification is equal to 46.51. The number of observations is 10,056. For the urban sample, also including all controls, the results are as follows: The coefficient on *Distance* from the probit estimation is -0.0119***(0.002); from the first stage of *Remit*, the coefficient on *Remit-hat* is 1.374***(0.224), and the F statistic is 19.1; from the first stage of *Remit·Crisis*, the coefficient on *Remit-hat·Crisis* is 1.089*** (0.118), and the F statistic is 46.18; the Kleibergen-Paap F statistic for weak identification is equal to 18.73. For this sample the number of observations is 19,160.

Life Survey 2005 (Rubalcava and Teruel, 2008), only 7.4% of rural migrant households have access to formal credit, while 25.6% of urban migrant households do.[17]

5. Conclusions

Previous papers in the literature have tried to identify the long term effects of migration and remittances on schooling in Mexican households and, in many cases, have found results that suggest a negligible or, in some cases, a negative effect. In this paper we focused on the short term effects of remittances on both school attendance and child labor, isolating our estimation from other type of longer-run effects that migration could have on these household choices. We used the 2008-2009 global economic crisis as an exogenous event that had a negative impact on remittance flows from the U.S. to Mexico to identify whether recipients react to this negative shock by increasing child labor or taking children out of school. The methodology consisted of a differences-in-differences strategy, where the treatment group was composed of children (aged 12 to 16) in remittance recipient households at the beginning of 2008. The control group was composed of children from never-recipient households. To account for possible endogeneity biases, we instrument for belonging to the treatment group with the distance from their municipality to the U.S. border along the 1920's rail network. We found that the negative shock on remittances caused an important increase in child labor and a decrease in school attendance of a similar magnitude.

A possible interpretation of our findings is that remittance-recipient households are credit constrained, since they seem to face the negative shock on remittances by sending their children to work. We leave for future research the task of conducting a more structural approach to identify whether the effects we found in this paper are related, for example, with the level of financial depth (bancarization) in the locality where households live, and whether migrant households are truly credit constrained. The results from that research could be relevant from a policy point of view.

[17] A household is considered to have access to formal credit in this survey if at least one household member has a credit card or has ever received a loan from a bank or a non-bank financial institution ("caja de ahorro").

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Table 1
Characteristics of children aged 12 to 16 and of their households in 2008-II by remittance status of the household

	OVERALL SAMPLE			RURAL SAMPLE		
	Non Recipients	Remittance Recipients	Difference	Non Recipients	Remittance Recipients	Difference
	(a)	(b)	(a-b)	(c)	(d)	(c-d)
Child characteristics						
Works (%)	14.6	15.7	-1.1	20.1	17.0	3.1
Attends school (%)	87.7	82.2	5.4 ***	83.2	78.0	5.2 ***
Male (%)	51.5	50.3	1.2	52.4	49.9	2.5
Age (years)	14.0	14.0	0.0	13.9	14.0	-0.1
Household characteristics						
Number of household members	5.3	5.2	0.1 *	5.8	5.4	0.4 ***
Number of household members under 18	2.8	3.0	-0.2 ***	3.3	3.2	0.0
Number of household members under 5	5.1	5.0	0.1	5.5	5.1	0.4 ***
Female household head (%)	18.3	55.2	-36.8 ***	13.9	51.7	-37.8 ***
Schooling of household head (years)	7.5	3.8	3.8 ***	5.4	2.6	2.8 ***
Age of household head (years)	45.1	47.1	-1.9 ***	45.2	47.2	-2.1 ***
Married household head (%)	83.8	78.0	5.8 ***	88.1	82.2	5.9 ***
Total household labor income ('000 pesos)	7.5	4.0	3.5 ***	5.0	2.3	2.7 ***
Per capita household labor income ('000 pesos)	1.5	0.8	0.8 ***	0.9	0.4	0.5 ***
Adjusted total household labor income ('000 pesos) [a]	7.4	3.8	3.5 ***	4.8	2.2	2.7 ***
Characteristics of municipality or locality						
Locality under 2,500 inhabs. (%)	20.0	41.2	-21.2 ***	59.9	71.9	-11.9 ***
Rural (under 15,000) (%)	33.3	57.3	-24.0 ***	100.0	100.0	0.0
Distance to border 1920 ('00 km) [b]	12.6	11.9	0.7	14.7	13.4	1.3 **
Health index[c]	0.90	0.87	0.03 ***	0.84	0.84	0.01
Education index[c]	0.84	0.81	0.03 ***	0.80	0.78	0.01 ***
Income index[c]	0.77	0.73	0.03 ***	0.70	0.69	0.01 **
Per capita annual income (ppp dollars)	10,916	9,077	1,839 ***	7,275	6,713	562 ***
Observations	13,950	658		4,651	377	

Sample: Children 12 to 16 years of age in the 2008-II wave of ENOE. Non-recipients includes households that did not receive remittances in 2008-II nor in 2009-I. Rural households are located in villages with a population smaller than 15,000.

[a] Excludes the child's labor income if she works.

[b] Minimum distance between five times the distance from the municipality to the rail network as it existed in 1920 plus the distance from that point to the border with the U.S. and the direct distance from the municipality to the border.

[c] Indexes used to compute the UNDP Human Development Index.

*** p<0.01, ** p<0.05, * p<0.1

Table 2

Simple Differences-in-differences estimations

Outcome variable: child labor

	2008-II	2009-I	Dif
Control	**0.146**	**0.162**	**0.016***** (0.004)
Treatment	**0.157**	**0.191**	**0.035*** (0.021)
Dif	**0.0106**	**0.029****	**0.019** (0.020)

Outcome variable: school attendance

	2008-II	2009-I	Dif
Control	**0.877**	**0.839**	**-0.037***** (0.004)
Treatment	**0.822**	**0.771**	**-0.052**** (0.022)
Dif	**-0.054*****	**-0.069*****	**-0.014** (0.020)

Note: The treatment group is composed of children aged 12 to 16 in 2008-II that belong to households that declared receiving remittances in 2008-II. The control group is composed of children aged 12 to 16 in 2008-II in households that did not receive remittances in 2008-II nor in 2009-I. Number of children: 658 for the treatment group and 13,950 for the control group. Standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 3

DiD results for child labor and school attendance

	Dep. Var.: Child Labor			Dep. Var.: School attendance		
	(1)	(2)	(3)	(4)	(5)	(6)
Crisis	0.016***	0.018***	0.018***	-0.037***	-0.039***	-0.039***
	(0.004)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Remit	0.0106	-0.023	-0.024	-0.0544***	-0.001	-0.002
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)	(0.016)
Remit · Crisis	0.019	0.02	0.02	-0.014	-0.013	-0.013
	(0.017)	(0.017)	(0.017)	(0.013)	(0.013)	(0.013)
Male		0.126***	0.126***		-0.029***	-0.029***
		(0.005)	(0.005)		(0.005)	(0.005)
Age		0.056***	0.056***		-0.069***	-0.069***
		(0.002)	(0.002)		(0.002)	(0.002)
Locality with under 2,500 inhabs.		0.057***	0.055***		-0.042***	-0.043***
		(0.007)	(0.007)		(0.008)	(0.008)
Number of household members		-0.011**	-0.01**		-0.009*	-0.009
		(0.005)	(0.005)		(0.005)	(0.005)
Number of members under 18		0.024***	0.023***		-0.016***	-0.016***
		(0.004)	(0.004)		(0.004)	(0.004)
Number of members under 5		0.004	0.004		0.002	0.003
		(0.004)	(0.004)		(0.005)	(0.005)
Female household head		-0.029***	-0.029***		0.024**	0.024**
		(0.010)	(0.010)		(0.011)	(0.011)
Schooling of household head		-0.008***	-0.007***		0.014***	0.014***
		(0.001)	(0.001)		(0.001)	(0.001)
Age of household head		-0.001***	-0.001***		0.002***	0.002***
		(0.000)	(0.000)		(0.000)	(0.000)
Married household head		-0.019*	-0.018*		0.047***	0.047***
		(0.011)	(0.011)		(0.012)	(0.012)
Adjusted household labor income			-0.0006*			-0.0003
			(0.000)			(0.000)
Constant	0.146***	-0.619***	-0.62***	0.877***	1.718***	1.717***
	(0.003)	(0.030)	(0.030)	(0.003)	(0.033)	(0.033)
Observations	29,216	29,216	29,216	29,216	29,216	29,216
R-squared	0.001	0.102	0.102	0.004	0.141	0.141

Sample: Children aged 12 to 16 in 2008-II. The Table presents the Linear Probability estimation of Equation (1). *Remit* is a dummy equal to one if the child belongs to a household that in 2008-II declared receiving remittances (treatment group), and it is equal to zero if the child belongs to a household that did not receive remittances in 2008-II nor in 2009-I (control group). *Crisis* is a dummy variable that takes the value of one for 2009-I and zero for 2008-II. The coefficient on interaction term *Remit ·Crisis* is the DiD estimate of the impact on the outcome variables (child labor and school attendance) of the negative shock on remittances due to the 2008 economic crisis.
Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 4
First stage estimations

	Probit Dep. var.: Remit				**First stage of *Remit*** Dep. var.: Remit				**First stage of *Remit·Crisis*** Dep. var.: Remit·Crisis			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Distance	-0.006***	-0.007***	-0.0095***	-0.0098***								
	(0.001)	(0.001)	(0.002)	(0.002)								
Remit-hat					1.392***	1.494***	1.336***	1.419***	0.137***	0.177***	0.116***	0.148***
					(0.134)	(0.132)	(0.126)	(0.125)	(0.045)	(0.043)	(0.041)	(0.040)
Remit-hat ·Crisis					-0.0051	-0.0057	-0.0052	-0.0055	1.1***	1.125***	1.089***	1.11***
					(0.008)	(0.006)	(0.007)	(0.006)	(0.069)	(0.069)	(0.068)	(0.068)
Crisis					0.0006	0.0005	0.0005	0.0004	-0.0042	-0.0054**	-0.0038	-0.0047*
					(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)	(0.003)	(0.003)
Male	-0.04	-0.04	-0.04	-0.04	0.0001	-0.0001	-0.0001	-0.0003	0.0001	-0.00003	-0.00001	-0.0001
	(0.028)	(0.029)	(0.029)	(0.029)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Age	0.0042	0.004	0.0035	0.0033	0.0001	0.0002	0.0001	0.0002	0.0001	0.0001	0.0001	0.0001
	(0.010)	(0.010)	(0.010)	(0.010)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Locality with under 2,500 inhabs.	0.44***	0.4***	0.35***	0.33***	-0.0155**	-0.0194***	-0.0103	-0.0131	-0.0072*	-0.0094**	-0.0048	-0.0064
	(0.032)	(0.033)	(0.038)	(0.038)	(0.007)	(0.007)	(0.008)	(0.008)	(0.004)	(0.004)	(0.004)	(0.004)
Number of household members	-0.1***	-0.08***	-0.09***	-0.07***	0.0029	0.0041	0.0019	0.003	0.0049**	0.0053**	0.0043**	0.0047**
	(0.025)	(0.025)	(0.025)	(0.025)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)
Number of members under 18	0.08***	0.05***	0.06***	0.04**	-0.0011	-0.0019	0.00003	-0.0007	-0.0014	-0.0017	-0.00076	-0.0011
	(0.018)	(0.019)	(0.019)	(0.019)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Number of members under 5	0.03	0.03	0.02	0.03	-0.0014	-0.0018	-0.0011	-0.0014	-0.0039**	-0.0039**	-0.0036*	-0.0036*
	(0.022)	(0.022)	(0.023)	(0.023)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Female household head	1.18***	1.19***	1.2***	1.2***	-0.06***	-0.08***	-0.05***	-0.06***	-0.03***	-0.04***	-0.02**	-0.03***
	(0.040)	(0.040)	(0.040)	(0.041)	(0.020)	(0.019)	(0.019)	(0.018)	(0.010)	(0.010)	(0.009)	(0.009)
Schooling of household head	-0.04***	-0.03***	-0.04***	-0.03***	0.001**	0.001***	0.001	0.001**	0.001**	0.001***	0.0004	0.001**
	(0.003)	(0.004)	(0.003)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Age of household head	0.01***	0.01***	0.01***	0.01***	-0.0005*	-0.0006**	-0.0005	-0.0006*	-0.0003*	-0.0003**	-0.0002	-0.0003*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married household head	0.79***	0.8***	0.79***	0.8***	-0.04**	-0.05***	-0.03**	-0.04***	-0.02**	-0.02***	-0.01*	-0.02***
	(0.046)	(0.046)	(0.046)	(0.046)	(0.016)	(0.016)	(0.015)	(0.015)	(0.008)	(0.008)	(0.008)	(0.008)
Adjusted household labor income		-0.02***		-0.01***		-0.0001		-0.0002		-0.0001		-0.0001
		(0.003)		(0.003)		(0.000)		(0.000)		(0.000)		(0.000)
Health index			0.58	0.5			-0.0156	-0.0207			-0.0067	-0.0098
			(0.408)	(0.409)			(0.075)	(0.075)			(0.038)	(0.038)
Education index			-1.86***	-1.81***			0.0923	0.106			0.043	0.0507
			(0.386)	(0.387)			(0.088)	(0.088)			(0.044)	(0.044)
Income index			-0.46	-0.29			-0.0046	-0.0012			-0.0024	-0.0004
			(0.324)	(0.327)			(0.055)	(0.055)			(0.027)	(0.028)
Constant	-2.86***	-2.87***	-1.43***	-1.54***	0.0374	0.0484*	-0.0293	-0.0297	0.0202	0.0266**	-0.0116	-0.0117
	(0.178)	(0.178)	(0.297)	(0.299)	(0.026)	(0.026)	(0.051)	(0.051)	(0.013)	(0.013)	(0.026)	(0.026)
Observations	29,216	29,216	29,216	29,216	29,216	29,216	29,216	29,216	29,216	29,216	29,216	29,216
R-squared					0.119	0.125	0.12	0.125	0.137	0.143	0.139	0.143
F statistic					56.86	67.4	58.93	66.89	135.9	138.6	140.4	142.3

	(1)	(2)	(3)	(4)
Kleibergen-Paap F statistic for weak identification (tests both instruments simultaneously)	53.44	63.75	55.99	64.09

Note: First stage results for the 2SLS estimation of Equation (1) where *Remit* is instrumented with *Remit-hat* and *Remit·Crisis* with *Remit-hat·Crisis*. *Remit-hat* is obtained from the probit estimation of *Remit* on *Distance* and controls *X*. *Remit* is a dummy equal to one if the child belongs to a household that in 2008-II declared receiving remittances (treatment group), and it is equal to zero if the child belongs to a household that did not receive remittances in 2008-II nor in 2009-I (control group). *Crisis* is a dummy variable that takes the value one for 2009-I and zero for 2008-II. The education, health, and income indexes correspond to the components of the UNDP Human Development Index.

Robust standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1.

Table 5
Instrumental variables estimation

	Dep. Var.: Child Labor				Dep. Var.: School attendance			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Crisis	0.013***	0.014***	0.014***	0.014***	-0.033***	-0.033***	-0.032***	-0.033***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Remit	-0.344***	-0.318***	-0.373***	-0.349***	0.015	0.028	0.008	0.016
	(0.069)	(0.064)	(0.071)	(0.067)	(0.074)	(0.069)	(0.074)	(0.070)
Remit·Crisis	0.114**	0.111**	0.0995**	0.098**	-0.154***	-0.142***	-0.167***	-0.156***
	(0.051)	(0.049)	(0.050)	(0.049)	(0.044)	(0.042)	(0.044)	(0.042)
Male	0.125***	0.125***	0.125***	0.125***	-0.03***	-0.03***	-0.029***	-0.029***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Age	0.056***	0.056***	0.056***	0.056***	-0.069***	-0.069***	-0.069***	-0.069***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Locality with under 2,500 inhabs.	0.069***	0.066***	0.056***	0.054***	-0.04***	-0.041***	-0.032***	-0.033***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.010)	(0.010)
Number of household members	-0.014***	-0.013***	-0.013***	-0.012***	-0.009*	-0.008	-0.01*	-0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Number of members under 18	0.027***	0.026***	0.025***	0.024***	-0.015***	-0.016***	-0.014***	-0.015***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Number of members under 5	0.005	0.005	0.004	0.004	0.002	0.002	0.002	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Female household head	0.019	0.015	0.026*	0.022	0.034**	0.03**	0.036**	0.033**
	(0.015)	(0.014)	(0.015)	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)
Schooling of household head	-0.009***	-0.008***	-0.008***	-0.008***	0.014***	0.014***	0.013***	0.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age of household head	-0.001**	-0.001**	-0.001**	-0.001**	0.002***	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married household head	0.017	0.014	0.02	0.017	0.054***	0.052***	0.056***	0.055***
	(0.014)	(0.013)	(0.014)	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)
Adjusted household labor income		-0.0007**		-0.0005		-0.0003		-0.0004
		(0.000)		(0.000)		(0.000)		(0.000)
Health index			0.278***	0.272***			0.038	0.0338
			(0.096)	(0.095)			(0.099)	(0.099)
Education index			-0.621***	-0.616***			0.401***	0.404***
			(0.095)	(0.095)			(0.108)	(0.108)
Income index			-0.041	-0.0319			-0.205***	-0.197***
			(0.066)	(0.066)			(0.069)	(0.069)
Constant	-0.646***	-0.645***	-0.347***	-0.352***	1.709***	1.711***	1.493***	1.489***
	(0.031)	(0.031)	(0.061)	(0.061)	(0.034)	(0.034)	(0.068)	(0.068)
Observations	29,216	29,216	29,216	29,216	29,216	29,216	29,216	29,216

Note: Second stage instrumental variables estimations of Equation (1). First stage results presented in Table 4. The coefficients on the interaction term *Remit·Crisis* indicate the effect of the negative shock on remittances on the variables of interest (child labor and school attendance). *Remit* is a dummy equal to one if the child belongs to a household that in 2008-II declared receiving remittances (treatment group), and it is equal to zero if the child belongs to a household that did not receive remittances in 2008-II nor in 2009-I (control group). *Crisis* is a dummy variable that takes the value one for 2009-I and zero for 2008-II. The education, health, and income index correspond to the components of the UNDP Human Development Index. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Table 6

Instrumental variables estimation for rural and urban samples

Coefficients on the interaction term *Remit·Crisis*

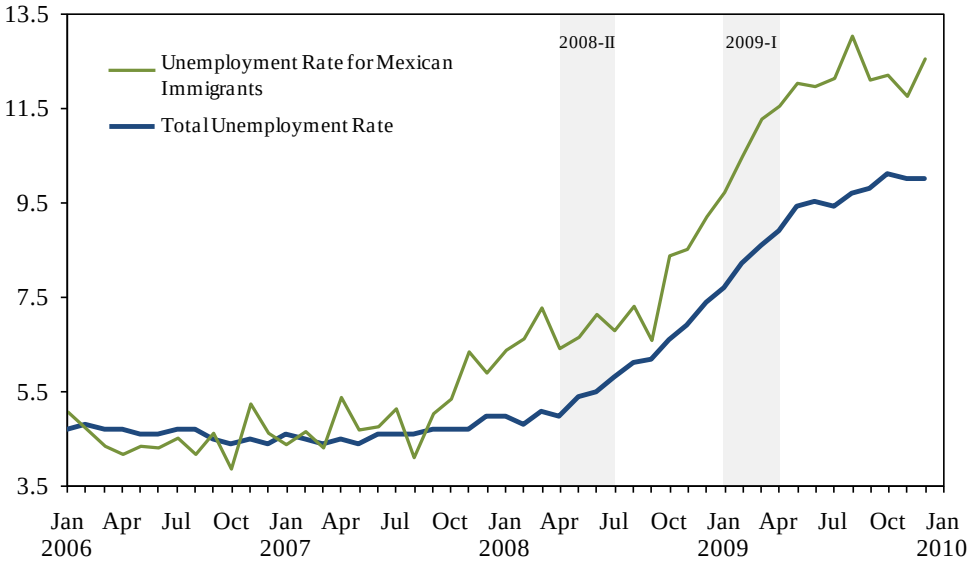
	Child Labor				School Attendance				Obs.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
Full Sample	0.114**	0.111**	0.0995**	0.098**	-0.154***	-0.142***	-0.167***	-0.156***	29,216
	(0.051)	(0.049)	(0.050)	(0.049)	(0.044)	(0.042)	(0.044)	(0.042)	
Rural Sample	0.141**	0.122**	0.134**	0.123**	-0.085*	-0.069	-0.096*	-0.079	10,056
	(0.061)	(0.056)	(0.060)	(0.056)	(0.051)	(0.049)	(0.051)	(0.049)	
Urban Sample	-0.008	-0.009	-0.018	-0.021	-0.124	-0.114	-0.124	-0.115	19,160
	(0.101)	(0.100)	(0.096)	(0.096)	(0.083)	(0.081)	(0.078)	(0.076)	
Additional controls									
Adjusted household labor income	No	Yes	No	Yes	No	Yes	No	Yes	
Development indexes	No	No	Yes	Yes	No	No	Yes	Yes	

Note: Second stage instrumental variables estimations of Equation (1). First stage results reported in footnote 16. The coefficients reported correspond to those on the interaction term *Remit·Crisis* and indicate the effect of the negative shock on remittances on the variables of interest (child labor and school attendance). *Remit* is a dummy equal to one if the child belongs to a household that in 2008-II declared receiving remittances (treatment group), and it is equal to zero if the child belongs to a household that did not receive remittances in 2008-II nor in 2009-I (control group). *Crisis* is a dummy variable that takes the value one for 2009-I and zero for 2008-II. The control variables included in all estimations are: Gender and age of the child, a dummy for villages with population under 2,500 persons, the number of household members, the number of household members under 18 years of age and under 5 years of age, and several characteristics of the household head (gender, years of schooling, age, and a dummy to identify whether he/she is married). Additionally, estimations 2, 4, 6, and 8 include household labor income excluding that of the child if she works. Development indexes indicates whether the education, health, and income indexes to construct the UNDP human development index have been included as controls or not. The rural sample is composed of children living in localities with a population of under 15,000 persons, while the urban sample considers children in localities above such threshold.
Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Figure 1

Unemployment Rates in the U.S.: Total Labor Force and Mexican Immigrants

(Seasonally Adjusted)

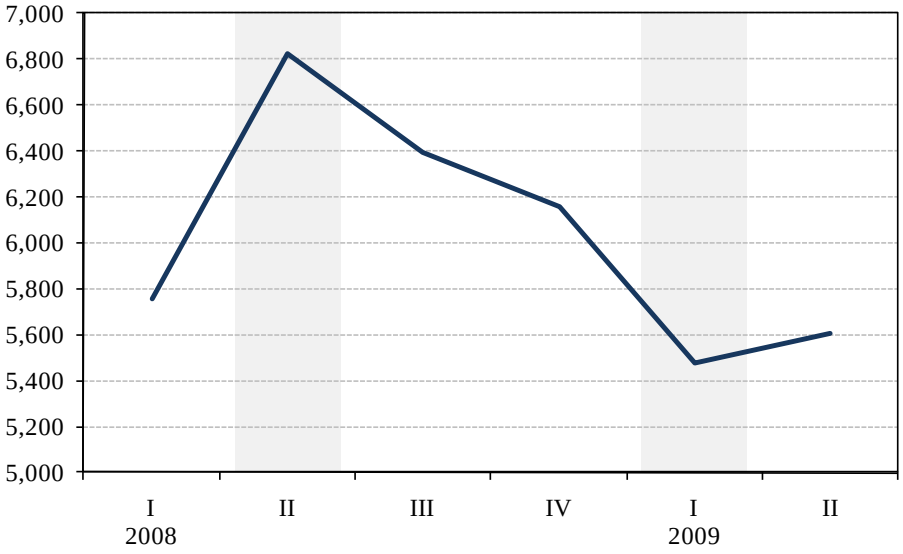


Source: Bureau of Labor Statistics. The shaded areas correspond to the second quarter of 2008 and to the first of 2009, which are the periods we use for the differences-in-differences approach we conduct in the paper.

Figure 2

Quarterly remittances from the U.S. to Mexico

Millions of dollars



Source: Banco de México. The shaded areas correspond to the second quarter of 2008 and to the first of 2009, which are the periods we use for the differences-in-differences approach we conduct in the paper.