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School-age exposure to conditional cash transfers and adult mental health: Evidence from Mexico's Progresa*

By Clotilde Mahé and Philipp Hessel

Abstract

Conditional cash transfer (CCT) programs have been shown to improve human capital and mental health in the short-run. However, it remains unclear whether those effects are long-lasting. Using data from Mexico, we test whether school-age exposure to the Progresa CCT program affects adult mental health. We exploit variation in the timing of the introduction of Progresa across municipalities before the program rollout, and compare adult mental health outcomes of cohorts that were differently exposed to Progresa transfers during expected schooling ages (5-17). Instrumental variables estimates reveal strong heterogeneity in the effect of Progresa exposure during schooling ages on adult mental health. A one standard deviation (SD) increase in per capita Progresa transfers in schooling ages implies a 1.64 and 1.87 percentage point decrease in self-reported mild anxiety among male and urban respondents during adulthood, respectively, equivalent to 0.08 and 0.07 SD effects, and a 0.69 (0.48) percentage point decrease in self-reported (severe) depression in rural areas, corresponding to a 0.06 (0.08) SD effect. In the longer term, Progresa exposure is also associated with more years of schooling, greater employability, and better self-rated health, possibly explaining the positive effect of the program on mental health we estimated.

Keywords: Conditional cash transfers, Mental health, Mexico, Latin America

JEL classifications: I10, I38

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Exposición en edad escolar a las transferencias monetarias condicionadas y la salud mental de los adultos: evidencia de Progresa en México

Por Clotilde Mahé y Philipp Hessel

Resumen

Se ha demostrado que los programas de transferencias monetarias condicionadas (CCT, por sus siglas en inglés) mejoran el capital humano y la salud mental a corto plazo. Sin embargo, no es cierto que esos efectos permanezcan a largo del plazo. Usando datos de México, evaluamos si la exposición en edad escolar al programa de CCT Progresa afecta la salud mental de los adultos. Explotamos la variación del momento de entrada a Progresa en los municipios antes de la implementación del programa y comparamos los resultados de salud mental en adultos de cohortes expuestas de manera diferente a las transferencias durante las edades escolares (5 a 17 años). Los resultados evidencian una alta heterogeneidad en el efecto de la exposición a Progresa durante las edades escolares sobre la salud mental en los adultos. Un aumento de una desviación estándar (SD, por sus siglas en inglés) en las transferencias per cápita de Progresa en edades escolares implica una disminución de 1,64 y 1,87 puntos porcentuales en la ansiedad entre los encuestados masculinos y urbanos durante la edad adulta, respectivamente, equivalente a efectos de 0,08 y 0,07 SD, y una disminución de 0,69 (0,48) puntos porcentuales en la depresión (grave) en las zonas rurales, lo que corresponde a un efecto de 0,06 (0,08) SD. A más largo plazo, la exposición a Progresa también se asocia con más años de escolaridad, mayor empleabilidad y mejor salud subjetiva, lo que posiblemente explique el efecto positivo del programa sobre la salud mental que obtenemos.

Palabras claves: Transferencias monetarias condicionadas, Salud mental, México, América Latina

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1 Introduction

Research has shown that receiving conditional cash transfer (CCT) programs in-utero, in early childhood, or in school-going ages, significantly improves schooling, cognition, learning, and socio-emotional skills in the short-run (Baird et al., 2013; Filmer and Schady, 2009; Macours et al., 2012; Ozer et al., 2009). Despite a wealth of studies on the protective and preventive immediate effects of CCTs, there is, to date, little evidence on the sustained effects of such interventions, with existing works indicate mixed impacts. In fact, several studies evidence persistent effects on schooling, but conflicting results on earnings and employment, whereas few studies find lasting effects on cognition, socio-emotional skills, or learning (Molina-Millan et al., 2019). Given that CCTs not only aim to alleviate poverty in the short term, but also to break the inter-generational cycle of poverty through human capital development, knowing whether their effects are long-lasting is essential.

One dimension increasingly acknowledged as a key determinant of future educational and career trajectories, as well as a barrier for exiting poverty, is mental health (Patel et al., 2007). While most cash transfer programs, such as CCTs, were not explicitly designed to improve mental health, several studies have revealed positive effects on psychological well-being in the short term (Ridley et al., 2020), including among children and adolescents (Zimmerman et al., 2021). Although evidence on the exact mechanisms at play is lacking, this positive mental health response might be explained by greater economic security,[1] school attendance,[2] better physical health,[3] social support,[4] or even reduced domestic violence.[5] Provided that cash transfers improved mental health, beneficiaries could then 'maximize' the positive effects of such programs in acquiring human capital and strengthening future career trajectories.

[1] Whether unconditional or conditional, cash transfers are intended to raise household income, which might directly alleviate stress associated with financial strain by ensuring economic security. Indirectly, they might reduce family conflict associated with poverty and financial stress, reducing mental health risks for all family members (Conger et al., 1994).

[2] Not only might cash transfers reduce the need for labor that place young people at risk of mental health disorders, but attaching cash transfers to education requirements is likely to increase school attainment – known to be associated with mental health problems (e.g. Currie and Stabile, 2006; Eisenberg et al., 2009; Fletcher and Wolfe, 2008; Fletcher, 2009) – as well as self-esteem and autonomy (Heckman et al., 2006) – two predictors of mental health in adolescence (Tait et al., 2003).

[3] Since poor physical and mental health are related, cash transfers might decrease psychological distress by limiting suffering from other illnesses, by, for instance, insuring regular medical check-ups and awareness (Baird et al., 2013).

[4] By allowing leisure activities, cash transfers might help foster more social interactions, which is considered a protective factor for mental well-being (Ridley et al., 2020)

[5] Among mothers, being household recipients of cash transfers is likely to limit stressful situations, and increase their sense of control (Ozer et al., 2011), thus decreasing stress among household children.

Still, the evidence on mental health response to CCTs remains geographically limited,[6] and tends to focus on short time spans. In addition, existing studies almost exclusively rely on experimental approaches, such as randomized control trials (RCTs) – with only one study assessing the mental health effects of a fully-scaled CCT program (Ozer et al., 2009). Moreover, with the exception of this article, existing studies on the effects of cash transfers on the mental health of children or adolescents focus on unconditional cash transfer (UCT) interventions (Zimmerman et al., 2021). When it comes to the potential effects of cash transfer programs on mental health, especially among children and adolescents, the distinction between conditional versus unconditional, on the one hand, and between experimental evaluations of programs during a relatively short period of time and evaluations of fully scaled programs over a longer period of time, on the other hand, is critical for at least two reasons. First, albeit considered 'gold standard' in impact evaluation, ethical, financial and logistical considerations inherent to RCTs often hinder assessing the persistence of treatment effects over time, at what age exposure to an intervention matters in explaining mental health response, or even heterogeneity in mental health effects. Most RCTs involve rather small cash transfers during a relatively short period of time, likely insufficient to generate lasting changes in mental health, especially since mental health improvements caused by cash transfers, when present, tend to decay over time (McGuire et al., 2022). In contrast, fully scaled cash transfer programs, such as Mexico's Progresa, have been implemented for nearly two decades, and have provided many families and children with continuous cash transfers over extended periods of time. Second, whereas UCT programs do not require any behavioral adjustments by definition, CCTs involve major behavioral changes, often in the form of mandatory school attendance or health checkups for children. These conditions might lead to adverse mental health effects as a result of stress because of (not) meeting these conditions and stigmatization (Layton, 2020). However, higher investments in children's and adolescents' human capital and health, as encouraged by the conditions attached to cash transfers, could translate into improved mental health in the short *and* longer-run, given the strong bi-directionality between socio-economic conditions and mental health (Ridley et al., 2020).

In this paper, we use secondary-source data to examine whether exposure to Mexico's CCT program *Programa de Educación, Salud y Alimentación* (Progresa) in expected school-

[6] See for instance Fernald and Gunnar (2009) and Ozer et al. (2009) for evidence from Mexico, Macours et al. (2012) from Nicaragua, Filmer and Schady (2009) from Cambodia, Baird et al. (2013) from Malawi, and Kilburn et al. (2019) for South Africa.

going ages affects adult mental health. This program aimed at alleviating poverty by, directly, providing cash, and, indirectly, investing in schooling and health –cash transfers were provided to eligible families, conditional on children regularly attending schools, and family members visiting health clinics for check-ups. Prior works have shown long-run improvements in adult education and employability (Molina-Milla et al., 2020; Caridad-Araujo and Macours, 2021; Parker and Vogl, 2018), and short-run decline in aggressive and oppositional symptoms, without any effect on anxiety nor depressive symptoms, among 4-6-year-old children (Ozer et al., 2011). Here, we focus on the lasting mental health effects of exposure to Progresa in childhood and adolescence (ages 5 to 17).

Identifying the (long-run impacts) of CCTs on mental health is challenging for several reasons. First, the possibility of self-selection implies that individuals with poor mental health are more likely to enroll in anti-poverty programs due to their increased vulnerability. Another major empirical challenge is the existence of reverse causality. For instance, poverty tends to raise vulnerability to mental illness, and mental illness simultaneously increases the risk of future poverty, resulting in a vicious circle of poor mental health and low socio-economic status (Patel and Kleinman, 2003; Ridley et al., 2020). A third source of possible bias is endogenous migration. On one hand, variation in coverage across a country might incite low-income families to migrate, if they were not provided with similar benefits in their current places of residence, as posited by the welfare magnet hypothesis (Moffitt, 1992).[7] On the other hand, the resulting alleviation of financial pressure, or acquired human capital, could encourage program beneficiaries to migrate, resulting in non-random sample attrition (Angelucci, 2015).

We overcome intrinsic biases in relying on plausibly endogenous observational changes in Progresa transfers by exploiting the staggered expansion of the program, comparing early beneficiaries to older cohorts too old to have received Progresa in expected school-going ages. We link administrative data on per capita Progresa transfers to survey data recording self-reported mental health of adults born between 1971 and 1990. These birth cohorts span the period in which Progresa was rolled out, and so were differentially exposed to Progresa transfers during schooling ages, depending on their place of residence and year of birth. In addition, we use the timing of the introduction of Progresa across Mexico, and the corresponding number of

[7] If individuals migrated to municipalities where benefits were offered in order to access them, observed mental health would be the result of individuals pulled to migrate to benefit from the program, rather than the intervention itself improving their well-being.

transfers as exogenous variations in Progresa transfers. Specifically, for each municipality and birth cohort combination, we predict per capita Progresa transfers during expected school going ages based on pre-Progresa poverty level in a difference-in-differences instrumental variables (DiD-IV) framework, to test whether 'exposed' cohorts – cohorts that were young enough to have been in school during or after Progresa was implemented nationwide – have better mental health than 'unexposed' cohorts – those that were too old to have received Progresa during schooling ages – in municipalities predicted to have experienced larger Progresa transfer increases, as per predetermined poverty level. Intuitively, we compare the difference in mental health outcomes of adult between birth cohorts from the same municipality exposed to Progresa for different amount of time (variation in exposure length), across municipalities with larger or smaller per capita Progresa transfers in expected school going ages (variation in exposure intensity). In practice, we estimate an intention-to-treat (ITT) effect of the absolute, long-run policy effect of increasing Progresa coverage during schooling ages in an individual's municipality of residence. Linking adult mental health with predicted Progresa transfer variations exclusively, rather than actual transfers, should get rid of the confounding influence of unobserved factors that might simultaneously determine actual Progresa transfers in schooling age and future mental health.

Several features make this setting particularly suitable for studying mental health response to conditional cash transfers. First, the availability of various sources of secondary source data sets allows combining administrative with household survey data to draw on the variation in Mexican Family Life Survey (MxFLS) respondents' year of birth and the timing of Progresa introduction in a particular municipality as per its staggered expansion. This enables assessing whether a fully scaled, nationwide CCT program received during schooling ages translates into persistent mental health, ensuring greater internal as well as external validity than previous studies (Muralidharan and Niehaus, 2017). Second, the availability of vital statistics during the study period permits connecting changes in household survey self-reported mental health to suicide-specific deaths, to identify links between conditional cash transfers and mortality. Third, while mental disorders are prevalent in Mexico, access to care remains unequal and, overall, limited (Instituto Nacional de Psiquiatría, 2010).[8] Given the number of empirical studies showing the human capital impacts of Mexico's flagship anti-poverty program, Progresa,

[8] In 2001, only 11.7% of Mexicans diagnosed with mental and behavioral disorders were provided formal or informal healthcare (Medina-Mora et al., 2003), leading to a treatment gap for major depression greater than the world median in rural Mexico (Kohn et al., 2004).

it is surprising that its long-run mental health implications have not been assessed yet. Understanding whether Progresa might have contributed to ‘breaking’ the so-called vicious circle between poverty and poor mental health in Mexico is a particularly relevant exercise as it might reveal its potentials for permanent increases in well-being, and offering an escape from inter-generational poverty. Ultimately, better understanding the link between anti-poverty measures and mental health is key for planning the coverage of mental health services in response to poverty conditions, and designing effective policies to close mental health treatment disparities. Two-stage least squares (2SLS) specifications reveal that a MXP1,000 per capita Progresa transfer increase in expected school-going ages decreases the propensity to report severe depression by 1.52 percentage point. While this effect seems, a priori, major in terms of sample mean – 0.38% of the estimation sample report severe depression – standardizing the independent variable of interest indicates a 0.24 percentage point decrease as a result of a one standard deviation (SD) increase in transfers, equivalent to 0.0390 SD effect. However, this estimate hides strong heterogeneity. A 1 SD increase in per capita Progresa transfers in schooling ages leads to a 1.64 and 1.87 percentage point decrease in self-reported mild anxiety among male and urban respondents, respectively, and 0.69 (0.48) percentage point decrease in (severe) depression in rural areas. Interestingly, although we do not obtain statistically significant effects for women’s self- reported mental health, vital statistics indicate a small but significant decrease in the likelihood to die to suicide as a result of receiving Progresa in expected school-going ages. Importantly, these modest improvements in adult mental health are driven by exposure to Progresa in adolescence – from 12 to 17 years old –rather than childhood– from 5 to 11, consistent with existing works highlighting adolescence as a critical period for developing mental and behavior disorders. We present several tests that support a causal interpretation of our results, and explore mechanisms, by documenting that Progresa transfer increases during school-going ages were associated with greater years of schooling, employability, and better health. In line with experimental and quasi- experimental studies finding that Progresa exposure in early childhood and during schooling ages improved educational attainment and employability in adulthood (Caridad-Araujo and Macours, 2021; Parker and Vogl, 2018), such human capital features might thus explain the modest improvements in adult mental health we identified.

While the precise magnitudes of these results might not be valid beyond the study context, they offer several contributions. First, our investigation advances the understanding of the long-run effects of anti-poverty programs –specifically, exposure to CCTs in school-going ages on adult outcomes. Second, we add to existing works on the relationship between CCTs and mental health outcomes. As Progresa aimed at improving the future of the next generations, and its education conditionality represented the greatest share of its expenditures, studying whether exposure in expected school-going ages did affect adult mental health is key to evaluate the efficiency of this intervention with regard to its long-run goal to break inter-generational poverty. Addressing this question is even more relevant since UCTs were found to explain most of the positive mental health effects of anti-poverty programs (McGuire et al., 2022), and that their mental health effects appear to dissipate within 12 months (Baird et al., 2013). We characterize the relationship between benefiting from CCTs in expected schooling ages, and self-reported and objective measures of mental well-being, to evidence modest and heterogeneous improvement in mental health in adulthood. While exposure during childhood and adolescence clearly leads to human capital development, our results suggest that the education component attached to Progresa could bear long-run, even if small, potentials for mental health for subsets of the eligible population. Simultaneously, findings caution against expecting unequivocally positive mental health effects for all beneficiaries, and highlight the importance of exposure in adolescence, rather than childhood. Last, by providing quasi-experimental evidence drawing on several sources of secondary data, we are able to connect changes in household survey mental health to suicide-specific deaths in vital statistics, to identify links between CCTs and mortality. Relying on observational data also permits addressing recent concerns over replicability, internal and external validity, and financial and logistical limitations of RCTs that prevent from assessing absolute, long-run effects of such interventions (Ravallion, 2020).

The remainder of the paper is organized as follows. Section 2 describes the features and implementation of Progresa. Section 3 introduces the data we use, and Section 4, the empirical strategy. Section 5 presents results. Section 6 concludes.

2 Mexico's Progresa conditional cash transfer program

Mexico's conditional cash transfer program, *Programa de Educación, Salud y Alimentación* (Progresa),[9] was introduced as a randomized pilot in 1997. In 1998, it was gradually rolled out throughout the country – rural areas were fully covered by 2000; the rest of the country enrolled at a slower rate. Within the first three years, the program benefited 300,000 to 2.5 million households; in 2004, 5 million were covered; figures that remained constant till 2010, when 6 million families were receiving the program, and then 2016, when Progresa merged with *Programa de Apoyo Alimentario* (PAL) (Yaschine, 2019). Post-pilot implementation was not randomized; it targeted in priority the poorest localities – the most *marginalized* – according to an index based on various locality characteristics.[10] Once eligible localities were selected according to their level of poverty, surveys were administered to identify which households would benefit from the program through proxy-means tests.

Targeting poor families, a cash transfer was regularly provided to women, conditional on health clinic visits for young children, and school enrolment and attendance for school-going age children. It also included social marketing – monthly talk at health clinics – to incite household investment in health and education. Its school component –scholarships and schooling equipment provided to primary and junior high school students first, and, since 2001, to high school students[11] – initially represented the greatest part of the program expenditures.[12] That human capital development was at the heart of the design of Progresa made its conditionality hard to escape. Students were allowed to repeat a grade once, but repeating twice would cancel benefits permanently. In fact, exploiting its experimental evaluation design, Progresa has been found to prevent dropout from primary to secondary school (Schultz, 2004), to decrease the tendency to repeat grades in primary school (Behrman et al., 2005), and, compared to those transitioning from primary to secondary school, to improve schooling and labor income for those exposed in-utero or in the first years of life (Caridad-Araujo and Macours, 2021). These findings have been confirmed with observational data in a quasi-experimental setting by (Parker and Vogl, 2018),

[9] In 2002, Progresa changed its name to *Programa de Desarrollo Humano Oportunidades* (Oportunidades), and again, in 2014, to *Programa de Inclusión Social* (Prospera). As we evaluate its long-run effects, we refer to its 1997 name, Progresa.
[10] This index includes measures of literacy, dwellings with running water, drainage, electricity, and dirt floor, average occupants per dwelling room, and labor force working in agriculture at the locality level.
[11] In Mexico, formal education starts with primary school (*primaria*) (grade 1), when a child is expected to be between 5 and 6 years, and ends with the last year of high school (*preparatoria*), grade 12, at ages 17-18.
[12] Up to 2008, Progresa school component represented 42 to 50% of the program total expenditures (Yaschine, 2019).

who show that benefiting from Progresa in childhood and early adolescence increased educational attainment and employability in adulthood, in particular for women.

3 Data

We compiled yearly municipality data on Progresa transfers, linked them to information on the year Progresa was introduced in a particular municipality, as well as the yearly number of household beneficiaries across Mexican municipalities, and assign these data by year of birth and municipality of residence to a survey that records adult mental health. Progresa spending data come from several sources we combine to form a panel of inflation-adjusted per capita transfers for the universe of 1995 Mexican municipalities annually from 1998 to 2008.[13,14,15] Spending data are then linked to information on the municipality-specific year of Progresa introduction, and longitudinal information on the share of beneficiary households between 1998 and 2008.[16,17] It is important to note that, although Progresa was expanded at the locality level, limitations in publicly available administrative data[18] and household survey data[19] constrain us to exploit municipality, rather than locality level variation in Progresa rollout, in line with past works, and without losing in precision (e.g. Barham, 2011; Barham and Rowberry, 2013; Parker and Vogl, 2018).[20]

Our measure of per capita Progresa transfers during childhood and adolescence, PCT_{5-17}, is constructed as the interaction of cumulative per capita Progresa transfers (in real 2010 MXP) during expected school-age years (ages 5-17) to an individual's municipality of residence m, belonging to state s, born in birth year b ($Transfers_{5-17,imsb}$) with the number of beneficiary

[13] Information on Progresa transfers to beneficiaries in a particular municipality, in a particular year, come from Mexico's statistical office, *Instituto Nacional de Estadística y Geografía* (INEGI), and were obtained through the platform *Sistema Estatal y Municipal de Base de Datos* (SIMBAD). 2000 population data at the municipality level is obtained from Mexico's 2000 population census. Per capita transfers are converted across all years to 2010 Mexican Pesos (MXP) using World Bank Consumer Price Indices.

[14] Missing values are proxied via linear interpolation. First-stage results hold when treating missing observations as such, or setting them to 0, as shown in Table A1.

[15] We use municipality boundaries that prevailed in 1995, and exclude municipalities that experienced administrative changes since 1995.

[16] Information on the year Progresa was introduced in a particular municipality, and the yearly municipality share of beneficiary households are based on administrative records of Progresa beneficiaries collected by *Consejo Nacional de Población* (CONAPO). Municipality-specific household numbers in 2000 come from Mexico's 2000 population census.

[17] A couple of observations have a share of beneficiary households slightly greater than unity. This is likely because (i) all localities in these municipalities received the intervention; (ii) due to measurement errors in population data; or (iii) lack of clarity in locality boundaries. These values are capped to one.

[18] Changes in locality identifiers were misreported, and Progresa information for small localities were absent from CONAPO data; INEGI data only provide Progresa transfers at the municipality level.

[19] Administrative locality identifiers are not provided by the MxFLS for confidentiality reasons.

[20] See for instance Parker and Vogl (2018) who show that patterns between enrolment ratio and marginality index are similar at the locality and municipality levels.

households in this municipality when s/he was 17 years old, their last year of expected schooling, divided by 2000 municipality population ($Intensity_{17,imsb}$):[21]

(1) $$PCT_{5-17,imsb} = Transfers_{5-17,imsb} \times Intensity_{17,imsb}$$

Data on adult mental health come from the last wave of the Mexican Family Life Survey (MxFLS), completed between 2009 and 2012, with the majority of respondents interviewed in 2009 and 2010. Our sample consists in MxFLS respondents born between 1971 and 1990, equivalent to 18 to 41 years old at the time of data collection. These cohorts span the staggered rollout of Progresa across Mexico, and are simultaneously old enough to have completed formal schooling by 2009. 52.61% of the sample reside in a municipality where Progresa was introduced before they reached 17 years old. The final sample includes, 461 adult respondents – 4,233 who were at least 17 years old in 1998, the year of Progresa nationwide introduction, and 5,228 who were younger – across 193 municipalities.

The MxFLS records self-reported depressive symptoms over the last four weeks by calculating a weighted score based on a series of questions – the first 20 questions of the emotional wellbeing module – following Calderon-Narvaez's (1997) approach, tailored to the Mexican context. The resulting mental health score is generated by summing the values attached to the answers of each question; it ranges from 20 to 80.[22] The higher the score, the greater a respondent's depressive symptoms. To ease interpretation, we standardize this mental health score, and construct three binary variables taking unity if a respondent reports (i) mild anxiety (score equal or greater than 36), (ii) (clinically) depressed (score equal or greater than 46); or (iii) severely (clinically) depressed (score equal or greater than 66); and 0 otherwise.

In addition, the MxFLS provides individual, household, and parental details on demographic and socio-economic characteristics. This allows exploring heterogeneity in mental health responses, controlling for migration, or examining mechanisms at play. In particular, it identifies and tracks all migrants, internal or international, even those who permanently left Mexico. Later, we will thus estimate specifications on a sample of respondents who never

[21] To ease interpretation, let's take an example. Marta was born in 1991, and resides in San Antonio de la Cal, Oaxaca, where Progresa was introduced in 1999. Assuming that she never migrated, she was exposed to Progresa from eight to 17 years old. During those nine years, the municipality received a total of per capita Progresa transfers of real MXP378. In the year Marta turned 17, her last year of expected schooling, there were about 18.62% household beneficiaries in this municipality. Consequently, Marta would have a PCT_{5-17} of per capita real MXP70. In contrast, Pedro, born in 1971, and residing in the same municipality, could not have been exposed to Progresa during expected schooling ages, from 1976 to 1988, as the intervention was introduced when he should have completed high-school. As a result, Pedro would have a PCT_{5-17} of MXP0.

[22] Each question offers four possible answers: 'No' (assigned 1 point), 'Yes, sometimes' (2), 'Yes, many times' (3), or 'Yes, all the time' (4).

migrated to assess whether results hold to endogenously changing residence to access benefits, or as a result of because of receiving Progresa.

We also process 2009-2012 vital statistics registries to assess whether any mental health effects identified in the household survey might be linked to suicide-specific deaths. Not only is psychiatric disorder history – in particular depression (Hawton et al., 2013) or stress (O'Connor and Nock, 2014) – the prime factor in determining suicides (Cavanagh et al., 2003), economic vulnerability has also been linked to suicides (Iemmi et al., 2016). In fact, existing research on nationwide conditional cash transfers, close in design to Progresa, evidences a decrease in suicides in Brazil (Alves et al., 2018) and Indonesia (Christian et al., 2019). In Mexico, Progresa has actually been found to decrease rural infant mortality (Barham, 2011), as well as deaths to infectious diseases and diabetes for people aged 65 or more (Barham and Rowberry, 2013). Analyzing whether suicide-specific mortality is associated with Progresa exposure in childhood and youth will thus provide a test of the robustness of survey-based estimates.

Yearly vital statistics registries from 2009 to 2012 are provided by INEGI. They include various information on the deceased, such as state, municipality, locality of residence, date of death, birth year, gender, and three-digit level cause of death, as listed by the International Classification of Diseases 10 (ICD-10). Data are processed as repeated cross-sections, and per capita Progresa transfers in expected school-going ages are assigned to the municipality of residence and year of birth of a deceased. Our outcome of interest, death to suicide, is constructed as a binary variable taking value 1 if a death occurred by suicide (ICD-10 X60-X84 codes), 0 otherwise. As for survey data, the estimation sample is limited to deceased born between 1971 and 199, leading to a sample of 203,511 deceased, across 2,152 municipalities.

Table 1 compares adult outcomes of individuals from different birth cohorts at similar ages, and from distinct levels of municipality poverty. A municipality is considered ‘poor’ if the value of its 1990 marginalization index is above the national median, that is it is listed as ‘high’ or ‘very high’ marginalization by INEGI, equivalent to poor and extremely poor areas. Municipalities that are below the 1990 marginalization median are considered as non-poor, including those with average, low, or very low marginalization. The average per capita Progresa transfers accumulated during expected school going ages is MXP285.40 (MXP169.2) in highly marginalized municipalities, and MXP30.8 (12.10) in non-poor areas according to survey data (vital statistics). Similarly, respondents eligible to receive Progresa in expected school-going ages, that is born in 1982 or after, who were less than 17 years old in 1998, year of the nationwide implementation of Progresa, display greater per capita Progresa transfers accumulated during schooling ages of MXP116.5 (MXP79.3), on average, according to survey data (vital statistics).

In addition, estimation sample descriptive statistics indicate that, on average, 8.19% respondents report mild anxiety, 1.80% to be depressed, and 0.38% to be severely depressed. 2009-2012 death records indicate that 4.09% deaths were suicides. 7.31% respondents deemed eligible to benefit from Progresa in expected school-going ages report to suffer from mild anxiety, 1.74% to be depressed, and 0.34% to be severely depressed. These figures contrast with non-eligible adults, out of which 9.28% report mild anxiety, 1.87% depression, and 0.43% severe depression. Self-reported mental health thus seems slightly worse among non-eligible respondents. In contrast, vital statistics indicate that 5.36% deaths among eligible individuals were suicides, compared to 3.26% for noneligible individuals, likely explained by the fact that suicide rates tend to be greater in younger populations. However, survey data and vital statistics show that adult mental health outcomes are less bad in poorer municipalities, that is municipalities above the 1990 marginalization median, the primary targets of Progresa, with self-reported depression and dying to suicide significantly higher in wealthier municipalities.

4 Empirical strategy

Our goal is to identify the causal effect of Progresa exposure during expected school-age years on subsequent mental health. The correlation between Progresa transfers in an area and the adult mental health of children and adolescents who were exposed might be confounded by several factors. First, Progresa targets low-income households, and poverty is correlated with poor mental health. It follows that those taking up the program might have different mental health markers than those not benefiting from the program per se. A naïve estimation might be positively biased if individuals living in marginalized communities, more likely to access Progresa, are more likely to have poor mental health; and, reversely, if the wealthiest, less likely to be eligible and benefit from the program, are more likely to report better mental health. Alternatively, there might be a negative bias if the poorest, more likely to access Progresa, are also less likely to report poor mental health; and if the wealthiest are more likely to express mental health concerns.

Second, there might be anticipatory effects – akin to an 'Ashenfelter dip' – by which eligible households worry about accessing Progresa, resulting in a case of reverse to the mean, had we observed better mental health upon access. On the other hand, their mental health might improve before actually receiving Progresa, had the mere announcement of the introduction of Progresa in their municipalities relieved stress.

Third, variations in conditional cash transfer coverage across Mexico could incite low-income families to migrate, if they were not provided with similar benefits in their current places of residence. According to Moffitt's (1992) welfare magnet hypothesis, this would result in endogenous migration. If individuals migrated to municipalities where Progresa was introduced to access this program, their (self-reported) well-being would be the result of individuals pulled to migrate to receive transfers. Since those households self-selecting into migration tend to have income, health, and personality features significantly different from non-migrants, endogenous migration might result in additional bias. For instance, if a family at the upper end of the income, health and education distribution of still eligible households, moves to a municipality where they know they would receive Progresa earlier than in their current place of residence, we would expect household members to be in relatively good (physical and mental) health, as per the 'healthy emigrant effect'.[23] Observing better mental health as result of Progresa exposure might then be explained by a positive bias. Reversely, those less likely to migrate to access Progresa in municipalities where it was introduced earlier, might exhibit worse mental (and physical) health, and any mental health improvements we would identify could be downward biased.

Accessing Progresa might as well enable beneficiaries to migrate.[24] There is, in fact, empirical evidence confirming Progresa did increase international migration (Angelucci, 2015), but did not have any clear effect on internal migration (Stecklov et al., 2005). Fortunately, the MxFLS identifies and tracks all migrants, internal or international, even those who permanently

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[23] Migrating often involves many obstacles. Existing research indicates that only the fittest, and so healthiest, successfully emigrate. The harder such obstacles, the stronger the positive health selection (Jasso et al., 2004).

[24] First, Progresa could support beneficiary households and their working-age members in looking for work, locally or outside their communities of origin, by alleviating financial stress, if financial constraints hindered their capacity to send migrants away. Progresa transfers, and their resulting effects on budget use, could directly finance migration (Ardington et al., 2009), or affiliation could be used to finance migration indirectly, if program entitlement acted as collateral and relaxed borrowing constraints (Angelucci, 2015). Second, the monetary transfers and health conditionality are expected to improve physical and mental health, likely to enhance the productivity of working-age beneficiaries, and strengthen their ability to migrate.

moved abroad. Table 1 indicates that, on average, 14.14% of the cohorts eligible to receive Progresa in schooling ages, ever migrated by the time of data collection, when 11.10% of non-eligible cohorts did. This is likely explained by the fact that younger individuals tend to migrate more than older individuals. There is, however, no statistically significant difference in migration propensity between respondents living in poor and non-poor municipalities. While these descriptive statistics are reassuring, we will later use information on respondents' migration history to assess the relevance of this bias.

Fourth, there might be measurement errors in self-reported beneficiary status. This is particularly true for the last MxFLS wave we use: whether households receive Progresa is not reported. While such measurement errors might (only) attenuate results, adopting a quasi-experimental approach that rely on administrative records will address bias induced by measurement errors; and, as a consequence, our results should be interpreted as intention-to-treat effects.

To address these potential sources of bias, we use variation in per capita Progresa transfers in expected school-age years that might be attributed exclusively to the introduction of the program. By design, Progresa was introduced earlier, and covered a greater share of the population, in the poorest areas of Mexico, while it was introduced later in other, wealthier areas, that also experienced lower spending. We therefore combined the variation in per capita Progresa transfers with its staggered rollout that directly resulted from its objective to focus first on the most deprived areas. We treat this variation as exogenous, and use the resulting natural experiment to estimate the causal effect of per capita Progresa transfers in childhood and adolescence on adult mental health in a combined difference-in-differences-instrumental variables (DiD-IV) setting.

Close in spirit to articles studying the effects of school-going age exposure to reform-induced school spending (Jackson et al., 2016), school construction (Duflo, 2001), or conditional cash transfers (Parker and Vogl, 2018) on educational and labor market outcomes in adulthood, we quantify the relationship between Progresa exposure and adult mental health by estimating the following system of equations by two-stage least squares (2SLS):

(2) $$Y_{imsb} = \delta \widehat{PCT_{5-17,imsb}} + \Phi C_{imsb} + \theta_m + \theta_b + \theta_{sb} + \varepsilon_{imsb}$$

(3) $PCT_{5-17,imsb} = \pi(NonELigible_{1998,ib} \times MarginalizationIndex_{1990,ms}) + \Pi C_{imsb} + \rho_m + \rho_b + \rho_{sb} + \xi_{imsb}$

Where Y is the outcome of interest, a series of indicators of poor mental health in the year of the survey for respondent i born in birth year b living in municipality m, in state s. PCT_{5-17} is our measure of per capita Progresa spending during childhood and adolescence as defined in the previous section, i.e. the cumulative per capita Progresa transfers (in real 2010 MXP) during expected school-age years (ages 5-17) by municipality of residence, interacted with the share of beneficiary households in this municipality when s/he was 17 years old, their last year of eligibility to benefit from Progresa education component. C_{imsb} is a vector of individual, household, and locality controls – gender, squared age, household health insurance, household death shock, and locality safety.[25] θ_m and ρ_m are municipality fixed effects. They are included to rely exclusively on variation across birth cohorts within municipalities. θ_b and ρ_b are cohort-of-birth fixed effects. They are included to account for general underlying differences across birth cohorts, irrespective of exposure to Progresa. With birth-cohort fixed effects, the estimated changes across birth cohorts in Progresa municipalities are interpreted as relative to changes across the same birth cohorts in municipalities where Progresa was not introduced during that time. ρ_{sb} and θ_{sb} are birth cohort-state fixed effects. They are included to avoid confounding estimated effects with that of other policies that might have been overlapping our study period. ξ_{imb} and ε_{imb} are the error terms. Standard errors are clustered at the locality level.[26]

Per capita Progresa transfers during expected schooling ages, PCT_{5-17}, is the endogenous variable of interest. We predict plausibly exogenous Progresa transfers within municipalities across birth cohorts using measures of birth cohort-specific exposure to Progresa interacted with predictors of municipality-specific coverage intensity. That is, underlying this 2SLS-DiD model is a first-stage DiD model predicting changes in per capita Progresa transfers for eligible cohorts that are greater in municipalities with higher coverage intensity, i.e. municipalities considered poorer before the Progresa introduction. We leverage exogenous variation in cohort-of-birth exposure to Progresa during the nationwide rollout of the program, i.e. when Progresa was differentially introduced across Mexican municipalities.

[25] When running specifications with vital statistics, we control for gender and squared age, as well as year of death, month of death, and day-of-week of death.
[26] While administrative locality identifiers are confidential, the MxFLS provides unique identifiers that ensure distinguishing MxFLS localities.

Specifically, we interact $NonEligible_{1998,ib}$, a binary variable taking value 1 if individual *i* was 17 years old or older in 1998, the year the program was introduced nationwide, and 0, otherwise, with $MarginalizationIndex_{1990,ms}$, a municipality-specific variable informing on 1990 poverty. Individuals who turned 17 years old or were older during the year of the nationwide introduction of Progresa, 1998, should have completed secondary school by the time it was implemented. These cohorts should not have been affected by the intervention while they were at school as they were too old to receive Progresa schooling component – they are classified as 'unexposed'. Reversely, individuals who turned 16 years old or were younger in 1998 would likely have been attending primary or secondary school when Progresa was implemented – these cohorts are classified as 'exposed'.

$MarginalizationIndex_{1990,ms}$ is a continuous variable, obtained from CONAPO, measuring municipality marginalization in 1990. 1995 marginalization was used by Mexican authorities to determine the expansion of Progresa across the country, and over time. 1990 marginalization is thus assumed to directly determine the year Progresa was introduced in a particular municipality as well as how many households received it, and, as a result, for how long an individual born in a particular year *b*, living in a particular municipality *m*, would have been exposed, but not to affect mental health directly, if not through Progresa.

In sum, this model compares the difference in mental health of adults between birth cohorts from the same municipality exposed to Progresa for different amounts of time (variation in length of exposure) across municipalities with larger or smaller Progresa-related per capita spending in expected schooling ages (variation in intensity of exposure). If exposed cohorts from municipalities that experience larger program-induced coverage increases tend to subsequently experience larger improvements in mental health outcomes (relative to unexposed cohorts), the coefficient δ from equation (3) will be negative. This coefficient will not be statistically significantly different from 0 if exposure to larger Progresa-induced coverage changes (across cohorts) are unrelated to changes in mental health outcomes. As long as the timing of the introduction of Progresa is exogenous to changes in outcomes across birth cohorts within municipalities, this coefficient should uncover the causal effect of Progresa on mental health outcomes. The credibility of this research design is based on the assumption that the timing of Progresa introduction in a particular municipality – its staggered rollout – was unrelated to other municipality level changes that directly influence outcomes (irrespective of intensity). In other words, our key identifying assumption is that the Progresa transfers in childhood and youth caused by Progresa introduction within municipalities were not related to other municipality-level changes that could directly affect adult outcomes. And, as our estimation sample respondents were born between 1971 and 1990 – before Progresa's nationwide expansion (1998) – an individual's year of birth and pre-Progresa poverty are deemed exogenous with regard to length and intensity of Progresa exposure in expected schooling ages.

It is important to note that because the childhood or youth municipality of residence prior to the introduction of the program may not be the same municipality an individual lives in at the time of data collection (due to migration), and since we do not observe a respondent's actual number of years of exposure to Progresa, this coefficient is an intention-to-treat (ITT) estimate that quantifies the absolute policy effect of increasing Progresa coverage in an individual's municipality of residence. In addition, since this program targets the poor, municipality-specific poverty could explain the rollout of its expansion across Mexico. It follows that differential changes in birth cohort outcomes between municipalities with differing enrolment intensities would reflect childhood and youth exposure to poverty rather than Progresa. Our specifications should eliminate bias associated with predetermined, time-invariant municipality conditions, thanks to municipality fixed effects, but they are less likely to control for time-varying shocks: Progresa might have been introduced in a municipality when it experienced shocks that increased deprivation. If this is the case, exposure to the intervention would be associated with exposure to adverse conditions. This bias would result in worse adult mental health, which would attenuate the possibility to find positive mental health effects of the program. Although the inclusion of state-birth cohort trends should attenuate this bias, we prefer to be conservative, and interpret our estimated effects as lower bound estimates of the long-run mental health effects of the program.

5 Results

5.1 Baseline

Our model estimates the effect of Progresa exposure in expected school-going ages on adult mental health for individuals living in a particular municipality, by comparing changes in outcomes between exposed and non-exposed cohorts of birth from that municipality. The key identifying assumption is that the Progresa transfers caused by its introduction within municipalities were not related to other municipality-level changes that could directly affect adult outcomes. Under this assumption, we can test the causal effect of per capita Progresa transfers during childhood and adolescence on adult mental health by estimating flexible DiD event-study effects of the first-stage of our model by different levels of the intensity measure, predetermined poverty.

In Table 2 and Figures 1-2, we assess the relevance of our instrumental variable. First, column (1), Table 2, indicates that the instrument is a strong predictor of per capita Progresa transfers in schooling ages, with a first-stage F-statistics of 278.85. The estimated relation is negative. We interpret the instrument coefficient estimate as being non-eligible in an equally poor municipality resulting in a decrease of MXP171.6 per capita Progresa transfers in school-going ages compared to being young enough at the time of Progresa's nationwide expansion to receive Progresa in schooling years.

Second, we assess whether outcome differences are related to the year of birth. We would expect statistically significant coefficient estimates for those born in 1982 and later. In columns (2)-(4), Table 1, and Figures 1-2, we substitute the binary variable $NonEligible_{1998,ib}$ of specification (3) with a series of binary variables taking unity for each birth year of estimation sample respondents. This is akin to an event-study specification, in which 1981, the last year of non-eligibility, is defined as base category. We see that the statistical strength and the magnitude of the instrument is greater for individuals who were born in 1982 or later, that is who were eligible to receive Progresa in expected school-going ages. Column (2), Table 2 confirms that coefficient estimates of pre-eligibility birth cohorts are, jointly, not statistically different from 0.

Third, we test whether outcome differences between exposed and unexposed cohorts residing in the same municipalities – the exposure, or intensity effect – are larger for 'poor' municipalities that experienced larger per capita Progresa spending increases across exposed and unexposed cohorts. In Figure 2, we estimate the first-stage of the above system of equations, specification (3), with two subsamples above ('poor') and below ('non-poor') median 1990 marginalization index. We see that the magnitude of the instrument is greater for post-1982 individuals living in poorer municipalities, i.e. those above median 1990 marginalization, compared to individuals living in wealthier municipalities, i.e. those below the median. Although there is no ideal test of the exogeneity assumption of our instrumental variable, these event-study figures based on different levels of years of exposure, or predetermined poverty confirm the variation underlying this 2SLS-DiD approach by visually assessing the credibility of our empirical design. In addition, that coefficient estimates are of greater magnitude for eligible birth cohorts in the poorest municipalities confirm monotonicity – that our model estimates a local average treatment effect (LATE) in the poorest areas.

We now turn to the DiD-IV specifications that use the event study patterns to predict changes in school-age exposure to per capita Progresa transfers. The 2SLS specifications provide a direct estimate of the effect of Progresa transfers in childhood and adolescence on adult mental health, allowing for tests of statistical significance. The 2SLS estimated effects for all respondents are presented in Table 3, Panel B, column (1), and Figure 3 (a). The explanatory variable of interest is the per capita Progresa transfers accumulated in expected school going ages in MXP1,000. The estimated coefficient of the first row is interpreted as the change in δ_b standard deviation (SD) in the mental health score, as a result of a MXP1,000 increase in per capita Progresa transfers in an individual's school-age years. In rows (2)-(4), the estimated coefficient is interpreted as the change in $\widehat{\delta} * 100$ percentage point in the probability to report depressive symptoms, as a result of a MXP1,000 increase in per capita Progresa transfers in an individual's school-age years, interacted with the share of household beneficiaries when a respondent was 17. The excluded instrument for this transfer variable is a binary variable for a respondent's non-eligibility to Progresa in schooling ages, interacted with a continuous variable measuring a municipality's pre-Progresa marginalization. The first-stage Kleipergen-Paap F-statistic is greater than 10 in all models. For comparison purposes, we also present in Panel A estimates from OLS regression models that do not account for the possible endogeneity of Progresa transfers.

In comparison to OLS estimates, 2SLS results indicate that increasing per capita transfers decreases symptoms of depression. Coefficient magnitudes are greater, in absolute value, suggesting OLS estimates might be upward biased. However, 2SLS estimates do not appear significantly different from 0 (at 10%). We are only able to precisely estimate the impact of per capita transfers on the most extreme form of self-reported mental health, severe depression. Specifically, 2SLS estimates indicate in Panel B, row (4), that increasing per capita transfers by MXP1,000 in all 12 school-age years significantly decreases the propensity to report severe depression by 1.52 percentage points on average among all respondents. To put this effect in perspective, we re-estimate specifications standardizing the explanatory variable of interest in

Figure 3 (a). A 1 SD increase in per capita transfers leads to a significant 0.24 percentage point decrease in the probability to report severe depression. This decrease is equivalent to a 0.0390 SD effect – it is rather modest.

5.2 Heterogeneity effect analysis

Baseline estimates show that increases in per capita Progresa transfers in schooling ages improve mental health moderately. Still, these estimates are average effects; they might hide differences in the mental health response to conditional cash transfers. It has indeed been documented that people are not equal when it comes to mental health disorders. For instance, in the Americas, women are more likely depressed and anxious than men. In contrast, men are more prone to substance addiction, self-harm, and suicide (Pan-American Health Organization, 2018). Similarly, depressed men are more likely to report alcohol or drug misuses, and risk taking or poor impulse than depressed women, who are, on the other hand, more prone to depressed mood, appetite disturbance or weight change (Cavanagh et al., 2017), with the gender differences in diagnosis peaking in adolescence (Salk et al., 2017). In addition, cash transfers have recently been found to have differential effect magnitudes on child and adult health (Cooper et al., 2020), and, in particular, to reduce depressive symptoms among men, but worsen mental health among women (e.g. Prencipe et al., 2021).

In Table 3, Panels B-E, and Figures 3 (b)-(e), we present 2SLS estimates for all (column 1), male (2), female (3), urban (5), and rural respondents (6). In columns (4) and (7), we present results of 2SLS models with all respondents, in which we included as additional regressor the explanatory variable of interest, interacted with a binary variable for male or urban, respectively. 2SLS estimates with all respondents appear to hide strong heterogeneity in the mental health response to Progresa transfers. Columns (2) and (5) indicate that a MXP1,000 increase in per capita Progresa transfers during expected schooling ages decreases mild anxiety by 10.83 and 29.66 percentage points for male and urban respondents, respectively. Standardizing the explanatory variable of interest, these estimates are equivalent to a 1 SD increase in per capita transfers reducing the propensity to report mild anxiety by 1.64 and 1.87 percentage points. The estimated effect magnitudes represent 0.0794 and 0.0656 SD effects, as per corresponding subsample SDs. In addition, rural respondents, in column (6), display a 3.18 percentage point decrease in reporting depression, and a 2.22 percentage point decrease in reporting severe depression – 0.0592 and 0.0806 SD effects, respectively.

Not only do mental and behavioral disorders tend to originate in adolescence of early childhood, but disorders developed in adolescence are likely to have negative repercussions on adult physical and mental health (Evans et al., 2007; McLoyd et al., 2009). And more, experimental and quasi-experimental evidence on Mexico's Progresa highlights some heterogeneity by age of exposure with large education gains for youth who had not yet reached the primary-to-secondary transition at the time of Progresa introduction (Behrman et al., 2011; Parker and Vogl, 2018). It follows that we might expect differential mental health response to transfers depending on the age at which one is exposed.

Accordingly, in Table 3, Panels C and D, and Figures 3 (f) and (g), we present 2SLS estimates for all respondents and subsamples with a slightly different explanatory variable of interest. In Panel C, the explanatory variable of interest is the per capita Progresa transfers accumulated in expected primary school going ages in MXP1,000, that is from 5 to 11 years old, interacted with the share of household beneficiaries when a respondent was 11. In Panel D, it is the per capita Progresa transfers accumulated in expected secondary school going ages in MXP1,000, that is from 12 to 17 years old, interacted with the share of household beneficiaries when a respondent was 17. Accordingly, in Panel C (D) specifications, the excluded instrument is a binary variable indicating whether a respondent was 11 (17) years old or older in 1998, interacted with a continuous variable measuring a municipality's pre-Progresa marginalization.

While the estimated decrease in reporting depression following Progresa exposure in primary school years is close in magnitude to the one associated with exposure in secondary school ages – equal to a 0.0395 SD decrease – subsample results reveal that the modest improvement in self-reported mental health is solely explained by increases in per capita Progresa transfers in youth. Specifically, a MXP1,000 increase in per capita Progresa transfers during expected secondary school ages (12-17) decreases mild anxiety by 14.11 and 37.95 percentage points for male and urban respondents, respectively. Standardizing the explanatory variable of interest, these estimates are equivalent to a 1 SD increase in per capita transfers reducing the propensity to report mild anxiety by 1.64 and 1.87 percentage points – 0.0794 and 0.0656 SD effects. Rural respondents display a 4.25 percentage point decrease in reporting depression, and a 2.96 percentage point decrease in reporting severe depression, i.e. 0.0592 and 0.0806 SD effects.

5.3 Robustness

5.3.1 Endogenous migration

To address concerns over endogenous residential mobility – migrating in order to access Progresa, or as a result of benefiting from the program – we re-estimate all specifications restricting the sample to respondents who never migrated. Results are presented in Table 3, Panel E, and Figure 3 (h). Estimates are almost identical to those with the full sample. While marginally significant estimates disappear, Panel E confirms that, in our analysis, endogenous migration is not a major source of bias: a MXP1,000 increase in per capita Progresa transfers during expected schooling ages decreases mild anxiety by 29.96 percentage points for urban respondents, equivalent to a 0.0659 SD effect, and decreases severe depression among rural respondents by 2.44 percentage point, corresponding to a 0.0826 SD effect.

5.3.2 Vital statistics

We now assess whether the improvements in psychological well-being we found in survey data translate into suicide-specific deaths in vital statistics. Not only is psychiatric disorder history – depression (Hawton et al., 2013) or stress (O'Connor and Nock, 2014) – the prime factor in determining suicides (Cavanagh et al., 2003), economic vulnerability has also been linked to suicides (Iemmi et al., 2016). In fact, prior research on nationwide conditional cash transfers, close in design to Progresa, evidences a decrease in suicides in Brazil (Alves et al., 2018) and Indonesia (Christian et al., 2019). Importantly, Progresa has been found to decrease rural infant mortality (Barham, 2011), as well as deaths to infectious diseases and diabetes for people aged 65 or more (Barham and Rowberry, 2013).

First, using the universe of vital statistics allows validating our identification strategy with other, nationally representative data, to ensure it generalizes to all municipalities, not only those in the MxFLS. Table A2 presents first-stage event-study estimates, and Figures A1-A2, corresponding plots, with 2009-2012 vital statistics. By providing estimates qualitatively similar to those obtained with MxFLS data, they confirm the validity of our identification strategy with the universe of Mexican municipalities.

Second, analyzing whether cause-specific mortality is associated with Progresa helps identify whether conditional cash transfers connect with changes in mortality, thus providing a falsification test on the magnitude of survey-based estimates. Given the MxFLS results, and in light of the existing literature, we expect to find modest negative changes in the incidence of death to suicide as a result of Progresa exposure in schooling ages. Table 4 and Figure 4 present estimates of the system of equations (2) and (3) with pooled cross-sections of individual deaths from 2009 to 2012.

The dependent variable a binary variable taking unity if a death was due to suicide, and 0 otherwise. In addition to fixed effects for birth year, municipality of death occurrence, and state-birth year, specifications control for year, month and day of death, as well as gender and squared age. Table 7 indicates that exposure to Progresa transfers decreases the probability to die to suicide among women. Specifically, a MXP1,000 increase in Progresa transfers in expected school going ages decreases death to suicide by 2.43 percentage points in adulthood, equal to a 0.0181 SD effect. In sum, using the universe of vital statistics, we find a very modest decreases in suicide for women, an objective, and likely the most extreme form of behavioral disorders, that might not be captured by survey-based self-reported mental health measures.

5.4 Exploring mechanisms

While anti-poverty programs are not explicitly designed to improve mental health, prior research has evidenced that they might affect psychological well-being directly, by ensuring greater economic security, and alleviating financial strain. Indirectly, they might reduce family conflict, including domestic violence,[27] associated with poverty and financial stress, reducing mental health risks for all family members (Conger et al., 1994). In addition, conditioning cash transfers on school enrolment and attendance for children, as it is the case for Progresa, is likely to increase school attainment, which is associated with mental health problems (e.g. Currie and Stabile, 2006; Eisenberg et al., 2009; Fletcher and Wolfe, 2008; Fletcher, 2009), as well as self-esteem and autonomy (Heckman et al., 2006) – two predictors of mental health in adolescence

[27] Among mothers, being household recipients of cash transfers is likely to limit stressful situations, and increase their sense of control (Ozer et al., 2011), thus decreasing stress among household children (Fernald and Gunnar, 2009).

(Tait et al., 2003). Going to school regularly should also foster social interactions, a protective factor for mental well-being (Ridley et al., 2020). And, given the link between poor physical and mental health, Progresa might decrease psychological distress by limiting suffering from other illnesses, and by insuring regular medical check-ups and awareness (Baird et al., 2013).

Accordingly, we next employ MxFLS data to understand how exposure to Progresa transfers in childhood and youth affects adult mental health. We assess the effects of exogenous transfer increases on human capital, objective and subjective indicators of health, and behaviors. In practice, we estimate a series of instrumental variables specifications, as in equations (2) and (3), where the dependent variables are alternatively, in Panel A, Table 5: (1) a binary variable taking unity if a respondent is literate; (2) a binary variable taking unity if a respondent ever attended school; (3) a continuous variable measuring the number of years of schooling; (4) a binary variable taking unity if a respondent worked in the last 12 months; (5) a binary variable taking unity if a respondent believes life will improve compared to the time of data collection; (6) a binary variable taking unity if a respondent is considered risk averse, as per a survey gambling experiment, (7) standardized score of a survey test measuring intellectual quotient.

In Panel B, the dependent variables are: (1) a binary variable taking unity if a respondent has high blood pressure; (2) a binary variable taking unity if a respondent has low hemoglobin; (3) a binary variable taking unity if a respondent considered their health improved compared to last year; (4) a binary variable taking unity if a respondent has diabetes; (5) a binary variable taking unity if a respondent was victim of crime in the last 12 months; (6) a binary variable taking unity if a respondent spends time socializing; (7) a binary variable taking unity if a respondent drinks alcoholic beverages at home. The endogenous variable is per capital Progresa transfers accumulated over a respondent's expected school-going ages, interacted with the share of households receiving Progresa in their expected last year of school. The instrumental variable is a binary variable taking unity if a respondent was not eligible to receive Progresa in their expected schooling ages, i.e. born before 1982, multiplied by 1990 municipality-specific marginalization.

Results are presented in Table 5 and Figure 5. Columns (1)-(4), Panel A, indicates that a MXP1,000 increase in Progresa transfers during schooling ages increases the probability to be literate by 14.34 percentage points, to have ever attended school by 11.95 percentage points, and the number of years of schooling by almost four years, leading to a rise in the likelihood to work in the last year preceding survey data collection by 27.73 percentage points, corresponding to 0.1188, 0.1313, 0.1762, and 0.0901 SD effects, respectively. While exposure to Progresa in expected school going ages does not seem to affect expectations (5) and risk aversion (6), we observe a 0.0728 SD increase in cognitive abilities, in column (7). Panel B indicates that a MXP1,000 increase in Progresa transfers during schooling ages improves subjective health by 11.96 percentage points – the probability to consider one's health better than last year – and decreases the propensity to have diabetes by 3.13 percentage points, in columns (4) and (5) respectively, equal to a 0.0725 SD increase and a 0.0459 SD decrease. However, in the full estimation sample, there is no clear link between Progresa exposure in expected school-going ages and (risky) behaviors, such as time allocated to social activities, drinking alcohol at home, or victimization.

Tables A3-A6 and Figure A3 confirm a 1 SD increase in Progresa transfers during expected schooling ages raises the number of years of schooling by 0.1860 SD, the propensity to work by 0.1588 SD, cognition by 0.0330 SD, and subjective health by 0.1069 SD increase, and decreases the propensity to have diabetes by 0.0755 SD among men. In comparison, while women display increases in literacy and school attendance, the corresponding increases in years of schooling, propensity to work, or subjective health, are lower in magnitude – 0.1732, 0.0675, and 0.0609 SD increases, respectively. In contrast, urban respondents show changes in subjective health closer to those men experience (0.1098 SD), although increases in years of schooling and employability of lower magnitudes (0.1084 and 0.0481 SD increases, respectively). Urban respondents also present decreases in risk aversion (0.0513 SD) and in reporting high blood pressure (0.0570 SD) as a result of increases in Progresa transfers in school going ages. Last, even though the increases in literacy and school attendance identified among rural respondents do not appear to translate significantly in additional years of schooling, we observe greater employability (0.1118 SD increase), and reported health (0.1226 SD increase).

Among the many mechanisms explaining the mental health response to Progresa exposure in childhood and youth, the above exercise thus indicates that the modest improvements in mental health we estimated might partly be driven by some elements of more years of schooling, a greater probability to work, and better subjective health. These are in line with experimental and quasi-experimental studies finding that Progresa exposure in early childhood and during schooling ages improved educational attainment and employability in adulthood (Caridad-Araujo and Macours, 2021; Parker and Vogl, 2018).

6 Concluding remarks

While there is emerging research indicating that anti-poverty programs, including CCTs, might have lasting impacts, and that benefiting from such measures concurrently improves mental health, few assess whether these mental health effects persist over time. We provide novel evidence on the causal effect of exposure to a fully scaled CCT program, Mexico's Progresa, in expected school-going ages on adult mental health.

We use survey data and vital statistics, and apply quasi-experimental methods to find that higher program exposure does not have clear lasting effects on mental health in the full sample. This is in line with growing evidence casting doubt on the persistence of anti-poverty program mental health impacts (Ozer et al., 2009; Garman et al., 2022; Kilburn et al., 2019). This is a priori surprising since such interventions have been shown to have lasting, non-negligible schooling and employability effects (by design) (Attanasio et al., 2021; Parker and Vogl, 2018) – mediators that tend to be associated with mental health. Why might it be the case? A possible explanation is that our outcomes of interest capture internalizing rather than externalizing behaviors, the latter being more malleable to cash transfers and schooling conditionalities (Ozer et al., 2009). Another reason might be the presence of strong heterogeneity in the adult mental health response to Progresa. In fact, 2SLS specifications show that a 1 SD increase in per capita Progresa transfers in expected school going ages decreases the propensity to report mild anxiety among male and urban respondents by 1.64 and 1.87 percentage points, equivalent to 0.0794 and 0.0656 SD effects, and 0.69 and 0.48 percentage point decrease in self-reported depression and severe depression, respectively, in rural areas, corresponding to 0.0592 and 0.0806 SD effects. While we do not find any effects on self-reported mental health, using the universe of vital statistics registry data over a four-year period, we evidence a significant decrease in suicide among women – we estimate a 0.0181 SD decrease in in the propensity to die to suicide. Importantly, these modest improvement in adult mental health are driven by exposure to Progresa in adolescence rather than childhood, consistent with existing works highlighting adolescence as a critical period for developing mental and behavioral disorders (Paus et al., 2008).

In sum, even though CCTs are unlikely panacea for mental health challenges in developing countries, there might be non-negligible positive effects on certain groups of beneficiaries in the long term. The particularly robust mental health impact we find for urban respondents contrasts with existing evidence pointing to positive response to conditional and unconditional cash transfers in rural areas in low-income African countries (Baird et al., 2013; Angeles et al., 2019). This might be explained by the fact that urban areas offer greater work opportunities making it possible to maximize the returns on schooling induced, by design, by Progresa. And, since higher socioeconomic status is associated with better mental health, residing in urban areas might increase chances to break up the vicious poverty-mental health circle. We indeed document that Progresa transfer increases during school going ages were associated with greater years of schooling, employability, and health, improvements that likely explain the modest improvements in adult mental health we identified. However, while sustained human capital effects might be linked to the mental health improvements we estimated, schooling, employment and health dissimilarities between gender and location do not appear to match exactly the estimated differential mental health effects.

Albeit modest in magnitude, and heterogeneous across subsets of the eligible population, we conclude that anti-poverty programs might offer opportunities, even if limited, to improve mental health of beneficiaries. In showing that being exposed to CCTs in school-going ages might have lasting positive mental health effects in adulthood, we believe our results might encourage future research aimed at better understanding mechanisms at play, in order to leverage their positive mental health effects by, for instance, introducing small modifications to these interventions.

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Table 1. Estimation sample descriptive statistics, MxFLS and vital statistics

	Full sample		Non-			1990 marginalization median		
	Mean	SD	eligible	Eligible	t-test	Above	Below	t-test
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mental health score	25.1726	7.1103	25.425	24.968	3.11***	24.936	25.209	-1.26
Mild anxiety	0.0819	0.2743	0.0928	0.0731	3.49***	0.0777	0.0826	-0.59
Depressed	0.018	0.1328	0.0187	0.0174	0.46	0.0096	0.0192	-2.39**
Severely depressed	0.0038	0.0616	0.0043	0.0034	0.64	0.0032	0.0039	-0.37
PCT	0.0644	0.1598	0	0.1165	-37.86***	0.2854	0.0308	62.30***
Non-eligible	0.4474	0.4973	-	-	-	0.4099	0.4531	-2.86***
1990 marginalization	-0.9882	0.8698	-1.055	-0.9341	-6.73***	-	-	-
Male	0.4319	0.4954	0.4297	0.4336	-0.38	0.4163	0.4342	-1.19
Age	27.3858	5.7992	32.94	22.888	165.33***	26.892	27.461	-3.23***
Urban	0.5569	0.4968	0.5901	0.53	5.86***	0.1441	0.6197	-33.32***
Ever moved[a]	0.1278	0.3338	0.111	0.1414	-4.40***	0.128	0.1277	0.03
Observations	9,461		4,233	5,228		1,249	8,212	
Suicide	0.0409	0.1981	0.0326	0.0536	-23.48***	0.0335	0.042	-6.31***
PCT	0.0315	0.1194	0	0.0793	-154.91***	0.1692	0.0121	216.27***
Non-eligible	0.603	0.4893	-	-	-	0.6107	0.6019	2.66***
1990 marginalization	-1.2052	0.9235	-1.2051	-1.2052	0.02	-	-	-
Male	0.7447	0.4361	0.7316	0.7646	-16.73***	0.7208	0.748	-9.27***
Age	30.7004	5.9395	34.761	24.533	705.80***	30.761	30.692	1.74*
Urban	0.8376	0.3688	0.8379	0.8371	0.46	0.4212	0.8961	-210.71***
Observations	203,511		122,713	80,798		25,072	178,439	

Notes: Estimation sample descriptive statistics. *** p<0.01, ** p<0.05, * p<0.1.
[a]Statistics based on 9,424 non-missing observations.

Table 2. First-stage estimates, 2009-2012 MxFLS

Variables	(1)	(2)	Above 1990 marginalization index (3)	Below 1990 marginalization index (4)
1990 MI X Non-eligible	-0.1716***			
	(0.0103)			
Pre-eligibility birth cohorts				
1990 MI X 1971		-0.0093*	-0.0061	-0.0039
		(0.0053)	(0.013)	(0.003)
1990 MI X 1972		-0.0032	0.0269	0.0007
		(0.005)	(0.021)	(0.0039)
1990 MI X 1973		-0.0021	-0.0268	-0.0057
		(0.0059)	(0.0231)	(0.0042)
1990 MI X 1974		-0.0055	-0.0179	-0.0103**
		(0.0037)	(0.021)	(0.0041)
1990 MI X 1975		0.002	-0.0595	-0.0037
		(0.0049)	(0.0374)	(0.0035)
1990 MI X 1976		-0.003	-0.0103	-0.0102**
		(0.0037)	(0.0184)	(0.0044)
1990 MI X 1977		0.0007	-0.0122	0.0006
		(0.0046)	(0.0245)	(0.0043)
1990 MI X 1978		0.004	0.0347	-0.0167***
		(0.0059)	(0.0316)	(0.0047)
1990 MI X 1979		-0.0008	0.073	-0.0076*
		(0.004)	(0.0626)	(0.004)
1990 MI X 1980		-0.0018	0.0064	-0.0057
		(0.0057)	(0.0221)	(0.0046)
R-squared	0.4966	0.9148	0.9421	0.8592
Observations	13,081	13,081	1,807	11,261
F-statistics	278.85***	29.71***	66.84***	11.07***
Pre-1982 F-statistics		1.35	1.08	4.75***

Notes: The dependent variable is $PCT_{5-17,imsb}$, per capita Progresa transfers accumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence. All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household, and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 2. First-stage estimates, 2009-2012 MxFLS (continued)

Variables	(1)	(2)	Above 1990 marginalization index (3)	Below 1990 marginalization index (4)
1990 MI X Non-eligible	-0.1716***			
	(0.0103)			
Post-eligibility birth cohorts				
1990 MI X 1982		0.0489***	0.1405***	0.0178***
		(0.0064)	(0.0303)	(0.0038)
1990 MI X 1983		0.0840***	0.1347***	0.0307***
		(0.0087)	(0.0225)	(0.0047)
1990 MI X 1984		0.1037***	0.1509***	0.0491***
		(0.0096)	(0.0322)	(0.008)
1990 MI X 1985		0.1633***	0.1613***	0.0883***
		(0.0118)	(0.0552)	(0.0118)
1990 MI X 1986		0.1773***	0.1947***	0.1004***
		(0.0134)	(0.0565)	(0.0127)
1990 MI X 1987		0.2041***	0.1229**	0.1203***
		(0.014)	(0.046)	(0.0136)
1990 MI X 1988		0.2134***	0.1055**	0.1306***
		(0.0137)	(0.0484)	(0.0127)
1990 MI X 1989		0.2407***	0.1499**	0.1341***
		(0.0161)	(0.0736)	(0.0129)
1990 MI X 1990		0.2467***	0.2607***	0.1334***
		(0.0167)	(0.0669)	(0.012)
R-squared	0.4966	0.9148	0.9421	0.8592
Observations	13,081	13,081	1,807	11,261
F-statistics	278.85***	29.71***	66.84***	11.07***
Pre-1982 F-statistics		1.35	1.08	4.75***

Notes: See Table 2.

Table 3. Baseline and heterogeneity effect analysis estimates, OLS and 2SLS estimates, 2009-2012 MxFLS

Variables	All (1)	Men (2)	Women (3)	(2)-(3) (4)	Urban (5)	Rural (6)	(4)-(5) (7)
Panel A: Baseline, OLS estimates							
Standardized score	-0.0172	-0.3539*	0.1227	-0.4586**	-0.3292	0.0619	-0.3878
	(0.0932)	(0.1891)	(0.1156)	(0.1907)	(0.3184)	(0.1293)	(0.3503)
Mild anxiety	0.0291	-0.0576	0.0807**	-0.1452**	-0.0329	0.0838*	-0.1163
	(0.0256)	(0.04)	(0.04)	(0.0603)	(0.081)	(0.0443)	(0.092)
Depressed	-0.0018	-0.0114	0.0058	-0.0183	-0.0162	0.0013	-0.0165
	(0.0099)	(0.0145)	(0.0156)	(0.0226)	(0.0303)	(0.0144)	(0.0337)
Severely depressed	-0.006	-0.0163	0.0028	-0.019	-0.0218	-0.0054	-0.0156
	(0.0059)	(0.0103)	(0.0094)	(0.0143)	(0.0141)	(0.0082)	(0.0163)
Observations	9,506	4,075	5,367	9,442	5,307	4,188	9,495
Panel B: Baseline, 2SLS estimates							
Standardized score	-0.1689	-0.4377	-0.1103	-0.2557	-1.1406**	-0.0934	-1.0837**
	(0.141)	(0.2864)	(0.2015)	(0.3334)	(0.4569)	(0.1943)	(0.5065)
Mild anxiety	-0.0264	-0.1083*	0.0062	-0.1146	-0.2966**	0.0583	-0.3549***
	(0.019)	(0.0595)	(0.0651)	(0.0959)	(0.1248)	(0.0514)	(0.1352)
Depressed	-0.0187	-0.0144	-0.0135	-0.001	-0.0597	-0.0318*	-0.0278
	(0.0156)	(0.0245)	(0.0252)	(0.0378)	(0.0612)	(0.0177)	(0.0637)
Severely depressed	-0.0152*	-0.0183	-0.0086	-0.0098	-0.0227	-0.0222*	-0.0005
	(0.0088)	(0.0147)	(0.0139)	(0.0213)	(0.0297)	(0.0124)	(0.0322)
Observations	9,461	4,052	5,345	9,397	5,262	4,188	9,450
Kleibergen-Paap F-statistics	267.128	248.224	247.618	126.105	72.898	137.146	68.442

Notes: OLS and 2SLS estimates for all (column 1), male (2), female (3), urban (5), and rural respondents (6). In columns (4) and (7), estimates with all respondents are presented, in which we included as additional regressor the explanatory variable of interest, interacted with a binary variable for male or urban, respectively. In Panel C, we present 2SLS estimates for all respondents and subsamples with per capita Progresa transfers accumulated in expected primary school going ages in MXP1,000, from 5 to 11 years old, interacted with the share of household beneficiaries when a respondent was 11, as endogenous variable of interest. In Panel D, it is the per capita Progresa transfers accumulated in expected secondary school going ages in MXP1,000, from 12 to 17 years old, interacted with the share of household beneficiaries when a respondent was 17. Accordingly, in Panel C (D) specifications, the excluded instrument is a binary variable indicating whether a respondent was 11 (17) years old or older in 1998, interacted with a continuous variable measuring a municipality's pre-Progresa marginalization. All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 3. Baseline and heterogeneity effect analysis estimates, OLS and 2SLS estimates, 2009-2012 MxFLS (continued)

Variables	All (1)	Men (2)	Women (3)	(2)-(3) (4)	Urban (5)	Rural (6)	(4)-(5) (7)
Panel C: 5-11 year-old exposure							
Standardized score	-0.3161	-0.4016	-0.2112	-0.1163	-1.6096	0.5134	-2.1488*
	(0.3548)	(0.636)	(0.4668)	(0.7048)	(1.1015)	(0.4937)	(1.2283)
Mild anxiety	-0.0233	0.0085	-0.0061	0.0147	-0.1468	0.1151	-0.2619
	(0.0877)	(0.121)	(0.1493)	(0.2115)	(0.3471)	(0.1128)	(0.365)
Depressed	-0.0622*	-0.0281	-0.0641	0.036	-0.2182	0.0036	-0.2218
	(0.0367)	(0.072)	(0.0614)	(0.1026)	(0.1354)	(0.0494)	(0.1443)
Severely depressed	-0.0207	-0.0319	-0.0019	-0.03	-0.0863	0.0088	-0.0951
	(0.0221)	(0.0381)	(0.0337)	(0.0525)	(0.0588)	(0.0385)	(0.0703)
Observations	9,461	4,052	5,345	9,397	5,262	4,188	9,450
Kleipergen-Paap F-statistics	128.248	112.341	131.793	64.647	35.404	110.636	17.669
Panel D: 12-17 year-old exposure							
Standardized score	-0.2167	-0.57	-0.1405	-0.3363	-1.4594**	-0.1249	-1.3814**
	(0.1808)	(0.377)	(0.2565)	(0.4335)	(0.5796)	(0.2593)	(0.6472)
Mild anxiety	-0.0339	-0.1411*	0.0079	-0.1491	-0.3795**	0.0779	-0.4574***
	(0.0505)	(0.0786)	(0.0829)	(0.1241)	(0.1585)	(0.0689)	(0.1731)
Depressed	-0.024	-0.0188	-0.0172	-0.0016	-0.0763	-0.0425*	-0.0338
	(0.02)	(0.0319)	(0.032)	(0.0486)	(0.0777)	(0.0234)	(0.0812)
Severely depressed	-0.0195*	-0.0239	-0.0109	-0.013	-0.029	-0.0296*	0.0006
	(0.0113)	(0.0191)	(0.0177)	(0.0275)	(0.038)	(0.0165)	(0.0415)
Observations	9,461	4,052	5,345	9,397	5,262	4,188	9,450
Kleipergen-Paap F-statistics	275.203	250.712	267.666	127.369	86.444	126.478	63.18
Panel E: Non-migrant							
Standardized score	-0.2085	-0.4076	-0.1503	-0.1923	-1.1221**	-0.007	-1.1444**
	(0.1394)	(0.3126)	(0.2033)	(0.3704)	(0.5135)	(0.1941)	(0.56)
Mild anxiety	-0.0371	-0.0733	-0.0272	-0.0461	-0.2996**	0.0753	-0.3749**
	(0.0418)	(0.067)	(0.0665)	(0.1007)	(0.1361)	(0.0525)	(0.1461)
Depressed	-0.0244	-0.0143	-0.0157	0.0014	-0.0848	-0.0272	-0.0575
	(0.0177)	(0.0272)	(0.0267)	(0.0397)	(0.075)	(0.0194)	(0.0776)
Severely depressed	-0.0147	-0.0198	-0.0059	-0.0139	-0.0249	-0.0244*	-0.0004
	(0.0095)	(0.0164)	(0.0156)	(0.0237)	(0.0322)	(0.0135)	(0.0349)
Observations	8,206	3,437	4,729	8,166	4,560	3,630	8,190
Kleipergen-Paap F-statistics	286.528	238.76	263.191	129.193	71.816	129.925	65.047

Notes: See Table 3.

Table 4. Suicide, 2SLS estimates, 2009-2012 vital statistics

	2SLS	Men	Women	(2)-(3)	Urban	Rural	(5)-(6)

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PCT	-0.0077	-0.0029	-0.0243*	0.0214	-0.0197	-0.0103	0.0094
	(0.0077)	(0.0097)	(0.0128)	(0.016)	(0.0128)	(0.0133)	(0.0182)
Observations	203,511	151,493	51,710	203,203	170,436	32,988	203,424
R-squared	0.0015	0	0.0001	0	0.0015	0.0016	0.0015
Kleipergen-Paap F-statistics	1493.8	1497.918	1183.47	816.065	747.685	6343.671	384.558
Mean DV	0.0409	0.0453	0.0283	0.041	0.0418	0.0366	0.0409
	[0.1981]	[0.2079]	[0.1659]	[0.1982]	[0.2000]	[0.1878]	[0.1981]

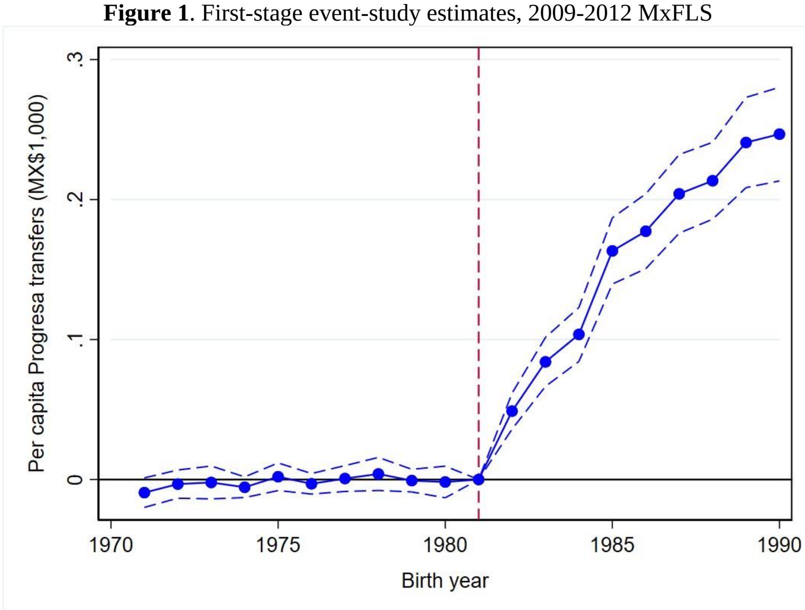
Notes: All regressions include birth year, municipality, state-birth year, year of death, month of death, and day-of-week of death fixed effects, as well as male and squared age variables. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. See Table 2.

Table 5. Mechanisms, 2SLS estimates, 2009-2012 MxFLS

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Human capital							
	Literate	Ever attended school	Years of schooling	Work	Life will improve	Risk averse	Standardized IQ
PCT	0.1434***	0.1195***	3.6629***	0.2773***	0.0598	-0.0641	0.4535**
	(0.033)	(0.0333)	(0.606)	(0.0678)	(0.0798)	(0.0741)	(0.1852)
Observations	9,654	9,654	12,990	9,653	9,653	9,582	7,372
R-squared	0.0022	0.0035	0.0143	0.0042	0.0025	0.0007	0.0049
K-P F-stat	267.036	267.036	277.98	267.209	267.015	265.748	265.297
Mean DV	0.9614	0.9784	9.2746	0.5902	0.422	0.4282	6.1980[a]
	[0.1927]	[0.1455]	[3.5841]	[0.4918]	[0.4939]	[0.4948]	[2.7393]
Panel B: Health and behaviors							
	High blood pressure	Low hemoglobine	Health improved	Diabetes	Victimized	Social	Drink at home
PCT	-0.1284	0.0807	0.1196***	-0.0313**	0.0111	0.0296	-0.0572
	(0.0798)	(0.0565)	(0.0453)	(0.0135)	(0.0732)	(0.0409)	(0.0534)
Observations	9,182	8,560	9,646	9,645	5,467	9,653	9,560
R-squared	0.1192	0.0978	0.0076	0.0018	0.0055	0.0434	0.0009
K-P F-stat	261.996	278.769	270.891	270.614	202.421	267.015	268.28
Mean DV	0.4555	0.1544	0.9249	0.012	0.0765	0.1449	0.0393
	[0.4980]	[0.3614]	[0.2635]	[0.1090]	[0.2658]	[0.3520]	[0.1944]

Notes: All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Non-standardized IQ score.

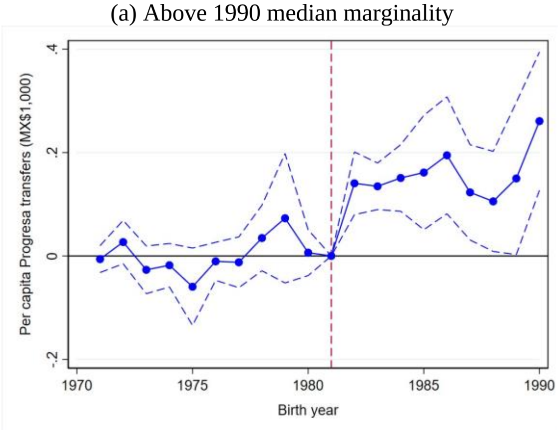
Figure 1. First-stage event-study estimates, 2009-2012 MxFLS



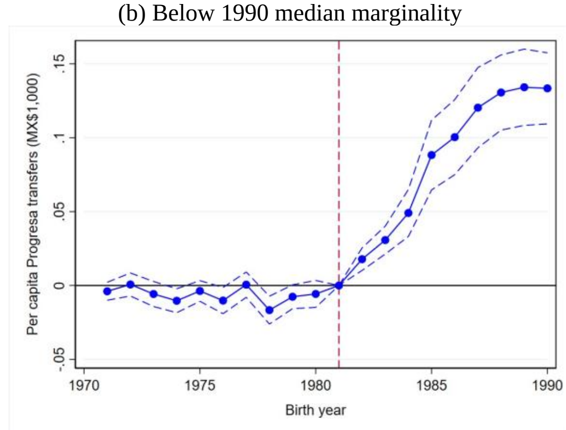
Notes: Event-study estimates for per capita Progresa transfers cumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence, before and after 1998, year of nationwide introduction of Progresa, according to specification (3). The omitted category is birth year 1981. The intervention cut-off is birth year 1982; it is represented by the dashed vertical red line. Dashed blue lines represent 95% confidence intervals.

Figure 2. First-stage event-study estimates by marginality intensity, 2009-2012 MxFLS

(a) Above 1990 median marginality

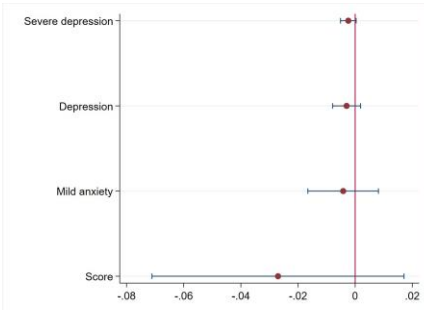


(b) Below 1990 median marginality

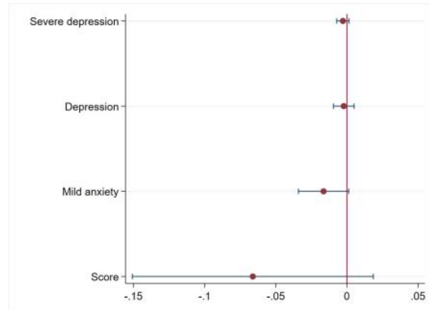


Notes: Event-study estimates for per capita Progresa transfers cumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence, before and after 1998, year of nationwide introduction of Progresa, according to specification (3). The omitted category is birth year 1981. The intervention cut-off is birth year 1982; it is represented by the dashed vertical red line. Dashed blue lines represent 95% confidence intervals. Panel (a) presents event-study estimates for respondents residing in municipalities with above median 1990 marginalization, deemed 'poor'. Panel (b) similarly shows estimates for respondents residing in municipalities with below median 1990 marginalization, i.e. 'non-poor'.

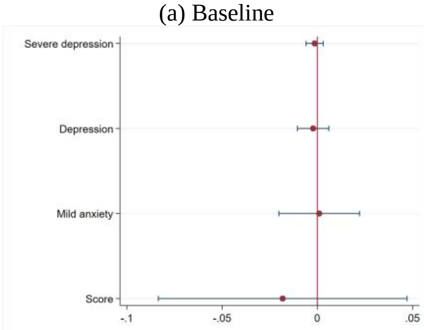
Figure 3. Baseline and heterogeneity effect analysis standardized estimates, 2009-2012 MxFLS



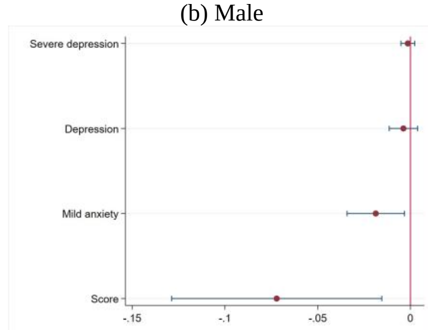
(a) Baseline



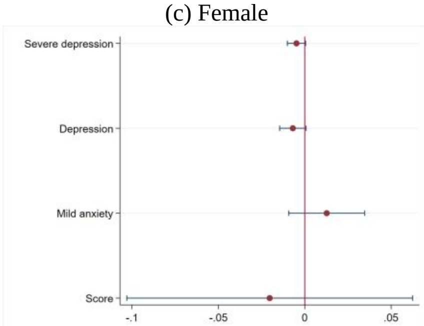
(b) Male



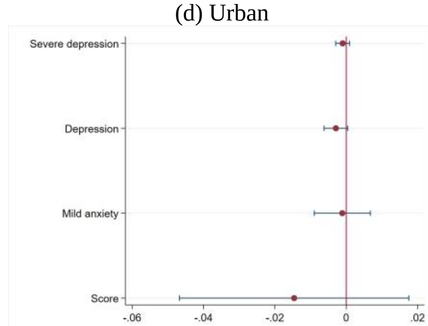
(c) Female



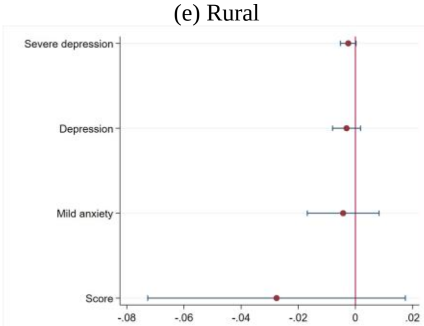
(d) Urban



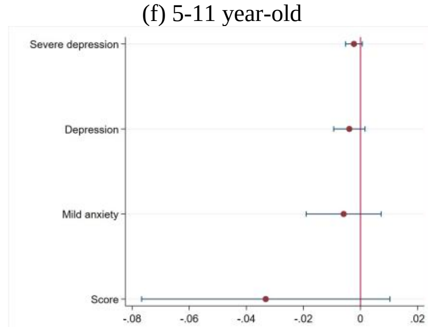
(e) Rural



(f) 5-11 year-old



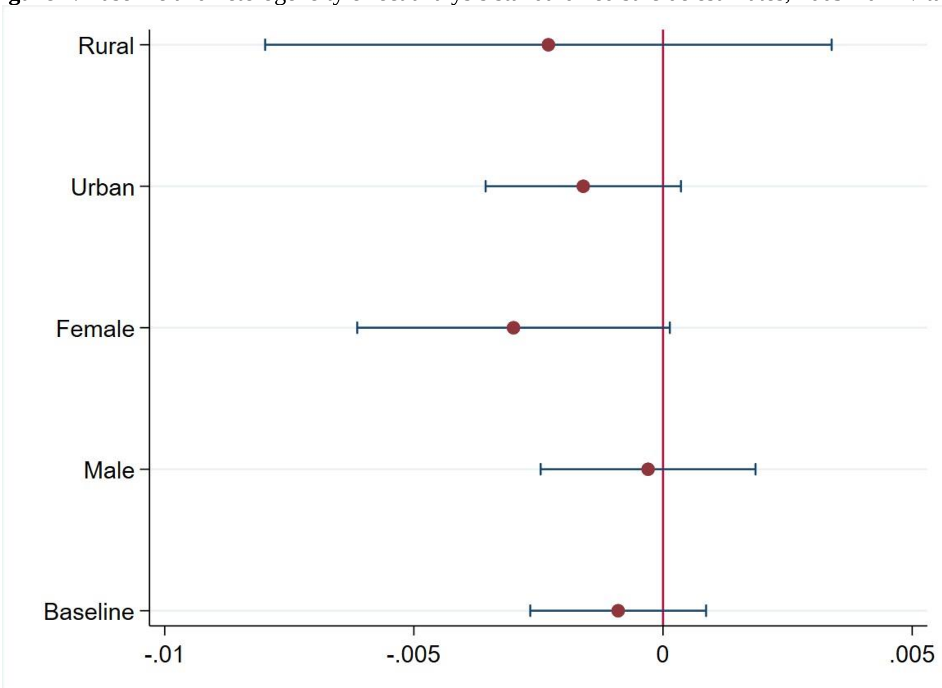
(g) 12-17 year-old



(h) Non-migrant

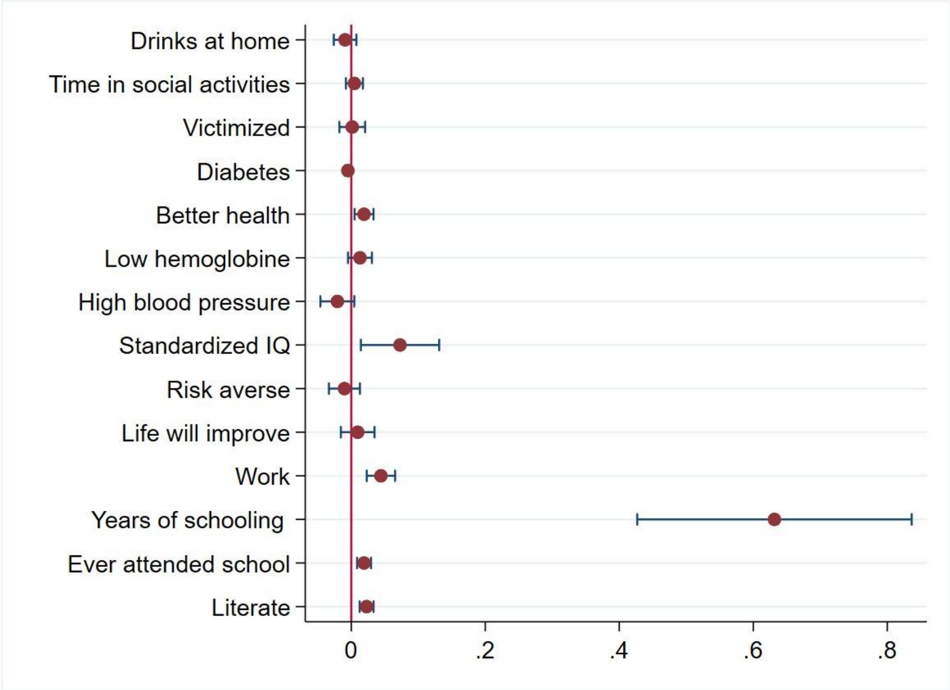
Notes: Lines are 95% confidence intervals, representing difference-in-differences instrumental variables coefficient estimates.

Figure 4. Baseline and heterogeneity effect analysis standardized suicide estimates, 2009-2012 vital statistics



Notes: Lines are 95% confidence intervals, representing difference-in-differences instrumental variables coefficient estimates.

Figure 5. Mechanisms, standardized estimates, 2009-2012 MxFLS



Notes: Lines are 95% confidence intervals, representing difference-in-differences instrumental variables coefficient estimates.

Appendices

Table A1. Dealing with missing values in Progresa transfers, 2009-2012 MxFLS

	Set to missing		Set to zero		Linear interpolation	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
1990 MI X Non-eligible	-0.1504***		-0.1504***		-0.1716***	
	(0.0097)		(0.0097)		(0.0103)	
Pre-eligibility birth cohorts						
1990 MI X 1971		-0.0081		-0.0081		-0.0093*
		(0.0052)		(0.0052)		(0.0053)
1990 MI X 1972		-0.0024		-0.0024		-0.0032
		(0.0050)		(0.0050)		(0.0050)
1990 MI X 1973		-0.0012		-0.0012		-0.0021
		(0.0059)		(0.0059)		(0.0059)
1990 MI X 1974		0.0008		0.0008		-0.0055
		(0.0035)		(0.0035)		(0.0037)
1990 MI X 1975		0.0028		0.0028		0.0020
		(0.0047)		(0.0047)		(0.0049)
1990 MI X 1976		0.0012		0.0012		-0.0030
		(0.0040)		(0.0040)		(0.0037)
1990 MI X 1977		0.0017		0.0017		0.0007
		(0.0046)		(0.0046)		(0.0046)
1990 MI X 1978		0.0036		0.0036		0.0040
		(0.0050)		(0.0050)		(0.0059)
1990 MI X 1979		0.0039		0.0039		-0.0008
		(0.0050)		(0.0050)		(0.0040)
1990 MI X 1980		0.0032		0.0032		-0.0018
		(0.0047)		(0.0047)		(0.0057)
R-squared	0.8174	0.8821	0.8174	0.8821	0.8711	0.9148
Observations	13,081	13,081	13,081	13,081	13,081	13,081
F-statistics	238.76***	31.56***	238.76***	31.56***	278.85***	29.71***
Pre-1982 F-statistics		1.03		1.03		1.35

Notes: The dependent variable is $PCT_{5-17,imsb}$, per capita Progresa transfers accumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence. All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household, and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A1. Dealing with missing values in Progresa transfers, 2009-2012 MxFLS (continued)

	Set to missing		Set to zero		Linear interpolation	
Variables	(1)	(2)	(3)	(4)	(5)	(6)
1990 MI X Non-eligible	-0.1504***		-0.1504***		-0.1716***	
	(0.0097)		(0.0097)		(0.0103)	
Post-eligibility birth cohorts						
1990 MI X 1982		0.0078		0.0078		0.0489***
		(0.0074)		(0.0074)		(0.0064)
1990 MI X 1983		0.0522***		0.0522***		0.0840***
		(0.0114)		(0.0114)		(0.0087)
1990 MI X 1984		0.0897***		0.0897***		0.1037***
		(0.0126)		(0.0126)		(0.0096)
1990 MI X 1985		0.1455***		0.1455***		0.1633***
		(0.0154)		(0.0154)		(0.0118)
1990 MI X 1986		0.1474***		0.1474***		0.1773***
		(0.0193)		(0.0193)		(0.0134)
1990 MI X 1987		0.1577***		0.1577***		0.2041***
		(0.0181)		(0.0181)		(0.0140)
1990 MI X 1988		0.2163***		0.2163***		0.2134***
		(0.0129)		(0.0129)		(0.0137)
1990 MI X 1989		0.2465***		0.2465***		0.2407***
		(0.0168)		(0.0168)		(0.0161)
1990 MI X 1990		0.2424***		0.2424***		0.2467***
		(0.0183)		(0.0183)		(0.0167)
R-squared	0.8174	0.8821	0.8174	0.8821	0.8711	0.9148
Observations	13,081	13,081	13,081	13,081	13,081	13,081
F-statistics	238.76***	31.56***	238.76***	31.56***	278.85***	29.71***
Pre-1982 F-statistics		1.03		1.03		1.35

Notes: See Table A1.

Table A2. First-stage estimates, 2009-2012 vital statistics

Variables	(1)	(2)	Above 1990 marginalization index (3)	Below 1990 marginalization index (4)
1990 MI X Non-eligible	-0.1491***			
	(0.0039)			
Pre-eligibility birth cohorst				
1990 MI X 1971		0.0004	-0.0017	0.0015
		(0.0011)	(0.0056)	(0.0011)
1990 MI X 1972		0.0003	-0.0012	0.0012
		(0.0010)	(0.0051)	(0.0011)
1990 MI X 1973		0.0002	0.0030	0.0015
		(0.0010)	(0.0055)	(0.0010)
1990 MI X 1974		0.0001	-0.0019	0.0014
		(0.0011)	(0.0055)	(0.0011)
1990 MI X 1975		0.0002	-0.0004	0.0000
		(0.0011)	(0.0059)	(0.0012)
1990 MI X 1976		-0.0014	0.0006	-0.0008
		(0.0011)	(0.0056)	(0.0011)
1990 MI X 1977		-0.0015	-0.0039	-0.0010
		(0.0011)	(0.0054)	(0.0012)
1990 MI X 1978		0.0001	-0.0024	0.0009
		(0.0011)	(0.0059)	(0.0011)
1990 MI X 1979		-0.0010	0.0010	0.0003
		(0.0011)	(0.0060)	(0.0011)
1990 MI X 1980		-0.0005	0.0051	0.0007
		(0.0012)	(0.0062)	(0.0011)
R-squared	0.8015	0.8541	0.9051	0.7662
Observations	203,511	203,511	25,030	178,439
F-statistics	1493.80	121.04	72.94	2.26
Pre-1982 F-statistics		1.03	0.32	38.22

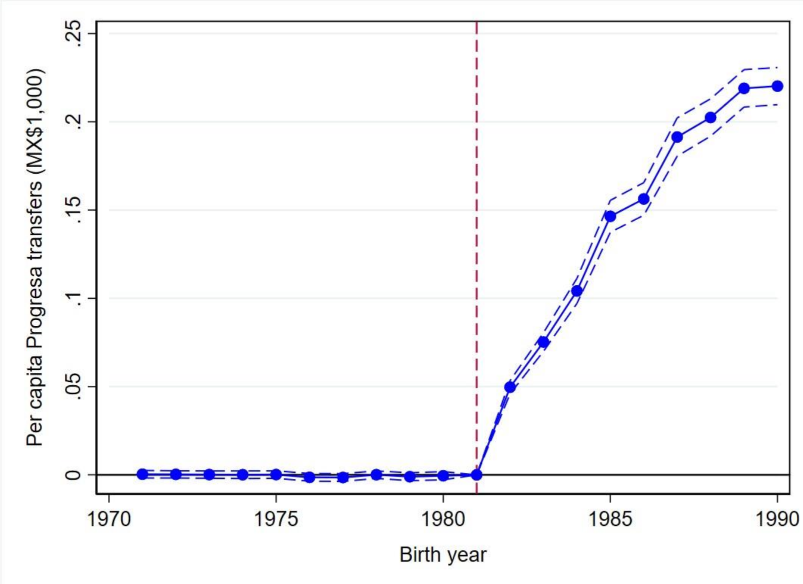
Notes: The dependent variable is $PCT_{5-17,imsb}$, per capita Progresa transfers accumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence. All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household, and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A2. First-stage estimates, 2009-2012 vital statistics (continued)

Variables	(1)	(2)	Above 1990 marginalization index (3)	Below 1990 marginalization index (4)
1990 MI X Non-eligible	-0.1491***			
	(0.0039)			
Post-eligibility birth cohorts				
1990 MI X 1982		0.0497***	0.0999***	0.0176***
		(0.0019)	(0.0062)	(0.0014)
1990 MI X 1983		0.0753***	0.1318***	0.0298***
		(0.0026)	(0.0070)	(0.0019)
1990 MI X 1984		0.1042***	0.1587***	0.0451***
		(0.0036)	(0.0087)	(0.0026)
1990 MI X 1985		0.1465***	0.2229***	0.0711***
		(0.0045)	(0.0126)	(0.0037)
1990 MI X 1986		0.1563***	0.2142***	0.0831***
		(0.0046)	(0.0122)	(0.0042)
1990 MI X 1987		0.1914***	0.2451***	0.1046***
		(0.0054)	(0.0200)	(0.0049)
1990 MI X 1988		0.2024***	0.2658***	0.1115***
		(0.0053)	(0.0139)	(0.0052)
1990 MI X 1989		0.2189***	0.3070***	0.1213***
		(0.0053)	(0.0127)	(0.0054)
1990 MI X 1990		0.2202***	0.3223***	0.1242***
		(0.0053)	(0.0134)	(0.0053)
R-squared	0.8015	0.8541	0.9051	0.7662
Observations	203,511	203,511	25,030	178,439
F-statistics	1493.80	121.04	72.94	2.26
Pre-1982 F-statistics		1.03	0.32	38.22

Notes: See Table A2.

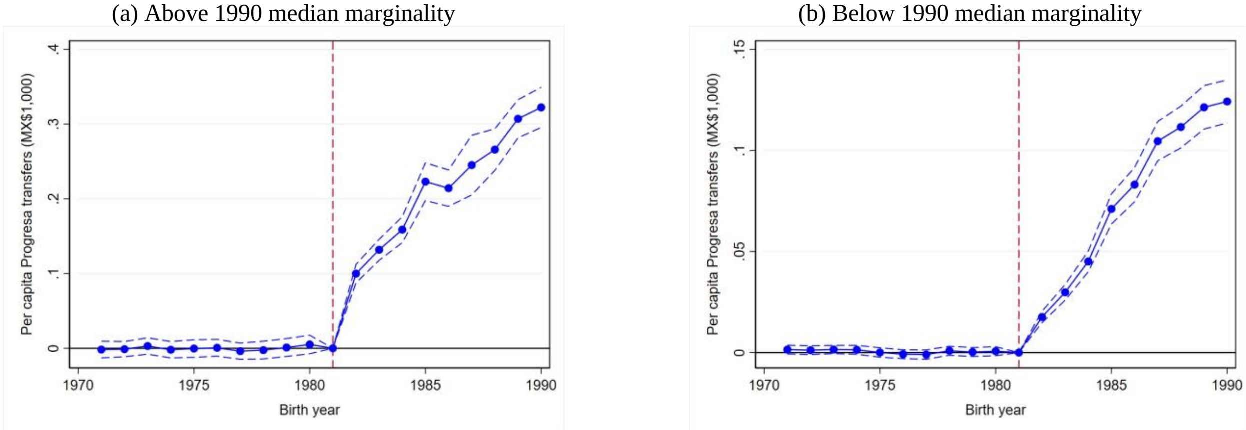
Figure A1. First-stage event-study estimates, 2009-2012 vital statistics



Notes: Event-study estimates for per capita Progresa transfers cumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence, before and after 1998, year of nationwide introduction of Progresa, according to specification (3). The omitted category is birth year 1981. The intervention cut-off is birth year 1982; it is represented by the dashed vertical red line. Dashed blue lines represent 95% confidence intervals.

Figure A2. First-stage event-study estimates by marginality intensity, 2009-2012 vital statistics

(a) Above 1990 median marginality (b) Below 1990 median marginality



Notes: Event-study estimates for per capita Progresa transfers cumulated during expected school going ages, interacted with the share of household beneficiaries when a respondent was 17 years old, in their municipality of residence, before and after 1998, year of nationwide introduction of Progresa, according to specification (3). The omitted category is birth year 1981. The intervention cut-off is birth year 1982; it is represented by the dashed vertical red line. Dashed blue lines represent 95% confidence intervals. Panel (a) presents event-study estimates for respondents residing in municipalities with above median 1990 marginalization, deemed 'poor'. Panel (b) similarly shows estimates for respondents residing in municipalities with below median 1990 marginalization, i.e. 'non-poor'.

Table A3. Exploring mechanisms, 2SLS estimates, 2009-2012 MxFLS, male respondents

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Human capital							
	Literate	Ever attended school	Years of schooling	Work	Life will improve	Risk averse	Standardized IQ
PCT	0.0824	0.0578	3.8794***	0.3110***	0.1241	-0.1474	0.6121**
	(0.0652)	(0.0469)	(0.7162)	(0.0684)	(0.1408)	(0.1219)	(0.3020)
Observations	4,132	4,132	6,228	4,131	4,132	4,106	3,267
R-squared	0.0026	0.0026	0.0142	0.0093	0.0014	0.0016	0.0074
Kleibergen-Paap F-statistics	246.535	246.535	268.662	246.617	246.535	247.716	250.009
Mean DV	0.9613	0.9782	9.1678	0.9044	0.4305	0.4165	6.3104 [a]
	[0.1930]	[0.1460]	[3.5537]	[0.2941]	[0.4952]	[0.4930]	[2.7598]
Panel B: Health and behaviors							
	High blood pressure	Low hemoglobine	Health improved	Diabetes	Victimized	Social	Drink at home
PCT	0.0360	-0.0233	0.1527**	-0.0485*	0.0709	0.0440	-0.0792
	(0.1109)	(0.0408)	(0.0650)	(0.0277)	(0.1349)	(0.0923)	(0.0858)
Observations	3,954	3,676	3,762	4,130	2,160	4,132	4,092
R-squared	0.0033	-0.0000	0.0039	0.0010	0.0030	0.0036	0.0011
Kleibergen-Paap F-statistics	241.184	244.914	250.575	250.575	160.863	246.535	243.954
Mean DV	0.6452	0.0258	0.9472	0.0094	0.0931	0.2270	0.0442
	[0.4785]	[0.1587]	[0.2236]	[0.0967]	[0.2906]	[0.4189]	[0.2056]

Notes: All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Non-standardized IQ score.

Table A4. Exploring mechanisms, 2SLS estimates, 2009-2012 MxFLS, female respondents

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Human capital							
	Literate	Ever attended school	Years of schooling	Work	Life will improve	Risk averse	Standardized IQ
PCT	0.1695***	0.1581***	3.5925***	0.2015**	0.0043	-0.0201	0.3809*
	(0.0618)	(0.0586)	(0.7113)	(0.0922)	(0.1200)	(0.1097)	(0.2201)
Observations	5,451	5,451	6,667	5,451	5,450	5,406	4,041
R-squared	0.0024	0.0051	0.0133	0.0082	0.0028	0.0002	0.0035
Kleibergen-Paap F-statistics	251.338	251.338	273.814	251.338	251.336	251.743	250.689
Mean DV	0.9611	0.9785	9.3654	0.4368	0.4143	0.4375	6.1074 [a]
	[0.1934]	[0.1449]	[3.6072]	[0.4960]	[0.4926]	[0.4961]	[2.7132]
Panel B: Health and behaviors							
	High blood pressure	Low hemoglobine	Health improved	Diabetes	Victimized	Social	Drink at home
PCT	-0.2093*	0.1530	0.1056*	-0.0199	-0.0061	-0.0492	-0.0077
	(0.1117)	(0.0991)	(0.0546)	(0.0199)	(0.0745)	(0.0498)	(0.0470)
Observations	5,162	4,819	5,446	5,445	3,257	5,397	5,450
R-squared	0.0026	0.0000	0.0015	0.0023	0.0014	0.0002	0.0021
Kleibergen-Paap F-statistics	238.017	256.182	255.496	254.847	187.583	251.336	254.080
Mean DV	0.3102	0.2534	0.9091	0.0140	0.0654	0.0815	0.0358
	[0.4626]	[0.4350]	[0.2875]	[0.1173]	[0.2473]	[0.2736]	[0.1857]

Notes: All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Non-standardized IQ score.

Table A5. Exploring mechanisms, 2SLS estimates, 2009-2012 MxFLS, urban respondents

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Human capital							
	Literate	Ever attended school	Years of schooling	Work	Life will improve	Risk averse	Standardized IQ
PCT	0.1321	0.1137	6.0104***	0.3560*	0.0688	-0.4012*	0.8988
	(0.0800)	(0.0699)	(1.3550)	(0.2124)	(0.1930)	(0.2237)	(0.7093)
Observations	5,364	5,364	7,263	5,363	5,364	5,316	4,195
R-squared	-0.0001	0.0020	0.0152	0.2027	0.0035	0.0007	0.0044
Kleibergen-Paap F-statistics	72.806	72.806	76.088	72.817	72.806	71.913	80.404
Mean DV	0.9696	0.9821	9.8415	0.6761	0.4228	0.4061	6.5411[a]
	[0.1717]	[0.1326]	[3.5135]	[0.4680]	[0.4941]	[0.4912]	[2.6864]
Panel B: Health and behaviors							
	High blood pressure	Low hemoglobine	Health improved	Diabetes	Victimized	Social	Drink at home
PCT	-0.4451*	0.1725	0.4781***	-0.0737	-0.0911	0.0080	-0.1257
	(0.2407)	(0.2066)	(0.1606)	(0.0450)	(0.2984)	(0.0790)	(0.1613)
Observations	5,002	4,800	5,348	5,347	3,030	5,300	5,364
R-squared	0.1100	0.0934	0.0104	0.0013	0.0048	0.0019	0.0397
Kleibergen-Paap F-statistics	73.389	70.557	73.068	73.028	48.870	72.806	72.874
Mean DV	0.4418	0.1485	0.9181	0.0123	0.1026	0.1670	0.0311
	[0.4967]	[0.3557]	[0.2742]	[0.1104]	[0.3035]	[0.3730]	[0.1737]

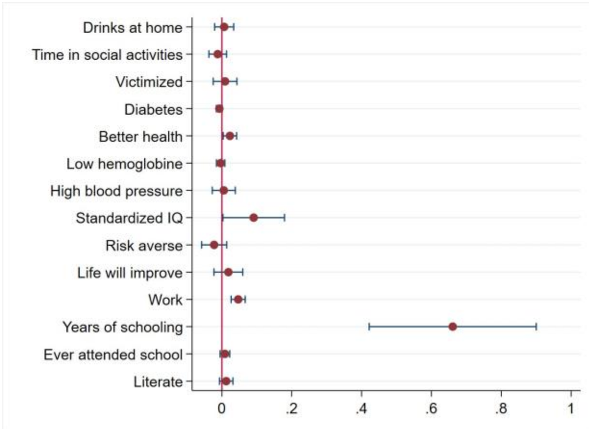
Notes: All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Non-standardized IQ score.

Table A6. Exploring mechanisms, 2SLS estimates, 2009-2012 MxFLS, rural respondents

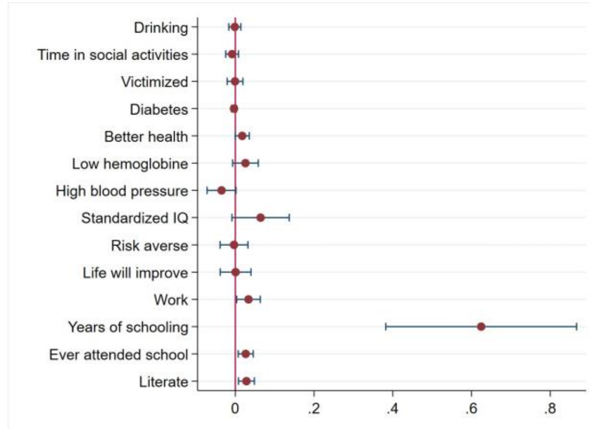
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Human capital							
	Literate	Ever attended school	Years of schooling	Work	Life will improve	Risk averse	Standardized IQ
PCT	0.1402***	0.1195**	0.9095	0.2520***	-0.1870	-0.0437	0.2255
	(0.0509)	(0.0484)	(0.8275)	(0.0904)	(0.1167)	(0.0924)	(0.2456)
Observations	4,278	4,278	5,721	4,278	4,277	4,254	3,161
R-squared	0.0022	0.0024	0.0081	0.3203	0.0018	0.0031	0.0060
Kleibergen-Paap F-statistics	134.957	134.957	127.692	134.957	134.963	137.405	141.763
Mean DV	0.9509	0.9736	8.5536	0.5940	0.4213	0.4558	5.7475^a
	[0.2161]	[0.1604]	[3.5442]	[0.4911]	[0.4938]	[0.4981]	[2.7450]
Panel B: Health and behaviors							
	High blood pressure	Low hemoglobine	Health improved	Diabetes	Victimized	Social	Drink at home
PCT	-0.0424	0.0937	0.1400**	-0.0125	-0.0197	-0.0002	0.0663
	(0.1299)	(0.0716)	(0.0638)	(0.0183)	(0.0542)	(0.0897)	(0.0609)
Observations	4,166	3,742	4,286	4,286	2,403	4,248	4,277
R-squared	0.1343	0.1047	0.0038	0.0024	0.0123	0.0028	0.0499
Kleibergen-Paap F-statistics	141.917	131.900	137.218	137.218	137.330	134.963	138.072
Mean DV	0.4722	0.1619	0.9337	0.0117	0.0429	0.1176	0.0497
	[0.4993]	[0.3685]	[0.2488]	[0.1074]	[0.2026]	[0.3222]	[0.2173]

Notes: All regressions include birth year, municipality, and state-birth year fixed effects, as well as individual, household and locality covariates. Robust standard errors clustered at the locality level are reported in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a]Non-standardized IQ score.

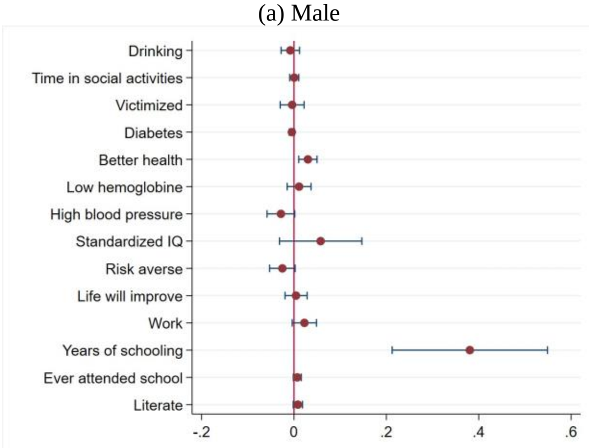
Figure A3. Mechanisms, standardized estimates, 2009-2012 MxFLS



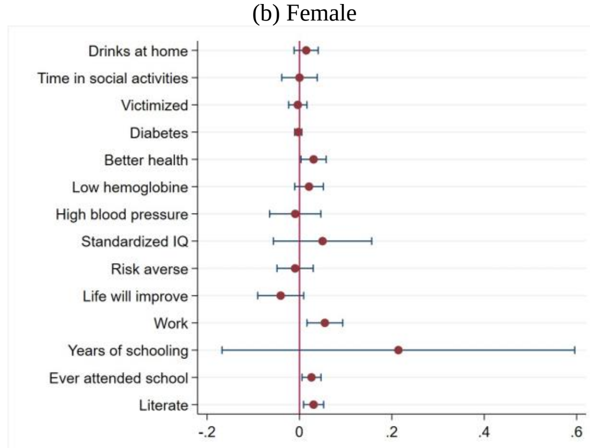
(a) Male



(b) Female



(c) Urban



(d) Rural

Notes: Lines are 95% confidence intervals, representing difference-in-differences instrumental variables coefficient estimates.

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