Growing Up Over the Social Safety Net: The Effects of a Cash Transfer Program on the Transition to Adulthood

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Abstract

Countries spend a large share of their budgets on aid to families with children, with cash transfers being one of the most used policy instruments for this purpose. This paper presents novel evidence about the effects of a permanent, large-scale, and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*. I focus on three critical dimensions of individuals' transition to adulthood: education, fertility, and labor market decisions. I use a unique combination of individual-level administrative records that exhaustively describes the year-by-year trajectory of the effects. Using a Regression Discontinuity Design that exploits the use of a poverty score to define eligibility to participate in the program, I show that the program reduces women's teenage pregnancies by 9.4p.p., increases participants' early adulthood labor market participation by 6.4p.p., months worked by 4.4, and earnings by about 12%. The evidence on education outcomes is mixed but suggests a stronger attachment to the secondary education system. Consistent with a postponement of women's first birth being the main driver, changes in labor market outcomes are observed exclusively for women. The evidence suggests that cash transfers may be viable policies to improve children's future life trajectories and contribute to reducing the labor market gender gap.

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1 Introduction

Worldwide, governments spend billions of dollars on social safety net (SSN) policies to reduce poverty, especially for vulnerable households with children.[1] Cash transfers are one of the most used policy instruments for this purpose. Because they represent sizable investments, have the potential to affect multiple generations through a wide range of mechanisms, and trigger ethical debates about who are the deserving beneficiaries, they are a highly controversial topic. For instance, some argue that cash transfers could be beneficial for children's life trajectories. They might help reduce child poverty, improve economic security, and connect vulnerable individuals to the labor force. Others argue that they could be inefficient or even hurtful for long-run upward mobility, especially when providing unconditional support.[2] The lack of consensus is not exclusive to the policy debate. The academic literature has yet to thoroughly describe whether and how cash transfers affect individuals' life trajectories, especially when the focus is on children. This has substantial policy implications since it could change the direction of the cost-benefit evaluation and the chances of the policy surviving the political cycle in cases where effects do not materialize immediately (Aizer et al., 2022).

This paper fills this gap by studying how a permanent, large-scale, and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*, affects the life trajectories in terms of education, fertility, and labor market decisions of individuals who benefited from the policy during their childhood. These outcomes are strongly correlated with poverty and opportunities for mobility and speak directly to the long-run goal of most cash transfer programs. I focus on individuals' decisions during the period that spans between 15 and 30 years old. This is a period that overlaps with what sociologists and psychologists usually refer to as "transition to adulthood" or "emerging adulthood" (Settersten Jr et al., 2008; Arnett, 2000). Adulthood is a distinct and socially recognized stage of life, usually defined by a series of markers related to the culmination of education cycles, labor market participation, residential independence, marriage, and fertility. Until recently, "adolescence" was the term used to describe the life stage between childhood and adulthood. However, this transition has become more nuanced in the last fifty years, and the idea of a uniform adolescence is becoming socially and economically inexact

[1] The world's average SSN expenditure is 1.93% of the GDP (e.g., in Japan) and ranges between 0.01% (Cote d'Ivoire) and 10.1% (South Sudan). In the US, SSN expenditure is 1.34% of the GDP, including programs such as TANF, Child Support programs, WIC, EITC, and Food Stamps. In OECD countries, the average SSN expenditure is 2.6% (e.g., Germany) and ranges between 0.7% (Turkey) and 4.9% (Denmark). In developing countries, expenditure is considerably lower, with an average of 1.7% (Thailand) and a median of 1.23% (China). Both for OECD and developing countries, this is larger, for instance, than the total tax revenue from property taxes.

[2] These two types of arguments can be found in recent discussions about the Child Tax Credit expansion in the United States For instance, in the blog post by Scott Winship from the conservative American Enterprise Institute (2021) and in a quote from Rep. Danny Davis (2021) in a press release from the "First Focus Campaign for Children". However, these expressions are representative of the typical discussion surrounding cash transfers across the world.

(Settersten Jr et al., 2008). Cash transfers might play a key role in shaping this transition, with long-term consequences in traditional socio-economic outcomes such as income, and human capital accumulation, among others. Understanding the dynamics of these effects is critical to assess if cash transfers are fulfilling their ultimate goal of reducing structural poverty and inequality and increasing opportunities for mobility.

The program that I study, *PANES/AFAM-PE*, was implemented in Uruguay in 2005 and remains in place until today. It consists of a cash transfer that represents, on average, more than 50% of the self-reported pre-program household income. To remain in the program, households were required to satisfy some conditions, such as school attendance and health check-ups, typical of Conditional Cash Transfer programs (CCTs). *PANES/AFAM-PE* has two main goals. First, in the short term, it aims to provide an additional source of income to help beneficiary households to overcome immediate needs related to their disadvantaged socio-economic status. Second, in the long run, the program aims to encourage human capital accumulation of beneficiary children for a more permanent transition out of poverty. *PANES/AFAM-PE* was broadly publicized, even before its implementation, and rapidly became the most generous anti-poverty program in the country's history (Manacorda et al., 2011). It accounts for 0.4% of the Uruguayan GDP and reaches more than 10% of Uruguayan households, comparable to programs such as *PROGRESA* (Mexico) and *Bolsa Familia* (Brazil). Since its inception, the program has only suffered minor changes aimed at increasing its coverage.

The ideal setting to analyze the causal effects of cash transfers on individuals' transition to adulthood would be to randomly assign a group of families to receive government assistance and wait for the children to grow up to compare some key socio-economic outcomes measured at different ages between treated and control families. The research hypothesis is that, compared to children that did not receive government assistance, children that grew up in families that did receive it have better outcomes during their transition to adulthood, such as lower teenage pregnancies or a higher probability of being employed in the formal labor market. Random assignment is atypical if one wants to analyze permanent, large-scale, and government-implemented programs. *PANES/AFAM-PE* is not an exception. Hence, I leverage some features of the program design and rely on a quasi-experimental Regression Discontinuity Design that closely mimics the ideal experiment. More specifically, I leverage the fact that eligibility to participate in *PANES/AFAM-PE* is determined based on a poverty score and exploit the sharp change in the probability of treatment just at the eligibility threshold. Intuitively, I compare individuals who obtained a poverty score just above the eligibility threshold with individuals that obtained a score just below. Under some assumptions (i.e., continuity and monotonicity), this comparison yields an estimate of the (local) average treatment effect of the program.[3]

[3]Because the program has been in place uninterruptedly since 2005, households might have applied to it more than once and obtained multiple scores. Following the approach proposed by Jepsen et al.

To conduct the empirical analysis, I have assembled a unique and exhaustive longitudinal dataset that contains individual-level information both on participation and outcomes of interest for the universe of Uruguayan individuals. This rare dataset is built on a series of administrative records provided by different government agencies that can be merged at the individual level using a unique masked identification number. In particular, I combine information on *PANES/AFAM-PE* applications and participation, births, education enrollment, and labor market participation from different institutions.

The main findings can be grouped into two. In the first place, *PANES/AFAM-PE* substantially affects individuals' transition to adulthood. In terms of fertility outcomes, measured at 18 years old, for instance, participating in the program reduces women's number of births by 0.108, or 41.95% of the control group average. This effect is statistically significant and economically relevant. First, in percentage terms, this reduction is equivalent to the reduction observed in Uruguay's adolescent fertility rate between 1960 and 2020. Second, the effect sizes are substantially larger compared to other policy changes that reduced teenage pregnancies in Uruguayan women, such as abortion legalization (Cabella and Velázquez, 2022) or a large-scale intervention that granted access to subdermal contraceptive implants (Ceni et al., 2021). This negative effect is also consistent with very recent findings for the EITC in the US (Michelmore and Lopoo, 2021), CCT programs in Latin America (e.g., Araujo and Macours, 2021; Attanasio et al., 2021; Barham et al., 2018), or Africa (e.g., Baird et al., 2011).

The effects on labor market outcomes are also strong. For instance, participating in the program causes an increase of 6.4p.p. (9.69%) in the probability of having worked four consecutive months in the formal sector at or before age 23. In addition, the program increases by 4.4 (or 19.77%) the cumulative number of months worked by age 23 and earnings by around 12%. As a reference, the size of the effect observed for participation is similar in size, but with the opposite sign, to the negative of *PANES/AFAM-PE* on parents' formal labor force participation (Bergolo and Cruces, 2021). These results are also in the same line that recent promising evidence for the US (Barr et al., 2022; Bailey et al., 2020; Bastian and Michelmore, 2018; Aizer et al., 2016), and Latin American countries (Araujo and Macours, 2021; Attanasio et al., 2021; Parker and Vogl, 2018).

Unlike the effects concerning fertility and employment decisions, the effects on education outcomes are more nuanced. The program weakly increases the number of years enrolled in secondary education at or before age 18 by 0.25 years (or 10%), with null effects on the extensive margin (i.e., ever being enrolled). This effect is consistent with related literature that usually finds increases in years of education that are close to 0.2-0.4 (e.g., Araujo and Macours, 2021; Aizer et al., 2016; Behrman et al., 2011). Exploratory

(2016), I use the score of the *first* application to the program as an instrument for treatment status. Section 5 discusses more in-depth the challenges of this type of setting and the reasons that lead to the use of the score of the first application as an instrument for the treatment variable.

evidence suggests that this less clear pattern of effects masks important heterogeneous effects by education level. In particular, the effects are observed exclusively for higher secondary education, with an increase in the maximum grade of high-school enrollment. This indicates that the relevant margin of response could be associated with academic progress rather than extensive margin responses. Moreover, the weak positive effects observed on education outcomes do not translate into increased enrollment in tertiary education. The mixed evidence found for education outcomes is not exclusive of *PANES/AFAM-PE*. For instance, despite the positive effects reported in Araujo and Macours (2021), Aizer et al. (2016), or Behrman et al. (2011), some other papers find null (Dustan, 2020; Barham et al., 2018), or even negative effects women's education (Bastian et al., 2022).

The second main set of findings suggests that the program's positive effects on employment, months worked, and earnings are explained by changes in the timing of women's fertility decisions. Two pieces of evidence support this interpretation. First, the effects estimated on the overall population, as described in the first group of findings, are driven exclusively for women. For instance, the effect of *PANES/AFAM-PE* on women's labor market participation measured at age 23 is 11.2p.p. (16.98%), while the effect on the number of months worked by this age is 5.92 (26.5%). On the contrary, the estimated effects for men are 4.4p.p. and -0.764 months, respectively, and both are statistically insignificant.

The second piece of supporting evidence is provided by the dynamic analysis. Comparing the age profiles of the effects for each outcome suggests a compelling story. The effects of *PANES/AFAM-PE* start to manifest as early as age 16, with the negative effects on teenage pregnancies. These effects peak (in absolute value) around the ages of 17-18 and continue being negative, although smaller in size, until women reach 27 years old. At this age, they become positive but statistically insignificant. The overall age profile of the effects suggests that *PANES/AFAM-PE* did not change women's overall fertility preferences. Rather, it led to a postponement of births that otherwise would have occurred in the late teens. A similar but oppositely signed pattern is observed when looking at women's labor market outcomes. The positive effects on labor market outcomes start as early as at age 18 and remain positive until the mid-twenties when they start to attenuate and trend toward 0. This attenuation coincides exactly with the attenuation (and even reversal) of the effects observed on fertility outcomes. The fact that labor market effects are exclusively observed on women, combined with such similar but inverse dynamics of fertility and labor market outcomes, suggests that *PANES/AFAM-PE* improves young women's labor market participation through a postponement of births.

The evidence reported in this paper has policy implications for the design, implementation, and evaluation of cash transfer policies. First, it illustrates that cash transfer policies may help reduce labor market gender gaps, even when they are not specifically designed for this purpose. Despite the signs of attenuation observed by the late twenties,

changes in the timing of events still have strong consequences from a life-cycle perspective due to the existence of fixed costs and flatter wage profiles for mothers (e.g., Bratti, 2015; Miller, 2011). Given that the motherhood penalty explains a sizable share of the labor market gender gap, policies that promote a postponement of pregnancies that otherwise would have occurred during teenage years might be particularly effective. Second, by having strong effects on critical decisions such as the age of first birth, cash transfers have the potential to spill over to future generations. For instance, later-life pregnancies could lead to higher test scores or improved educational and psychological outcomes for children of the third generation (e.g., as discussed in Sobotka, 2010). These results suggest that cash transfer programs can have long-lasting effects that should be considered when assessing their effectiveness, making them much more attractive. Overall my paper shows that cash transfers can be a viable policy instrument to reduce long-run poverty by improving participants' labor market outcomes.

This paper makes two main contributions. First, it contributes to the literature that analyzes the effect of cash transfers on children's outcomes. In its current status, this literature can be synthesized into two snapshots. On the one hand, there is well-documented evidence about positive early-file effects of cash transfers on children's health and education outcomes in high-, low-, and middle-income countries (Hoynes and Schanzenbach, 2018; Bastagli et al., 2019, 2016; Bosch and Manacorda, 2012; Fiszbein et al., 2009). This literature usually measures children's outcomes at around age 18. On the other hand, some incipient literature focuses on later-life outcomes. This recent literature provides promising evidence for the US (e.g., Barr et al. 2022; Bailey et al. 2020; Price and Song 2018; Bastian and Michelmore 2018; Hoynes et al. 2016), Mexico's *PROGRESA* (Araujo and Macours, 2021), Nicaragua's *Red de Proteccion Social* (Barham et al., 2018), and Colombia's *Familias en Accion* (Attanasio et al., 2021).[4] In general, this literature focuses on outcomes measured later in life, at around the age of 30. However, to my knowledge, there is still no evidence that describes the full dynamic of the effects of cash transfers on transitions to adulthood. Doing so represents a major empirical challenge, mostly because it is extremely data-demanding.

By collecting administrative records from different data sources that capture both a long period of time and a wide range of outcomes, I am able to overcome this critical empirical challenge. To my knowledge, this is the first paper to exhaustively describe the effects of cash transfers on education, fertility, and labor market decisions during children's transition to adulthood between 18-30 years old with this degree of detail and based on high-quality administrative records. Furthermore, this paper shows that understanding the full dynamics of the effects is key for a correct assessment of the effects of cash transfers on individuals' life trajectories. For instance, if the effects of the program on

[4]Some recent reviews can be found in (Aizer et al., 2022) for the US social safety net, or (Molina Millán et al., 2019) for conditional cash transfers in low- and middle-income countries

fertility were measured only at age 18, one would conclude that the program led to a reduction in fertility. On the other hand, focusing on the effects measured at age 30 would lead to the conclusion that the program did not have an effect on fertility. In both cases, one would have completely overlooked the postponement effect.

This paper also presents some additional methodological improvements within this literature strand, which has usually been affected by some data or research design limitations. One example is the use of geographic or temporal variation in the rollout of a program as a source of exogenous variation. Using aggregated units of analysis leads to intention-to-treat estimates rather than average treatment effects. This could be a considerable limitation in contexts where take-up is imperfect.[5] Having access to individual-level data, jointly with a research design that exploits changes in treatment status at the individual level, allows me to provide estimates of (local) average treatment effects in addition to the intention to treat effects. Local effects may be the parameters of interest from a government's perspective, for instance, when considering an expansion of the program.

Another limitation of the existing literature, particularly relevant in developing countries, is the lack of high-quality administrative records. This implies that researchers usually need to conduct their own follow-up surveys to collect information on the post-intervention period. The high costs associated with this strategy usually result in follow-up surveys comprising very few data points for very specific cohorts. Access to high-quality administrative records allows me to overcome some of the attrition or small sample size issues associated with survey data. Finally, it is also important to note that sometimes the existing evidence on the effects of cash transfers corresponds to very specific settings. For instance, temporary interventions in rural areas, conducted by local or international NGOs. The analysis presented in this paper not only provides the first characterization of the effects of cash transfers on individuals' transition to adulthood but does so in the context of a permanent, large-scale, and government-implemented policy. This is presumably a more general context compared to other small-scale, context-specific interventions.

The second main contribution is to the literature on gender inequality in the labor market (see general surveys in Altonji and Blank, 1999, Blau and Kahn, 2017, or Olivetti and Petrongolo, 2016), but with a focus in the relationship between motherhood and labor market outcomes (e.g., Bratti, 2015; Miller, 2011; Waldfogel, 1998 or more recent works such as Kleven et al., 2019,?). Regarding this literature, my paper provides complementary evidence highlighting how fertility decisions, particularly during the critical adolescent ages, might have long-lasting consequences in terms of labor market participation, experience, and earnings. Furthermore, this paper illustrates how cash transfer

[5]There are some exceptions, such as Aizer et al. (2016) or Price and Song (2018) that do analyze the effects of a cash transfer at the individual level, but they are subject to additional limitations. For instance, Aizer et al. (2016) restrict their analysis to male children, who do not tend to change their surnames and, therefore, can be tracked over time. Price and Song (2018) propose a matching algorithm that only allows measuring effects on families with more than one child.

policies can be useful in reducing labor market gender gaps, even when they are not specifically designed for this purpose.

This paper makes contributions to two additional broader strands of literature. On the one hand, to the literature in Demography that analyzes the causes and consequences of the "postponement transition." The postponement transition describes the increase in the mean age of first birth that has affected rich countries since the 1970s and, more recently, Latin-American countries (Rosero-Bixby et al., 2009). This transition has been explained by several factors, such as the spread of modern contraception or legalization of abortion, but also by changes in socio-economic trends, such as prolonged education, women's emancipation, and the postponement of other adulthood milestones such as finishing education, leaving the parental home, or forming a couple (see Sobotka, 2010; Mills et al., 2011 for exhaustive reviews). Hence, the fertility postponement is strongly connected to the idea of a more diffuse transition to adulthood. This literature has discussed the relationship between fertility postponement and labor market decisions mostly based on macro-level correlations. My paper provides additional evidence using micro-level data that shows a causal relation between improvements in socioeconomic conditions of the households and changes in fertility patterns.

Finally, this paper contributes to a broader literature on the role of household income on children's outcomes. The bulk of the empirical literature has found that early childhood interventions have strong effects on long-term outcomes (see Almond et al., 2018 for a thorough review). However, a growing literature shows that shocks to household income when children are older may also be effective (Bulman et al., 2021; Manoli and Turner, 2018; Cesarini et al., 2016; Akee et al., 2010; Dynarski, 2003). I contribute to this literature by showing the effects of a policy-driven income shock on household income for children that were, on average, 13 years old when they first applied to the program.

The rest of the paper is structured as follows. In Section 2, I describe the main features of *PANES/AFAM-PE*. Then, in Section 3, I discuss the main mechanisms that could drive the effects of cash transfers on the outcomes of interest, with a specific focus on how these mechanisms might evolve over time. In Section 4, I describe the data used in the analysis, while in Section 5, I describe the main features of the Regression Discontinuity approach used to estimate the causal effects of the program. In Section 6, I report the main results from the empirical analysis. Section 7 discusses the main theoretical mechanisms that could explain the results. Finally, Section 8 concludes and discusses the main policy implications.

2 Institutional Background: *PANES/AFAM-PE*

2.1 Context of Implementation

Uruguay is a middle-high-income country in South America with a population of about 3.5 million inhabitants. In 2018, Uruguay had the second largest GDP in the region (USD 23,585), only led by Chile (USD 25,526).[6] In the same year, Uruguay was ranked 55th in the world in terms of Human Development Index and classified within the very high HDI group. Uruguay's lower secondary completion rate in 2018 was 56.8%, which is comparable to Argentina's but lower than in Mexico, Brazil, and Chile; and way behind richer countries such as the United States, Sweden, or even Italy and Spain. Uruguay's adolescent fertility rate (i.e., births per 1,000 women aged 15-19) is 58.24, similar to Brazil and Argentina, but higher than in Chile and Costa Rica, and substantially higher compared to the United States, Norway, Sweden, Spain, and Italy.

Uruguay has a well-established tradition of a strong public sector. In 2018, Uruguay's tax revenue as a percentage of the GDP was 29.2%, the largest in the region, only behind Brazil. Compared to the rest of the world, this share is higher than in the United States and close to the OECD average. In terms of its social protection system, Uruguay has one of the oldest and most developed systems in the region.[7] In 1943, Uruguay implemented family allowances for families with underage children for the first time. However, until the end of the 90s, these benefits were restricted to registered employees.

The program I focus on, *PANES/AFAM-PE*, was implemented in 2005. It was conceived as a temporary social relief program in response to the economic downturn that affected most Latin American countries in the early 2000s, and it remained in place until December, 2007.[8] In the next section, I describe in detail the key elements of its design.

2.2 Design of *PANES/AFAM-PE*

The implementation of *PANES/AFAM-PE* can be divided into two phases. The first started in 2005 under the name of *PANES* and remained in place until 2007. The second, *AFAM-PE*, started immediately after. The program was widely publicized and rapidly became the largest anti-poverty program in the country's history (Manacorda et al., 2011). *PANES/AFAM-PE* is comparable both in its design and in its relative size to programs such as *PROGRESA-Oportunidades* (Mexico) and *Bolsa Familia* (Brazil). Its total cost has been consistently around 0.4% of the Uruguayan GDP.

The main component of *PANES* was a cash transfer targeted at the poorest 150,000

[6] See Table A.1 in Appendix A.1 for further details.

[7] For instance, old age pensions were established for the first time in 1919; maternity leave was implemented in 1937; sickness and disability insurance in 1950; and unemployment benefits in 1958.

[8] After the economic crisis of the early 2000s, unemployment and poverty sky-rocketed. By the end of 2004, the poverty rate for urban areas reached 40%, and the unemployment rate was close to 15%.

households in the country. The program had two primary goals. First, in the short run, it aimed to alleviate the high poverty levels caused by the economic crisis.[9] Second, in the medium- and long-run, its goal was to encourage human capital accumulation in poor households to help them move out of structural poverty. The base cash transfer was USD 133, expressed in January 2008 PPP terms.[10] In addition, the program provided a supplementary transfer between USD 29 and USD 78 to households with underage children (70% of the participant households). Overall, the cash transfer represented more than 50% of the average self-reported household income in the application forms.[11]

Between 2005 and 2007, more than 180,000 different households (about 17.6% of all households in the country) applied to *PANES/AFAM-PE*. Eligibility was determined based on two criteria. First, applicant households must have a per-capita household income below USD 131 (or between 27.9% and 41.7% of the April 2005 poverty line). Second, households must have a poverty score below an arbitrarily defined threshold that varies by region. Households were visited by program officials who conducted a thorough interview to evaluate their socio-economic situation. This information was used to compute the poverty score, which consists of the predicted probability of being below a critical per capita income level.[12] Households with a poverty score above a certain threshold are eligible to participate, while households with a score below the threshold are deemed ineligible. After being accepted, participant households were supposed to satisfy school attendance, regular health check-ups, and monthly per-capita income requirements, but the program did not rigorously enforce these conditions until April 2013.

On January 1st, 2008 *PANES* was expanded and re-branded into *AFAM-PE*. While formally, *AFAM-PE* was a new program that substituted the original *PANES*, in practice, it was implemented as an expansion with very slight differences. The program's main components - i.e., eligibility criteria and type of benefits and conditionalities - remained the same. There were only three differences between *PANES* and *AFAM-PE*. The first one is that *AFAM-PE* established the presence of underage children in the household as a requirement for eligibility. The second is a more lenient poverty score eligibility threshold. This change aimed to increase the coverage of the program. Finally, the program changed

[9] In 2005, the country's poverty rate was close to 21%. However, the child poverty rate was even higher: 36.6% for all children in urban areas and 60% for children between 0-5 years old.

[10] In local currency, this corresponded to UYU 1,360. In what follows, all income variables are converted to 2008 PPP using CPI and PPP conversion factors.

[11] See Appendix **??** for a more detailed description of the characteristics of the universe of application forms. It is important to note that the income used as a reference to calculate this share is self-reported. However, since the program also had an income threshold rule to define eligibility, households may have misreported income to become eligible. Therefore this share must be interpreted as an upper bound. As an alternative reference, in April 2005, the household *per capita* poverty line was USD 314.19 for rural areas and USD 469.95 for urban areas in 2008 PPP terms.

[12] The variables used to calculate the score included the overall quality of the building, the number of people living in the household, the number of rooms, the presence of underage children, average years of education, and type of employment, among others. More details about how the poverty score was computed can be found in Appendix **??** and in Manacorda et al., 2011; Amarante et al., 2016.

the formula used to define the amount to be transferred. The new structure established a baseline payment of USD 57 per child from 0-17 but was subject to an equivalence scale of 0.6. In addition to the base payment, each household would receive an additional USD 24 per child enrolled in the secondary education system, also subject to an equivalence scale of 0.6. Finally, *AFAM-PE* beneficiaries were also supposed to fulfill education and health check-up conditionalities. However, these started to be enforced only beginning in April 2013. In subsequent years the enforcement quality depended on the will of the Ministry of Social Development and other high-ranked officials.

The transition between the two phases was straightforward. Provided that families had underage children, *PANES* participants were automatically enrolled in *AFAM-PE*. Furthermore, households rejected during the first phase were automatically enrolled in the second phase if they satisfied the new eligibility requirements. Figure 1 presents a summary of the main components of *PANES/AFAM-PE*.

3 Conceptual Framework: Cash Transfers and Decisions Within the Household

Cash transfers may cause behavioral responses in several margins across all household members. In its simplest form (i.e., unconditional), cash transfers induce a pure income effect that leads households to increase their demand for normal goods (e.g., consumption goods or leisure). However, cash transfers contingent on certain behaviors (i.e., conditional) are associated with a more complex set of potential behavioral responses. These may trigger reactions associated with a substitution effect due to changes in the relative opportunity costs of the alternatives included in individuals' choice sets. The analysis becomes even more convoluted when decisions are allowed to interact inter-temporarily or when market imperfections such as information frictions or collective household models are considered. In this section, I motivate the research hypotheses by broadly discussing how *PANES/AFAM-PE*, or any other similar CCT program, may affect education, labor market, and fertility decisions of individuals who benefited from the program in their childhood.[13] The list of mechanisms discussed in this section is not intended to be exhaustive. The goal is to provide an overview of what the literature has proposed and discussed when analyzing the effects of CCTs on education, fertility, and labor market decisions.

[13]Developing a theoretical model that contemplates all these potential interactions is beyond the scope of this paper. However, it is important to mention that Keane and Wolpin (2010), for instance, formalize a similar decision process, focusing exclusively on women's decisions. More specifically, they estimate a structural model in which women's choice set is comprised of work, marriage, schooling, fertility, and welfare participation. A very simple but illustrative example of the complexity of this setting is that women make between 18 and 36 mutually exclusive choices in each period, depending on their fecundity stage.

Income and substitution effects: Consider a simple unitary model where households decide over leisure, school, fertility, and labor market activities. In this setting, CCTs could imply both income and substitution effects. The income effect, associated with additional household resources, would increase the quantity demanded of normal goods (e.g., leisure) to the detriment of labor market activities for all household members. Furthermore, if households obtain direct utility from children's current human capital or schooling (e.g., Todd and Wolpin 2006, 2008; Keane and Wolpin 2010), the income effect could also lead to an increased demand for children's education.[14] Income effects are crucial for poor households in the presence of credit constraints. In such settings, families might decide not to send their children to school because they cannot afford it. The cash transfer would work as a mechanism that relaxes those constraints, allowing beneficiary households to increase their expenditure on school-related goods and services. This would enable children to enroll and remain at school (e.g., books, clothing, transportation costs, etc.). A similar mechanism could also explain changes in fertility decisions if, for instance, there is a direct dis-utility associated with early life childbearing, and household members cannot buy contraceptives in the absence of cash transfers.[15]

CCTs can also affect household decisions through a substitution effect since they make participation contingent on specific behaviors, typically school enrollment, attendance, and health check-ups. Education requirements reduce the opportunity cost of schooling, and make it more attractive compared to any other non-education-related activity such as labor market participation or becoming a parent (e.g., Parker and Todd, 2017). A substitution effect could also affect children's education enrollment through parents' time allocation if children's engagement with the education system depends, at least partly, on the time they spend together (e.g., in the spirit of Martinelli and Parker, 2008). In this case, the reduction in parents' time allocated to labor market activities through the substitution effect would free time that could be re-directed toward time spent with children. A reduction in parents' time allocated to labor market activities also increases children's share of supervised time. This reduces the possibility of engaging in risky behaviors that could lead to early-life pregnancies. In sum, both income and substitution effects are expected to reduce children's labor market participation, increase children's education enrollment and reduce young women's fertility when they receive the cash transfer.

Dynamic effects: The effects discussed so far correspond to a static model. When

[14] It is beyond the scope of this paper to discuss the non-pecuniary benefits of schooling or if it should be considered a (normal) consumption good. Oreopoulos (2011) and MacLeod and Urquiola (2019) provide in-depth reviews about the status of this discussion in the literature.

[15] One alternative way in which the cash transfer can affect fertility rates of young women through an income effect is when their labor market activities are associated with transactional sex activities (see Baird and Özler 2016; LoPiccalo et al. 2016 for a review of the relation between income and transactional sex)

individuals make decisions that have consequences for multiple periods, the set of potential behavioral responses becomes broader and even more complex. One example is what Black et al. (2008) refers to as the "future human capital effect". Consider the income and substitution effects discussed in the previous paragraph as effects in the "current" or today's time. The reduction in the marginal cost of schooling increases current investment in education. However, additional education today also increases the opportunity cost of education tomorrow. The more schooling children accumulate, the higher the wage offers they receive. If there are diminishing marginal returns to schooling, there would be a point where the marginal cost of an additional year of schooling will be larger than the marginal benefit. This would lead some individuals to choose labor market participation instead of more schooling (Behrman et al., 2011).

A similar reasoning can be applied to fertility decisions. There is a strong link between expected future labor market income and fertility decisions. Models that aim to characterize early fertility decisions propose that young women compare the lifetime expected utility of having vs. not having a teen birth (e.g., Duncan and Hoffman, 1990; Wolfe et al., 2001). Because they reduce the marginal cost of schooling, CCTs increase the expected utility of delaying fertility through higher expected adult wages. This leads to more women deciding to delay fertility. While delaying fertility might seem relatively costless in the short run, it is also reasonable to expect these costs to increase in the long run, for instance, due to a reduced probability of having a successful healthy pregnancy or due to an increase in biological or psychological costs associated with later-life pregnancies (Schmidt et al., 2012; Gustafsson, 2001). Hence, at some point, even when the opportunity cost of having a child is large due to high wages, the marginal cost associated with keep delaying childbearing might be sufficiently high to more than offset the potential gains in earnings. Under such circumstances, it is reasonable to expect that initial negative effects on fertility might start to fade or even reverse in the long run. In this case, one should be cautious in how the early negative effects are interpreted. These could be more associated with delays rather than actual changes in fertility preferences. However, potential effects of CCTs on overall preferences for fertility cannot be ruled out ex-ante.

Another example of how current decisions might have strong implications on future choices stems from the models of skill formation in the presence of dynamic complementarities (Cunha and Heckman, 2007). In these models, today's education investments increase education returns in subsequent stages, which promote a more extended stay in the education system to the detriment of other activities, such as labor market participation and childbearing.

It is important to note that besides the direct effects of CCTs on education, fertility, and labor market decisions, these decisions might also have direct effects on each other. For instance, education could affect fertility decisions if there is a trade-off between quality and quantity of children (Becker and Lewis, 1973); if it improves current women's

ability to predict better labor market outcomes associated with delaying childbearing (referred to as "current human capital effect" in Black et al., 2008); if it improves access to contraceptives and family planning and health care services which are critical determinants of fertility decisions (e.g., as in Kearney and Levine, 2009; Bailey, 2006; Lundberg and Plotnick, 1995); or by changing women empowerment, attitudes, and values toward maternity, just to name a few.[16,17] Fertility could also affect education decisions, for instance, through the effect of child care time on the marginal cost of school time (Klepinger et al., 1999). Similarly, education can affect labor market decisions by affecting children's perceptions about how the process of earning better wages works, the current sacrifices required for better future wages, by improving expectations about achievable goals, or by providing different role models, etc.

Other mechanisms: While in a friction-less model, conditionalities associated with cash transfers would cause efficiency losses, they usually aim to correct potential sub-optimal decisions due to market failures, such as information frictions, differences in discount rates, or intra-household bargaining problems (Parker and Todd, 2017; Baird et al., 2014). Hence, under more realistic circumstances, CCTs may also affect households' decisions through mechanisms other than the standard income and substitution effects. For instance, CCTs are usually entitled to the mother of the eligible children. Moving from a unitary to a collective household decision model (e.g., Chiappori, 1988, 1992; Browning and Chiappori, 1998) opens the door for CCTs to change household members' bargaining power (Martinelli and Parker, 2003, 2008; Attanasio et al., 2012), which could re-direct part of the household expenditure toward goods and services that are more favorable to children (e.g. Thomas, 1990; Duflo, 2003, or more specifically about *PANES/AFAM-PE* Bergolo and Galván, 2018).

The information environment and expectations about returns to education are also key determinants of current education decisions (Jensen, 2010). For instance, by participating in a CCT, parents are more exposed to highly educated professionals, which could change their expectations about the opportunities for their children and the investment required to reach them. Parents' improved expectations can also be transmitted to their children. This would lead to higher enrollment and permanence in the education system (Attanasio and Kaufmann, 2014; Chiapa et al., 2012). On the contrary, children's expected returns to education can be negatively affected by the CCT if parents

[16]Related literature (e.g., Black et al., 2008) also defines an "incarceration effect" of education on fertility, i.e., more time spent at school reduces the time available to engage in risky behavior. While this mechanism is plausible, in this discussion, it is captured by the idea that education and fertility are mutually exclusive or highly substitutes

[17]Alternatively, attending school might also increase the social interactions of young girls with other potential sex partners that they meet at school or in related environments. However, for this to have an effect, the new interactions should more than offset the existing interactions outside the school that are lost due to the increased time at education institutions.

substantially increase their time allocated to leisure activities because of the income effect. Perceptions about expected future outcomes are also highly relevant for fertility decisions. Kearney and Levine (2014) propose a model where fertility decisions are determined by the perceived probability of achieving a high utility state, which is only feasible if women delay childbearing. Perceptions of the likelihood of success are a function of current socio-economic status and inequality. Hence, CCTs may also affect fertility decisions by changing the current socio-economic situation of poor women or, more generally, by reducing inequality in their society.

Finally, alternative mechanisms such as a reduction in household economic stress that could create a better environment for child development (Gershoff et al., 2007; Yeung et al., 2002; Conger et al., 1993); or improved children's health outcomes due to better parental socioeconomic conditions (e.g., Currie, 2009); or social interactions and peer effects (e.g. Bobonis and Finan, 2009; Lalive and Cattaneo, 2009) might also affect education, fertility, labor market participation decisions.

In sum, the related theoretical literature provides mostly unambiguous predictions about the short-run effects of CCTs on education, fertility, and labor market decisions for individuals that benefited from a CCT program when they were young. More specifically, CCTs are expected to reduce teenage pregnancies, increase education enrollment, and reduce children's labor market participation. However, in a dynamic setting, the expected effects are ambiguous and depend on individual preferences and institutional characteristics. The fact that these effects can interact in complex and theoretically ambiguous ways illustrates the need for a dynamic analysis to understand how CCTs affect the current and future lives of the beneficiaries and the mechanisms involved. In the end, the effects of CCTs on the trajectories that mark children's transition to adulthood and early adulthood outcomes is mostly an empirical matter.

4 Data Sources, Measurement, and Sample of Interest

The analysis of the effects of CCTs on the transition to adulthood is highly data demanding for two reasons. First, it requires information on a large number of individual characteristics. Because adulthood is defined not just by one but by a series of markers in different life spheres - including education, fertility, and labor market markers among the most important ones - the transition to it also needs to be characterized in terms of such dimensions. Second, because transitions are a dynamic phenomenon by nature, its analysis requires longitudinal information that allows for a complete description of the individual trajectories. Having both is extremely difficult and costly.

The data used in this paper satisfy these two requirements. First, the empirical analysis is based on an exhaustive compilation of administrative records from different sources for the universe of applicants to *PANES/AFAM-PE*. These can be linked at the individual level and contain information about fertility, education, and labor market outcomes. Second, because the data is based on administrative records, all of these variables are observed for a long span of years and for the universe of interest. In the next section, I explain in detail the main features of the dataset assembled for the analysis, as well as the key outcome variables.

4.1 Data Sources and Measurement

***PANES/AFAM-PE* records: Application and participation variables.** These records are used to measure all the application- and participation-related variables, which are mostly used as treatment or control variables. They were provided by the Ministry of Social Development, which is in charge of implementing the program, and contain information about the universe of successful and unsuccessful applications to *PANES/AFAM-PE* between April 2005 and December 2017 at the form, household, and individual level. The information at the form level includes city, date of application, poverty score, resolution, and in case of acceptance, the participation history. Information at the household level includes the house's building materials, structure, appliances, and access to public services, among other information used to compute the poverty score. Individual level information contains the baseline information about education, employment status, income, date of birth, and gender, for each household member reported in the application form. The total number of application forms included in the raw participation data is 747,204, corresponding to 1,476,696 unique individuals.[18]

Birth Records: Fertility outcomes I use birth records to measure the fertility outcomes reported throughout the paper. These were provided by the Ministry of Health and consist of an individual-level dataset that includes the universe of births in Uruguay between 2005 and 2019. Birth records contain information such as birth date, type of institution where the child was born (public, private, or others), the mother's age, birth weight, and gestation weeks. In addition, they also include identification information of the mother, which allows me to link this information with *PANES/AFAM-PE* participation records at the individual level.

Concerning fertility outcomes, it is important to note that these variables are defined exclusively for women due to the typical limitations in the information reported on birth certificates about newborns' fathers. As for every outcome variable described hereon, I define two types of variables. A binary variable that indicates whether a woman has

[18] Appendix B contains a more detailed description of the participation data.

given birth *at or before* a certain age and a continuous variable that reports the number of births by a given age. The binary variable is associated with extensive margin responses, i.e., it will capture the effect of *PANES/AFAM-PE* on giving birth versus not giving birth. The continuous variable will also capture responses in the intensive margin. In addition, I define different variables for each age between 15 and 30 years old. This allows me to provide a full description of whether and how the effects of *PANES/AFAM-PE* materialize throughout the transition to adulthood. All the outcome variables are defined exclusively based on the post-application period. In the specific case of fertility outcomes, I define the post-treatment period as starting seven months after the application date. As a robustness test, I will also report estimates based on a binary variable defined *at* a given age, as opposed to *at or before*.[19]

Secondary and tertiary education administrative records: Education outcomes. I use the secondary and tertiary education administrative records to measure the by-age effects on education enrollment. These records come from three different public institutions: 1) National Council of Secondary Education, 2) National Council of Technical and Professional Education, and 3) *Universidad de la Republica*, which is the largest public university in the country. Information from the National Council of Education corresponds to the traditional public secondary education system, which is analogous to grades 1-6 of middle and high school education in the United States. These records contain yearly information for the universe of students enrolled in secondary public schools in 2006-2012, 2014, 2017, and 2018. National Council of Technical and Professional Education records contain information on vocational and technical public school enrollment for the same period. Careers offered by technical and vocational schools can be classified into middle school, high school, and tertiary careers, based on enrollment requirements.[20] Finally, the information provided by *Universidad de la Republica* consists of individual-level information that identifies the year of enrollment of every student enrolled at the University between 2005 and 2020.[21]

To analyze the effects of *PANES/AFAM-PE* on education decisions, I focus specifically on enrollment outcomes. These variables are the most complete and reliable among the ones included in the administrative records. They are also easily comparable across

[19] Appendix C provides summary statistics for each of the outcome variables based on the samples that are defined in Section 4.2

[20] For instance, a middle-school-analogous vocational education program is a program that requires individuals to have completed primary school. A high-school-analogous vocational educational program is a program that requires to have completed middle school and so on.

[21] While I do not have access to education enrollment information in private institutions for any of the education systems or levels, it is important to note that the (free) public education system is probably the relevant choice set of schools for the population of interest, given that private institutions usually offer a limited number of grants and have relatively expensive tuition. For instance Ramírez Leira (2021) shows that the probability of enrolling in a public institution for individuals in the first quintile of the income distribution is larger than 95% in 2017

levels and institutions.[22] I define education enrollment outcomes using binary and continuous variables. For secondary enrollment, The binary variable indicates whether an individual was enrolled at any secondary education institution, either traditional or vocational, at or before a given age. The continuous variable measures the total years of enrollment. In both cases, different variables are computed for ages 12-23. Enrollment in tertiary education works differently than enrollment at secondary education institutions. In particular, once registered for the first time, students are not required to re-enroll periodically to take classes. For this reason, the information provided by *Universidad de la Republica* only allows me to define a binary variable that indicates if the individual has ever been enrolled at the university or any other tertiary level course at the vocational/technical institutions, by a certain age. Tertiary education outcomes are computed between 18-23 [23]

Social Security Agency (SSA) labor histories: Labor market outcomes. I use SSA labor histories to construct the labor market outcomes. They contain monthly individual-level information about wages, hours worked, activity type, and employers' industry sector for each position for the universe of registered employees. The main limitation of SSA labor histories is that they provide information only on the formal labor market. However, it is important to note that the informal labor sector in Uruguay is currently relatively small compared to other non-high-income countries, and it only represents 17% of total employment. In any case, to interpret the results reported in this paper, one should keep in mind that someone that does not show up in SSA labor histories can be either unemployed or employed in the informal sector. Therefore, results must be interpreted exclusively as concerning the formal employment sector.

To analyze the effect of *PANES/AFAM-PE* on labor market outcomes, the baseline binary labor market outcome indicates whether someone has had a registered employment spell that lasted at least four consecutive months at or before a given age. Using spells of four consecutive months rules out potential temporary employment, such as Summer jobs, and reflects more stable links to the labor market. In addition, I define two complementary continuous variables. First, the total number of months an individual has worked in the formal sector by a certain age. Second, a similar variable that measures cumulative earnings, i.e., the sum of all labor income earned by a given age. These three variables are calculated for ages between 14 and 30 years old. As in the other dimensions, I report

[22] It is important to note that students who promoted the current grade are automatically enrolled for the next academic year. Hence, enrollment variables do not necessarily represent an explicit decision to sign up for the current academic year. Moreover, to some extent, individual enrollment for a given grade could be interpreted as a signal of academic progress.

[23] 95% of the individuals in the sample who were ever enrolled in tertiary education did it before the age of 23. Because the share of individuals ever enrolled in tertiary education is already extremely low, first-time enrollment between 23-30 only corresponds to a handful of cases. Hence, for simplicity, I report estimates for tertiary enrollment only until age 23.

robustness tests using the same variables defined *at* a given age, instead of *at or before*.

4.2 Sample of Interest: Definition

Since this paper focuses on the effects of *PANES/AFAM-PE* on the transition to adulthood for children that benefited from the program during their childhood, the empirical analysis is restricted to individuals of households that applied to the program when they were eighteen years old or younger and had at least fifteen years old by April 2018. The latter restriction ensures that every individual included in the analysis has had the chance to enroll in high secondary education for at least one year. In addition, the analysis is always restricted to individuals of households who applied for the first time between 2005-2012, which represent 95% of the sample.

A dynamic analysis of the age-by-age effects of the program on transitions to adulthood presents some challenges associated with the definition of the sample of interest, especially for a permanent and relatively young program such as PANES/AFAM-PE. For instance, it is impossible to calculate the effect of the program at age 30 for someone that has not turned that age by the time the outcomes are last observed.[24] The simplest alternative would be to keep the sample composition constant and only use individuals who have already turned 30 on a given date. However, this would substantially reduce the sample size and compromise estimates' statistical precision. To balance the trade-off between keeping the sample composition constant and maximizing the use of information available, I present two main sets of results. First, a group of estimates based on a *main sample* comprised of 23 years old or older individuals in December 2019. Second, for the dynamic analysis and estimates at older ages, I use a series *dynamic samples* that aim to include as many observations as possible. These *dynamic samples* vary their composition depending on the age at which an outcome is measured but use the maximum amount of information available.[25] However, for the sake of transparency, I will also report the most conservative estimates based on an extreme-balanced sample of individuals who were 30 years old by December 2019. Estimates based on this fully-balanced sample allow us to completely rule out that the results are driven by changes in the composition, although at the cost of statistical precision.

[24] Appendix C reports the distribution of ages on December 31, 2019. This corresponds to the last day available in birth records.

[25] For instance, estimates of the effect of the program at age 27 will be based on individuals who had already turned 27 by the time an outcome is last measured, which depends on the outcome.

4.3 Sample of Interest: Description

Table 1 describes the main characteristics for the *main* (columns 1 and 2) and the *dynamic* (columns 3 through 6) samples.[26] Columns 3 and 4 describe the sample of analysis used for fertility estimates at age 30, while columns 4 and 5 do the same for the sample used for labor market outcomes. These are two extreme examples of the *dynamic samples* used in the longer run analysis. Odd columns include all individuals in each of the samples. Even columns are restricted to individuals with an application score within the optimal RDD bandwidth chosen for the baseline estimates. The procedure used to select the optimal bandwidth is explained in detail in Section 5.[27]

Panel a. focuses on individual characteristics. There are 224,413 individuals in the *main sample* who are equally split between men and women, were on average 26.9 years old by December 31, 2019, and belonged to about 1.8 households. Individuals are typically included in 2.6 application forms. The average age at first application is 13.4 years old. About 84.2% of these individuals were accepted to *PANES/AFAM-PE* at least once before age 18.[28] 78.4% of the individuals show up in at least one application form to the *PANES* phase. At the same time, 96.1% are included in at least one *AFAM-PE* form. Panel b. describes the characteristics of the application forms for these individuals. Because they may be included in multiple application forms, this table reports the characteristics of the earliest application form filled.[29,30] The average centered poverty score is 0.18. This means that the average application corresponds to an eligible form. Consistently, the share of individuals whose first application form was accepted is 71.8%. The first application form for most individuals (78.4%) was filled during the *PANES* phase, and 31% of the applications corresponded to individuals of households in the capital city (Montevideo). This means that individuals from the capital city are under-represented in this sample since about half of Uruguay's population lives in Montevideo. Finally, panel c. describes the household characteristics. Individuals in the sample belong to households that are comprised, on average, of 4.9 individuals, of whom 2.9 are underage children. Slightly less than half of the households correspond to single-parent households, and the average age of household members (including children) is 23.1. Household heads have, on average low education levels, i.e., about seven years, slightly more than the equivalent of completed primary school. 63.4% of them are employed, and their average income is USD 144.33, which is comparable in size to the cash transfer value, as described in Section 2.

[26] Appendix C report more detailed descriptive statistics at the form level, including information about the 747,204 application forms filled between 2005 and 2017

[27] For exposition purposes, to describe the samples used in the analysis, I selected the largest optimal bandwidth among the estimates that use each specific sample.

[28] Appendix C provides the full distribution of age at first application and age at first acceptance for both *main* and *dynamic* samples.

[29] A more detailed discussion of the reasons for selecting this application form is provided in Section 5, since this is also a critical decision for the empirical design

[30] Appendix C compares the characteristics of *all* application forms versus *first* application forms.

The sub-group of individuals that belong to the *main sample* and have a poverty score within the optimal bandwidth has very similar characteristics to the full *main sample* in terms of variables that are not related to the poverty score. These are different by construction. Besides these variables, the exception is on the share of individuals from the capital city, which is smaller than in the full *main sample*. The *dynamic samples* also closely resemble the *main sample* except for age-related variables, which are mechanically different. Individuals in the *dynamic samples* are, on average, between 3 and 4 years older than in the main sample (31.1 and 29.97 compared to 26.91), and were also older when they applied to the program for the first time (16.8 and 15.98 years old compared to 13.4). Because of the age restriction used to define these *dynamic samples*, most of the first application forms for these individuals corresponded to *PANES* applications. This also implies that they had less potential time of exposure before turning 18 years old and translates into a lower share of individuals ever accepted to *PANES/AFAM-PE* (71.0% and 80.9% compared to 84.2%). Except for the mechanical differences in variables related to the definition of each sample, the subgroup of individuals that comprise the *dynamic samples* is very similar to the full *main sample*.

5 Empirical Strategy

As described in Section 2, eligibility to participate in *PANES/AFAM-PE* is based on a poverty score. More specifically, let z be the poverty score centered around the eligibility threshold and D an indicator variable such that positive values of z indicate eligibility (i.e., $D = 1$) and negative values indicate ineligibility (i.e., $D = 0$). The use of an arbitrary threshold to define whether a household is eligible to participate in *PANES/AFAM-PE* provides a quasi-random source of exogenous variation to identify the causal effects of the program using a Regression Discontinuity Design (RDD) (Thistlethwaite and Campbell, 1960). Intuitively, under perfect compliance and a continuity assumption, (local) average treatment effects of the program can be obtained by comparing the regression functions of the outcome of interest at both sides of the threshold (Hahn et al., 2001).[31]

To illustrate how the *PANES/AFAM-PE* eligibility rule works, Figure 2 describes the relation between a variable that indicates if an application was successful (*y-axis*) and the centered poverty score (*x-axis*). Panel a. depicts this relation for the full support of the running variable. Each bin in the figure represents the percent of accepted forms within the bin.[32] In the background, vertical bars represent the distribution of the poverty

[31] Formally, let Y be any of the outcomes of interest. Under perfect compliance, the key identification assumption in RDD is that Y is continuous at $z = 0$ if the regression functions for the outcome variable - $\mathbb{E}[Y(1)|Z = z]$ and $\mathbb{E}[Y(0)|Z = z]$ - are continuous functions at $z = 0$, then: $\mathbb{E}[Y(1) - Y(0)|Z = z] = \lim_{z \downarrow 0} \mathbb{E}[Y|Z = z] - \lim_{z \uparrow 0} \mathbb{E}[Y|Z = z]$.

[32] To the left of 0, observations are binned in ten quantile-spaced bins. To the right of 0, observations are binned in fifty quantile-spaced bins. The relation in the number of bins used at each side of 0 is based

score. Panel b. zooms into observations close to the threshold, i.e., within five percentage points distance, grouped into 10 bins of half percentage point width. From the program administrator's perspective, the eligibility rule was applied correctly, although not perfectly. Figure 2 shows a pronounced change in the acceptance rate just at 0, i.e., at the eligibility threshold. The size of the change is 60.0p.p., and it is statistically significant at usual levels ($p-value < 0.001$).[33] Different reasons can explain the fuzziness observed on both sides of the threshold. For instance, to the left of 0, it could be due to applications below the eligibility threshold that were rejected when filed but were automatically enrolled after the threshold became more lenient.[34] On the right-hand side, it could be due to rejections based on reasons other than the poverty score, such as income or no qualifying underage children.

Despite this sharp change, the use of an RDD in the *PANES/AFAM-PE* setting presents one additional challenge. As discussed in Section 2, *PANES/AFAM-PE* has been in place uninterruptedly since 2005. This means that households might have applied to the program multiple times, introducing two concerns about key elements of the research design. The first one is how to define the running variable when households have multiple application scores. The second is how to address the possibility of endogenous sorting around the eligibility threshold induced by re-applications. This could happen if re-applicant households that are close to the threshold and were originally rejected are different from non-reapplicant households, and these differences are correlated to the outcomes of interest. In this case, the RDD estimates will be biased.[35] To address these concerns, I follow the approach proposed by Jepsen et al. (2016) who suggest implementing a fuzzy RDD where eligibility based on the first application score is used as an instrument for treatment in contexts where there are re-applications. The intuition is that the first score is presumably the score that is less subject to manipulation.[36] Hence, the RDD will be based on the following variables:

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on the relative number of observations between the two sides.

[33]The procedure used to calculate the change in the probability of acceptance is based on the data-driven approach proposed by Calonico et al. (2019). Hence, the optimal bandwidth is selected such that it optimizes the Mean Squared Error (MSERD). This will be explained in detail before the end of this section.

[34]Unfortunately, the administrative records do not identify these cases, and date of application corresponds to the day on which the application was submitted.

[35]Endogenous sorting in settings with multiple applications is also an issue in different contexts such as close elections (e.g. Cellini et al., 2010), analysis of returns to education using test scores (e.g. Clark and Martorell, 2014); or evaluation of the effects of remedial education(e.g., Martorell and McFarlin, 2011).

[36]Jepsen et al. (2016) analyze the effects of GED scores on employment and earnings. In this setting, the discontinuity exploited is the passing grade of the exam, and concerns about endogenous sorting arise because students can take the exam multiple times. The issue for identification is that re-takers can be different from non-re-takers in ways that are also correlated with the outcome of interest. If this is the case, using the final score obtained in the GED exam will not provide an adequate source of identification for the effects of the GED.

Exogenous variable: eligibility based on the score of the first application form (D^{1st}): I define the first application form (or reference form) as the earliest application form by any of the households that an individual has ever belonged to, as long as the individual had not left the household by the time of application. By going as far back as possible when defining the value of the running variable, I am taking a conservative approach to minimize any possible concern about endogenous sorting.[37] Hence, eligibility based on the first application is a binary variable that takes the value of 1 if the score obtained in the first application corresponds to an eligible form and 0 otherwise.

Endogenous variables: participation in *PANES/AFAM-PE* (T): The baseline treatment variable (T) is a binary variable that indicates whether an individual was ever accepted to *PANES/AFAM-PE* before turning eighteen years old. In addition, I define analogous variables for ages twelve through seventeen that will be used in estimates where the outcome is measured before 18 years old. As a robustness test, I present estimates based on two complementary continuous treatment variables: 1) the number of months treated and 2) the net present value of the cash transfer collected by the household. Hence, the analysis of the causal effects of *PANES/AFAM-PE* on the different outcomes of interest is based on the following specification:

$$Y_i = \mu + \tau T_i + \beta_1 Z_i^{1st} + \beta_2 Z_i^{1st} T_i + u_i \tag{1}$$

where Y_i is the outcome of interest for individual i, (Z_i^{1st}) is the score obtained in the first application, and T_i corresponds to i's treatment status. Because T_i and Y_i are endogenous, T_i is instrumented using D_i^{1st} based on the following first-stage equation:

$$T_i = \alpha + \delta D_i^{1st} + \gamma_1 Z_i^{1st} + \gamma_2 Z_i^{1st} D_i^{1st} + \epsilon_i \tag{2}$$

Following Imbens and Lemieux (2008) and Calonico et al. (2014), I estimate this model using local linear regressions fitted separately to each side of the threshold with observations that are sufficiently close to it. The estimation procedure follows Calonico et al. (2014), who provide robust standard errors and confidence intervals. The threshold is defined optimally following the data-driven approach by Calonico et al. (2019) and the default options: selection of bandwidth by optimization of Mean Squared Error (MSERD) and a triangular kernel function that puts more weight on observations that are close to the threshold. To assess the robustness of the results to these arbitrary choices, I present specification curves based on all possible combinations of options for each baseline outcome. In all cases, standard errors are clustered at the household level.

[37] For instance, household h_1 applied to *PANES/AFAM-PE* with forms $f_{h_1,A}$ and $f_{h_1,B}$. Individual i was born in h_1 after $f_{h_1,A}$ was filed, but before $f_{h_1,B}$ was filed. In this case, $f_{h_1,A}$ is still the reference form for individual i, even when she was not included in $f_{h_1,A}$.

The described strategy provides an estimate that should be interpreted as a local average treatment effect. In addition, I will also report the intention to treat effects. The difference between the ITT and LATE effects is that the latter scales up the ITT effect by the size of the first stage, i.e., by the actual change in the probability of participation at the eligibility threshold. ITT estimates are based on the following specification:

$$Y_i = \tilde{\mu} + \tilde{\tau}\mathbb{1}(Z_i^{1st} > 0) + \tilde{\beta}_1 Z_i^{1st} + \tilde{\beta}_2 Z_i^{1st}\mathbb{1}(Z_i^{1st} > 0) + u_i \tag{3}$$

Equation 3 is the reduced form specification for equation 1, but using an indicator variable for eligibility ($\mathbb{1}(Z_i^{1st} > 0)$) instead of the treatment binary variable (T). The coefficient of interest is $\tilde{\tau}$, the ITT effect, which measures the difference in the intercepts of the two local linear regressions fitted separately to each side of the eligibility threshold within the optimal bandwidth. ITT estimates are important to provide an idea of the magnitude and standard errors associated with the visual representations of the RDD.

Finally, it is important to note that, compared to sharp RDDs, fuzzy RDDs require an additional identifying assumption of monotonicity or "no defiers" (Imbens and Lemieux, 2008; Cattaneo et al., 2019). In this paper, monotonicity implies that an application form with a score z that is rejected when the threshold is set at 0 would also be dismissed for any alternative threshold greater than 0. Conversely, any application form with a score z that is accepted when the cutoff is 0, would also be accepted if the cutoff is $\tilde{z} < 0$.

6 Results

In this section, I present the main empirical analysis. First, I illustrate the validity of the RDD by reporting first-stage results, manipulation, and balance tests used typically in these settings. Second, I report the reduced form estimates (ITT), including visual evidence, and the LATE effects for a group of baseline results measured at ages 18, 23, and 30. For fertility and labor market outcomes, I report estimates for each of these three ages. For education outcomes, I focus exclusively on outcomes measured up to the age of 23. This is because after 23 years old, there are almost no changes in education enrollment variables. Education variables at age 18 correspond to secondary education, while at 23 correspond to tertiary education. Traditional and vocational/technical school enrollment variables are pooled in both cases. Third, I report analogous estimates focusing on heterogeneity by sex. Finally, I report the full dynamic analysis measuring the effects of *PANES/AFAM-PE* at all possible ages.

It is important to note that the choice of the specific age cutoffs used for the baseline results is arbitrary and mainly for illustration purposes. Estimates on outcomes measured at 18 and 23, based on the *main sample*, are chosen to coincide with the age at which someone on track would complete secondary and tertiary education, respectively. Esti-

mates on outcomes measured at age 30, based on the *dynamic sample*, are the last ones to provide a reasonable sample size to implement an RDD. This way of presenting the results is also illustrative of the two typical snapshots of early- and later-life results (18, and 30 years old, respectively) that can be found in the literature. In any case, the full set of results is discussed in the dynamic analysis, which provides the full description of the trajectory of the effects.

6.1 Validity of the RDD Design

In this section, I report evidence that supports the use of an RDD to analyze the causal effects of *PANES/AFAMPE*. First, Figure 3 depicts the relation between the score obtained by an individual in her first application form (Z^{1st}) - measured in the *x-axis* - and *PANES/AFAM-PE* participation before eighteen years old (T) - measured in the *y-axis*. Panel a. reports the relation for the full support of Z^{1st}, while panel b. zooms into a narrower bandwidth of 5p.p.. In all cases, as in Figure 2, each circle represents the average value of the treatment variable within a 0.5p.p. width bin.

Overall, Figure 3 shows an abrupt discontinuity in the probability of ever being accepted into the program before turning eighteen years old, just at the eligibility threshold. This probability changes by 50% (29.3p.p.), and the change is statistically significant at traditional levels ($p-value \leq 0.001$). Table 2 presents the analogous regression estimates. Column (1) reports the baseline estimates using a linear polynomial function and a triangular kernel function, while columns (2) through (4) show that the baseline specification is robust to changes in the polynomial degree and kernel function.[38]

In addition, Figure 4 and Table 3 report some of the typical tests performed when using RDD to validate the identification assumption of continuity. Panel a. in Figure 4 illustrates that the distribution of the poverty score is smooth around the threshold. Panel b. provides a formal test of continuity of the running variable based on Cattaneo et al. (2018) and McCrary (2008). This test provides no evidence to reject the null hypothesis of continuity ($p-$value=0.715). Table 3 reports the RDD analogous to a balance table comprised of a series of falsification tests that replicate the baseline RDD strategy on pre-treatment covariates. As expected, Table 3 shows that baseline variables are continuous at the threshold. When p-values are adjusted by the expected false discovery rate (Anderson, 2008), all estimates are statistically insignificant.[39] Furthermore, when the falsification test is conducted on a variable that predicts the eligibility status based

[38] Appendix D reports several additional robustness tests, such as using alternative endogenous variables, estimates on the dynamic samples, and falsification tests.

[39] When taken individually, in some cases there are statistically significant differences, but in all cases, these are economically irrelevant. For instance, the average age of household members for eligible individuals in the *main sample* is 0.31 years higher compared to ineligible individuals. Eligible individuals also live in households with, on average, 0.07 fewer members and are also 0.13 years younger by December 2019.

on all the other baseline covariates, there are no signs of discontinuity at the threshold (p-value = 0.635). This indicates that observable characteristics do not change abruptly at the threshold.[40] Overall, the tests reported in this section provide robust support for the validity of the identification strategy. However, out of caution, the empirical analysis will be complemented with several tests to further prove their robustness.

6.2 Baseline Estimates

Figures 5 and 6 depict the corresponding binary or continuous outcome variable as a function of the score obtained in the first application. For an easier comparison across outcomes, visual evidence is reported for a bandwidth of ± 5p.p.. Observations at each side of the threshold are grouped into bins of 0.5p.p. Table 4 reports the analogous regression estimates for a bandwidth chosen by optimizing MSERD (Section 5 described this in detail). For consistency, and to provide the most transparent representation possible, Table 4 and Figures 5 and 6 are based on specifications without additional covariates. Table 5 reports the baseline LATE results.

Fertility outcomes

Panel a1 in Figures 5 and 6 already show signs of a discontinuity at 0 both for the probability of having a birth before the age of 18, and the number of births by the same age. The fact that the visual evidence already shows clear signs of a discontinuity, even when a large share of ineligible individuals - based on the first score - has participated in the program, is suggestive of the strength of the effects. Table 4 reports formal estimates of the size of these changes. When measured at age 18, eligibility to participate in *PANES/AFAM-PE* has an ITT effect on the probability of giving birth of -3.1p.p. This effect represents 13.5% of the number of women in the ineligible group within the optimal bandwidth who gave birth by age 18, and is statistically significant at traditional levels (p-value = 0.005). A similar effect is observed in panel b. for the number of births, which decreased by 0.038 (or 14.9% with a p-value = 0.003). LATE estimates reported in Table 5, which re-scale the ITT effects by the size of the first stage, show effects of -9.4p.p. (-41.2%, p-value = 0.015), and -0.108 (-41.9%, p-value=0.015), respectively.

Both the visual and the econometric evidence suggest a decay in the effects when measured at age 23. For instance, the estimated LATE effect in the probability of having a birth before the age of 23 is -0.047 (-8.52%) and statistically insignificant. The effect on the number of births remains statistically significant at a 10% level, but is less than half of the effect estimated for age 18 (-41.9% vs. -16.86%). A more drastic attenuation,

[40] Appendix D reports visual evidence about the continuity of the predicted eligibility status and similar estimates for the *dynamic samples*. Both pieces of evidence also support the baseline variables' continuity at the threshold.

or even reversal, is observed when the outcome is measured at age 30. In this case, both the binary and continuous variables seem to have been unaffected by the program. Furthermore, in the case of the probability of having at least one birth before the age of 30, the effect has even changed its sign, although it is statistically insignificant (p-value = 0.288). Overall, the results suggest that *PANES/AFAM-PE* negatively affects fertility at early ages, but these effects attenuate at age 23 and even reverse by age 30. I will go back to this discussion in Section 6.4 when I report the age-by-age results for the whole period covered by the data.

The results reported on the baseline set of fertility outcomes indicate a strong and negative effect of *PANES/AFAM-PE* on the probability of having a teenage pregnancy. This effect is statistically significant and economically relevant. For instance, a 41.9% reduction in the number of births by age 18 is equivalent in percentage terms to the reduction observed in Uruguay's adolescent fertility rate between 1960 and 2020, which changed from 5% to 3% in the period. Compared to other policy interventions carried out in Uruguay, the effects of *PANES/AFAM-PE* are substantially larger than, for instance, legalization of abortions (Cabella and Velázquez, 2022), or a large-scale intervention that granted access to subdermal contraceptive implants (Ceni et al., 2021). The effect is also consistent with very recent empirical evidence from other programs in high-, middle-, and low-income countries. For instance, in the US, Michelmore and Lopoo (2021) find that additional exposure to the EITC during childhood leads to a 2%–3% decline in a woman's likelihood of having a first birth by her early 20s. Perhaps in a more similar context, Attanasio et al. (2021) find remarkably similar estimates of the effects of an expansion of *Familias en Accion* on teenage pregnancies measured at age 18 of -9.3p.p., while Barham et al. (2018) find that a CCT in Nicaragua reduced the number of women's births at ages 18-21. A qualitatively similar result is observed for a temporary cash transfer implemented in rural Malawi, although, in this case, the effects were observed for an unconditional type of transfer (Baird et al., 2011).

Education outcomes

The effects on education outcomes are more nuanced. In the case of secondary education, measured at age 18, the visual evidence reported in panels b1. of Figures 5 and 6 shows mixed evidence. First, there is no sign of a discontinuity when using the binary variable as the outcome variable. This is confirmed by the econometric evidence reported in Tables 4 and 5 that show ITT and LATE effects of 0.008 (1.06%) and 0.017 (2.21%), respectively, both statistically insignificant (p-value=0.204 and p-value=0.469). On the contrary, when looking at the number of years enrolled in secondary education, both the visual and the econometric evidence suggest a positive effect, with an ITT effect of 0.086 years (3.41%) and a LATE effect of 0.253 (10.05%), both statistically significant (p-value=0.029 and p-value=0.027).

The program does not seem to affect individuals' enrollment in tertiary education, measured at age 23. The ITT estimates show an increase of 0.5p.p. (5.83%) on the probability of enrollment, but imprecisely estimated and statistically insignificant (p-value=0.445). A similar null effect is observed for LATE estimates (p-value=0.526). Unfortunately, the data available does not allow me to analyze any measure of academic progress in tertiary education.

The more nuanced evidence on the effects of cash transfers on secondary education enrollment outcomes is consistent with findings in related literature. On the one hand, the moderate increase of about a quarter of a year in secondary education enrollment is similar to previous findings that show that conditional cash transfers programs improve years of education between 0.2-0.4 years (Araujo and Macours, 2021; Behrman et al., 2011; Barham et al., 2018). In a different setting, but also focusing on the role of a cash transfer program, Aizer et al. (2016) find that the Mothers' Pension program in the US increased children's years of education by 0.3-0.4 years. Additional literature provides evidence of both stronger and weaker effects. For instance, Attanasio et al. (2021) report that *Familias en Accion* strongly reduced dropouts by 18p.p.; Molina Millán et al. (2020) find that a CCT program in Honduras led to a large increase in secondary completion rates; and Cahyadi et al. (2020) find an increase of 29% in high school completion for an Indonesian cash transfer program. On the contrary, some additional evidence suggests null (Dustan, 2020), or even negative (Bastian et al., 2022) effects of cash transfers or expanded access to welfare.[41] Focusing on tertiary education outcomes, the null effects of *PANES/AFAM-PE* are in contrast with Molina Millán et al. (2020) who find a strong increase in the probability of reaching university, or with Attanasio et al. (2021) who find an increase in tertiary education enrollment for men. Similarly, some evidence about the effects of social safety net policies in the US point in the same direction and find positive effects of increased income on college enrollment (Bastian and Michelmore, 2018; Manoli and Turner, 2018).

Together, the results reported in this section indicate that the mixed evidence for education outcomes is explained by the margin of response considered. In particular, behavioral responses seem to be associated with the intensive margin, i.e., an increase in the number of years enrolled, rather than by changes in the extensive margin, i.e., the probability of being enrolled. Howecver, the comparison in the previous paragraph must be taken with a grain of salt since the estimates reported so far only inform about enrollment. Additional evidence reported in Section 6.4 provides preliminary evidence of the effects on *PANES/AFAM-PE* academic progress.

[41] For instance, Dustan (2020) finds null effects of a CCT implemented in Mexico City that paid students to be enrolled in a public high school. Bastian et al. (2022) find evidence that an expansion of the safety-net reform in the US might have reduced educational attainment for women and had small positive effects on men.

Labor market outcomes

Effects on labor market outcomes measured at age 18 are null. First, the visual evidence reported in panels c1 of Figures 5 and 6 does not suggest any evidence of discontinuity at the threshold neither for the binary or the continuous variable. The regression estimates for ITT and LATE both indicate similar patterns, with the effects being small and statistically insignificant.

When looking at age 23, the picture is extremely different. The visual evidence reported in panel c2 depicts a sizable jump in both outcome variables. The regression estimates show an ITT effect on the probability of having had at least one spell of four consecutive months in the formal labor market of 2.0p.p. (3.85%) and of 0.816 (3.65%) on the number of months worked. Both effects are statistically significant (p-value=0.022 and p-value = 0.045). Similarly, the estimated LATE effects are 6.4p.p. (9.69%) for the binary variable (p-value=0.062) and 4.4 months (19.77%) for the continuous variable (p-value=0.005). Effects of this size are not rare for this program. For instance, Bergolo and Cruces (2021) find that *PANES/AFAM-PE* had a similarly-sized effect but with the opposite sign on parents' adult formal labor market participation. It is important to note that this effect is explained by the income threshold used to define eligibility for the program, which mostly affects adults' labor market decisions.

As in the case of women's fertility, by age 30, both the ITT and LATE effects on labor market outcomes seem to have attenuated. For instance, the estimated LATE effect on the extensive margin of participation is 1.6p.p. (1.98%) and statistically insignificant ($p - value = 0.570$), and 1.259 months (2.31%) for the number of months worked, also statistically insignificant (p-value=0.324).

Positive effects on labor market outcomes have been found in recent literature for other cash transfer programs. For instance, Barham et al. (2018) find that a CCT program in Nicaragua increases employment and earnings at the ages 19-22. However, the attenuation of the effects by the age of 30 contrasts with Araujo and Macours (2021) who find that, by this same age, PROGRESA improved earnings by 16%. For the US, most of the evidence also suggests a positive effect of increased exposure to social safety net policies (Barr et al., 2022; Bailey et al., 2020; Bastian and Michelmore, 2018; Aizer et al., 2016), although the only purely experimental piece of evidence suggests that SIME/DIME had null effects on children's labor market outcomes (Price and Song, 2018).

The set of results reported so far illustrates the overall effects of the program on fertility, education, and labor market decisions in the way that snapshots would do. Taken individually, the effects are consistent with most of the literature but fail to explain how *PANES/AFAM-PE* has affected the full transition to adulthood. The fact that the effects are substantially different depending on the outcome and the age at which they are estimated highlights the need to analyze the trajectories more in detail.

Robustness Tests

Appendices E and F report additional robustness and sensitivity tests to validate the ITT and LATE results reported in the previous sections.

Randomization Inference: First, in the spirit of randomization inference, I replicate the baseline ITT estimates using different placebo cutoffs. More specifically, I iterate the baseline ITT specification using every possible cutoff in the range [-0.08,0.50] in steps of 0.0025, excluding values close to the actual threshold, i.e., between -0.01 and 0.01. These tests show that estimates that are statistically significant using the true cutoff fall in the extremes of the distribution of the placebo estimates. A similar pattern is obtained when looking at the sorted p-values. Furthermore, as expected, all the distributions of the placebo estimates are centered around 0 and have averages that are very close to 0.

Specification curves: Second, to rule out that the effects are driven by specific choices of the RDD parameters, I report specification curves for each estimate included in Tables 4 and 5. More specifically, I plot the point estimate and 90% confidence intervals for all possible combinations of choices of 1) criteria used to define optimal bandwidth, 2) kernel functions, 3) polynomial degree, and 4) use of covariates, sorted by point estimate. Overall, the specification curves illustrate that the size and direction of the effect are not driven by a specific choice of one of these parameters. Furthermore, the baseline estimates are usually close to the median estimates and, if anything, err toward null effects.

Inclusion/exclusion of covariates Third, I test whether ITT estimates are robust to the inclusion/exclusion of additional baseline variables as control variables. Both for ITT and LATE estimates, the inclusion/exclusion of covariates provides estimates that closely resemble the baseline specifications in magnitude, size, and statistical significance.

Balanced sample: Fourth, I report the baseline estimates but using a fully balanced sample instead of the *main sample*. The fully balanced sample is comprised exclusively of individuals who were 30 or older in December 2019. Hence, the sample composition is held constant for every estimate reported in these tables. ITT estimates are very similar in direction and slightly stronger in size. Furthermore, the balanced sample shows a weak positive ITT effect on secondary enrollment by age 18, even when using the binary outcome variable. However, in most cases, the effects are more imprecisely estimated because of the substantial reduction in the sample size. LATE estimates based on the balanced sample are also very similar to the baseline estimates based on the *main sample*, although with some differences in the magnitude and, in some cases, in the statistical significance of the effects due to the reduced sample size. The more pronounced difference is observed

in the years of secondary education enrollment, which is substantially smaller and statistically insignificant, contributing to the nuanced pattern of effects in this dimension

Alternative endogenous variables: Finally, I replicate the baseline LATE analysis but use alternative definitions of the endogenous treatment variable. First, I substitute the binary treatment variable for a continuous variable that indicates the number of years in the program before turning 18. Estimates based on this alternative definition are almost identical in direction, statistical significance, and size when scaled up by the average value of the treatment variable. The same is true for estimates based on a continuous variable that measures the net present value of the total cash transfer amount collected by the household before the individual turns eighteen years old.

6.3 Heterogeneous Responses by Sex

In this section, I replicate the baseline estimates but split the sample by gender. Tables 6 and 7 report the LATE estimates for men and women respectively, while Figure 7 summarizes these results and reports the p-value of a test of difference of coefficients between these two groups. Panel a. reports estimates for the binary outcome variables, while panel b. reports the estimates associated with the continuous variables. To make the comparison easier, the figure reports standardized effects.[42]

First, the estimated effects on the probability of ever being enrolled in secondary education are small (4.55% and 0.65%) and statistically insignificant for both groups (p-value = 0.341 and p-value = 0.807). The effects on the number of years enrolled are also very similar (10.89% and 8.60%) but only statistically significant for men. In both cases, the differences between men and women are statistically insignificant (p-value = 0.549 and p-value = 0.767, respectively). The same is observed for tertiary education enrollment, where differences between men and women are not significant either (p-value = 0.392). The existing evidence on the effects of cash transfers on education outcomes does not provide a clear pattern of heterogeneous effects by gender either. For instance, while Araujo and Macours (2021) find that educational gains from PROGRESA are slightly larger for women, Parker and Vogl (2018) find stronger effects on college enrollment for men, consistent with the evidence reported for Colombia in Attanasio et al. (2021). On the other hand, for a CCT in Honduras, Molina Millán et al. (2020) find similar effects for men and women.

Unlike education outcomes, the effects of *PANES/AFAM-PE* on labor market outcomes present a clear and strong differential pattern between men and women. Measured at age 18, the LATE effect on having worked four consecutive months is 2.1p.p. (9.7%) for women, but -0.5p.p. for men (-2.1%). However, even in spite of the different signs

[42]Because fertility variables are only measured for women, the estimates reported in Figure 7 are the same as in Table 4, except for the standardization.

of the effect, both coefficients taken individually are statistically insignificant, and so is the difference between them (p-value = 0.710). Stronger differences are observed when comparing the effects on the number of months worked (-0.253 vs. 0.455). However, it still cannot be ruled out that both coefficients are the same ($p - value = 0.344$). When measured at age 23, the differential effects become larger. When looking at the binary outcomes, the effect of *PANES/AFAM-PE* on women is 11.2p.p. (17.0%, p-value = 0.051) versus 3.4p.p. (6.7%, p-value = 0.268) for men. As in previous cases, I still cannot rule out that both effects are the same ($p - value = 0.369$). Regarding the number of months worked, for men, the estimated effect on the number of months worked is -0.764 (-3.40%) and statistically insignificant ($p - value = 0.975$). For women, the estimated effect is 5.92 months (26.5%), and statistically significant ($p - value = 0.009$). The differences between the two coefficients now become statistically significant at a 5% level ($p - value = 0.046$). Finally, a similar comparison can be made for estimates measured at age 30. However, the effects are more imprecisely estimated due to the reduced sample size, and neither men nor women show statistically significant effects on their labor market outcomes by this age. Despite being less precisely estimated, the effects are still larger for women, and we can rule out that both effects are the same at a 10% (p-value = 0.099). A more in-depth discussion of this differential pattern is presented in the next section when the full dynamic effects are described.

6.4 Dynamic Effects

In this section, I provide a more thorough analysis of the effects of *PANES/AFAM-PE* measured age-by-age. In particular, I report results for outcomes measured as early as age 12 in the case of education outcomes and at age 15 for fertility and labor market outcomes. To maximize the use of information, the effects reported in this section are based on the *dynamic samples*. However, to rule out that the estimated trajectories of the effects are driven by changes in the sample's composition, Appendix G reports estimates based on the fully balanced sample. The main caveat with using the balanced sample is that estimates are more imprecisely estimated due to the reduced sample size. Given the strong heterogeneous effects reported in the previous section, especially for labor market outcomes, the dynamic analysis is presented for men and women separately.

Fertility outcomes

Figures 8 and 9 report the main findings for the binary and continuous variables. To make comparisons easier, estimates for continuous variables are expressed in standard deviations.[43] In panel a. I report estimates of the effect of *PANES/AFAM-PE* on fertility

[43] Appendix G include Tables with point estimates, standard errors, robust p-values, and p-value of the equality of coefficients tests.

outcomes measured at different ages. For instance, the estimated effect on the binary outcome reported at age 25 corresponds to the effect on the probability that a woman has given birth at or before age 25. The trajectory of the effects on fertility outcomes is similar for the binary and continuous variables. In both cases, the program's effects are strong, negative, and statistically significant when measured at ages around 17 and 18 years old. Estimates are also negative between 20 and 25 years old but slightly smaller in magnitude and statistically insignificant.[44] The effects start to trend toward the positive side starting at age 25. Overall, this pattern suggests that the effect of the program, at least until the age of 30, is associated with a postponement of the age of women's first birth rather than changes in overall preferences for the number of children.

The postponement effect is consistent with the scarce existing related literature that also finds stronger effects of cash transfers on fertility at early ages. For instance, using cross-section data, Araujo and Macours (2021) find that PROGRESA increased the age at which women had their first child by 0.5 years. In a very different context, Michelmore and Lopoo (2021) shows that exposure to EITC benefits in the US has stronger effects on early-life pregnancies around the age of 20 compared to the effects estimated around the mid-twenties. However, their analysis only covers the 16-25 period, so it is not clear what the trajectory of the effects on later-life outcomes is going to be.

Education outcomes

Panel b. in Figures 8 and 9 report estimates for secondary education outcomes. The effects on the extensive margin of secondary enrollment are mostly null for men and women. A slightly different story is observed when the effects are measured using the continuous variable. First, between ages 18 and 22 there is a consistently positive and statistically significant effect for men. During these four years, point estimates are between 0.15 and 0.2 standard deviations (i.e., a third of a year, or about 14% of the control mean). In all cases, the estimates are statistically significant ($p - values$ range between 0.019 and 0.058). On the contrary, estimates for women are smaller and statistically insignificant. However, the equality of coefficients cannot be rejected.[45] In sum, the age-by-age estimates on education outcomes again yield mixed evidence about the program's effects when estimated separately on men and women.

One alternative way of looking at effects on education outcomes in a dynamic setting is to analyze the effects of *PANES/AFAM-PE* on secondary enrollment for each grade

[44]The only exceptions are the coefficients on the number of births measured at ages 23 and 24 (-0.14 and -0.17 births, respectively), which are negative and statistically significant ($p - value = 0.067$ and $p - value = 0.031$). While the magnitude of these coefficients measured in percentage points is larger compared to the effects measured at 17 or 18 years old, the size of the effect relative to the control mean is much smaller (16.7% and 18.6%).

[45]For instance, $p - values$ are between 0.107 and 0.130 for estimates measured between ages 18 and 20 and between 0.220 and 0.525 for ages 21 and 22.

separately. Appendix F reports these results. First, when considering men and women together, the results indicate that *PANES/AFAM-PE* has null effects on enrollment in grades 1-3 (middle school), while it has strong and positive effects on enrolment in grades 4-6 (high-school). More specifically, *PANES/AFAM-PE* increases enrollment in 4th grade by 9.3p.p. (32.07%), in 5th grade by 5.9p.p. (28.83%), and in 6th grade by 4.7p.p. (32.36%). In each of these three cases, the effects are statistically significant (p-values of 0.003, 0.024, and 0.030, respectively). Combined with the baseline estimates reported above, the fact that there is an increase in the number of years enrolled in secondary education, which is driven mostly by changes in high-school enrollment, suggests that academic progress might be playing a role.

Ideally, one would want to test this hypothesis directly and report estimates of the effects on actual years completed. However, this information is not available in the current data. For this reason, I conduct an alternative approach which could also be informative about potential effects on academic progress. First, I report estimates on the number of different grades in which an individual was enrolled to, separated by secondary education level. This variable takes a value between 0-3, where 0 corresponds, for instance, to someone that was not enrolled in middle school, while 3 corresponds to someone that was enrolled in grades 1st to 3rd. Consistent with previous findings, the effects of *PANES/AFAM-PE* are null on the number of middle school grades while positive and statistically significant for the number of high school grades. In this case, the effect is 0.187 (30.39%) with a p-value=0.002. In addition, I also define a variable that contains the maximum grade in which someone was enrolled from 1st to 6th. A positive effect on this variable would provide some suggestive evidence of academic progress. Consistent with this hypothesis, the estimated effects of *PANES/AFAM-PE* on the maximum grade enrolled is 0.35 (9.49%) and statistically significant (p-value=0.018).

However, when looking at the effects on education outcomes by grade and gender, there are some differential patterns between men and women. First, for men, only the effect on the probability of being enrolled in 4th grade is statistically significant. The size of this effect is large, 9.2p.p. (42.35%) and statistically significant (p-value = 0.001). In addition, there is an effect of *PANES/AFAM-PE* on men's number of grades of high school - 0.139 (31.68%) with a p-value of 0.014 - but there is no effect on the maximum grade in which an individual was enrolled. In this case, the effect is still sizable - 0.324 (9.63%) - but statistically insignificant (p-value=0.157). For women, the effects are larger and more widespread. For instance, there is a positive effect of the program both on 4th and 5th grade enrollment. These effects are 8.2p.p. (21.56%) and 10.1p.p. (36.78%), respectively, and statistically significant (p-value=0.084 and p-value=0.012). The effect on 6th grade enrollment is still sizable - 6.4p.p. (32.29%) - but statistically insignificant (p-value=0.119). The effects on women's number of grades enrolled in middle school are null, while for high school they are positive - 0.226 (27.75%) - and statistically significant (p-

value=27.75%). Furthermore, the effect on the maximum grade enrolled is 0.366 (9.06%), which is slightly larger than for men but statistically significant (p-value = 0.047).

The main lessons that can be learned from the estimates on education outcomes are the following. First, *PANES/AFAM-PE* does not seem to affect enrollment in the secondary education system in the extensive margin. On the contrary, effects seem to be associated with changes in the number of years of secondary enrollment, and in particular, to an increase in the number of years enrolled in high school rather than middle school. In terms of differential patterns by gender, changes in the number of years of secondary education enrollment are driven by men. However, even though women do not show changes in the number of years enrolled in secondary, they do show an increase in the number of grades enrolled in high school and, most importantly, in the maximum grade in which they were enrolled. Together, these results suggest that effects on men's enrollment could be associated with a more passive enrollment, where they just stay in the system for longer but do not make significant academic progress, while women do. However, the evidence is not conclusive, and more work is required to confirm this interpretation.

Finally, panel c. in Figure 8 depicts the analogous estimates for tertiary education enrollment. The trajectory of the effects is consistent with the results discussed in Section 6.2. *PANES/AFAM-PE* does not seem to affect the probability of ever being enrolled in tertiary education for any of the ages considered.

Labor Market Outcomes

Binary labor market outcomes are reported in panel d. of Figure 8. Estimates for the continuous variables are reported in panels c. and d. of Figure 9. Panel d. of Figure 9 presents additional complementary evidence of effects on cumulative earnings.

The differences in the effects of the program between men's and women's labor market outcomes are striking. For men, *PANES/AFAM-PE* does not seem to have affected either labor market participation, months worked, or total cumulative earnings. Only 3 out of 45 p-values estimated for the 15 ages, and the 3 outcomes are smaller than 0.100, and those who are, do not follow any clear pattern. On the contrary, estimates for women provide substantial, robust evidence of positive effects on each of the three variables. These start as early as around 17-18 years old and continue relatively stable until their mid- or late-twenties. The differences between men and women are large and, in some cases, statistically significant. For instance, the effect of *PANES/AFAM-PE* on women's probability of having at least one four-month spell in the formal labor market measured at or before age 18 is 13p.p. (56.38%), while for men is -3p.p. and statistically insignificant ($p-value = 0.568$). The test of equality of coefficients for this outcome is rejected at a 5% level ($p-value = 0.027$). The differences are even larger for months worked and earnings. For instance, at age 24, the effect of *PANES/AFAM-PE* on women's months worked is 9.62 (40.79%), while for men, it is -3.22 (-9.97%) and statistically insignificant (p-value=0.497).

The differential in the effects of the program on months worked between men and women measured at age 24 is 12.83, and it is statistically significant (p-value=0.004). More generally, this differential remains statistically significant between ages 17 and 27. The heterogeneous responses between men and women are not as large when comparing effects on cumulative earnings.

Finally, for each of the three labor market outcomes utilized, the effects attenuate by the late twenties and become null when reaching the 30s. Measured at 28, 29, or 30 years old, the program's effects are null both for men and women. Furthermore, by this age, there are no signs of the strong positive differential in favor of women's outcomes observed in the early twenties.

Stronger effects on women's labor market outcomes are also observed in related literature. For instance, in the US, Bastian et al. (2022); Hoynes et al. (2016); Bitler and Figinski (2019) find that the effect of different social safety net policies on children's adult outcomes is stronger for women. For PROGRESA, both Araujo and Macours (2021) and Parker and Vogl (2018) find similar heterogeneous responses where effects are more pronounced on women, although in some cases, the differences are not statistically significant. One recent piece of evidence that, at first glance, goes in the opposite direction is Barr et al. (2022), who show that the effects of additional exposure to EITC during childhood on early adulthood labor market outcomes are mostly driven by men. However, when making a more detailed comparison, the contrasting pattern is not so strong, and it could be explained mostly due to differences in the periods covered by the analysis. More specifically, Barr et al. (2022) do not report effects measured in the early 20s, which is the period where I find stronger effects on women's outcomes. Furthermore, unlike the estimates I provide in this paper, their analysis extends to the mid-thirties. At these ages effects on women's labor market outcomes become stronger. If one extrapolates this finding to the setting of *PANES/AFAM-PE*, this could suggest that the improved labor market outcomes might show up again after a period of high fertility. However, this is just speculative and can only be tested in the future when women are older.

Summary of results

The results discussed in this section illustrate the trajectory of the effects measured for ages 15 to 30 for each one of the dimensions included in the analysis. Comparing the age profiles of the effects for each outcome suggests a compelling story. Figure 10 illustrates this story by putting together the two main results, i.e., the age trajectories of the effects on women's fertility and labor market outcomes. This exercise aims to provide visual evidence of regular trends in the trajectories of the effects on different outcomes that could suggest the potential mechanisms behind these responses. When the two series are put together, a clear pattern emerges. Figure 10 illustrates that effects on fertility and labor market outcomes evolve inversely. The effects of *PANES/AFAM-PE* start to manifest

as early as age 16, with negative effects on teenage pregnancies. These effects peak (in absolute value) around the ages of 17-18 and continue being negative, although smaller in size, until women reach 27 years old. At this age, they become positive but statistically insignificant. The overall age profile of the effects suggests that *PANES/AFAM-PE* did not change women's overall fertility preferences. Rather, it led to a postponement of births that otherwise would have occurred in the late teens. A similar but oppositely signed pattern is observed when looking at women's labor market outcomes. The positive effects on labor market outcomes start as early as age 18 and remain positive until the mid-twenties when they start to attenuate and trend toward 0. This attenuation coincides exactly with the attenuation (and even reversal) of the effects observed on fertility outcomes. The fact that labor market effects are exclusively observed on women, combined with such similar but inverse dynamics of fertility and labor market outcomes, suggests that *PANES/AFAM-PE* improves young women's labor market participation through a postponement of births.

In addition, while weaker and perhaps not as conclusive, the effects reported on women's academic progress in secondary education are also consistent with the trajectories observed for fertility and labor market outcomes. In particular, it is important to note that the effects on labor market outcomes become statistically significant only after age 18. Altogether, these results could indicate that, first, women make additional progress in secondary education, and then, they anticipate their entry into the formal labor market. However, as explained in the previous section, results on education outcomes must be interpreted cautiously because of the mixed and weaker patterns.

In the following section, I discuss in detail some of the main theoretical mechanisms that could explain these results.

7 Discussion

The results presented in previous sections indicate that *PANES/AFAM-PE* had strong effects on women's transition to adulthood, mostly on labor market outcomes, and coinciding with a postponement of their first birth. In addition, there is mixed evidence about the effect of the program on education decisions. While effects on tertiary education enrollment are consistently null, effects on secondary education seem to be strong for women, particularly in terms of high school enrollment. Furthermore, suggestive evidence shows that the effect on women's education might be explained by academic progress.

As discussed in Section 3, there are several potential mechanisms that could explain the effects reported in the previous section. For instance, the negative effects of *PANES/AFAM-PE* on teenage pregnancies can be explained by increased household income. On the one hand, higher income could lead to increased access to contraceptives, which in turn could reduce teenage pregnancies (e.g., as in Kearney and Levine, 2009;

Bailey, 2006; Lundberg and Plotnick, 1995). Similarly, higher household income might reduce households' economic stress, making them a more attractive place to stay and reducing the incentives for young children to leave their parents' household to form a new one.

The suggestive evidence on education outcomes might also help explain reductions in teenage fertility. First, additional years in the education system might lead to a reduction in activities associated with risky behaviors that could lead to early-life pregnancies (Black et al., 2008; Berthelon and Kruger, 2011). Second, in a human capital framework, increased education improves expectations about future labor market outcomes, which is one of the key components of the opportunity costs of motherhood. In particular, this mechanism is supported by Araujo and Macours (2021) who show that PROGRESA improves children's earnings expectations, and also with literature on career choice that shows that the expected starting wage and the steepness of the earnings profile are strongly associated with fertility postponement (Van Bavel, 2010). This mechanism is also consistent with the literature in Demography that explains fertility postponement, partly by an increase in women's education (see Sobotka, 2010 for a thorough review).

The dynamic analysis discussed in Section 6.4 shows that fertility postponement was strongly associated with earlier participation in the labor market. This finding is also consistent with a whole strand of literature in Demography that discusses the relationship between fertility and labor market outcomes. For instance, since 1995, countries with higher delays in fertility have been associated with an increase in labor market participation and better wages for women (See Bratti, 2015 for a brief review). The micro-level evidence that uses biological fertility shocks to analyze the causal effects of fertility postponement on labor market outcomes also points in the same direction (Miller, 2011; Bratti and Cavalli, 2014).

The findings in this paper, at least for women's labor market outcomes, are also consistent with a broader literature that discusses the role of income during childhood on adulthood labor market outcomes (Akee et al., 2010; Bulman et al., 2021; Cesarini et al., 2016). However, if income *per se* was the main mechanism, one would also expect to observe a positive effect on men's outcomes. Overall, the fact that labor market effects are exclusively driven by women, jointly with such similar but oppositely signed trajectories for the effects on the women's labor market and fertility outcomes, are consistent with the interpretation that a postponement of women's age of first birth is one of the main channels for an earlier entry to the formal labor market and for the increased months and earnings.

8 Conclusion

Worldwide, governments spend billions of dollars on social safety net (SSN) policies to reduce poverty and inequality. With different designs depending on the context, cash transfers are one of the simplest and most used policy instruments for this purpose. Cash transfers can affect the lives of beneficiary household members in several ways. For instance, they can change parents' time allocation between labor, leisure, and housework, or children's school enrollment and healthcare decisions. All these changes affect the current lives of individuals but can also have long-lasting consequences, especially for children who benefited from the program in early life. This paper presents evidence of how a large-scale and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*, affects the transition to adulthood of individuals that benefited from the program when they were young.

Using a Regression Discontinuity Design that exploits the use of a poverty score to define eligibility to participate in the program, I show that the program reduces women's teenage pregnancies by 9.4p.p., increases participants' early adulthood labor market participation by 6.4p.p., months worked by 4.4, and earnings by about 12%. The evidence on education outcomes is mixed but suggests a stronger attachment to the secondary education system. For women, this stronger attachment is possibly explained by academic progress. Consistent with a postponement of women's first birth being the main driver, changes in labor market outcomes are observed exclusively for women.

The evidence reported in this paper has implications for the design, implementation, and evaluation of cash transfer policies. In particular, it suggests that cash transfers may play a key role in reducing labor market gender gaps, even when they are not specifically designed for this purpose. A back-of-the-envelope calculation illustrates that the differential effects of *PANES/AFAM-PE* on women's earnings represent a large share of the earnings gender gap. For instance, at age 25, the differential effect observed for women represents 57% of the earnings gap of a "pure" control group that is close to the threshold but never participated in the program.[46] At later ages, the positive effects on women's labor market outcomes seem to attenuate. Hence, one could be worried that the reduction in the labor market gender gap is only short-lived. However, changes in the timing of events still have strong consequences from a life-cycle perspective due to the existence of fixed costs and flatter wage profiles for mothers (e.g., Bratti, 2015; Miller, 2011). Cash transfers might help mitigate the motherhood penalty by delaying the time of a woman's first birth, even if they do not change the overall number of children. . This is particularly important in the context of an anti-poverty program in Uruguay, where the motherhood penalty is larger for low-income mothers, although it has reduced over time (Querejeta

[46] One could be worried that never-treated are a group of reference that is strongly selected. Using the group of ineligible individuals based on the score obtained in the first application yields similar results. The differential effects represent about 39.7% of the gender gap, conditional on having earned income

and Bucheli, 2022). One relevant question, that exceeds the case of Uruguay, is what is the role of public policy in reducing the motherhood penalty and, in general, the labor market gender gap. My paper illustrates that public policy has the potential to play a key role. Given that the motherhood penalty explains a sizable share of the labor market gender gap, policies that promote a postponement of pregnancies that otherwise would have occurred during teenage years might be particularly effective in reducing long-term labor market gender gaps.

In addition, the evidence reported in this paper suggests that cash transfers might also induce strong intergenerational effects, for instance, by affecting critical decisions such as the age of first birth. In this regard, the literature has shown that later-life pregnancies are associated to higher test scores or improved educational and psychological outcomes for children of the third generation (e.g., as discussed in Sobotka, 2010).

Overall, the study of the effects of *PANES/AFAM-PE* on individuals' transition to adulthood suggests that cash transfer programs can have long-lasting effects that should be considered when assessing their effectiveness, making them much more attractive. In particular, the positive effect on women's labor market outcomes combined with potential improvement in subsequent generations suggests that cash transfers can be a viable policy instrument to reduce long-run poverty and inequality in the long run.

Despite the thorough analysis reported in this paper, there are at least two key questions that remain unanswered since they require waiting for a longer time to observe these same individuals at older ages. The first one corresponds to the intergenerational effects of welfare participation. In particular, it is important to understand whether children that benefited from parents participating in welfare programs will also increase their own participation as adults. The empirical literature provides mixed evidence in this regard (Dahl and Gielen, 2021; Dahl et al., 2014; Hartley et al., 2022; Deshpande, 2016; Price and Song, 2018). Overall, improved labor market outcomes could suggest that they will not require to participate in welfare programs as adults. However, the attenuation observed by the late twenties weakens this interpretation. Unfortunately, the participation records used in the analysis only contain information until 2017. Hence, they do not allow me to provide precise estimates on children's adult participation in *PANES/AFAM-PE*, yet. However, this is a key topic for future research.

The second important question that remains unanswered is how the program would end up affecting overall fertility and, most importantly, what are the welfare implications. While the postponement of fertility has improved women's labor market outcomes, these improvements might come with a cost. In particular, birth at later ages, especially after mid-thirties, might lead to higher risks such as a higher probability of infertility, increased risk of miscarriage, and higher risk of pregnancy complications, among others, which also entail higher expected pecuniary and psychological costs of pregnancies (Schmidt et al., 2012; Gustafsson, 2001). In addition, one must consider that by delaying the age of first

birth, the whole fertility cycle becomes shorter, and some women might be prevented from achieving their desired fertility plans. In this regard, demographers have suggested that the postponement transition is one of the reasons that explain a reduction in the total fertility rate observed in some societies for more than three decades (Kohler et al., 2002; Sobotka, 2004). The effects found in this paper correspond mostly to a postponement of birth that otherwise would have happened in the teenage years. Hence, the increased costs of postponing the age of firth birth are probably not as significant as they would be if the delay corresponded to pregnancies in the mid-, late-twenties. Furthermore, such early-life pregnancies also have some additional non-pecuniary costs that must be considered in an overall welfare assessment. In any case, a correct evaluation of welfare effects must weigh the positive effects on labor market outcomes and reduction in labor market gender gaps against potential changes in pecuniary and non-pecuniary costs of changes in the timing of the pregnancies. Furthermore, this has implications for policy design. For instance, if the costs of postponement are too large, it would be better from a social welfare perspective for governments to find a way to balance fertility and labor market decisions without inducing large delays. One way to go could be to increase investment in family-friendly policies such as early care education centers or parental leave policies.

Finally, the results in this paper illustrate the importance of dynamic analysis to assess the effects of public policy correctly. The findings on fertility outcomes provide a clear example. If the effects of the program on fertility are measured at around age 18, one could conclude that the program led to a reduction in fertility. On the other hand, focusing on the effects measured at age 30, one could conclude that the program did not have an effect on fertility. In both cases, one would have completely overlooked the postponement effect. Such a finding resembles the literature that discusses non-linear trajectories in the dynamic effects of early childhood policy interventions Almond et al. (2018); Chetty et al. (2011). This paper provides evidence based on a different type of intervention, i.e., cash transfers, at ages not necessarily restricted to early childhood. Furthermore, Almond et al. (2018) argue that the potential existence of non-linear patterns is one of the main reasons that justify studying the "missing middle". In Almond et al. (2018), this missing middle refers to the lack of knowledge about the years between early childhood and adulthood in terms of developmental trajectories. The idea of an understudied missing middle that prevents us from understanding individuals' life trajectories also relates to the primary motivation for this paper. My paper contributes to this strand of literature by providing novel evidence about a different "missing middle".

Figures

Figure 1: Description of the Program

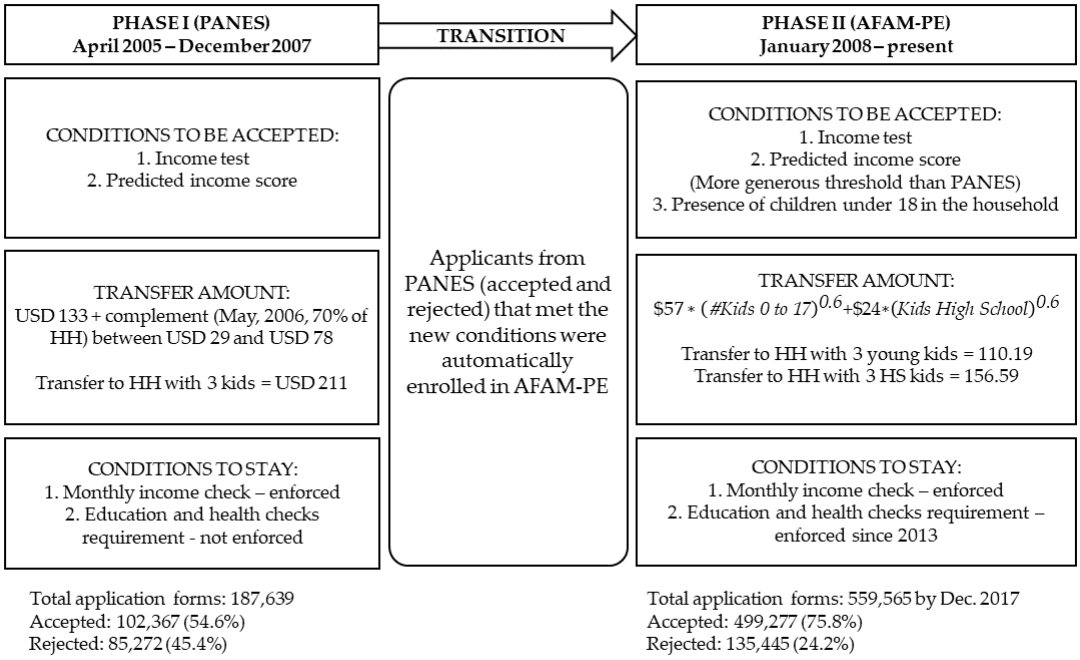
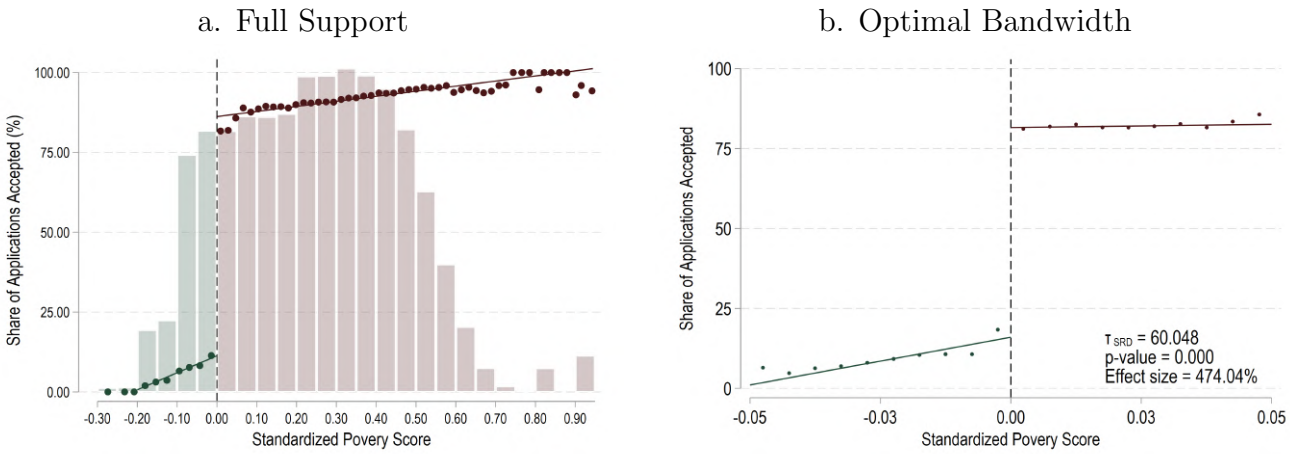


Figure 2: Relation Between Application Form Eligibility and Resoultion - Main Sample

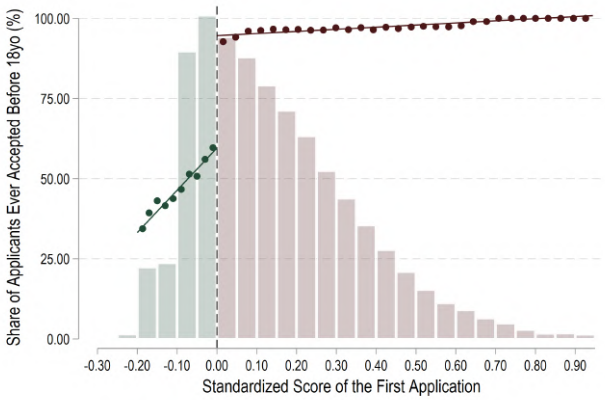


Notes:This figure reports the share of application forms that were accepted as a function of the standardized poverty score (z) for the forms corresponding to individuals in the *main sample* as defined in Section 4.2.. Each observation used to construct this figure corresponds to an application form. Panel a. reports this relation for the full support of z. Negative values of z (depicted in green) indicate that the application does not meet the eligibility requirements, while positive values (depicted in red) correspond to eligible applications. Bars in the background depict the distribution of z. Each dot in the figure represents the average share of application forms accepted within a bin. The number of bins was selected manually such that the number of bins for negative values of z relative to the number of bins for the positive values of z represents the distribution of z. Panel b. focuses on application forms that are located within a bandwidth of 5p.p. of the eligibility threshold. In this case, observations are grouped into bins of 0.5p.p. width. In addition, the figure reports the point estimate of the local difference in the share of application forms accepted just at the threshold (τ_{SRD}), the p-value corresponding of a test of continuity, and the effect size expressed as a percent of the share of applications accepted for the ineligible group within the bandwidth depicted. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), the bandwidth used is selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights and robust standard errors clustered at the household level. For transparency and consistency between the point estimates reported and the graphical representation, estimates are based on a specification that does not include any additional covariates.

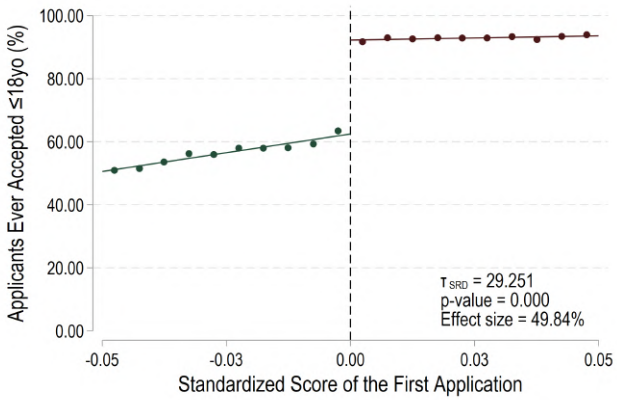
Figure 3: Participation Rule Using First Application Form - Main Sample

Ever Treated Before 18yo

a. Full Support



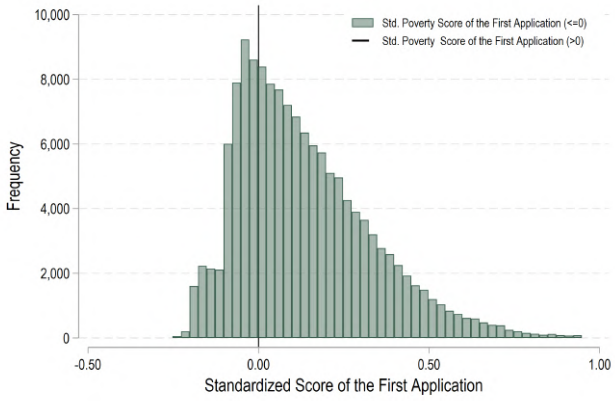
b. Optimal Bandwith



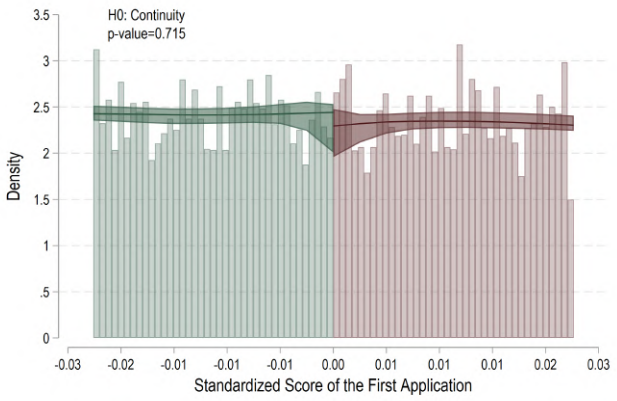
Notes: This figure reports the share individuals that were ever accepted to *PANES/AFAM-PE* before turning eighteen years old as a function of the standardized poverty score obtained in the first application (Z_i^{1st}) for the *main sample* as defined in Section 4.2.. Each observation used to construct this figure corresponds to an individual of the main sample as defined in Section XX. Panel a. reports this relation for the full support of z. Negative values of z (depicted in green) indicate that the score obtained in the first application form does not meet the eligibility requirements, while positive values (depicted in red) indicate that it does. Bars in the background depict the distribution of z. Each dot in the figure represents the average share of application form accepted within a bin. The number of bins was selected manually such that the number of bins for negative values of z relative to the number of bins for the positive values of z represents the distribution of z. Panel b. focuses on application forms that are located within a bandwidth of 5p.p. of the eligibility threshold. In this case, observations are grouped into 0.5p.p. width bins. In addition, the figure reports the point estimate of the local difference in the share of individuals ever treated just at the threshold (τ_{SRD}), the p-value corresponding of a test of continuity, and the effect size expressed as a percent of the share of ever treated individuals in the ineligible group within the bandwidth depicted. Estimates are based on the specification reported in equation 3. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), the bandwidth used is selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights and robust standard errors clustered at the household level. For transparency and consistency between the point estimates reported and the graphical representation, estimates are based on a specification that does not include any additional covariates. Additional details on the estimation procedure are reported in Table 2.

Figure 4: Continuity of the Poverty Score in 1st. Application Form - Main Sample

a. Distribution of the Poverty Score



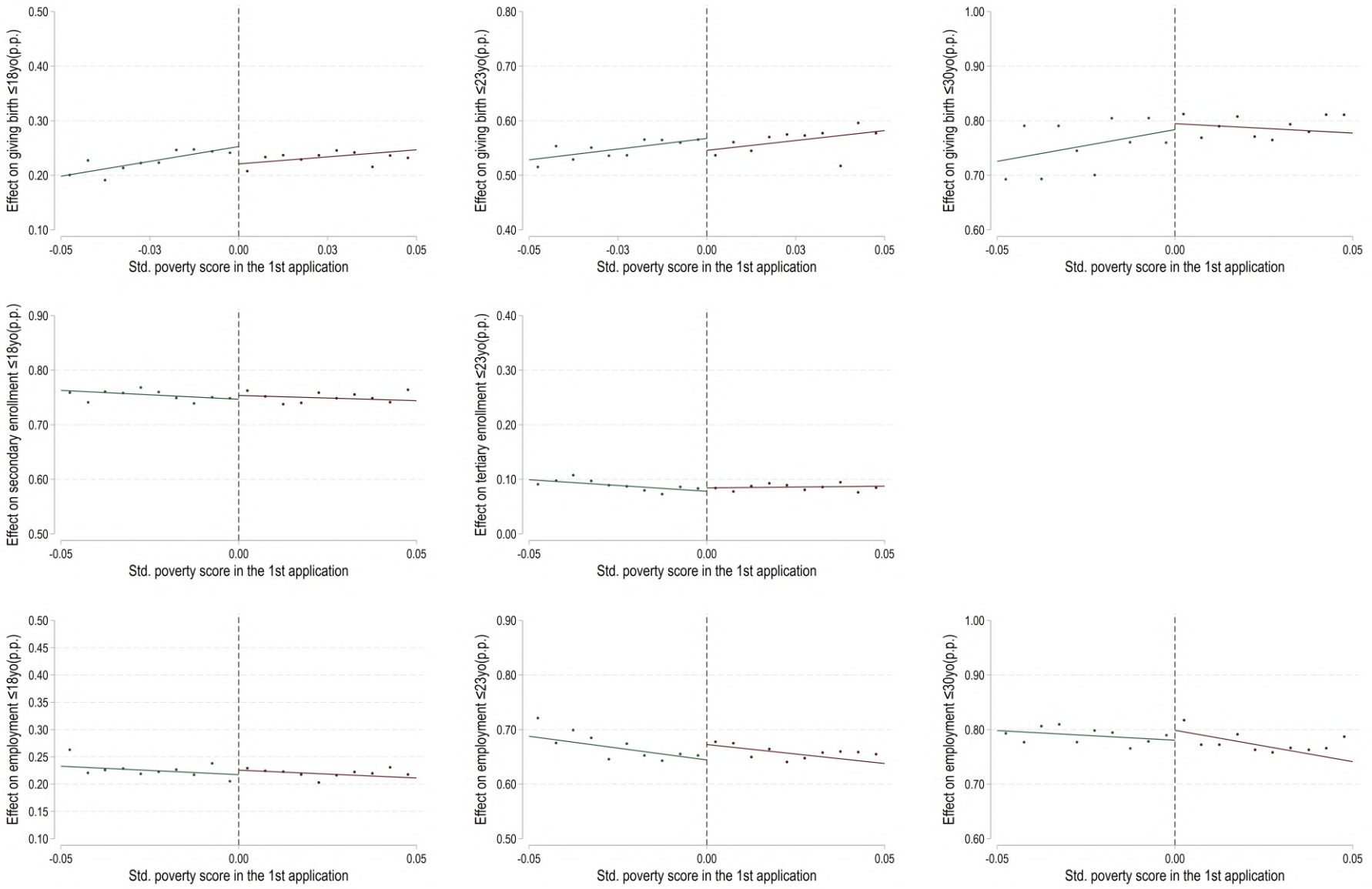
b. Cattaneo, Jansson, and Ma Test



Notes: This figure illustrates the distribution of the standardized poverty score obtained in the first application form (Z_i^{1st}) for the *main sample* as defined in Section 4.2.. Panel a. reports the distribution of Z_i^{1st} for its full support. Panel b. provides an illustration of a continuity test of Z_i^{1st} at the eligibility threshold as proposed by Cattaneo, Janson, and Ma (2020) and using the default options in the *rddensity* Stata command.

Figure 5: Graphic Evidence: Intention to Treat Effects, by Age - Binary Variables

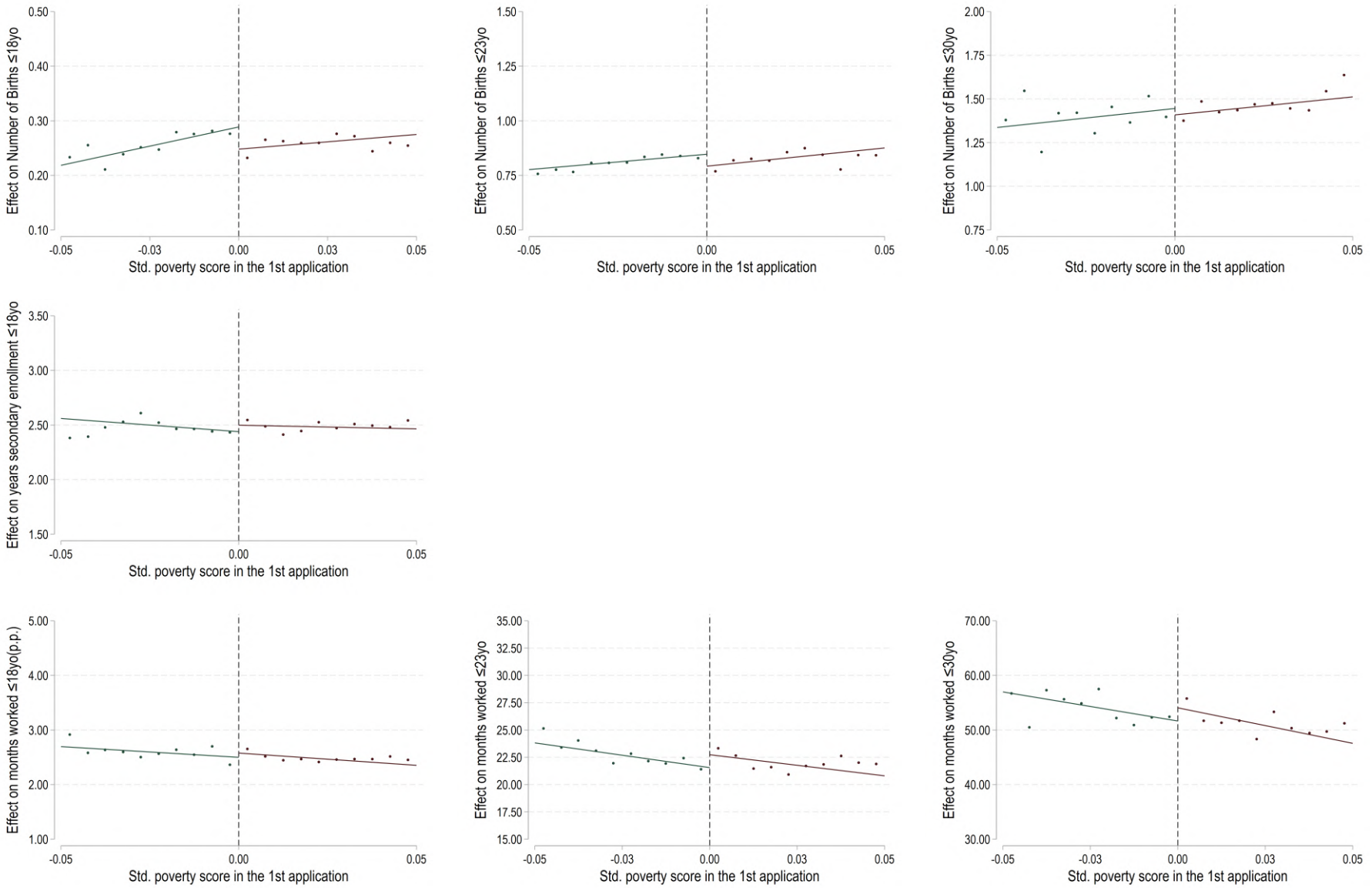
a. 18 years old b. 23 years old c. 30 years old



Notes: This figure illustrates the local intention to treat effects of the program on the different outcomes of interest using the score obtained in the first application (Z_i^{1st}) as the running variable. Panel a. depicts these effects for outcomes measured at the age of 18, panel b. does the same for outcomes measured at the age of 23, while panel c. uses outcomes measured at the age of 30. As explained in Section 4.2., panels a. and b. are based on the *main sample*, while estimates reported in panel c. are based on the *dynamic sample* of individuals with at least 30 years old by December, 2019. Row 1 focuses on women's fertility outcomes. In this case, the outcome variable takes the value of 1 if a woman has given birth by the corresponding age and 0 otherwise. Row 2 is focused on men and women's education outcomes. The outcome variable reported in panel a. takes the value of 1 if an individual has ever been enrolled into secondary education by the age of 18 and 0 otherwise. In panel b., the outcome variable is defined similarly but for tertiary education enrollment. As explained in Section 5, education outcomes are not meaningful at older ages. Therefore, I do not include estimates for tertiary enrollment by the age of 30. Finally, row 3 illustrates the effects of the program for men a women's labor market outcomes. In this case, the outcome variable is defined as 1 if the individual has had a four-consecutive-months employment spell in the labor market by the corresponding age and 0 otherwise. For comparison purposes all panels in this figure focus on application forms that are located within a bandwidth of 5p.p. of the eligibility threshold. In this case, observations are grouped into bins of 0.5p.p. width. Point estimates, standard errors, and additional details about the estimation procedure are reported in Table 4.

Figure 6: Graphic Evidence: Intention to Treat Effects, by Age - Continuous Variables

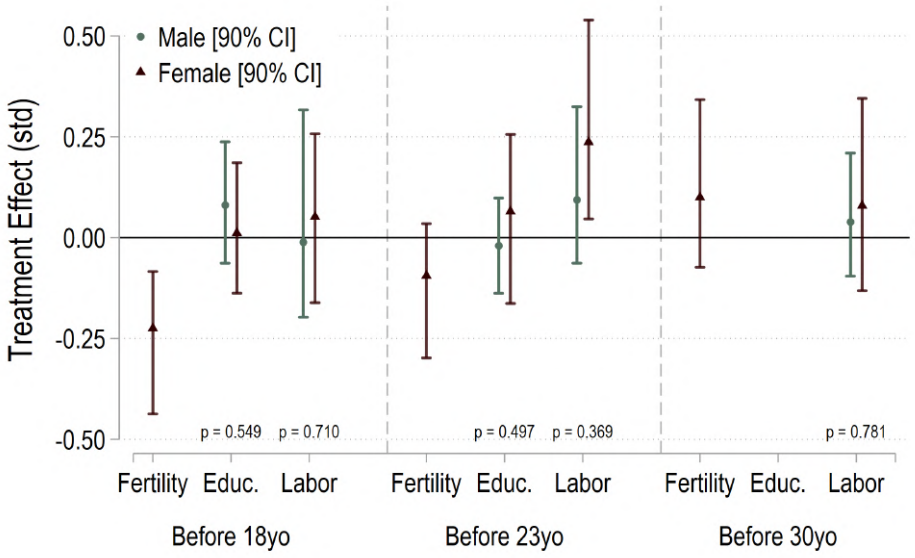
a. 18 years old b. 23 years old c. 30 years old



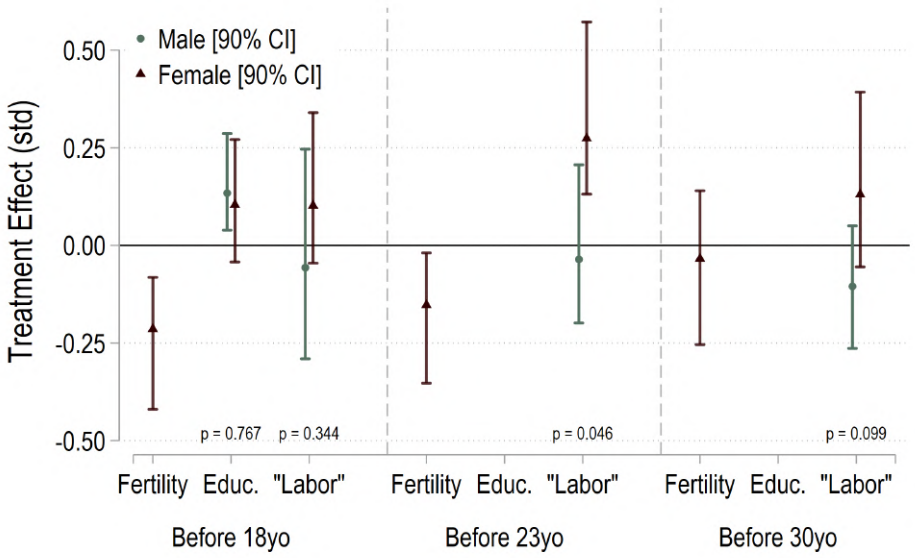
Notes: This figure illustrates the local intention to treat effects of the program on the different outcomes of interest using the score obtained in the first application (Z_i^{1st}) as the running variable. Panel a. depicts these effects for outcomes measured at the age of 18, panel b. does the same for outcomes measured at the age of 23, while panel c. uses outcomes measured at the age of 30. As explained in Section 4.2., panels a. and b. are based on the *main sample*, while estimates reported in panel c. are based on the *dynamic sample* of individuals with at least 30 years old by December, 2019. Row 1 focuses on women's fertility outcomes. In this case, the outcome variable measures the number of births that a woman has had by the corresponding age and 0 otherwise. Row 2 is focused on men and women's education outcomes. The outcome variable reported in panel a. measures the number of years that an individual has been enrolled into secondary education by the age of 18. As explained in Section 5, the information available for tertiary education only allows to measure enrollment. Therefore, tO do not estimate effects of the program on the number of years enrolled in tertiary education. Finally, row 3 illustrates the effects of the program for men a women's labor market outcomes. In this case, the outcome variable is defined as the number of months that an individual has worked in the formal labor market by the corresponding age. For comparison purposes all panels in this figure focus on application forms that are located within a bandwidth of 5p.p. of the eligibility threshold. In this case, observations are grouped into bins of 0.5p.p. width. Point estimates, standard errors, and additional details about the estimation procedure are reported in Table 4.

Figure 7: Heterogeneity by Gender

a. Binary outcomes

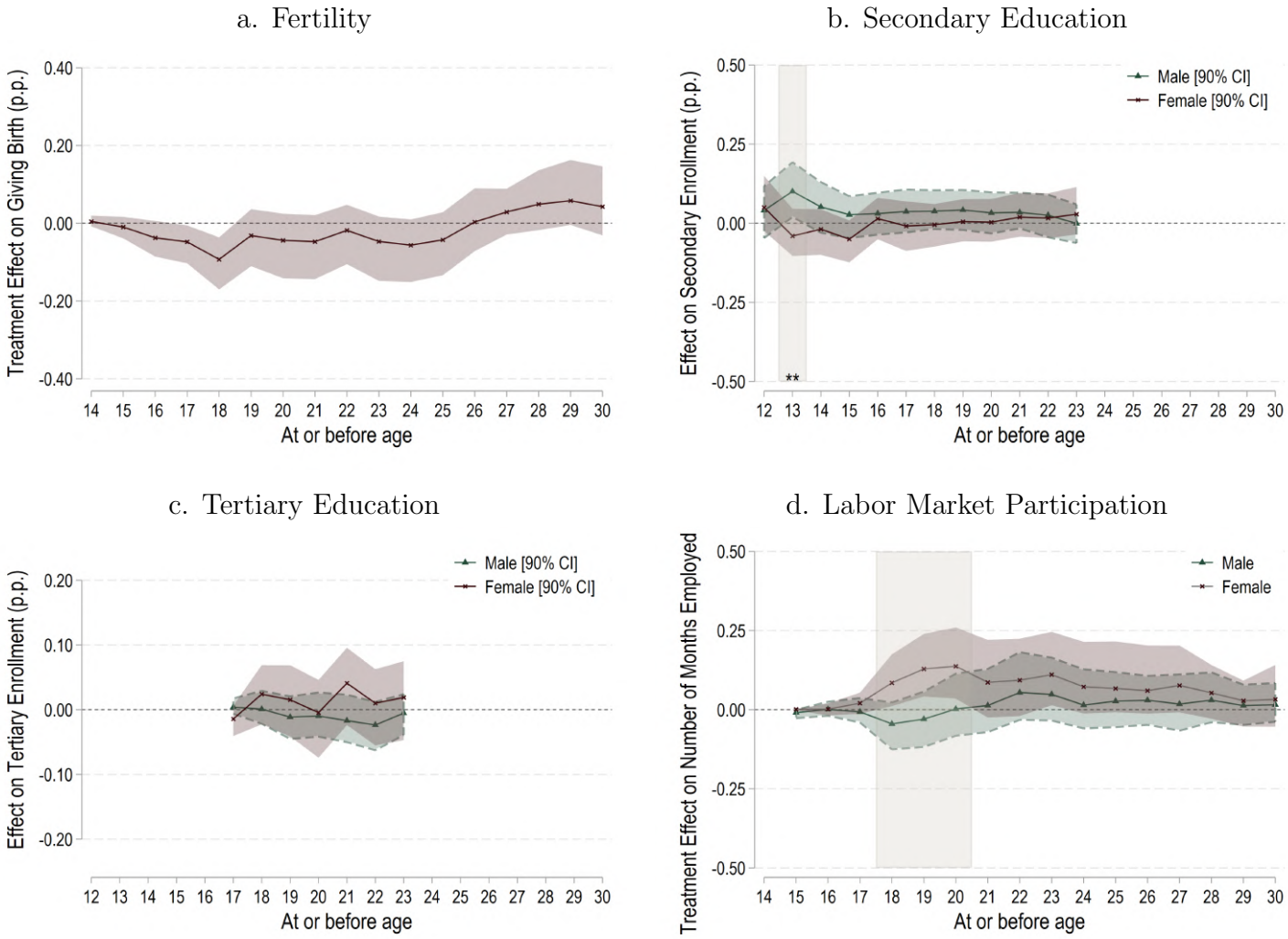


b. Number of events



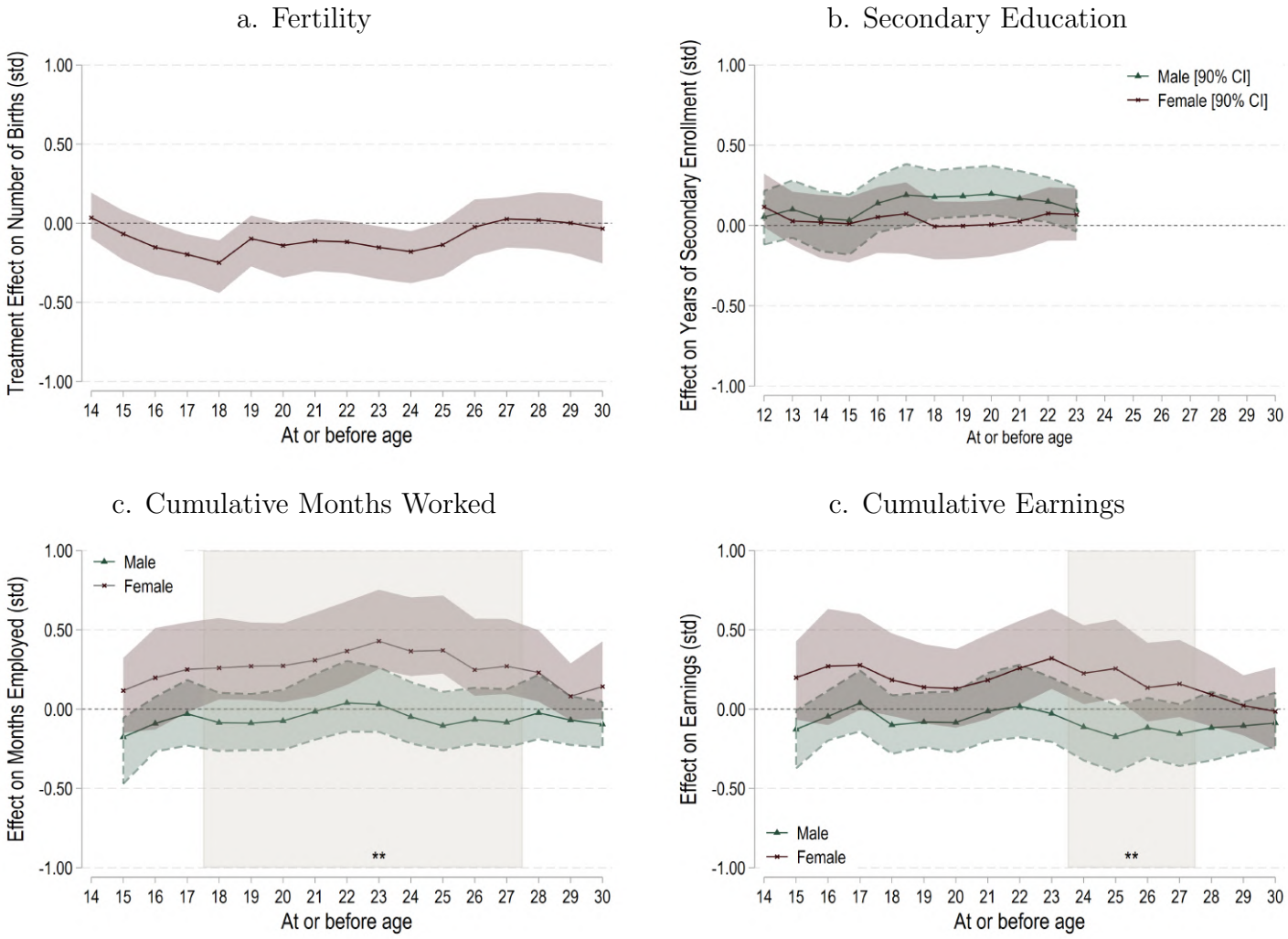
Notes: This figure illustrates the local average treatment effects of the program on the different outcomes of interest, measured at different ages, for men (green) and women (red) separately. As explained in Section 4.2., estimates measured at 18 and 23 years old are based on the *main sample*, while estimates measured at the age of 30 are based on the *dynamic sample* of individuals with at least 30 years old by December, 2019. Panel a. reports estimates that correspond to the binary outcome variables, as defined in Figure 5. Panel b. replicates the analysis for the continuous outcome variables, as defined in Figure 6. In both cases, effects on fertility outcomes are reported exclusively for women. In all cases, for a simpler comparison across groups and outcomes, estimates are expressed in standard deviations. In addition to the point estimates and the 90% robust confidence intervals based on Cattaneo, Janson, and Ma (2020), the figure also reports the p-value for a test of equality of coefficients between men and women. See notes in Tables 6 and 7 for additional details about the estimation procedure.

Figure 8: Dynamic Effects, by outcome and gender - Binary Variable



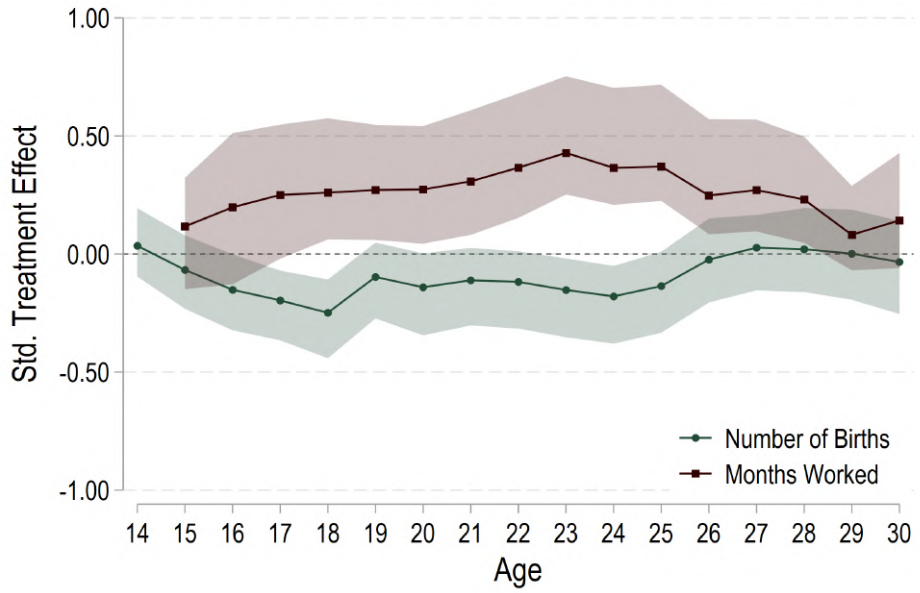
Notes: This figure reports the age-by-age estimates of the local average treatment effects on a set of outcomes of interest for men (green) and women (red) separately. Estimates are based on the *dynamic samples* as defined in Section 4.2.. Each dot in a figure represents a different RDD estimate. These estimates are based on the specification described by equation 1 estimated using 2SLS where eligibility based on the score obtained in the first application is used as an instrument for the endogenous variable of ever treatment. Panel a. reports the estimates for the binary fertility outcome. This variable takes the value of 1 if a woman gave birth by a certain age, measured in the x-axis. For instance, when the x-axis takes the value of 25, the outcome variable is defined as 1 if a woman has given birth at or before the age of 25 and 0 otherwise. Panel b. reports the estimates corresponding to the binary education outcomes, i.e. a variable that takes the value of 1 if an individual was enrolled in secondary education at or before a certain age and 0 otherwise. Panel c. focuses on a similarly defined variable but for tertiary education enrollment. Finally, panel d. reports estimates corresponding to a binary variable that takes the value of 1 if an individual has ever had an employment spell that lasted at least for consecutive months at or before a given age and 0 otherwise. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), each estimate corresponds to an optimal bandwidth selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights. All estimates correspond to a model that includes the pre-treatment variables described in Table 3 (except predicted eligibility) as control variables. 90% confidence intervals correspond to robust standard errors clustered at the household level. Full estimates are reported in Table XXX in Appendix XXX as well as additional details on the estimation procedure.

Figure 9: Dynamic Effects, by outcome and gender - Continuous Variable



Notes: This figure reports the age-by-age estimates of the local average treatment effects on a set of outcomes of interest for men (green) and women (red) separately. Estimates are based on the *dynamic samples* as defined in Section 4.2.. Each dot in a figure represents a different RDD estimate. These estimates are based on the specification described by equation 1 estimated using 2SLS where eligibility based on the score obtained in the first application is used as an instrument for the endogenous variable of ever treatment. In all cases, estimates are standardized for an easier comparison across groups and outcomes. Panel a. reports the estimates for the continuous fertility outcome. This variable measures the number of births a woman has had by a certain age, measured in the x-axis. For instance, when the x-axis takes the value of 25, the outcome variable is defined as the number of births of a woman by the age of 25. Panel b. reports the estimates corresponding to the continuous education outcomes, i.e. a variable that counts the number of years an individual was enrolled in secondary education by a certain age. Panel c. focuses on a continuous measure of labor market participation. This variable counts the number of months that an individual has worked in the formal sector by a given age. Finally, panel d. reports an analogous measure but for the cumulative earnings by a given age. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), each estimate corresponds to an optimal bandwidth selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights. All estimates correspond to a model that includes the pre-treatment variables described in Table 3 (except predicted eligibility) as control variables. 90% confidence intervals correspond to robust standard errors clustered at the household level. Full estimates are reported in Table XXX in Appendix XXX as well as additional details on the estimation procedure.

Figure 10: Dynamic Effects, Combined



Notes: This figure reports the age-by-age estimates of the local average treatment effects of the program on women's fertility and labor market outcomes. These series of effects correspond to the ones reported in Figure 9, panels a. and c. See notes in Figure 9 for details on the estimation procedure.

Tables

Table 1: Descriptive Statistics: Individual Characteristics - By Sample

	Main Sample: ≥ 23 years old At Dec, 2019		Dynamic Sample: ≥ 30 years old Fertility		Dynamic Sample: ≥ 30 years old Labor Market	
	All	Opt. Bandwidth	All	Opt. Bandwidth	All	Opt. Bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)
a. Individual Characteristics						
Female (%)	50.32	50.70	49.88	51.02	52.13	53.60
	(50.00)	(50.00)	(50.00)	(49.99)	(49.96)	(49.87)
Number of HH.	1.77	1.63	1.93	1.67	1.99	1.80
	(1.10)	(0.97)	(1.20)	(1.03)	(1.20)	(1.05)
Age at 31 Dec. 2019	26.91	27.11	31.11	31.09	29.97	30.01
	(2.56)	(2.61)	(0.68)	(0.66)	(1.20)	(1.19)
Age of 1st application	13.42	13.40	16.84	16.87	15.98	15.98
	(2.59)	(2.61)	(0.69)	(0.67)	(1.22)	(1.19)
Accepted before 18yo (%)	84.16	73.62	71.04	53.30	80.94	65.63
	(36.52)	(44.07)	(45.36)	(49.89)	(39.28)	(47.49)
Number of app. forms	2.56	2.47	2.44	2.23	2.71	2.62
	(1.44)	(1.35)	(1.31)	(1.22)	(1.32)	(1.23)
In *PANES* form (%)	78.39	86.89	100.00	100.00	91.27	94.94
	(41.16)	(33.75)	(0.00)	(0.00)	(28.23)	(21.93)
In *AFAM-PE* form (%)	96.08	93.21	91.40	85.94	99.85	99.73
	(19.42)	(25.16)	(28.04)	(34.77)	(3.92)	(5.20)
b. Reference Form						
Std. Score	0.18	-0.00	0.16	-0.00	0.18	-0.00
	(0.25)	(0.05)	(0.27)	(0.03)	(0.26)	(0.05)
App. Accepted (%)	71.82	49.08	69.58	50.52	73.27	50.69
	(44.99)	(49.99)	(46.01)	(50.00)	(44.25)	(50.00)
PANES (%)	78.39	86.89	100.00	100.00	91.27	94.94
	(41.16)	(33.75)	(0.00)	(0.00)	(28.23)	(21.93)
Capital City (%)	31.25	18.18	29.64	15.64	31.27	17.90
	(46.35)	(38.56)	(45.67)	(36.33)	(46.36)	(38.34)
c. Household characteristics (ref. form)						
Single Parent (%)	46.73	48.96	48.30	54.65	47.14	49.56
	(49.89)	(49.99)	(49.97)	(49.79)	(49.92)	(50.00)
Number of members	4.92	4.38	4.98	4.23	5.06	4.45
	(1.98)	(1.82)	(2.12)	(1.91)	(2.07)	(1.87)
Number of children	2.95	2.41	2.91	2.27	3.02	2.43
	(1.69)	(1.45)	(1.78)	(1.46)	(1.76)	(1.48)
Avg. age	23.13	25.13	24.35	26.65	23.59	25.79
	(7.60)	(8.00)	(7.85)	(8.16)	(7.62)	(8.02)
Household Head: Ed. years	6.83	7.20	6.72	6.94	6.71	7.05
	(3.41)	(3.55)	(3.47)	(3.51)	(3.42)	(3.55)
Household head: Employed (%)	63.43	64.55	65.31	63.48	64.36	64.29
	(48.16)	(47.84)	(47.60)	(48.15)	(47.89)	(47.92)
Household head: income	143.33	159.37	128.79	133.69	130.82	147.87
	(172.54)	(171.86)	(140.96)	(134.38)	(145.62)	(152.77)
Observations	224,413	76,593	34,754	7,971	59,667	21,779

Notes: Table 1 reports a series of descriptive statistics for some of the samples used in the analysis. Columns (1) and (2) are focused on the *main sample* as described in Section 4.2.. Columns (3) and (4) are based on the *dynamic sample* used for estimates on fertility outcomes. This dynamic sample is defined as individuals who were younger than 18 years old at the time of their first application, and at least 30 years old by December, 2019, the latest date included in birth records. Columns (5) and (6) are based on the *dynamic sample* used for estimates on labor market outcomes that is defined analogously but for individuals with at least 30 years old by October, 2021, the latest date included in labor histories. Odd columns report statistics that describe individuals across the full support of the running variable, while even columns report statistics corresponding to individuals that are within the optimal bandwidths used in the RDD estimates of the effects of the program. Panel a. reports information on a series of characteristics at the individual level. Panel b. focuses on the characteristics of the reference form, i.e., the application form corresponding to the first application. Finally, panel c. reports information about the characteristics of the household defined in the first application form.

Table 2: First Stage - Main Sample

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
a. Dep. Var: Ever Treated Before 18 Years Old				
Eligibility	29.251***	29.089***	29.677***	29.620***
	(0.892)	(1.013)	(0.921)	(1.067)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	49.84%	52.37%	50.08%	51.72%
Bwd.	[0.033;0.033]	[0.054;0.054]	[0.026;0.026]	[0.043;0.043]
Observations	31,413	52,538	24,551	40,813
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Uniform	Uniform

Notes: Table 2 reports the first stage coefficients based on the *main sample* defined as in Section 4.2. (equation 2). These coefficients measure the effect of obtaining a poverty score above the eligibility threshold in the first application (Z_i^{1st}) on the probability of ever participating in the program. In this case, the outcome variable takes the value of 100 if an individual ever received the benefits of the program, while takes the value of 0 otherwise. Column (1) reports the estimates for the baseline specification that uses an optimal MSERD bandwidth (Calonico, Cattaneo, Farrell, and Titiunik, 2018), a triangular kernel function, and a linear local polynomial. Columns (2) through (4) reports a series of estimates based on alternative specification as robustness tests. Column (2) changes the polynomial degree from linear to quadratic, column (3) uses a uniform kernel function and a linear local polynomial, while column (4) uses a uniform kernel function and a quadratic local polynomial. In addition to the point estimate, this table reports robust standard errors clustered at the household level, the robust p-value, the effect size measured as a percent of the mean for the ineligible group within the optimal bandwidth, the optimal bandwidth, and the total number of effective observations used in the estimation.

Table 3: Balance of Baseline Covariates - Main Sample

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	p-value Robust	Sharpened FDR q-value
	(1)	(2)	(3)	(4)	(5)
Predicted Eligibility	0.66	0.66	0.003	0.635	0.651
HH - Avg. Age	25.11	25.41	0.306	0.113	0.276
HH - Avg. age adults	39.92	40.53	0.607	0.016	0.118
HH - Capital City	0.19	0.17	-0.023	0.021	0.118
HH - Number of people	4.27	4.20	-0.073	0.126	0.276
HH - Number of children	2.35	2.32	-0.030	0.358	0.416
HH - Single Parent	0.53	0.55	0.024	0.091	0.276
HHH - Income (IHS)	4.32	4.33	0.010	0.690	0.651
HHH - Employed	0.61	0.62	0.008	0.517	0.603
HHH - Years of Educ.	6.98	7.06	0.080	0.348	0.416
Age at 1st. App.	13.40	13.41	0.011	0.645	0.651
Age (Dec. 31, 2019)	29.04	28.91	-0.130	0.010	0.118
Number of Apps.	2.80	2.81	0.012	0.896	0.916
Female	51.17	50.40	-0.768	0.367	0.416
Number of HH.	1.64	1.61	-0.026	0.098	0.276

Notes: Table 3 is the RDD-analogous to a balance table in an experimental design. All variables included in the Table are measured at the baseline, i.e., at the moment of application, and correspond to the *main sample* as defined in Section 4.2.. For each variable included in the Table, I replicate the baseline estimation procedure used on the main outcomes and test whether they are continuous at the threshold. In all cases, estimates are based on an optimal MSERD bandwidth (Calonico, Cattaneo, Farrell, and Titiunik, 2018), a triangular kernel function, and local linear regressions. Column (1) reports the intercept of the regression function estimated on the ineligible side of the running variable. Column (2) does the same but for observations in the eligible side. Column (3) reports the difference between columns (2) and (1), i.e, the sharp RDD estimate. Column (4) reports the robust p-value of the continuity test. Column (5) reports the sharpened FDR q-values that adjust for multiple hypotheses testing. The variable predicted eligibility correspond to predicted values estimated based on a probit model where the dependent variable is defined as 1 if the individual has a score in their first application that is above the eligibility threshold, and 0 otherwise. All the other variables in the table are included as regressors in this probit model and used to calculate the predicted eligibility.

Table 4: Intention to Treat Effects

	18 years old			23 years old			30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.031***	0.008	0.004	-0.023*	0.005	0.020**	0.011	0.006
	(0.011)	(0.007)	(0.007)	(0.014)	(0.005)	(0.008)	(0.022)	(0.012)
Robust p-value	0.005	0.204	0.559	0.078	0.445	0.022	0.822	0.632
Effect Size (%)	-13.52%	1.06%	1.77%	-4.14%	5.83%	3.05%	1.49%	0.75%
Bwd.	[0.050;0.050]	[0.063;0.063]	[0.055;0.055]	[0.042;0.042]	[0.048;0.048]	[0.059;0.059]	[0.071;0.071]	[0.068;0.068]
Observations	24,078	61,225	46,474	20,292	45,194	49,828	6,504	17,964
				b. Dep. Var.: Number of Events				
Ever Treated	-0.038***	0.086**	-0.009	-0.060**		0.816**	-0.041	2.001
	(0.013)	(0.039)	(0.076)	(0.026)		(0.395)	(0.068)	(1.540)
Robust p-value	0.003	0.029	0.865	0.016		0.045	0.381	0.145
Effect Size (%)	-14.93%	3.41%	-0.36%	-7.36%		3.65%	-2.96%	3.72%
Bwd.	[0.052;0.052]	[0.046;0.046]	[0.060;0.060]	[0.040;0.040]		[0.054;0.054]	[0.057;0.057]	[0.052;0.052]
Observations	25,258	43,514	49,987	19,294		44,910	5,245	13,560
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes: Table 4 reports the effects of obtaining a score of the first application above the eligibility threshold on a series of outcomes of interest. These must be interpreted as intention to treat effects (ITT), or reduced form effects and are based in the specification described in equation 3. Columns (1) through (3) correspond to ITT effects of the program on outcomes measured at the age of 18, columns (4) through (6) correspond to outcomes measured at the age of 23, and columns (7) and (8) correspond to outcomes measured at the age of 30. Panel a. reports a series of estimates based on binary outcome variables, as defined in Figure 5. Panel b. reports estimates on a series of continuous outcome variables as defined in Figure 6. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), the bandwidth used is selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights and robust standard errors clustered at the household level. For transparency and consistency between the point estimates reported in this table and the graphical representation in Figures 5 and 6, estimates are based on a specification that does not include any additional covariates. In addition to the point estimate, this table reports robust standard errors clustered at the household level, the robust p-value, the effect size measured as a percent of the mean for the ineligible group within the optimal bandwidth, the optimal bandwidth, and the total number of effective observations used in the estimation.

Table 5: Local Average Treatment Effects

	≤ 18 years old			≤ 23 years old			≤ 30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.094**	0.017	0.021	-0.047	0.013	0.064*	0.043	0.016
	(0.039)	(0.026)	(0.034)	(0.042)	(0.015)	(0.032)	(0.045)	(0.028)
Robust p-value	0.015	0.469	0.313	0.193	0.526	0.062	0.288	0.570
Effect Size (%)	-41.19%	2.21%	9.36%	-8.52%	13.72%	9.69%	5.62%	1.98%
Bwd.	[0.044;0.044]	[0.042;0.042]	[0.044;0.044]	[0.050;0.050]	[0.072;0.072]	[0.065;0.065]	[0.040;0.040]	[0.068;0.068]
Observations	20,033	37,050	33,594	22,711	66,259	51,206	3,343	16,875
				b. Dep. Var.: Number of Events				
Ever Treated	-0.108**	0.253**	0.250	-0.137*		4.401***	-0.039	1.259
	(0.044)	(0.114)	(0.382)	(0.078)		(1.871)	(0.114)	(2.674)
Robust p-value	0.015	0.027	0.253	0.067		0.005	0.635	0.324
Effect Size (%)	-41.95%	10.07%	9.84%	-16.86%		19.77%	-2.78%	2.31%
Bwd.	[0.049;0.049]	[0.041;0.041]	[0.041;0.041]	[0.046;0.046]		[0.041;0.041]	[0.046;0.046]	[0.082;0.082]
Observations	22,536	36,257	31,203	20,776		31,602	3,760	19,810
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes: Table 5 reports the local average treatment effects of being ever treated on a series of outcomes of interest. These estimates are based on the specification described by equation 1 estimated using 2SLS where eligibility based on the score obtained in the first application is used as an instrument for the endogenous variable of ever treatment. Columns (1) through (3) correspond to the effects of the program on outcomes measured at the age of 18, columns (4) through (6) correspond to outcomes measured at the age of 23, and columns (7) and (8) correspond to outcomes measured at the age of 30. Panel a. reports a series of estimates based on binary outcome variables, as defined in Figure 5. Panel b. reports estimates on a series of continuous outcome variables as defined in Figure 6. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), the bandwidth used is selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights and robust standard errors clustered at the household level. All estimates correspond to a model that includes the pre-treatment variables described in Table 3 (except predicted eligibility) as control variables. In addition to the point estimate, this table reports robust standard errors clustered at the household level, the robust p-value, the effect size measured as a percent of the mean for the ineligible group within the optimal bandwidth, the optimal bandwidth, and the total number of effective observations used in the estimation.

Table 6: LATE Effects, by Age - Estimates With Covariates - Male

	18 years old		23 years old		30 years old
	Education	Labor	Education	Labor	Labor
	a. Dep. Var.: Dummy Variable				
Ever Treated	0.035	-0.005	-0.006	0.044	0.016
	(0.034)	(0.054)	(0.018)	(0.046)	(0.031)
Robust p-value	0.341	0.702	0.782	0.268	0.538
Effect Size (%)	4.55%	-2.09%	-6.30%	6.70%	1.96%
Bwd.	[0.052;0.052]	[0.052;0.052]	[0.064;0.064]	[0.066;0.066]	[0.122;0.122]
Observations	23,189	19,451	29,094	25,170	12,197
	b. Dep. Var.: Number of Events				
Ever Treated	0.280**	-0.253		-0.764	-4.698
	(0.135)	(0.607)		(2.314)	(3.665)
Robust p-value	0.031	0.894		0.975	0.263
Effect Size (%)	10.89%	-10.03%		-3.40%	-8.49%
Bwd.	[0.061;0.061]	[0.050;0.050]		[0.070;0.070]	[0.138;0.138]
Observations	27,878	18,780		26,603	13,116
Parameter Selection:					
Pol. Degree	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular

Notes: Table 6 reports the local average treatment effects of being ever treated on a series of outcomes of interest for the sub-sample of men. These estimates are based on the specification described by equation 1 estimated using 2SLS where eligibility based on the score obtained in the first application is used as an instrument for the endogenous variable of ever treatment. Columns (1) and (2) correspond to the effects of the program on outcomes measured at the age of 18, columns (3) and (4) correspond to outcomes measured at the age of 23, and column (5) correspond to outcomes measured at the age of 30. As explained in Section 4, due to data limitations, there is no reliable information available about men's fertility decisions. Panel a. reports a series of estimates based on binary outcome variables, as defined in Figure 5. Panel b. reports estimates on a series of continuous outcome variables as defined in Figure 6. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), the bandwidth used is selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights and robust standard errors clustered at the household level. All estimates correspond to a model that includes the pre-treatment variables described in Table 3 (except predicted eligibility) as control variables. In addition to the point estimate, this table reports robust standard errors clustered at the household level, the robust p-value, the effect size measured as a percent of the mean for the ineligible group within the optimal bandwidth, the optimal bandwidth, and the total number of effective observations used in the estimation.

Table 7: LATE Effects, by Age - Estimates With Covariates - Female

	18 years old			23 years old			30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.094**	0.005	0.021	-0.047	0.019	0.112*	0.043	0.032
	(0.039)	(0.036)	(0.044)	(0.042)	(0.031)	(0.060)	(0.045)	(0.052)
Robust p-value	0.015	0.807	0.706	0.193	0.717	0.051	0.288	0.462
Effect Size (%)	-41.19%	0.65%	9.73%	-8.52%	20.76%	16.98%	5.62%	4.12%
Bwd.	[0.044;0.044]	[0.050;0.050]	[0.047;0.047]	[0.050;0.050]	[0.050;0.050]	[0.046;0.046]	[0.040;0.040]	[0.045;0.045]
Observations	20,033	22,751	18,913	22,711	22,778	18,375	3,343	5,740
				b. Dep. Var.: Number of Events				
Ever Treated	-0.108**	0.216	0.455	-0.137*		5.920***	-0.039	5.855
	(0.044)	(0.166)	(0.435)	(0.078)		(2.417)	(0.114)	(5.237)
Robust p-value	0.015	0.231	0.209	0.067		0.009	0.635	0.215
Effect Size (%)	-41.95%	8.60%	17.98%	-16.86%		26.50%	-2.78%	10.93%
Bwd.	[0.049;0.049]	[0.045;0.045]	[0.048;0.048]	[0.046;0.046]		[0.047;0.047]	[0.046;0.046]	[0.041;0.041]
Observations	22,536	20,375	19,214	20,776		18,756	3,760	5,210
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes: Table 7 reports the local average treatment effects of being ever treated on a series of outcomes of interest for the sub-sample of women. These estimates are based on the specification described by equation 1 estimated using 2SLS where eligibility based on the score obtained in the first application is used as an instrument for the endogenous variable of ever treatment. Columns (1) through (3) correspond to the effects of the program on outcomes measured at the age of 18, columns (4) through (6) correspond to outcomes measured at the age of 23, and columns (7) and (8) correspond to outcomes measured at the age of 30. Panel a. reports a series of estimates based on binary outcome variables, as defined in Figure 5. Panel b. reports estimates on a series of continuous outcome variables as defined in Figure 6. Following Calonico, Cattaneo, Farrell, and Titiunik (2018), the bandwidth used is selected by minimizing the mean squared error (MSE). Estimates reported are based on local linear regressions using triangular weights and robust standard errors clustered at the household level. All estimates correspond to a model that includes the pre-treatment variables described in Table 3 (except predicted eligibility) as control variables. In addition to the point estimate, this table reports robust standard errors clustered at the household level, the robust p-value, the effect size measured as a percent of the mean for the ineligible group within the optimal bandwidth, the optimal bandwidth, and the total number of effective observations used in the estimation.

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Online Appendix

The Medium-/Long-run Effects of a Conditional Cash Transfer Program on Early Adulthood Outcomes

Matías Giaccobasso

September 20, 2023

Appendix

A Further Details on the Institutional Background

A.1 Uruguay's Background and Comparison to Other Countries

Table A.1: Uruguay's Background and Comparison to Other Countries

Country	GDP PPP	HDI	HDI Rank	Tax Revenues (% gdp)	Completed Lower Secondary	Adolescent Fertility (per 1000s)	Unemployment rate
Argentina	23,290	0.842	46	28.78	57.16	62.57	9.22
Brazil	15,018	0.762	84	33.08	60.01	57.89	12.33
Chile	25,526	0.849	43	21.07	79.60	40.14	7.23
Colombia	15,044	0.764	83	19.45	54.44	65.48	9.11
Costa Rica	21,319	0.808	61	24.04	53.11	52.52	9.63
Italy	43,036	0.890	29	42.05	78.46	5.11	10.61
Mexico	20,096	0.776	74	16.13	63.26	59.45	3.28
OECD (Avg.)	45,008			34.26		19.73	5.29
Spain	40,696	0.905	25	34.40	77.72	7.49	15.25
Sweden	53,521	0.943	8	43.93	91.07	5.12	6.35
United States	62,805	0.925	17	24.33	96.03	18.56	3.68
Uruguay	23,585	0.816	55	29.17	56.83	58.24	8.34

A.2 Phase I: PANES

PANES was created in April, 2005 by the new center-left government as a response to the economic downturn that affected most Latin American countries in the early 2000s and it remained in place until December, 2007. The main component of this phase was a cash transfer targeted to the poorest 150,000 households in the country.

As most CCTs, *PANES* had two main goals. The first one, more related to a short run critical socio-economic context, was to alleviate the extremely high poverty levels in the country. The overall poverty rate in urban areas for 2005 was 36.6%. Specifically for children, poverty incidence was even more extreme being 61.30% for children between 0-5, 58.00% for children between 6-12, and 49.30% for children between 13-17. The second goal was more related to the medium- and long-run, and consisted of encouraging human capital accumulation in poor households, in particular for the children in those households. In this case, the cash transfer was supposed to ease the way out from poverty circles for many of these households who might had very few resources to dedicate to human capital investment of their children.

PANES was widely publicized, and it rapidly became the most generous anti-poverty program in the country's history up to 2005. The most important component of PANES was the cash transfer, but it was also comprised by other small-sized programs such as temporary public employment programs, education and training for the job market, and other minor interventions such as access to public utilities, building materials, and free dental and eye health care. While 96.7% of the participant households received the

cash transfer, less than 20% participated in the remaining components. Hence, *PANES* can be interpreted as mostly a cash transfer program, despite that for a few households it could have represented a wider set of benefits.[47]

The base cash transfer consisted of around USD 133.[48] In addition, a complementary transfer that ranged between USD 29 and USD 78 was provided to households with underage children (70% of the participant households). Overall, the cash transfer represented more than 50% of the average self-reported pre-program income. It is important to note that the income used as a reference to calculate this share is self-reported income. However, since the program also had an income threshold rule to define eligibility, households may have under-reported income to become eligible, and therefore this share must be interpreted as an upper bound. As an alternative reference, in April, 2005 the household per capita poverty line was USD 314.19 for rural areas and USD 469.95 for urban areas, in 2008 PPP terms.

The total number of applications to phase I – that were filled mostly between April, 2005 and May, 2005 - was 187,639, corresponding to 625,208 individuals. This represents about 17% of Uruguayan households and about 20% of the total population. Eligibility to participate on the program was determined based on two criteria. First, applicant households must had a per-capita income below USD 131 PPP. This restriction affected 10% of the applications. Second, households must had a poverty score below an arbitrarily defined threshold that varied by region. Regarding the first requirement, household per-capita income was calculated as the sum of each individual's income divided by the number of household components. Individual's income was defined as the maximum between the self-reported income at the moment of the household interview and the income registered with the Social Security Agency. Regarding the second requirement, households were visited by program officials who conducted a thorough interview to evaluate the socio-economic situation of the household. The information collected in this interview is used to compute a poverty score, which consists of a prediction of the probability of being below a critical per capita income level. If the value of this score was below a certain arbitrarily defined threshold, the household is eligible to receive the cash transfer. The use of a poverty score and an arbitrarily defined threshold to define eligibility to participate in the program was proposed and designed by researchers of the largest university in the country. Next, I list the variables used to calculate the poverty score as well as the regions used to define the eligibility threshold. It is important to note that neither the income nor the poverty score threshold were informed to the participants or publicly revealed. The participants were not informed about their poverty score either.

Predicted income based on probit model using the following covariates:

- Indicator for public employees in the households
- Indicator for pensioners
- Average years of education for individuals over 18 years old
- Number of members of the HH
- Indicator for children between 0-5 and 12 and 17
- Indicator for private health insurance

[47] See Manacorda et al. (2011) for more details about the program.

[48] In local currency, this corresponded to UYU 1,360. In what follows all income variables are converted to 2008 PPP using CPI and PPP conversion factors.

- Residential overcrowding
- Toilet facilities
- Wealth index based on household durables

Eligibility thresholds were set for five regions:

- Montevideo (capital city)
- North: Artigas, Salto and Rivera
- Center-North: Paysandu, Rio Negro, Tacuarmebo, Duranzo, Treinta y Tres and Cerro Largo
- Center-South: Soriano, Florida, Flores, Lavalleja and Rocha
- South: Colonia, San Jose, Canelones, Maldonado

After accepted, participant households were supposed to satisfy school attendance, regular health checks and per-capita income requirements. However, these conditions were not enforced at all due to administrative constraints. Furthermore, there is no evidence of participants being excluded of the program due to non-compliance with the requirements established by the program. Further details about the model used to calculate the poverty score as well as other details about the program implementation can be found in (Manacorda et al., 2011; Amarante et al., 2016).

A.3 Phase II: AFAM-PE

AFAM-PE is the name given to the program that replaced *PANES* in January 2008. While *PANES* was conceived as a temporary program, *AFAM-PE* was implemented as a part of a larger and more structural bundle of public sector reforms. These reforms included changes to the social safety net structure, but also to the tax structure and health insurance system. Unlike *PANES*, *AFAM-PE* was thought of, and legislated as, a permanent component of the social safety net in the country.

In practice, *AFAM-PE* was implemented as an expansion of *PANES*. The total number of applications – until December, 2017 - was 559,565, corresponding to about 1,349,292 different individuals. This represents a substantial increase compared to the population covered by *PANES* both because the program was intended to be expanded, but also because it was in place for a significantly larger period of time. There were only three differences between PANES and AFAM-PE. The first one is that AFAM-PE established as a requirement for eligibility the presence of underage children in the household, while PANES did not include any restriction related to household composition. The second was that the eligibility poverty score threshold became more lenient, aiming to expand the coverage of the program. Finally, the formula used to define the transfer amount was changed. The new structure established a baseline payment of USD 57 per children from 0-17 but subject to a equivalence scale of 0.6. In addition to the base payment, each household would receive an additional USD 24 per children enrolled in the secondary education system, also subject to an equivalence scale of 0.6.

Conditionalities to remain in the program also remained unenforced during the first years of implementation. However, in April, 2013 the government started to require household to present proof that they were actually fulfilling the requirements. Just as an example, in April, 2013 more than 30,000 children were unenrolled from the program because of non-compliance with the education enrollment requirements.

In subsequent years, enforcement strongly depended of who was the person in charge and it was relatively intermittent.

B Data Construction and Methodological Decisions

B.1 Description of Raw Data

Participation data

Participation data contains form-level information about successful and unsuccessful applications to PANES/AFA PE between April 2005 and December 2018. Information comes from a detailed socio-demographic questionnaire implemented by program officials to households that applied to the program. The information collected in this questionnaire was used to compute the poverty score that defines the eligibility condition. Table B.1 describes the raw variables included in these administrative records.

Table B.1: Description of Information Contained in Participation Records

Type	**Variable**
Application forms	Application number
	Resolution
	Application data
	Self-reported per-capita household Income
	Poverty score
	Application status
	History of participation
Household Characteristics	Application Number
	Department
	Housing characteristics and quality
	Ownership and value
	Access to utilities
	Appliances
Individual Characteristics	Application and masked national ID number
	Birth date
	Gender
	Education (current level and attendance)
	Activity and occupation status
	Income
Household Roster	Application and Masked national ID numbers
	Relation with household head

Phase I participation records contain information of 187,881 application forms. Of these, 102,436 were accepted applications and 85,291 were rejected. The total number of observations in the individual characteristics dataset is 708,622, which includes repeated individuals. In phase II, the total number of application forms is 639,167. This also includes households that were transferred automatically from phase I to phase II. Of the 639,167 phase II application forms, 499,277 were accepted and 139,890 were rejected. Phase II individual level data contains information of 1,249,466 individuals in the accepted applications data and 392,207 in the rejected data. Since individuals can show up in both datasets, the final number of unique individuals will be smaller.

In the process of cleaning the data some application forms were discarded due to different reasons. First, 190 application forms in phase I are excluded because they cannot be linked to a household or

individual level information, are special application forms or do not contain information of the score, place or date in where the application form was submitted. In phase II, I exclude 4,031 forms for similar reasons. In addition, there are 29,101 duplicated phase II forms that show up both in the accepted and rejected applications datasets. Second, since I observe rejected applications only until December, 2017, I drop all accepted applications forms corresponding to 2018 and 2019 (29,692 forms). In addition, 6,223 applications forms are dropped because they only contain individuals with missing id number, which is the variable used to link participation and other administrative records used to build the outcome variables. Finally, there are some cases where application forms for the same household are almost identical - including the application date - but they differ in some very specific variables. In cases where the only difference is the number of application, I keep one of them randomly and drop 6,922 forms. As to identical application forms with different identification numbers and rejection reason, I collapse the reasons and then drop 743 duplicated forms. Finally, for the remaining same day applications I drop 3,048 forms keeping the application form with the largest score and another 3 forms that have multiple applications the same day with different form number but the same score. The resulting number of forms is 747,134 and the corresponding number of unique individuals is 1,812,495

Birth Records

The birth certificates records come from the Ministry of Health and include the universe of births in the country (780,490) between 2003 and 2019. [49] The variables included in the raw dataset are: birth date, type of institution where the child was born (public, private or others), the age of the mother at the time of birth, birth weigth and gestation weeks. The number of mothers included in the dataset is 473,483 and 60% of them only gave birth to a single child, 29% gave birth two children, 8% gave birth three children and the remaining 3% to 4 or more children.

Education records

Education records come from three different sources: 1) National Council of Secondary Education, 2) National Council of Technical and Professional Education and 3) *Universidad de la Republica*, which is the largest public university in the country (more than 80% of total university enrollment).

Information from the National Council of Education provides enrollment data for 2006-2012, 2014, 2017 and 2018 for middle- and high-school levels in the traditional secondary education system. For 2008-2009, the information provided by the officials is incomplete and total enrollment is about 25% smaller compared to previous and subsequent years. The reason is most likely that an additional filter was used by the officials at the moment of extracting the data, which for these two years is restricted to students enrolled in more recent study plans. In any case, this is unrelated to treatment status or the running variable. If anything it represents measurement error that will bias the estimated treatment effects toward zero.

Information from the National Council of Technical and Professional Education contains enrollment data in vocational schools for the same period of time. In both cases, for 2004-2011 and 2014 there is also

[49] Actually, the original dataset contains information of 780,490 births but I drop 1,127 that correspond to 2002 and 3 which are mislabeled

additional information about promotion, courses passed and total absences during the school year.

Information from *Universidad de la Republica* contains enrollment data for the universe of students that ever enrolled to any major between 2005 and 2020. In this case, the only information available is whether the student was enrolled, and no additional information about major, progress, performance and completion status is available at this moment.

Labor market records

Labor market records come from the Social Security Agency and contain information about registered employment between 2005-2015. This dataset consists of individual-month-job level information and contains employment status, wage/income, and hours worked for all registered employees in the country.

In this case, the one-to-one matching that uses the masked ID number can only be performed for individuals that ever applied or were transferred to phase II of PANES/AFAM-PE. The reason is that the masked ID number required to do this merge is only available for individuals that ever showed up in phase II participation records. Out of the 1,900,000 masked national ID numbers that show up in participation records of PANES/AFAM-PE, 75% of them show up at least once in phase II records. Of these more than 95% have a masked ID number that can be used to link participation records and SSA data.

B.2 Creation of household identifier for Phase I data

Unlike phase II data, phase I only contains application form and personal identifiers, but it does not include a household identification number, which is key to detect which households applied multiple times to the program and to define the first application at the household level. One way to detect if two application forms correspond to the same household is to compare the personal identifiers of the individuals included in the applications. If the two forms include the exact same personal identifiers, they can be attributed to a same household. However, since household composition is dynamic (e.g. a new child could have being born or someone may leave), the process of constructing a household identifier is more challenging. For this reason, I create a phase I household identifier based on the following procedure that establishes a set of rules to define when application forms correspond to the same household. After identifying same-household applications in phase I, the next step is to match the phase I household identifier with phase II household identifiers. This procedure is explained in section B.3

1. Identify individuals individuals who are included in more than one application form:
    - Individuals with the same ID number
    - Individuals with the same name and birth date (some IDs corresponding to recently newborns are missing)
2. Create a list of forms that do not include any individual whose personal identifier shows up in multiple forms. These are the vast majority of forms (91.3%) and represent households with only one phase I application.
3. Check if there are forms with identical composition. 1.55% of the forms matched other forms with identical composition only, and 0.12% had at least one identical match but also some other non-identical match.

4. For the 7.15% forms remaining, I created an algorithm that defines whether two forms correspond to the same household. This algorithm compares each form with all the forms that contain at least one repeated individual and defines that two forms correspond to the same household if at least one of the following conditions hold:
    - Forms have the same household head, and the matching rate of individuals between the forms is larger than 50%
    - Forms have different household head, include two persons or more, and the matching rate of individuals between the forms is larger than 80%
    - Forms have different household head, one is included in the other (i.e., all individuals in the smaller form are included in the larger form) and the matching rate is larger than 60%
    - Forms have different household heads, one is not included in the other but some members intersect, the number of members is 3 or more, and matching rate is larger than 60%
5. After running the algorithm, 8,933 forms satisfied at least one of these rules and 4,493 did not. The latter correspond to individuals that are repeated across forms, but forms cannot be linked to the same household. This could be the case of an individual that left the original household, created a new one and apply to phase I.

B.3 Matching household identifiers between phases I and II

After creating the household identifiers for phase I, I harmonize the household identifiers of phase I and phase II. To do this, I merge the participation data of the two phases using the unique national identification number. For every individual included in the phase II data, I observe a list of phase I household identifiers where the national id number was included. 64.32% of all the national ID numbers observed in the pooled data correspond to phase II data only, while 35.68% show up both in phase I and phase II data. 99.66% of the national ID numbers that are included both in phase I and phase II data are associated to a unique household in phase I data. This is reasonable since phase I only lasted two years. 27.62% of the individuals show up only in phase I data. 99.66% of them are associated to a unique household.

The key challenge to link phase II and phase I households IDs correspond to cases where different individuals in the same phase II household can be linked to different phase I households. The actual percentage of households that are in this situation is very small (1.31% or 4,980 out of 380,040 household ids in phase II). For this 1.31% of the households I implemented the following rules:

1. For households in phase II that merge with multiple households in phase I, I assign the match to the household match that appears the most (of the total number of households in phase II this rule lefts 1,872 remaining cases. Note that this rule includes households with only one match, which as mentioned before are 98.17% of the households)
2. For ties, I assign the match observed for the household head. This rule left 750 households to match
3. In case of ties and when there is not match for a household head, I assign the match observed for the wife/husband/spouse of the household head. After this match, there are 537 households left to match
4. For the remaining cases I pick one of the matches randomly

B.4 Baseline Covariates

Based on the information available in the application form used the first time that a household applied to PANES/AFAM-PE, I create two sets of baseline variables that will be used as control variables to increase precision in the estimates and to test balance on observables in the baseline period: 1) household characteristics, 2) household head characteristics.

Table B.2: Description of Baseline Covariates

Group	**Variable**
Household Characteristics	Number of household members
	Number of children
	Single parent household
	Avg. age of the adults
Household Head Characteristics	Educ. years
	Working status
	Income reported head/partner

C Further Results on Descriptive Statistics

Column (1) reports the information corresponding to all application forms that have ever been filed between April, 2005 and December, 2017. The share of application forms in the full sample that correspond to 2005-2007 - i.e., phase 1 of *PANES/AFAM-PE* - is 25.11%. The average standardized poverty score is 0.21. Because the standardized score is centered around the eligibility threshold, positive values mean that individuals are eligible. Hence, the average individual in the full sample is an eligible individual. This is also reflected in the average acceptance rate, which is 70.46. 35.58% of the applications correspond to the capital city, which is under-represented compared to the distribution of the whole population. Regarding household structure, the average number of individuals in an application form is 3.92, of whom 1.96 are children. 51.44% of the application forms correspond to single-parent families. The average age of all household members is 24.01. Finally, household heads can be described as mostly low-educated individuals, with an average years of education that is just 0.77 years more than a level of complete primary education. Household heads have also low rates of labor market participation with an average employment rate of 55.40%. The average total household income is USD 313.77, while the average household head income is USD 164.80.

Column (2) reports the summary statistics corresponding to application forms that have at least one individual that belongs to the main sample (i.e., individuals that belong to households who applied for the first time when they were younger than 18 years old, have at least 15 years old by April, 2018, and applied for the first time to the program before between 2005-2012). In general, the overall characteristics of the application forms for this sub-sample are very similar to the full sample. However, there are some differences which are related to the selection criteria. For instance, the average number of individuals is 0.49 larger and this is mostly explained by a larger presence of children. Similarly, the average age of the household members is 2 years younger, compared to the full sample. Mechanically, the share of applications between 2005-2007 is also larger (6p.p.) since the main sample is restricted to applications

between 2005-2012. In terms of all the other variables, the forms are very similar between samples. This is also true for Column (3) that describes the characteristics of the forms that contain individuals from the balanced sample.

Table C.1: Descriptive Statistics: Outcome Variables at the Individual Level - By Sample

	Main Sample			Restricted Sample Fertility			Restricted Sample Labor		
	All	$[-0.08; 0]$	$(0; 0.08]$	All	$[-0.046; 0]$	$(0; 0.046]$	All	$[-0.082; 0]$	$(0; 0.082]$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
a. Fertility Outcomes									
Birth before 18yo	25.18	22.26	23.22	17.73	15.26	14.64	23.20	21.12	21.35
	(43.40)	(41.60)	(42.23)	(38.19)	(35.97)	(35.36)	(42.21)	(40.82)	(40.98)
Birth before 23yo	59.28	55.08	56.30	61.73	55.78	57.88	65.88	63.25	63.26
	(49.13)	(49.74)	(49.60)	(48.61)	(49.68)	(49.39)	(47.41)	(48.22)	(48.21)
Birth before 30yo	80.46	76.21	79.53	80.46	76.38	78.65	84.69	82.50	83.57
	(39.66)	(42.58)	(40.35)	(39.66)	(42.48)	(40.99)	(36.01)	(38.00)	(37.06)
Number of births before 18yo	0.29	0.25	0.26	0.18	0.16	0.15	0.25	0.23	0.23
	(0.53)	(0.50)	(0.50)	(0.41)	(0.38)	(0.38)	(0.48)	(0.46)	(0.47)
Number of births before 23yo	0.90	0.80	0.83	0.90	0.79	0.81	0.99	0.92	0.92
	(0.93)	(0.89)	(0.89)	(0.89)	(0.85)	(0.84)	(0.92)	(0.88)	(0.89)
Number of births before 23yo	1.57	1.42	1.47	1.57	1.41	1.45	1.69	1.57	1.57
	(1.20)	(1.16)	(1.13)	(1.20)	(1.15)	(1.13)	(1.19)	(1.15)	(1.12)
b. Education Outcomes									
Enrolled sec. educ. before 18yo	62.58	67.94	66.11	38.06	45.31	41.29	49.44	56.58	54.29
	(48.39)	(46.67)	(47.33)	(48.55)	(49.79)	(49.24)	(50.00)	(49.57)	(49.82)
Enrolled tert. educ. before 23yo	7.16	8.96	8.44	3.82	5.80	4.76	4.15	5.59	5.18
	(25.79)	(28.56)	(27.80)	(19.16)	(23.38)	(21.30)	(19.94)	(22.97)	(22.17)
Number of years enrolled sec. educ. by 18yo	2.30	2.58	2.48	0.57	0.69	0.63	0.99	1.15	1.12
	(2.01)	(2.08)	(2.05)	(0.83)	(0.88)	(0.86)	(1.25)	(1.30)	(1.30)
c. Labor Market Outcomes									
Worked four cons. months before 18yo	21.30	19.41	20.57	13.16	11.37	12.47	17.22	17.20	16.56
	(40.94)	(39.55)	(40.42)	(33.81)	(31.75)	(33.05)	(37.75)	(37.74)	(37.17)
Worked four cons. months before 23yo	60.98	58.03	60.82	49.83	45.17	46.62	63.67	64.65	63.03
	(48.78)	(49.35)	(48.82)	(50.00)	(49.77)	(49.89)	(48.10)	(47.81)	(48.27)
Worked four cons. months before 30yo	67.50	62.96	65.75	63.93	58.90	60.58	77.67	79.22	77.43
	(46.84)	(48.29)	(47.46)	(48.02)	(49.21)	(48.87)	(41.65)	(40.58)	(41.81)
Months worked before 18yo	2.39	2.21	2.30	1.41	1.27	1.30	1.89	1.95	1.81
	(4.22)	(4.24)	(4.18)	(3.14)	(3.09)	(2.96)	(3.61)	(3.82)	(3.56)
Months worked before 23yo	20.33	19.69	20.26	14.25	13.18	13.35	19.46	20.36	19.22
	(21.27)	(21.52)	(21.25)	(17.84)	(17.95)	(17.67)	(19.29)	(19.85)	(19.33)
Months worked before 30yo	43.89	43.12	43.42	40.08	38.57	39.10	50.65	54.52	51.31
	(44.23)	(45.56)	(44.55)	(42.87)	(43.80)	(43.31)	(43.88)	(44.97)	(44.18)
Earnings by 18yo (1000s)	1.04	0.98	1.02	0.45	0.43	0.42	0.66	0.70	0.65
	(2.21)	(2.20)	(2.21)	(1.21)	(1.23)	(1.18)	(1.52)	(1.61)	(1.55)
Earnings by 23yo (1000s)	12.83	12.63	13.00	7.12	6.90	6.86	10.76	11.62	10.82
	(17.64)	(17.77)	(17.76)	(11.42)	(11.75)	(11.43)	(14.34)	(14.90)	(14.37)
Earnings by 30yo (1000s)	32.42	33.25	32.78	28.42	28.72	28.63	37.52	42.15	38.86
	(45.51)	(47.89)	(46.49)	(42.18)	(44.75)	(44.07)	(47.11)	(50.49)	(48.32)
Observations	224,413	40,602	35,991	34,754	4,275	3,696	59,667	11,475	10,304

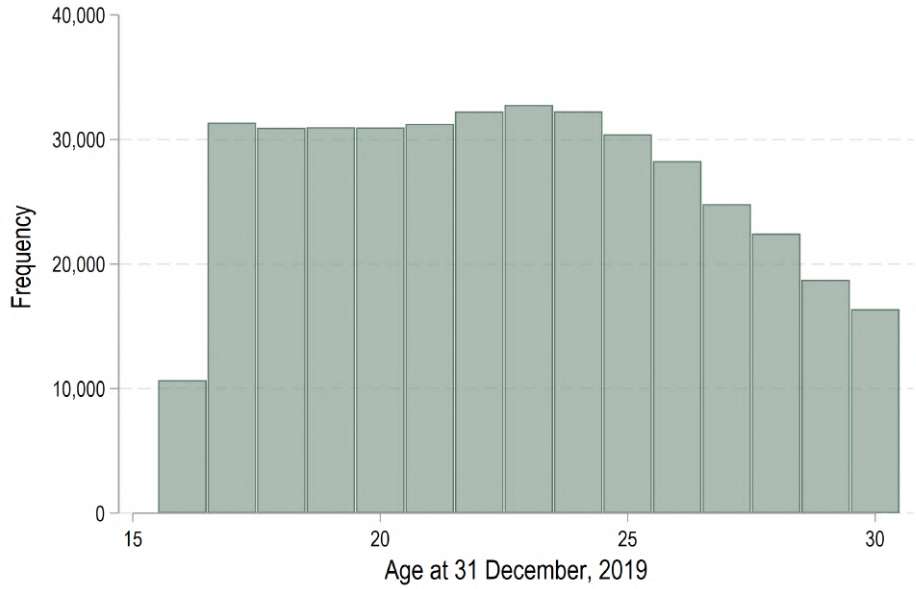
Notes:

Table C.2: Descriptive Statistics at the Application Form Level - By Sample

	All Forms			Reference Forms						
	Full Sample (1)	Main Sample (2)	Restricted Sample (3)	Full Sample (4)	Main Sample (5)	Main Sample Opt. Bdw. (6)	Restricted Sample Fertility (7)	Restricted Sample Fertility Opt. Bdw. (8)	Restricted Sample Labor (9)	Restricted Sample Labor Opt. Bdw. (10)
PANES Form (%)	25.11	32.33	43.39	52.72	70.09	81.81	100.00	100.00	87.16	92.44
	(43.37)	(46.77)	(49.56)	(49.93)	(45.79)	(38.58)	(0.00)	(0.00)	(33.45)	(26.43)
Std. Score	0.21	0.25	0.27	0.12	0.15	-0.00	0.15	-0.00	0.17	0.00
	(0.23)	(0.22)	(0.23)	(0.22)	(0.23)	(0.05)	(0.26)	(0.04)	(0.25)	(0.06)
Capital City (%)	35.58	32.98	32.37	34.14	31.33	18.41	29.14	15.21	30.68	17.65
	(47.87)	(47.01)	(46.79)	(47.42)	(46.38)	(38.76)	(45.44)	(35.92)	(46.12)	(38.13)
App. Accepted (%)	70.46	77.80	78.97	59.55	66.98	47.08	68.27	50.27	72.92	51.40
	(45.62)	(41.56)	(40.75)	(49.08)	(47.03)	(49.92)	(46.54)	(50.00)	(44.44)	(49.98)
Single Parent (%)	51.44	49.31	47.56	50.88	47.76	50.84	48.58	54.87	48.09	50.82
	(49.98)	(50.00)	(49.94)	(49.99)	(49.95)	(49.99)	(49.98)	(49.77)	(49.96)	(49.99)
Number of members	3.92	4.71	5.10	3.48	4.35	3.92	4.81	4.11	4.78	4.24
	(1.86)	(1.96)	(2.28)	(1.61)	(1.74)	(1.58)	(2.05)	(1.82)	(1.93)	(1.74)
Number of children	1.96	2.64	2.62	1.63	2.41	2.01	2.77	2.17	2.84	2.32
	(1.45)	(1.57)	(1.73)	(1.32)	(1.45)	(1.22)	(1.70)	(1.38)	(1.64)	(1.37)
Avg. age	24.01	22.75	22.18	26.85	24.37	26.19	24.58	26.83	23.47	25.44
	(11.83)	(7.70)	(7.48)	(14.33)	(8.30)	(8.58)	(8.05)	(8.27)	(7.69)	(8.04)
Household Head: Ed. years	6.77	6.52	6.42	7.06	6.89	7.18	6.73	6.93	6.75	7.09
	(3.26)	(3.25)	(3.24)	(3.49)	(3.43)	(3.55)	(3.49)	(3.51)	(3.42)	(3.55)
Household head: Employed (%)	55.40	58.30	57.94	57.44	62.51	63.62	65.08	63.19	64.14	64.24
	(49.71)	(49.31)	(49.37)	(49.44)	(48.41)	(48.11)	(47.67)	(48.23)	(47.96)	(47.93)
Household head: income	164.80	141.32	127.83	162.11	147.30	155.85	128.13	131.59	131.83	146.87
	(240.05)	(182.56)	(161.72)	(246.83)	(182.06)	(173.41)	(140.49)	(133.72)	(148.35)	(153.96)
Total household income	313.77	266.44	267.93	293.26	257.91	269.77	229.82	232.24	229.99	256.45
	(781.79)	(289.59)	(285.55)	(378.03)	(262.89)	(255.56)	(209.54)	(197.70)	(222.06)	(229.54)
Observations	747,204	265,350	62,734	342,412	126,041	46,810	29,713	7,000	39,763	14,661

Notes:

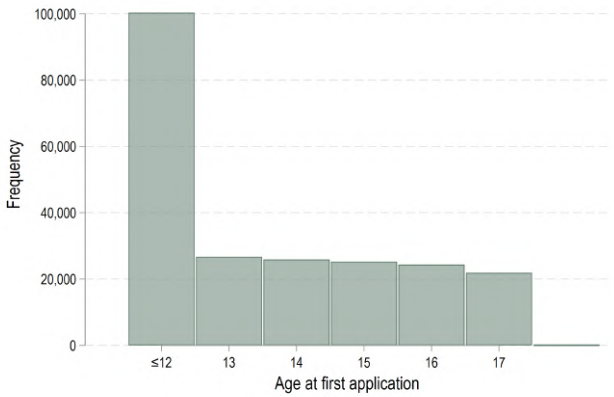
Figure C.1: Distribution of Age at 31 December, 2019



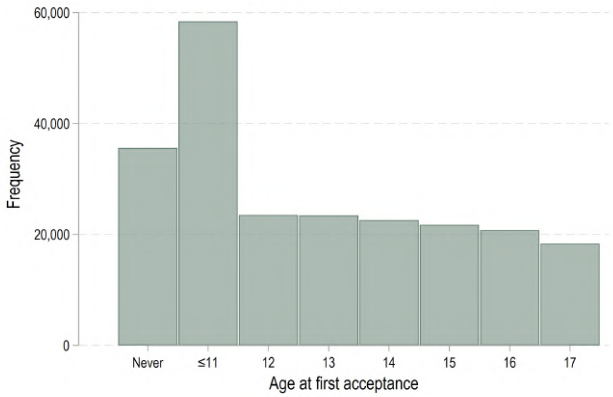
Notes:

Figure C.2: Descriptive Statistics - Application and Treatment - Sample 23yo at Dec, 2018

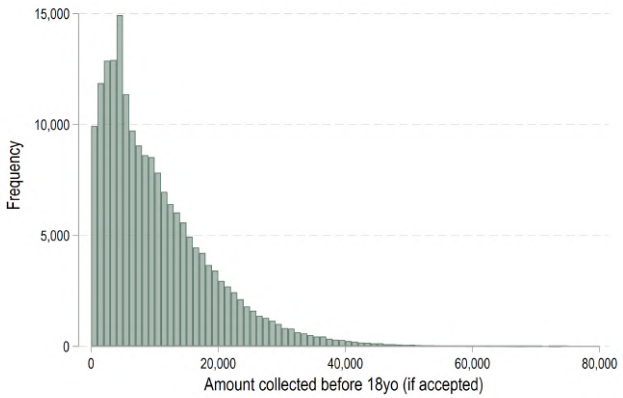
a. Age at first application



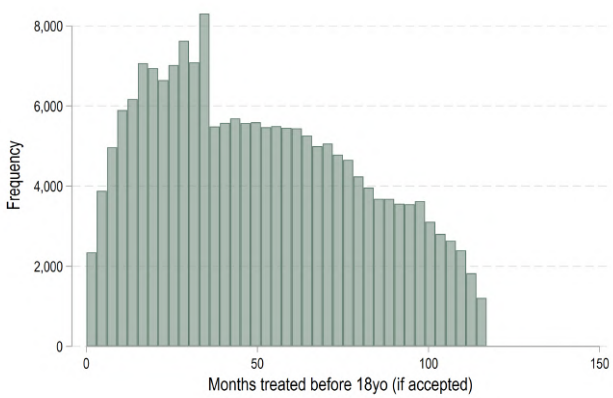
b. Age at first acceptance



c. Amount collected before 18yo



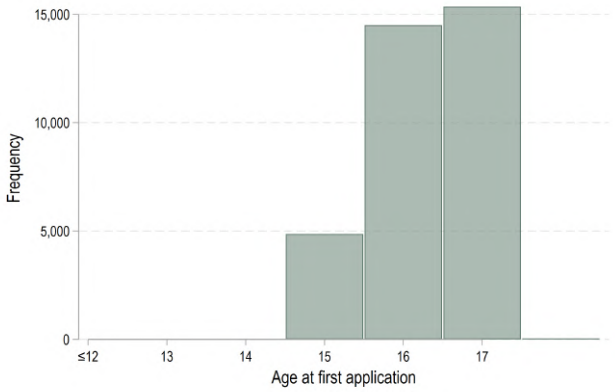
d. Months treated before 18yo



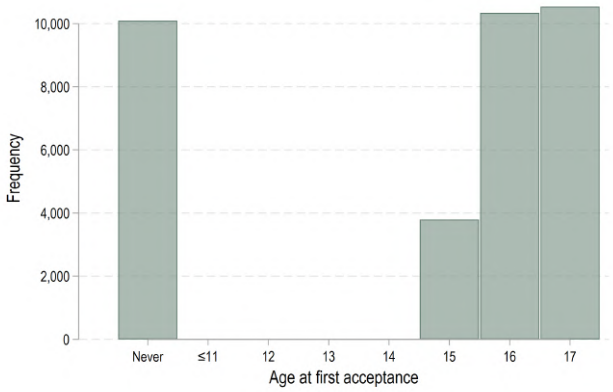
Notes:

Figure C.3: Descriptive Statistics - Application and Treatment - Dynamic Sample - Fertility Outcomes

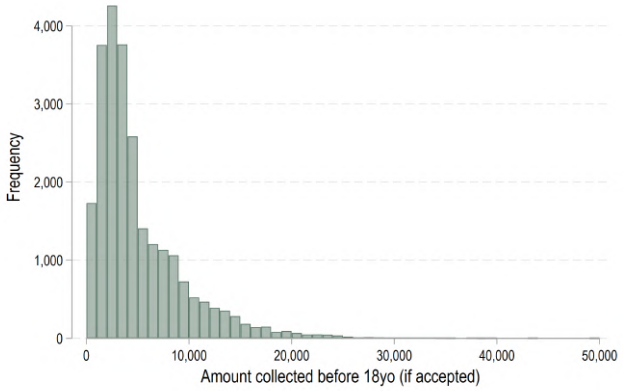
a. Age at first application



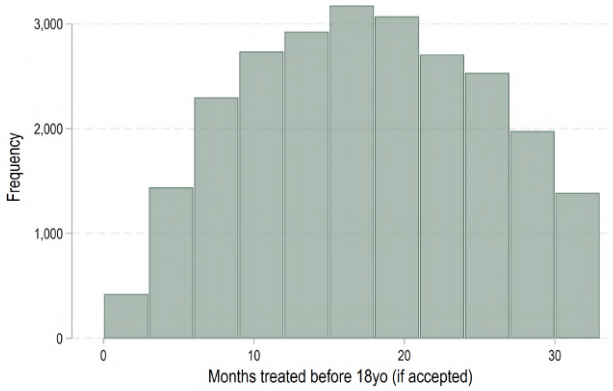
b. Age at first acceptance



c. Amount collected before 18yo



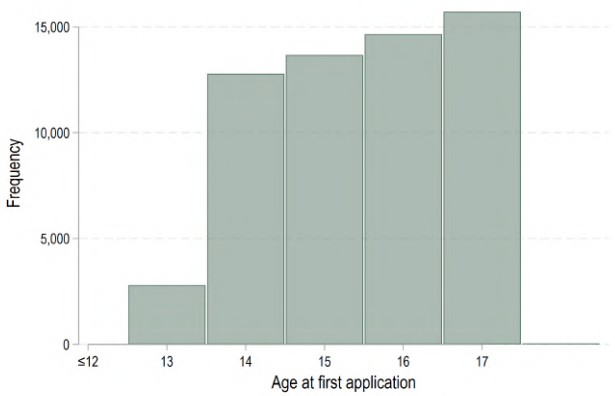
d. Months treated before 18yo



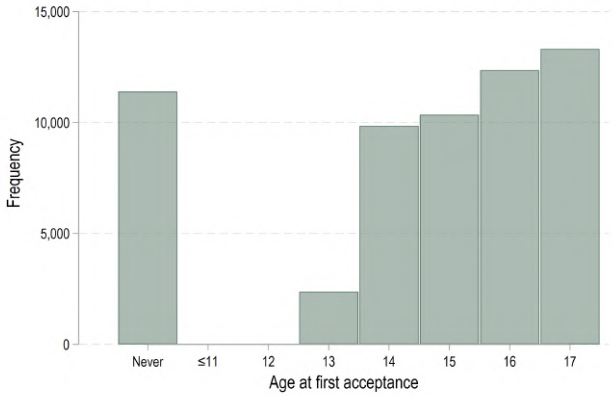
Notes:

Figure C.4: Descriptive Statistics - Application and Treatment - Dynamic Sample - Labor Market Outcomes

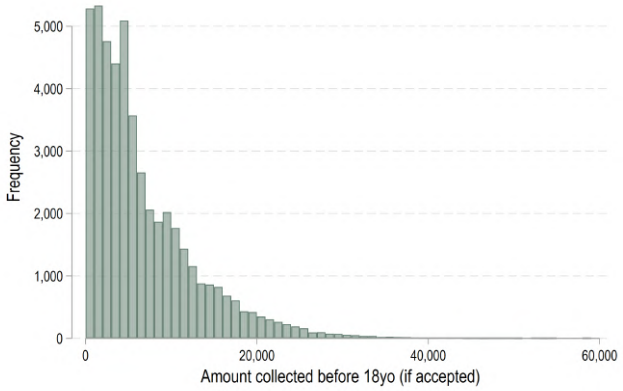
a. Age at first application



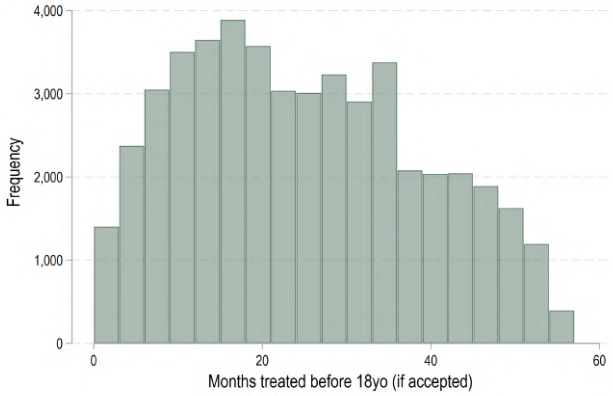
b. Age at first acceptance



c. Amount collected before 18yo



d. Months treated before 18yo



Notes:

D Further Details on Validity of the RDD

First, I report that first-stage estimates are identical when using two alternative treatment variables: 1) months treated and 2) the net present value of the total cash transfer amount collected by the household. Second, I report the first stage estimates for the two examples of the *dynamics sample*. It is important to note that first-stage estimates are larger in older samples. This difference is expected given that individuals who were closer to turning 18yo when applied for the first time to the program have fewer opportunities to re-apply before turning that age. Therefore, the score obtained in the first application is a stronger indicator of treatment before 18. Finally, I report a series of falsification tests on the *main* and *dynamic samples*. These illustrate that the discontinuity observed in the treatment variable at the eligibility threshold is not observed in any other arbitrarily defined placebo threshold.

Overall, Figure **??** in the main text shows persuasive evidence of an abrupt discontinuity in the treatment variable just at the eligibility threshold. The change in the probability of being ever accepted into the program before turning eighteen years old changes by 50% (29.3p.p.) just at the centered value of the first application poverty score, and this difference is statistically significant at traditional levels ($p - value \leq 0.001$). Table **??** presents the analogous regression estimates. Column (1) reports the

baseline estimates using a linear polynomial function and a triangular kernel function, while columns (2) through (4) present sensitivity tests based on alternative polynomial degrees and kernel functions.

A similarly sized discontinuity can be observed if we consider either the number of months treated or the total amount collected variables. For the number of months, individuals who are just to the right of the threshold have been exposed to the program before the age of 18 on average 15 months more compared to individuals just to the right. Expressed as a percentage of the average number of months for individuals to the left of the threshold, this is a difference of 47.38%. If we consider the *amount_treated* variable, the jump observed is of a slightly larger magnitude (58.93%), but qualitatively identical.

Table D.1: First Stage Estimates, Multiple Endogenous Variables - Main Sample

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
	a. Dep. Var: Ever Treated Before 18 Years Old			
Eligibility	29.677***	29.620***	29.251***	29.089***
	(0.921)	(1.067)	(0.892)	(1.013)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	50.08%	51.72%	49.84%	52.37%
Bwd.	[0.026;0.026]	[0.043;0.043]	[0.033;0.033]	[0.054;0.054]
Observations	24,551	40,813	31,413	52,538
	b. Dep. Var: Months Treated Before 18 Years Old			
Eligibility	15.174***	14.829***	15.130***	15.298***
	(0.649)	(0.735)	(0.620)	(0.811)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	55.27%	57.20%	56.67%	58.63%
Bwd.	[0.028;0.028]	[0.049;0.049]	[0.037;0.037]	[0.047;0.047]
Observations	26,893	46,967	35,527	44,592
	c. Dep. Var: Amount (USD 1,000) Received Before 18 Years Old			
Eligibility	3.243***	2.928***	3.291***	3.290***
	(0.136)	(0.145)	(0.136)	(0.171)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	69.19%	67.54%	72.01%	75.24%
Bwd.	[0.029;0.029]	[0.059;0.059]	[0.036;0.036]	[0.048;0.048]
Observations	28,133	57,694	34,028	45,177
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Uniform	Uniform	Triangular	Triangular

Notes:

Table D.2: First Stage Estimates - Dynamic Sample - Fertility Outcomes at Age 30

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
a. Dep. Var: Ever Treated Before 18 Years Old				
Eligibility	60.728***	57.054***	55.005***	59.210***
	(2.583)	(2.891)	(2.847)	(2.589)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	255.16%	272.47%	231.93%	325.01%
Bwd.	[0.017;0.017]	[0.030;0.030]	[0.017;0.017]	[0.043;0.043]
Observations	3,006	5,341	3,069	7,513
b. Dep. Var: Months Treated Before 18 Years Old				
Eligibility	10.413***	9.855***	9.846***	10.006***
	(0.473)	(0.581)	(0.552)	(0.540)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	294.65%	318.58%	271.73%	370.93%
Bwd.	[0.025;0.025]	[0.038;0.038]	[0.023;0.023]	[0.051;0.051]
Observations	4,377	6,577	4,098	8,886
c. Dep. Var: Amount (USD 1,000) Received Before 18 Years Old				
Eligibility	2.415***	2.382***	2.419***	2.397***
	(0.167)	(0.210)	(0.173)	(0.207)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	253.64%	295.18%	273.41%	300.29%
Bwd.	[0.032;0.032]	[0.045;0.045]	[0.037;0.037]	[0.055;0.055]
Observations	5,633	7,881	6,436	9,966
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Uniform	Uniform	Triangular	Triangular

Notes:

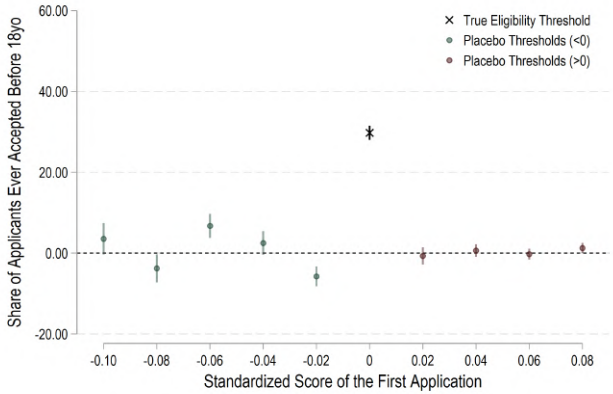
Table D.3: First Stage Estimates - Dynamic Sample - Labor Market Outcomes at Age 30

	First Stage Estimates - Alternative Specifications			
	(1)	(2)	(3)	(4)
a. Dep. Var: Ever Treated Before 18 Years Old				
Eligibility	42.846***	40.788***	40.291***	40.658***
	(1.707)	(2.093)	(1.989)	(2.048)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	98.17%	98.95%	91.51%	103.09%
Bwd.	[0.030;0.030]	[0.046;0.046]	[0.027;0.027]	[0.055;0.055]
Observations	8,240	12,115	7,411	14,731
b. Dep. Var: Months Treated Before 18 Years Old				
Eligibility	12.961***	12.852***	12.847***	12.522***
	(0.684)	(0.661)	(0.620)	(0.676)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	153.52%	180.14%	161.78%	172.34%
Bwd.	[0.022;0.022]	[0.051;0.051]	[0.032;0.032]	[0.057;0.057]
Observations	5,851	13,697	8,619	15,691
c. Dep. Var: Amount (USD 1,000) Received Before 18 Years Old				
Eligibility	3.141***	3.132***	3.112***	3.095***
	(0.204)	(0.218)	(0.182)	(0.220)
Robust p-value	0.000	0.000	0.000	0.000
Effect Size (%)	163.75%	199.40%	182.40%	206.85%
Bwd.	[0.024;0.024]	[0.048;0.048]	[0.037;0.037]	[0.054;0.054]
Observations	6,569	12,851	9,906	14,503
Parameter Selection:				
Pol. Degree	1	2	1	2
Bwd. Method	mserd	mserd	mserd	mserd
Kernel	Uniform	Uniform	Triangular	Triangular

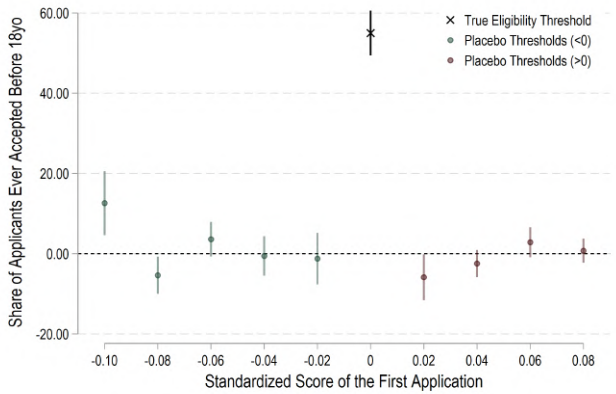
Notes:

Figure D.1: First Stage Falsification Tests

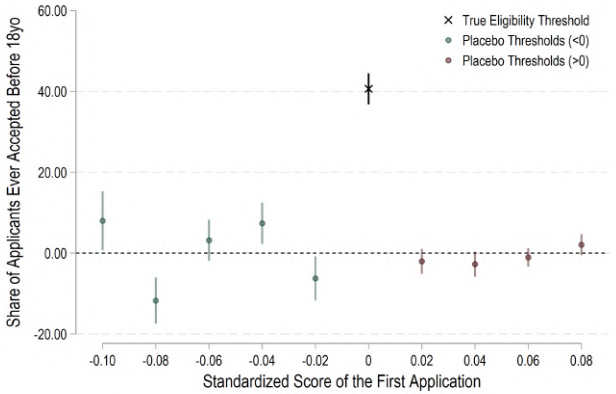
a. Main Sample



b. Dynamic Sample: Fertility at 30



c. Dynamic Sample: Labor Market at 30

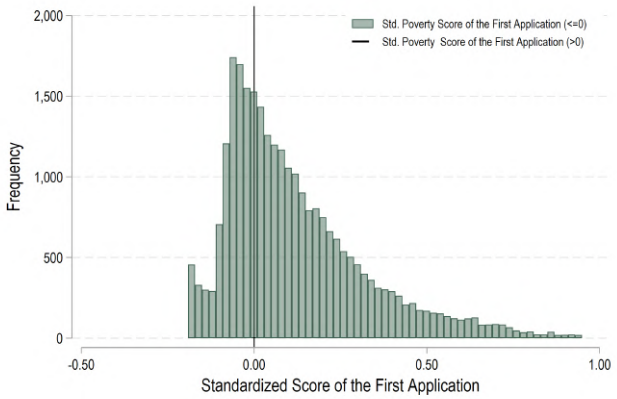


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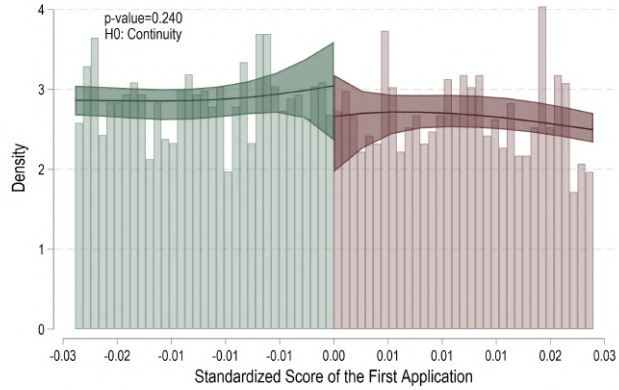
Figure D.2: Continuity of the Poverty Score in 1st. Application Form - Dynamic Sample

Fertility Outcomes at 30

a. Distribution of the Poverty Score

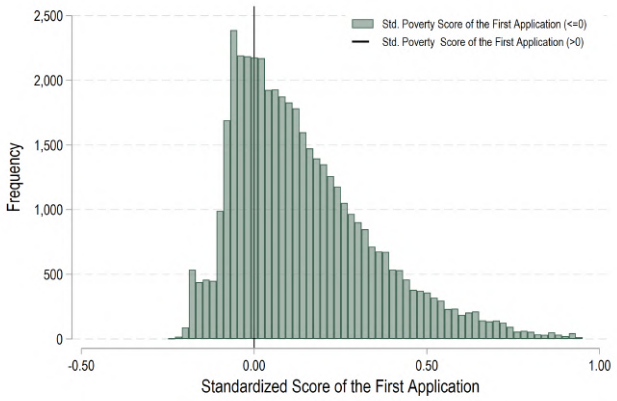


b. Continuity Test

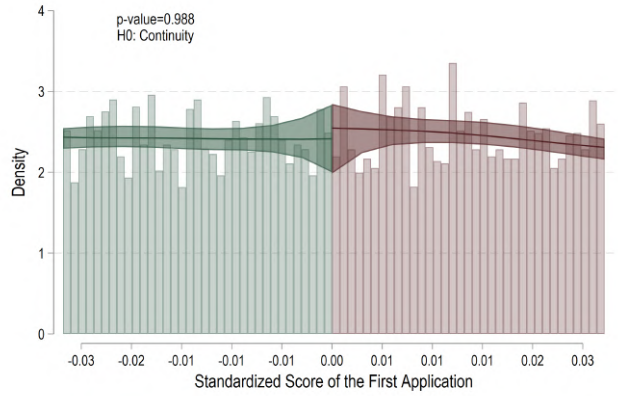


Labor Market Outcomes at 30

a. Distribution of the Poverty Score



b. Continuity Test



Notes:

Table D.4: Balance of Baseline Covariates - Dynamic Sample - Fertility Outcomes at 30

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	p-value Robust
	(1)	(2)	(3)	(4)
Predicted Eligibility	0.61	0.60	-0.013	0.189
HH - Avg. Age	26.79	27.30	0.503	0.155
HH - Avg. age adults	41.39	41.95	0.555	0.213
HH - Capital City	0.18	0.13	-0.049	0.035
HH - Number of people	4.24	4.25	0.007	0.902
HH - Number of children	2.28	2.24	-0.046	0.487
HH - Single Parent	0.55	0.55	0.001	0.887
HHH - Income (IHS)	4.20	4.22	0.027	0.760
HHH - Employed	0.60	0.62	0.012	0.789
HHH - Years of Educ.	6.76	6.89	0.128	0.536
Age at 1st. App.	16.88	16.86	-0.023	0.373
Age (Dec. 31, 2019)	31.13	31.09	-0.036	0.203
Number of Apps.	2.81	2.79	-0.014	0.653
Female	49.98	51.04	1.056	0.501
Number of HH.	1.71	1.65	-0.060	0.210

Notes:

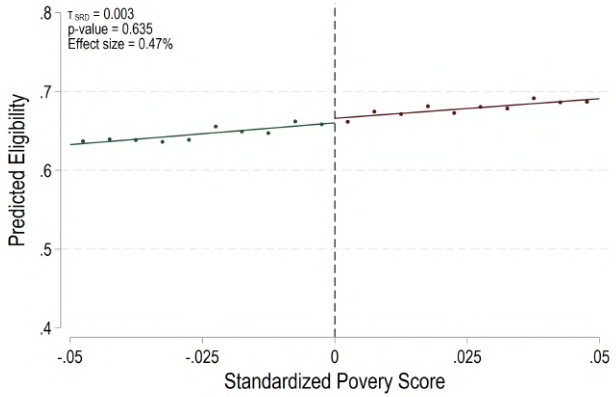
Table D.5: Balance of Baseline Covariates - Dynamic Sample - Labor Market Outcomes at 30

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	p-value Robust
	(1)	(2)	(3)	(4)
Predicted Eligibility	0.67	0.66	-0.007	0.267
HH - Avg. Age	25.97	25.97	0.004	0.769
HH - Avg. age adults	40.84	40.89	0.050	0.731
HH - Capital City	0.19	0.16	-0.033	0.083
HH - Number of people	4.39	4.40	0.008	0.964
HH - Number of children	2.41	2.39	-0.016	0.723
HH - Single Parent	0.52	0.56	0.037	0.100
HHH - Income (IHS)	4.32	4.35	0.025	0.686
HHH - Employed	0.62	0.61	-0.010	0.464
HHH - Years of Educ.	6.73	6.95	0.217	0.181
Age at 1st. App.	16.02	15.94	-0.084	0.057
Age (Dec. 31, 2019)	30.05	29.99	-0.063	0.107
Number of Apps.	3.03	3.05	0.018	0.805
Female	52.58	53.24	0.665	0.541
Number of HH.	1.81	1.75	-0.061	0.089

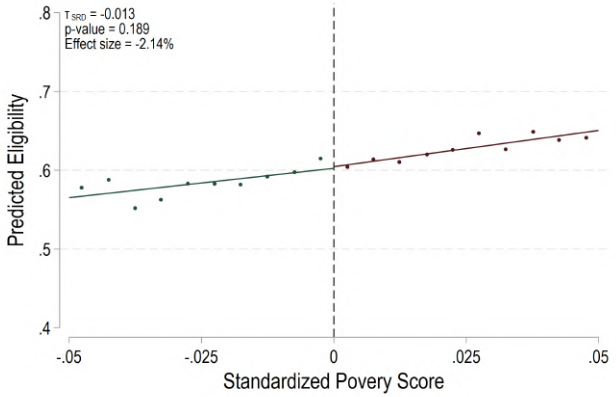
Notes:

Figure D.3: Balance on Baseline Covariates - Predicted Eligibility

a. Main Sample



b. Dynamic Sample: Fertility at 30



c. Dynamic Sample: Labor Market at 30



Notes:

E Further Results - Intention to Treat

Appendices E and F report additional robustness and sensitivity tests to validate the ITT and LATE results reported in the previous sections.

Randomization Inference: First, in the spirit of randomization inference, I replicate my baseline ITT estimates using different placebo cutoffs. The intuition behind this test is simple. The idea is to analyze whether estimates that are similar to the ones obtained using the true cutoff can be obtained by replicating the baseline strategy on randomly defined placebo thresholds. Having similarly sized and statistically significant effects when using these placebo cutoffs, where treatment status does not change, would suggest that RDD estimates could be capturing just noise. More specifically, I iterate the baseline ITT specification on every possible cutoff in the range [-0.08,0.50] in steps of 0.0025, excluding values close to the actual threshold, i.e., between -0.1 and 0.1. I use two illustrations to depict the results of these tests. First, I report the distribution of the estimates obtained from these falsification tests, jointly with a vertical line that represents the ITT effect estimated at the true cutoff. Second, I report the sorted p-values, highlighting the one that corresponds to the true cutoff. I do this both for the binary and the continuous variables. Both illustrations suggest that the RDD is capturing the true effects of the program. In particular, estimates that are statistically significant using the true cutoff lie on the extremes of the distribution. Furthermore, as expected, all these distributions are centered around 0, and have averages that are very close to 0. Figures depicting the ranked p-values show consistent evidence too. p-values estimated using the true cutoff have extremely low-rank values when they correspond to estimates that are computed at the true cutoff and are statistically significant.

Specification curves: Second, to rule out that the effects are driven by specific choices of the parameters involved in the RDD estimates, I report the specification curves for each estimate included in Tables 4 and 5. Specification curves are extremely useful to rule out that the estimated effects are driven by specific arbitrary technical choices in the parameters required to estimate an RDD using local polynomial regressions. More specifically, I plot the point estimate and 90% confidence intervals for all possible combinations of choices of 1) criteria used to define optimal bandwidth, 2) kernel functions, 3) polynomial degree, and 4) use of covariates, sorted by point estimate. Overall, the specification curves illustrate that the size and direction of the effect are consistent across different combinations of parameters and that estimates from the preferred specification (i.e., MSERD, triangular, and including covariates) are usually very close to the median estimates. Moreover, if anything, the baseline specification errs toward null effects.

Inclusion/exclusion of covariates Third, I test whether ITT estimates are robust to the inclusion/exclusion of additional baseline variables as control variables. Baseline ITT estimates were reported based on a specification without covariates to make them consistent with the visual evidence and as transparent as possible. For LATE estimates, the baseline results were reported based on a specification that includes additional covariates. Hence, for ITT estimates, Appendix D reports the estimates with covariates, while for LATE estimates, appendix E reports estimates without covariates. In both cases, the inclusion/exclusion of covariates provides estimates that closely resemble the baseline specifications in magnitude, size, and sta-

tistical significance.

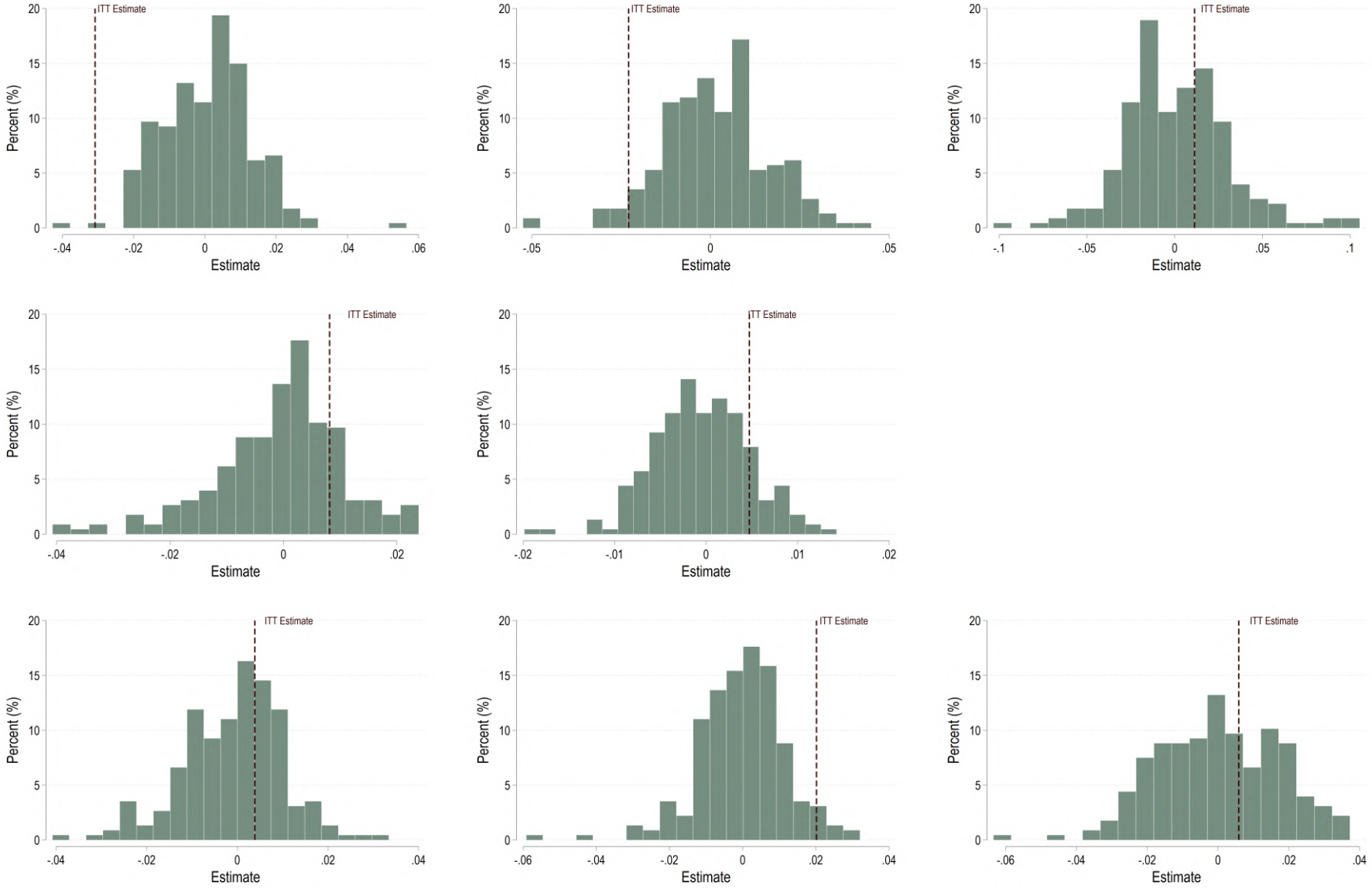
Balanced sample: Fourth, I report estimates in Table 4 measured at 18, 23, and 30 but using a fully balanced sample instead of the *main sample*. The fully balanced sample is comprised exclusively of individuals who were 30 years old or older in December 2019. Hence, the sample composition is held constant for every estimate reported in these tables. Overall, estimates are very similar in direction and slightly stronger in size. Furthermore, additional evidence supports the existence of weak positive ITT effects on secondary enrollment at or before age 18, even when using the binary outcome variable. However, in most cases, the effects are more imprecisely estimated because of a substantial reduction in the sample size, and the $p - values$ are moderately larger.

As discussed for ITT estimates, the LATE estimates based on the balanced sample are very similar to the estimates reported in Table 5 with only a few minor differences. For instance, the effects on fertility are slightly larger when using the fully balanced sample (-11.7p.p., and -0.123 births). Still, these are not statistically different from the estimates obtained in the baseline specification (9.4p.p. and 0.108 births). Relative to the estimates obtained using the *main sample*, the positive effect on the continuous education variable measured at age 18 is substantially smaller and statistically insignificant. This is consistent with the pattern of mixed evidence for secondary education outcomes. The effects on labor market participation also resemble the baseline results. The positive effects observed on the labor market variable are also present at age 23, but the magnitude is smaller than in the baseline specification, and the effect is statistically significant only in the extensive margin of labor market participation. Estimates for outcomes measured at age 30 are also extremely similar to the ones reported in Table 4.

Alternative endogenous variables: Finally, I replicate the baseline LATE analysis but using alternative definitions of the endogenous treatment variable. First, I substitute the binary treatment variable for a continuous variable that indicates the number of years in the program before turning 18. Estimates based on this alternative definition are almost identical in direction, statistical significance, and size when scaled up by the average value of the treatment variable. The same is true for estimates based on a continuous variable that measures the net present value of the total cash transfer amount collected by the household before the individual turns eighteen years old.

Figure E.1: Falsification Tests: Distribution of Estimate - Binary Variables

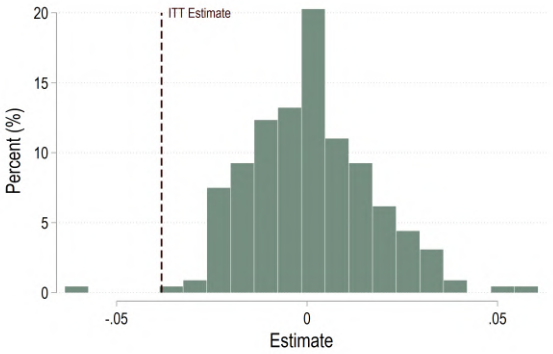
a. 18 years old b. 23 years old c. 30 years old



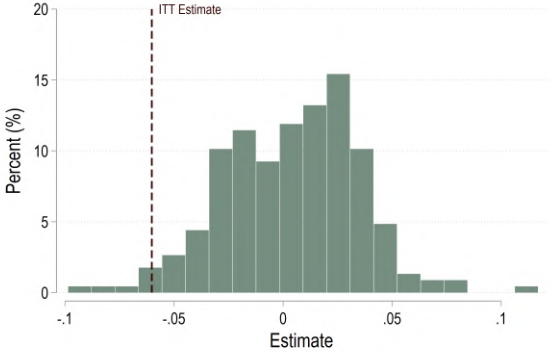
Notes:

Figure E.2: Falsification Tests: Distribution of Estimate - Continuous Variables

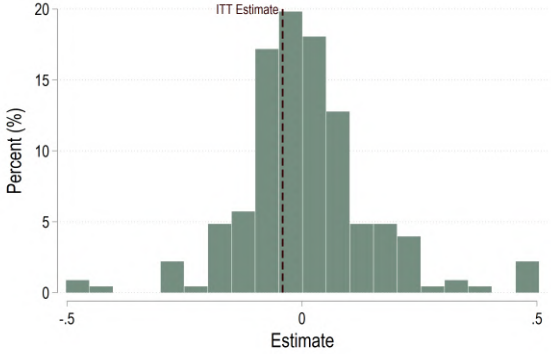
a. 18 years old

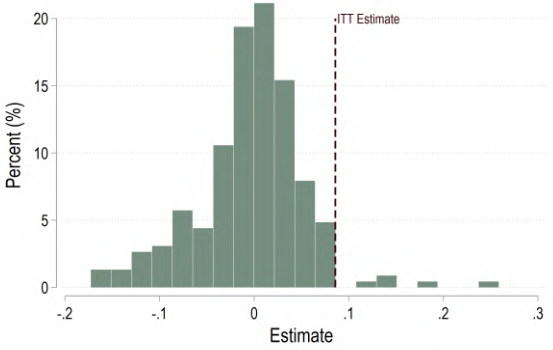


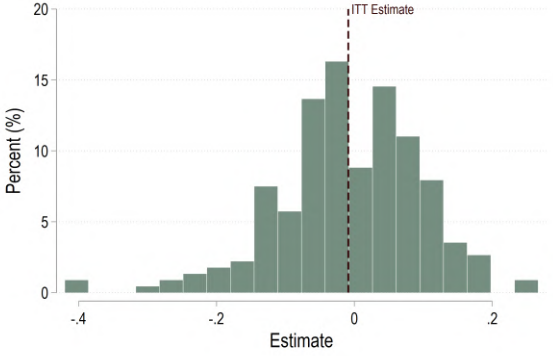
b. 23 years old

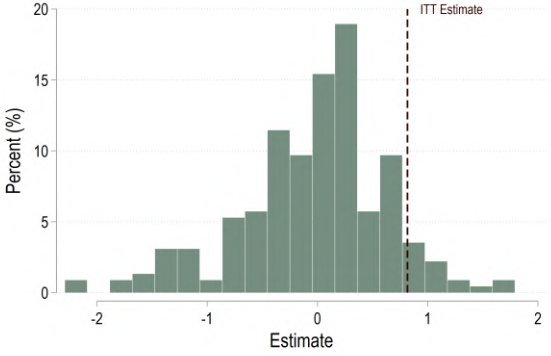


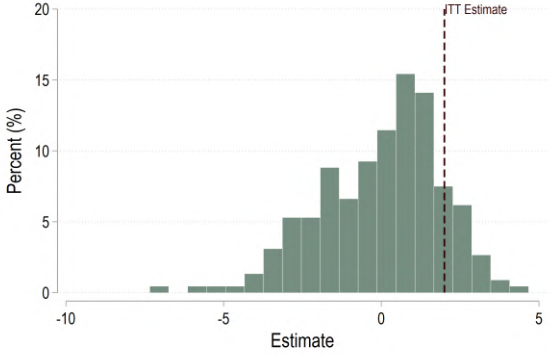
c. 30 years old







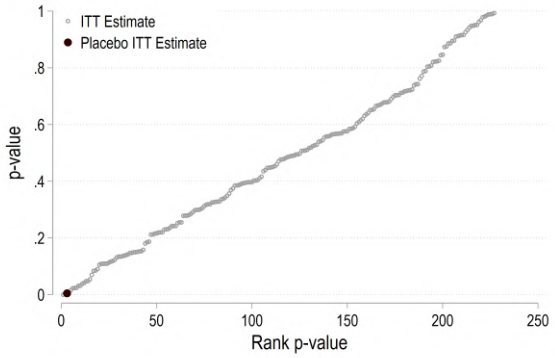




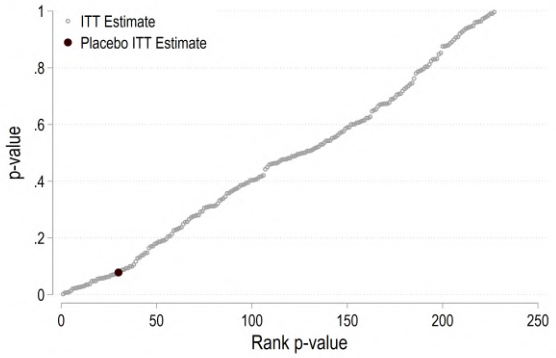
Notes:

Figure E.3: Falsification Tests: Sorted p-values - Binary Variables

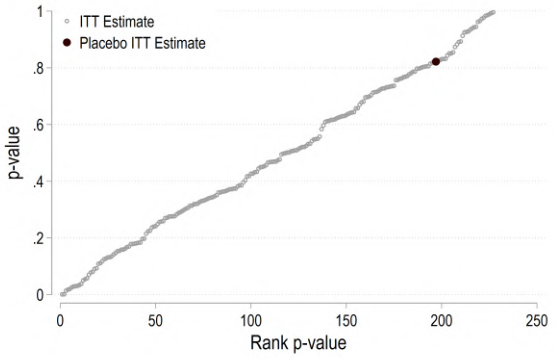
a. 18 years old

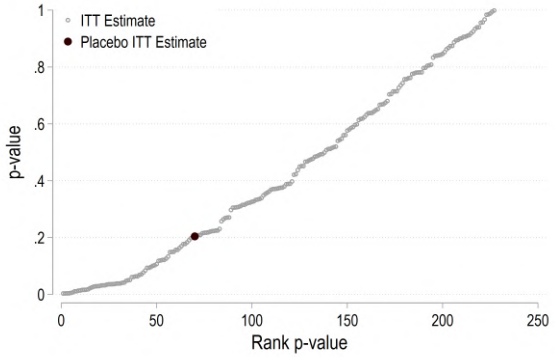


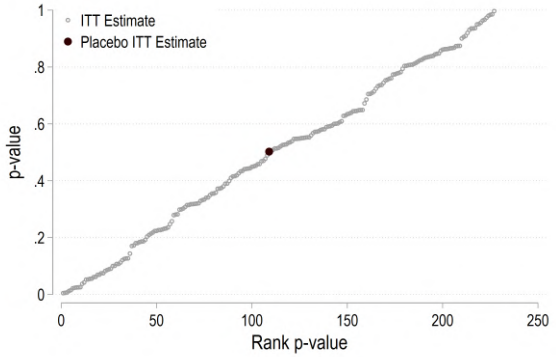
b. 23 years old

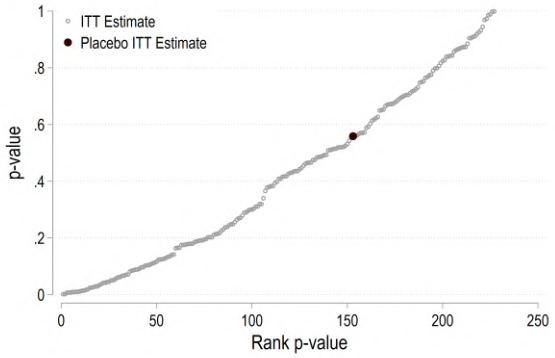


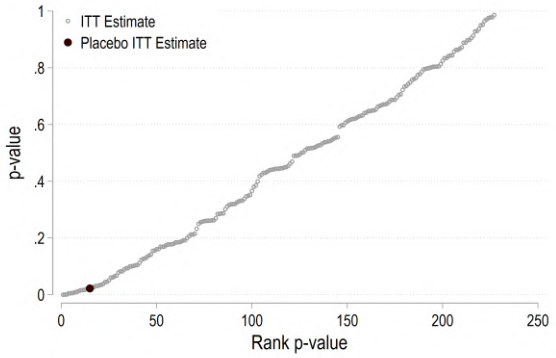
c. 30 years old

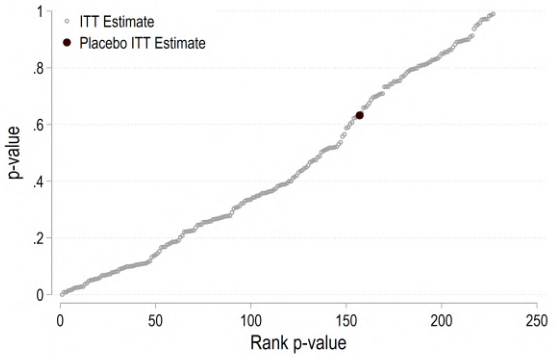








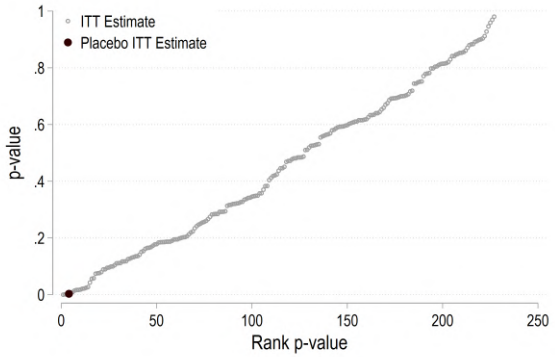




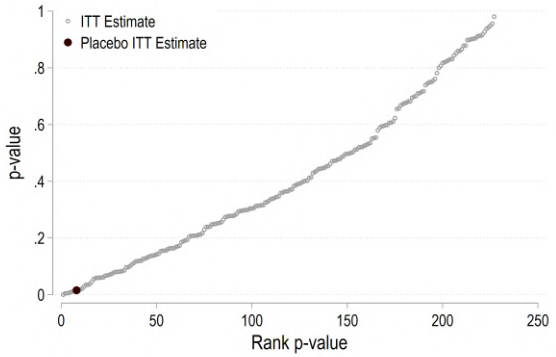
Notes:

Figure E.4: Falsification Tests: Sorted p-values - Continuous Variables

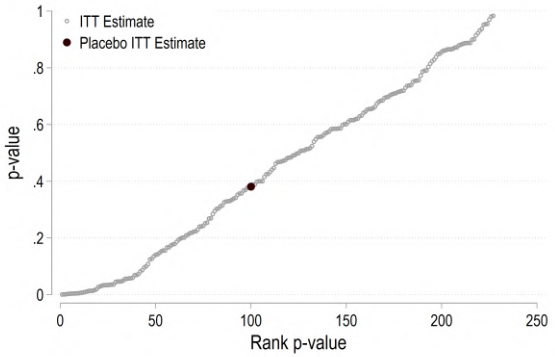
a. 18 years old

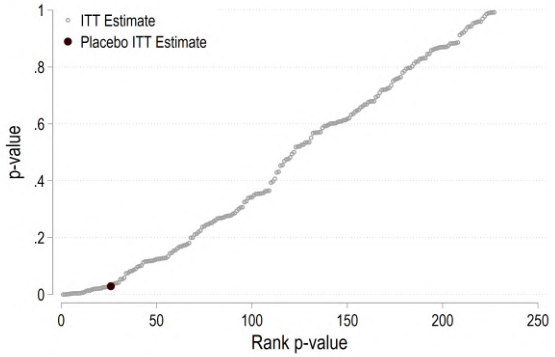


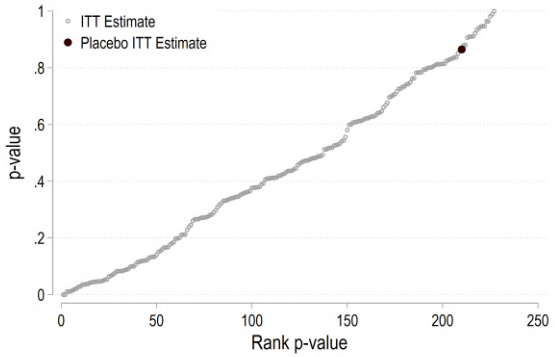
b. 23 years old

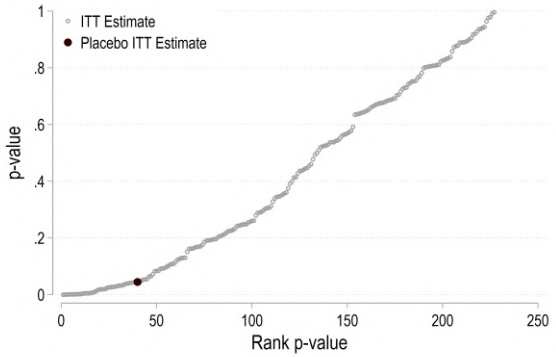


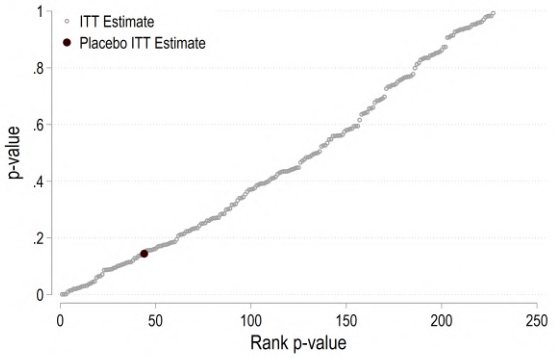
c. 30 years old







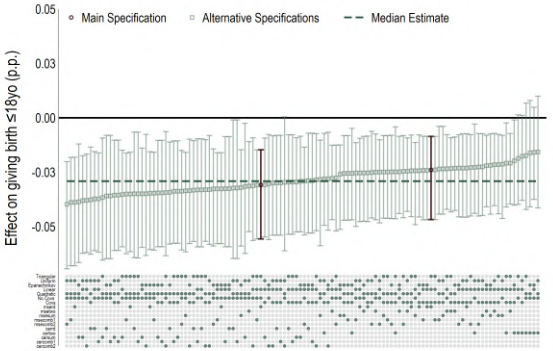




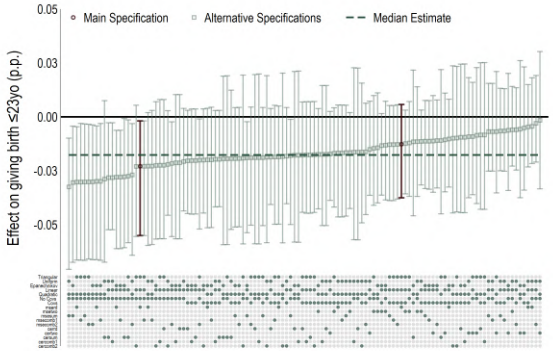
Notes:

Table E.1: Specification Curves - ITT Effects - Binary Variables

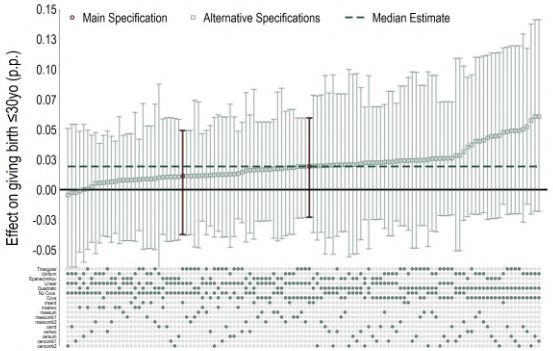
a. 18 years old

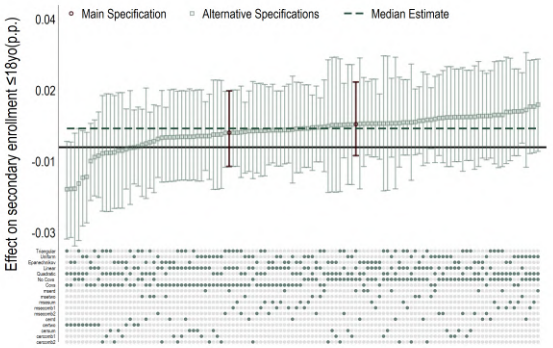


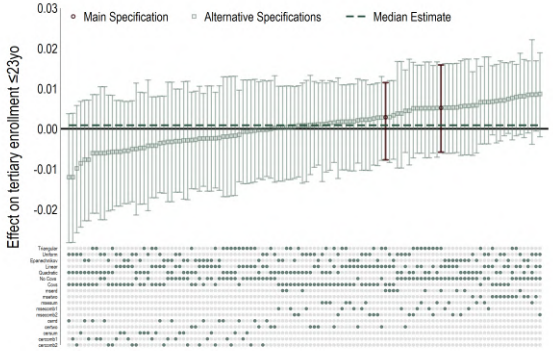
b. 23 years old

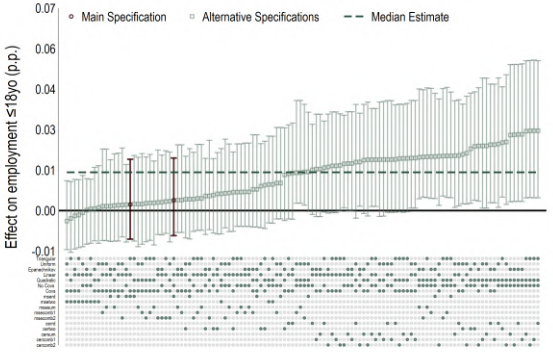


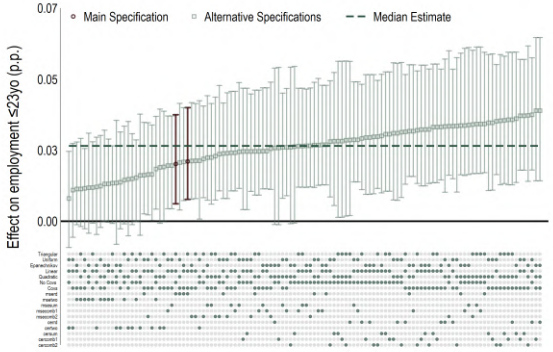
c. 30 years old

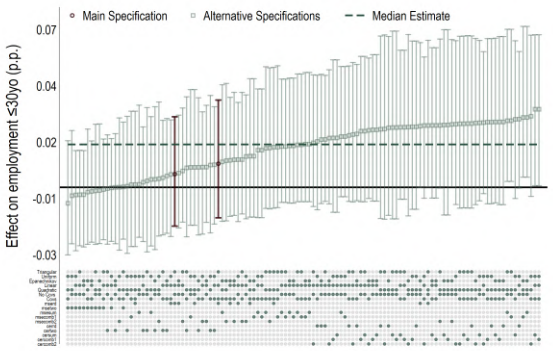








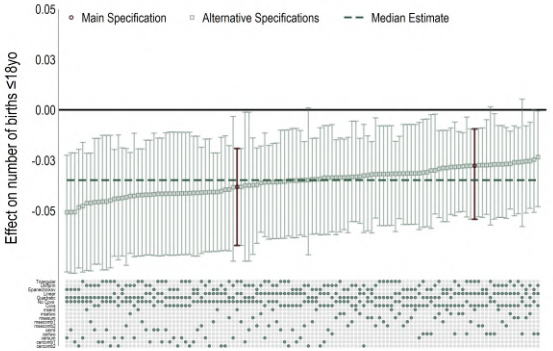




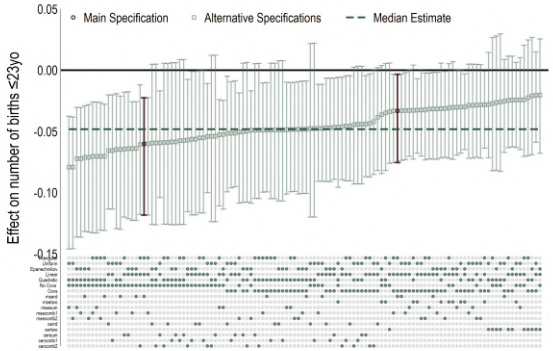
Notes:

Table E.2: Specification Curves - ITT Effects - Continuous Variables

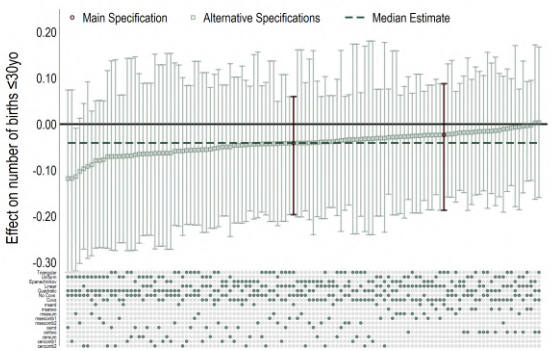
a. 18 years old

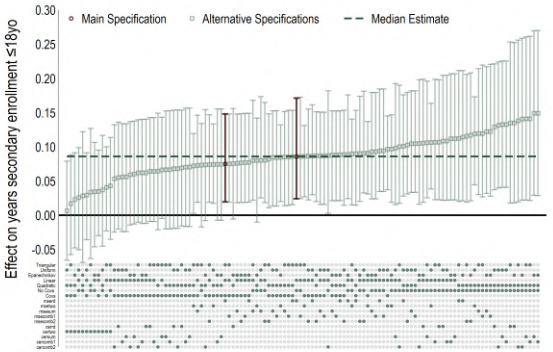


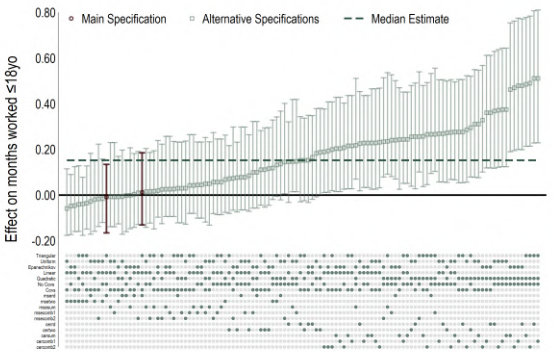
b. 23 years old

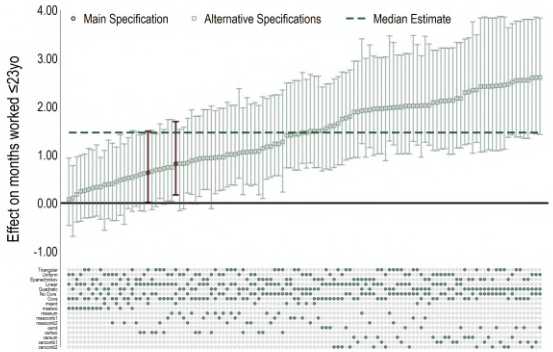


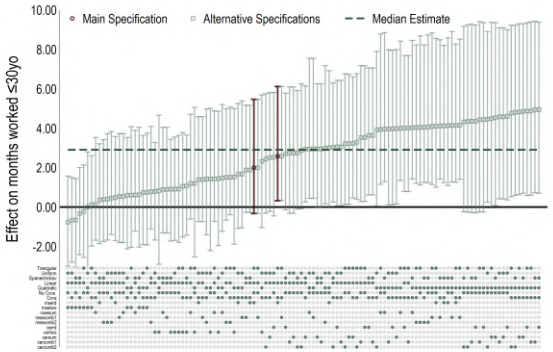
c. 30 years old











Notes:

Table E.3: Intention to Treat Effects, by Age - Estimates With Covariates

	18 years old			23 years old			30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.024**	0.005	0.002	-0.013	0.003	0.021**	0.019	0.011
	(0.010)	(0.007)	(0.008)	(0.011)	(0.005)	(0.009)	(0.021)	(0.013)
Robust p-value	0.017	0.418	0.639	0.227	0.748	0.015	0.465	0.428
Effect Size (%)	-10.56%	0.67%	1.04%	-2.30%	3.18%	3.19%	2.54%	1.34%
Bwd.	[0.052;0.052]	[0.053;0.053]	[0.051;0.051]	[0.056;0.056]	[0.059;0.059]	[0.054;0.054]	[0.078;0.078]	[0.060;0.060]
Observations	23,877	48,514	39,807	26,519	54,702	42,185	6,491	14,800
				b. Dep. Var.: Number of Events				
Ever Treated	-0.028**	0.075**	0.013	-0.033*		0.633*	-0.023	2.582*
	(0.012)	(0.034)	(0.081)	(0.019)		(0.385)	(0.072)	(1.564)
Robust p-value	0.019	0.032	0.776	0.073		0.094	0.553	0.067
Effect Size (%)	-10.88%	2.99%	0.51%	-4.11%		2.85%	-1.65%	4.81%
Bwd.	[0.055;0.055]	[0.043;0.043]	[0.053;0.053]	[0.061;0.061]		[0.056;0.056]	[0.049;0.049]	[0.050;0.050]
Observations	26,074	38,012	41,699	28,857		44,898	4,118	12,194
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

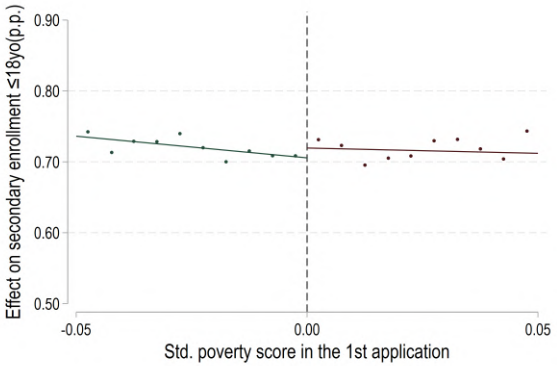
Table E.4: Intention to Treat Effects, by Age - At least 30 years old at 31 December, 2019

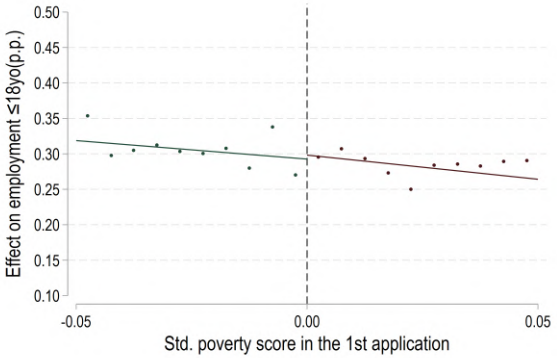
	18 years old			23 years old			30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.066***	0.038*	0.019	-0.030	-0.020*	0.057**	0.011	0.009
	(0.024)	(0.023)	(0.018)	(0.032)	(0.012)	(0.026)	(0.022)	(0.018)
Robust p-value	0.005	0.062	0.342	0.248	0.075	0.034	0.822	0.602
Effect Size (%)	-42.82%	8.33%	12.36%	-5.41%	-35.28%	9.66%	1.49%	1.22%
Bwd.	[0.044;0.044]	[0.046;0.046]	[0.051;0.051]	[0.051;0.051]	[0.033;0.033]	[0.049;0.049]	[0.071;0.071]	[0.075;0.075]
Observations	3,888	7,956	6,598	4,622	5,828	6,234	6,504	9,653
				b. Dep. Var.: Number of Events				
Ever Treated	-0.065***	0.086**	-0.006	-0.090*		1.022	-0.041	3.163*
	(0.025)	(0.045)	(0.138)	(0.055)		(0.820)	(0.068)	(1.966)
Robust p-value	0.009	0.049	0.911	0.066		0.222	0.381	0.091
Effect Size (%)	-40.66%	12.57%	-0.34%	-11.51%		5.75%	-2.96%	6.16%
Bwd.	[0.046;0.046]	[0.036;0.036]	[0.073;0.073]	[0.050;0.050]		[0.067;0.067]	[0.057;0.057]	[0.063;0.063]
Observations	4,034	6,292	9,453	4,527		8,695	5,245	8,194
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

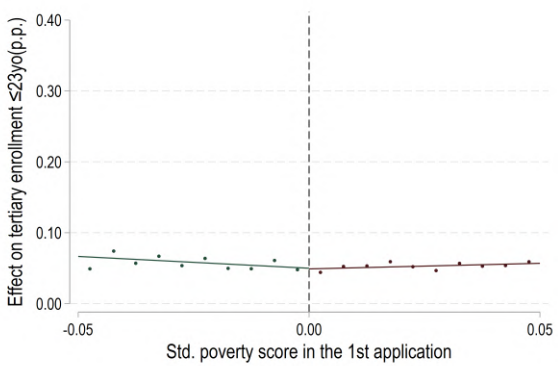
Figure E.5: Graphic Evidence: Intention to Treat Effects, by Age - Binary Variables - Male

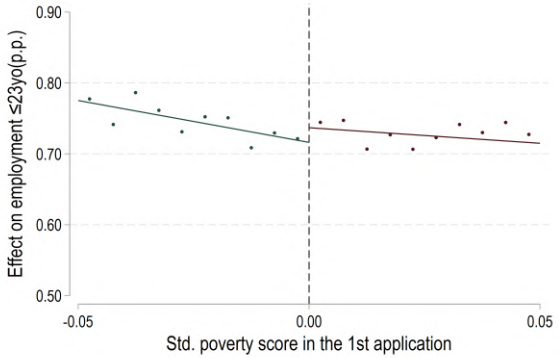
a. 18 years old



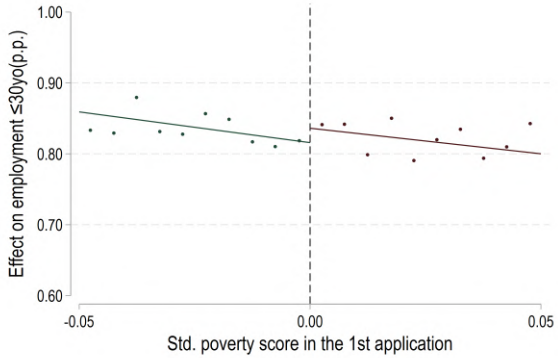


b. 23 years old





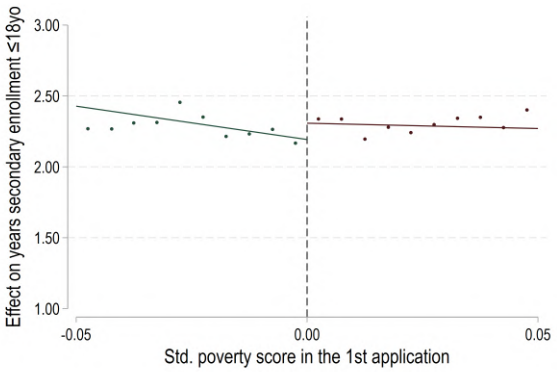
c. 30 years old

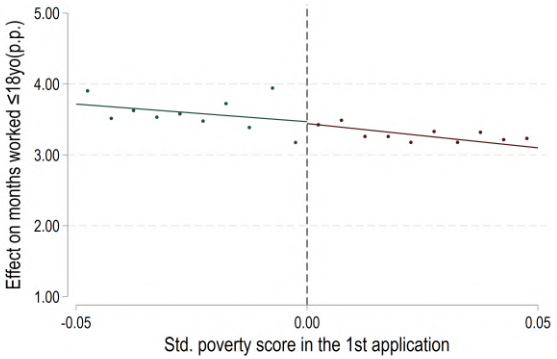


Notes:

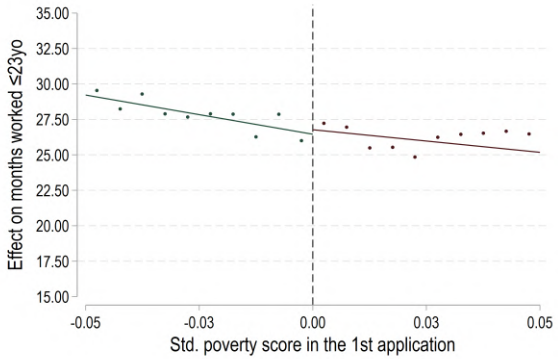
Figure E.6: Graphic Evidence: Intention to Treat Effects, by Age - Continuous Variables - Male

a. 18 years old

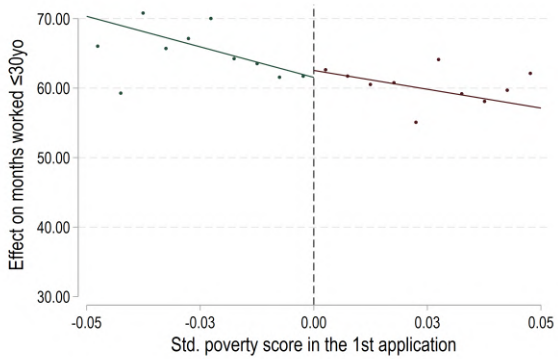




b. 23 years old



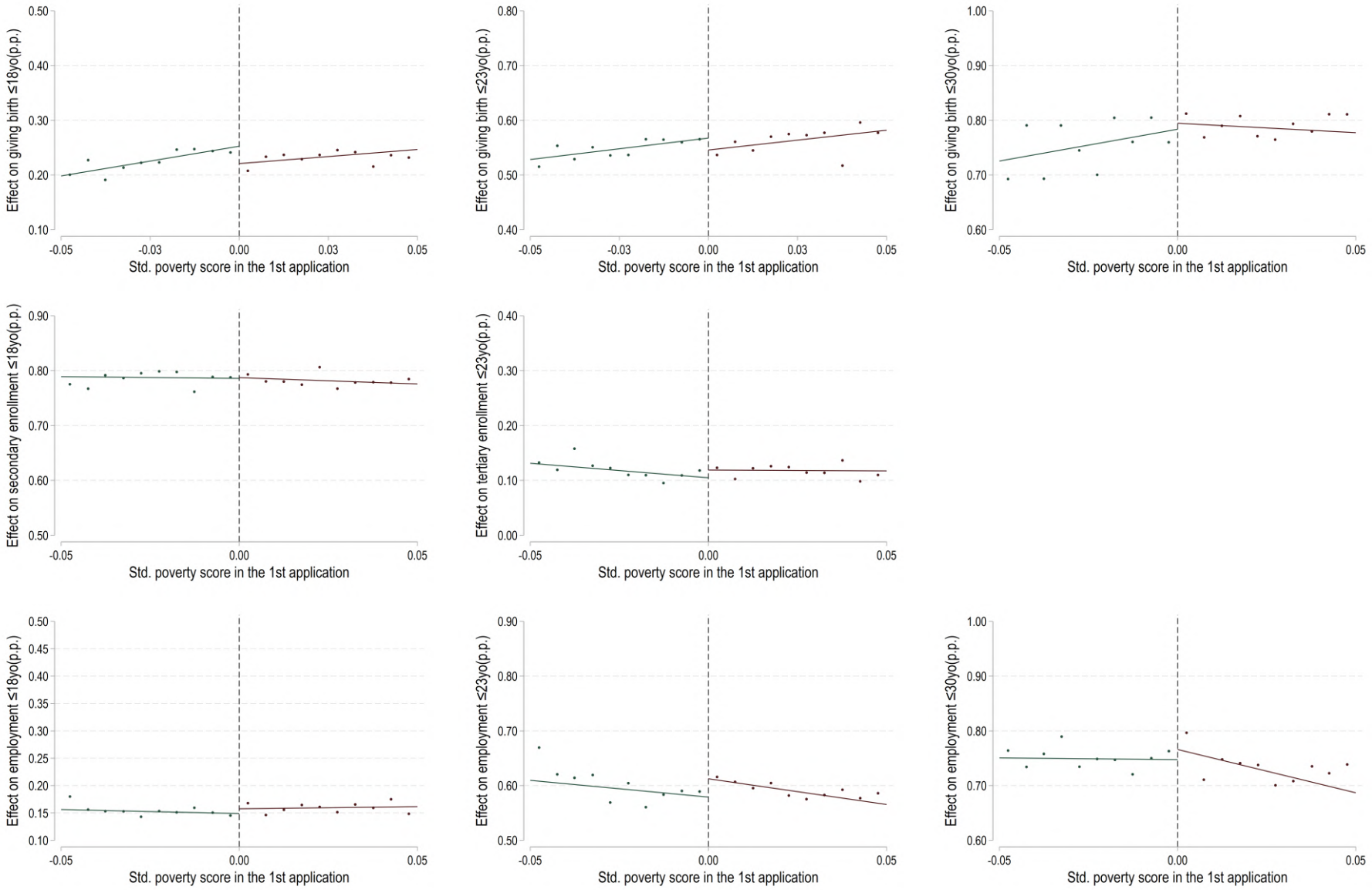
c. 30 years old



Notes:

Figure E.7: Graphic Evidence: Intention to Treat Effects, by Age - Binary Variables - Female

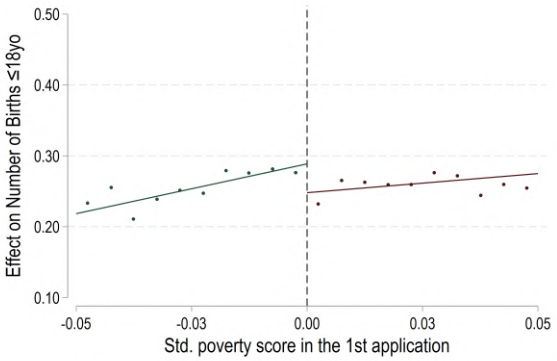
a. 18 years old b. 23 years old c. 30 years old

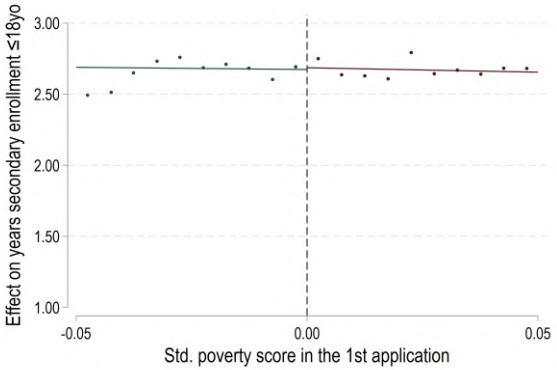


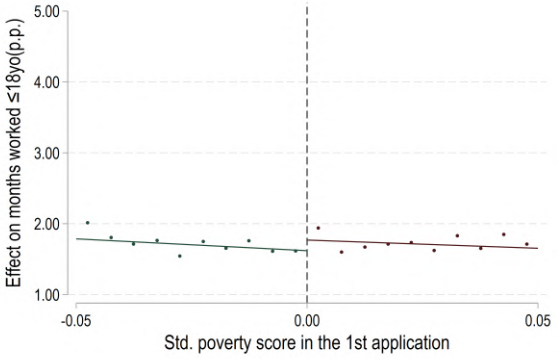
Notes:

Figure E.8: Graphic Evidence: Intention to Treat Effects, by Age - Continuous Variables

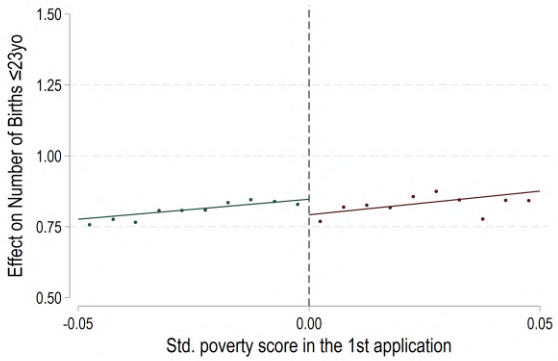
a. 18 years old

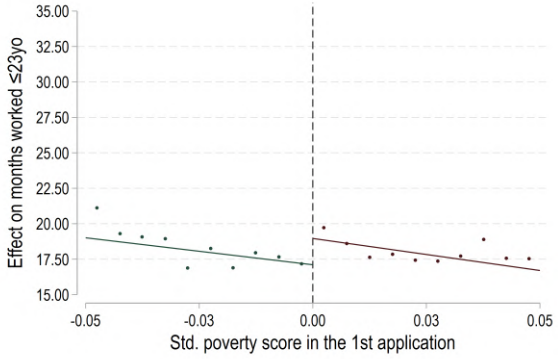




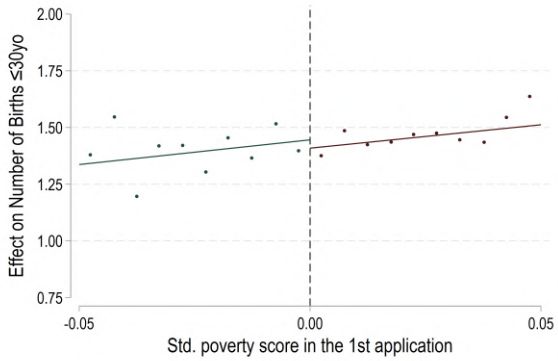


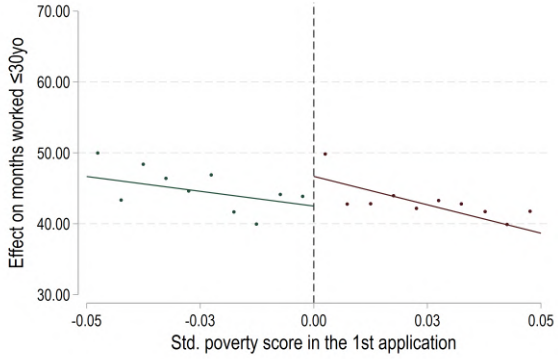
b. 23 years old





c. 30 years old





Notes:

F Further Results - LATE

Table F.1: LATE Effects, by Age - Estimates With Covariates- Balanced 30

	≤ 18 years old			≤ 23 years old			≤ 30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.117**	-0.003	0.044	-0.029	-0.010	0.068**	0.043	0.021
	(0.046)	(0.025)	(0.029)	(0.038)	(0.008)	(0.031)	(0.045)	(0.025)
Robust p-value	0.011	0.858	0.151	0.377	0.244	0.015	0.288	0.458
Effect Size (%)	-72.92%	-0.66%	28.23%	-5.17%	-16.59%	11.16%	5.62%	2.65%
Bwd.	[0.031;0.031]	[0.079;0.079]	[0.049;0.049]	[0.069;0.069]	[0.186;0.186]	[0.079;0.079]	[0.040;0.040]	[0.093;0.093]
Observations	2,599	12,858	5,812	5,915	21,563	9,550	3,343	10,868
				b. Dep. Var.: Number of Events				
Ever Treated	-0.123**	-0.025	0.231	-0.136		0.660	-0.039	1.906
	(0.049)	(0.031)	(0.259)	(0.100)		(0.862)	(0.114)	(2.087)
Robust p-value	0.013	0.273	0.441	0.236		0.206	0.635	0.214
Effect Size (%)	-73.26%	-3.52%	13.58%	-16.92%		3.55%	-2.78%	3.60%
Bwd.	[0.031;0.031]	[0.152;0.152]	[0.050;0.050]	[0.036;0.036]		[0.135;0.135]	[0.046;0.046]	[0.134;0.134]
Observations	2,586	19,285	6,034	2,985		13,602	3,760	13,554
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.2: LATE Effects, by Age - Estimates Using Years Treated as Endogenous Variable

	≤ 18 years old			≤ 23 years old			≤ 30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.023**	0.004	0.008	-0.012	0.002	0.014**	0.029	0.018
	(0.009)	(0.006)	(0.008)	(0.011)	(0.004)	(0.006)	(0.032)	(0.022)
Robust p-value	0.018	0.524	0.231	0.203	0.881	0.031	0.357	0.437
Avg. Treatment	3.75	3.78	3.97	3.75	3.81	4.03	1.29	1.33
Effect Size (%)	-37.00%	1.96%	13.58%	-8.39%	8.07%	8.28%	4.96%	3.17%
Bwd.	[0.042;0.042]	[0.041;0.041]	[0.034;0.034]	[0.043;0.043]	[0.053;0.053]	[0.062;0.062]	[0.042;0.042]	[0.061;0.061]
Robust p-value	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
				b. Dep. Var.: Number of Events				
Ever Treated	-0.027**	0.061**	0.115	-0.034*		1.407***	-0.043	2.713
	(0.011)	(0.028)	(0.087)	(0.019)		(0.445)	(0.096)	(1.886)
Robust p-value	0.015	0.047	0.128	0.075		0.001	0.564	0.115
Avg. Treatment	3.76	3.78	3.97	3.75		3.97	1.29	1.34
Effect Size (%)	-39.58%	9.19%	17.97%	-15.38%		25.24%	-3.94%	6.99%
Bwd.	[0.044;0.044]	[0.038;0.038]	[0.031;0.031]	[0.042;0.042]		[0.029;0.029]	[0.035;0.035]	[0.081;0.081]
Observations	19,946	34,264	23,864	19,022		22,563	2,954	9,694
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

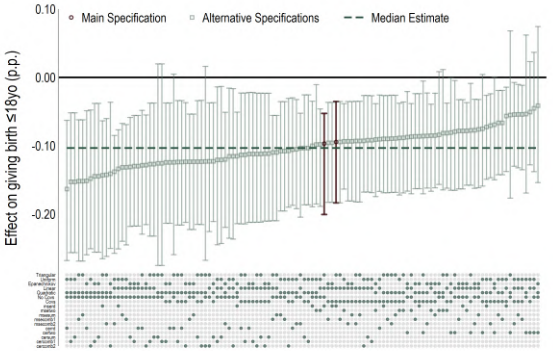
Table F.3: LATE Effects, by Age - Estimates Using NPV of Total Amount Collected (USD 10,000) Treated as Endogenous Variable

	≤ 18 years old			≤ 23 years old			≤ 30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.066**	0.014	0.027	-0.030	0.002	0.086***	0.107	0.047
	(0.028)	(0.024)	(0.027)	(0.030)	(0.017)	(0.032)	(0.111)	(0.061)
Robust p-value	0.016	0.539	0.240	0.292	0.885	0.006	0.307	0.436
Avg. Treatment	0.90	0.87	0.91	0.90	0.87	0.91	0.39	0.43
Effect Size (%)	-26.37%	1.62%	11.02%	-4.93%	2.39%	11.97%	5.52%	2.55%
Bwd.	[0.053;0.053]	[0.036;0.036]	[0.034;0.034]	[0.063;0.063]	[0.044;0.044]	[0.032;0.032]	[0.037;0.037]	[0.085;0.085]
Robust p-value	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
				b. Dep. Var.: Number of Events				
Ever Treated	-0.073**	0.222**	0.364	-0.117*		4.571***	-0.135	7.895
	(0.032)	(0.100)	(0.303)	(0.065)		(1.514)	(0.305)	(5.837)
Robust p-value	0.018	0.018	0.161	0.061		0.002	0.566	0.153
Avg. Treatment	0.90	0.87	0.91	0.89		0.91	0.39	0.43
Effect Size (%)	-25.85%	7.69%	13.05%	-12.63%		18.84%	-3.76%	6.46%
Bwd.	[0.059;0.059]	[0.041;0.041]	[0.032;0.032]	[0.042;0.042]		[0.031;0.031]	[0.036;0.036]	[0.092;0.092]
Observations	27,866	36,342	24,813	19,043		24,148	2,994	10,838
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

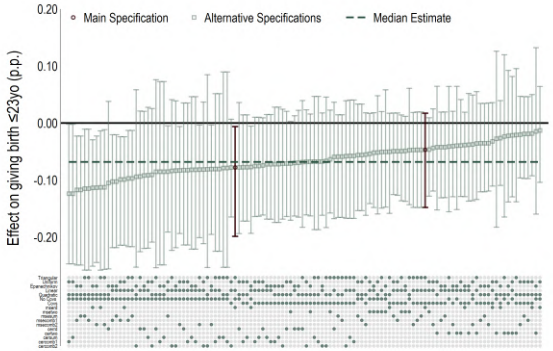
Notes:

Table F.4: Specification Curves - LATE Effects - Binary Variables

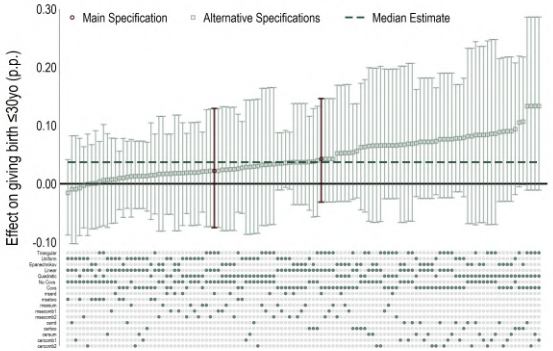
a. 18 years old

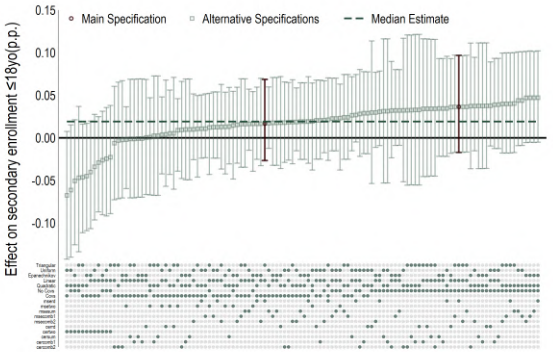


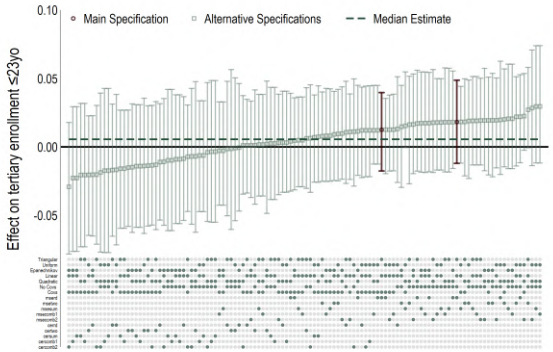
b. 23 years old

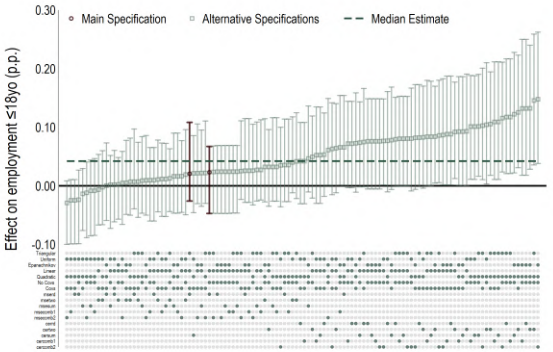


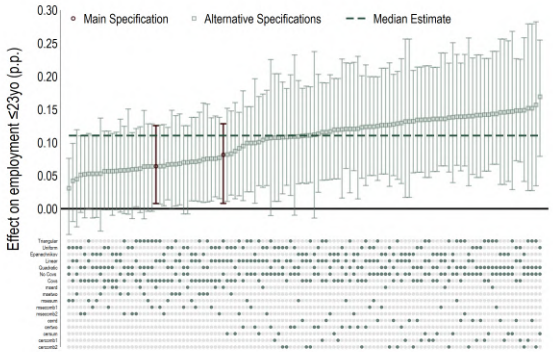
c. 30 years old

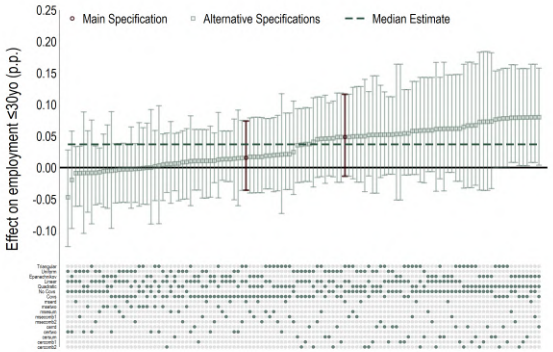








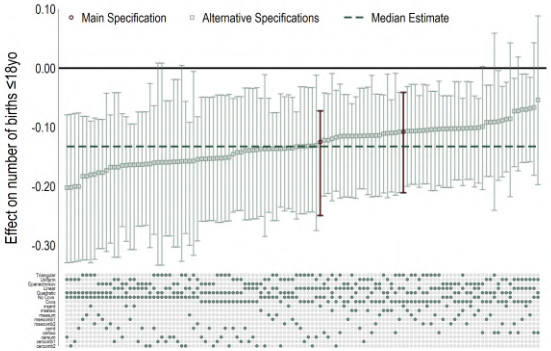




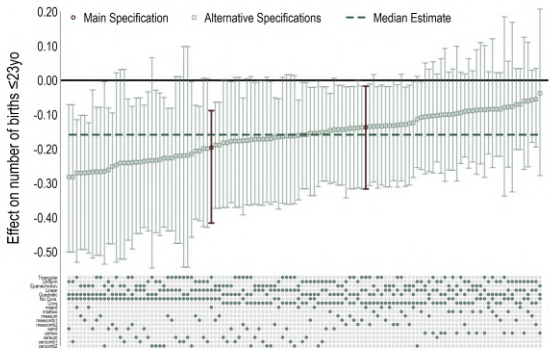
Notes:

Table F.5: Specification Curves - LATE Effects - Continuous Variables

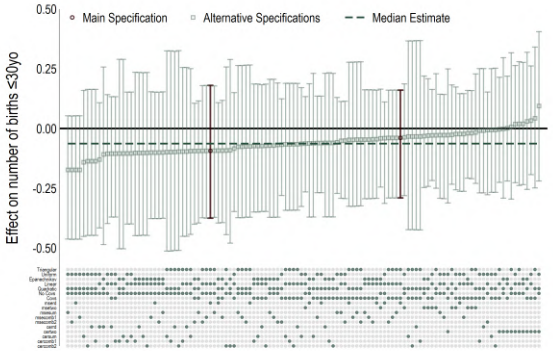
a. 18 years old

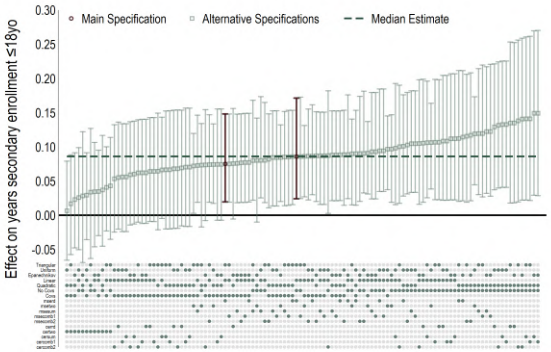


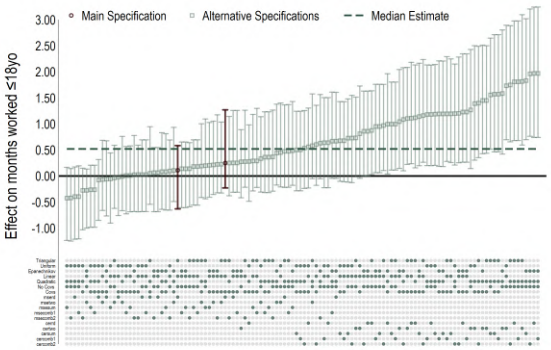
b. 23 years old

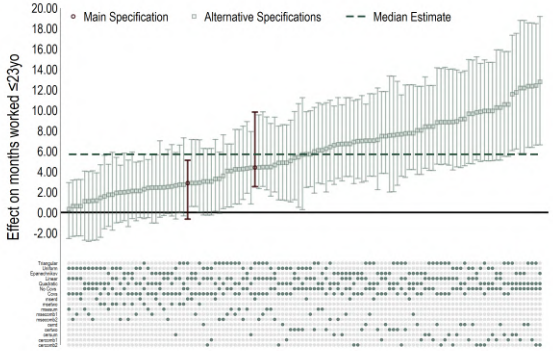


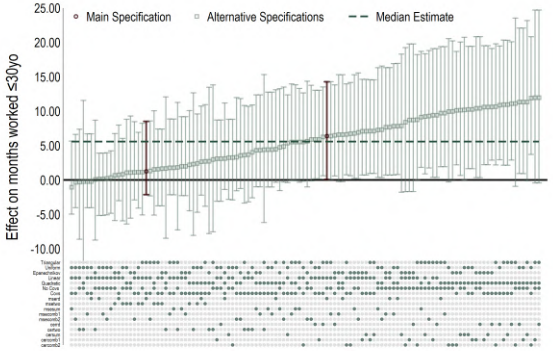
c. 30 years old











Notes:

Table F.6: LATE Effects, by Age - Estimates With Covariates - Male - Balanced Sample

	18 years old		24 years old		30 years old
	Education	Labor	Education	Labor	Labor
	a. Dep. Var.: Dummy Variable				
Ever Treated	0.023	0.026	-0.026*	0.013	0.029
	(0.037)	(0.034)	(0.015)	(0.037)	(0.031)
Robust p-value	0.416	0.320	0.062	0.697	0.494
Effect Size (%)	5.67%	10.24%	-64.80%	1.79%	3.51%
Bwd.	[0.077;0.077]	[0.126;0.126]	[0.067;0.067]	[0.120;0.120]	[0.120;0.120]
Observations	6,108	5,990	5,431	5,814	5,806
	b. Dep. Var.: Number of Events				
Ever Treated	0.001	0.145		0.089	1.445
	(0.061)	(0.342)		(1.585)	(3.615)
Robust p-value	0.773	0.381		0.780	0.653
Effect Size (%)	0.13%	4.97%		0.35%	2.23%
Bwd.	[0.075;0.075]	[0.111;0.111]		[0.130;0.130]	[0.125;0.125]
Observations	5,977	5,580		6,084	5,947
Parameter Selection:					
Pol. Degree	1	1	1	1	1
Bwd. Method	mserd	mserd	mserd	mserd	mserd
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.7: LATE Effects, by Age - Estimates With Covariates - Female - Balanced Sample

	18 years old			24 years old			30 years old	
	Fertility	Education	Labor	Fertility	Education	Labor	Fertility	Labor
				a. Dep. Var.: Dummy Variable				
Ever Treated	-0.117**	0.013	0.017	-0.029	-0.027	0.079**	0.043	0.012
	(0.046)	(0.050)	(0.028)	(0.038)	(0.030)	(0.038)	(0.045)	(0.037)
Robust p-value	0.011	0.459	0.605	0.377	0.281	0.013	0.288	0.683
Effect Size (%)	-72.92%	2.67%	20.31%	-5.17%	-37.68%	15.28%	5.62%	1.66%
Bwd.	[0.031;0.031]	[0.046;0.046]	[0.059;0.059]	[0.069;0.069]	[0.038;0.038]	[0.099;0.099]	[0.040;0.040]	[0.080;0.080]
Observations	2,599	3,853	3,964	5,915	3,126	6,268	3,343	5,252
				b. Dep. Var.: Number of Events				
Ever Treated	-0.123**	0.079	0.121	-0.136		2.391*	-0.039	5.249
	(0.049)	(0.096)	(0.240)	(0.100)		(1.428)	(0.114)	(3.659)
Robust p-value	0.013	0.301	0.661	0.236		0.067	0.635	0.104
Effect Size (%)	-73.26%	10.26%	13.70%	-16.92%		19.43%	-2.78%	12.51%
Bwd.	[0.031;0.031]	[0.038;0.038]	[0.057;0.057]	[0.036;0.036]		[0.070;0.070]	[0.046;0.046]	[0.071;0.071]
Observations	2,586	3,106	3,887	2,985		4,709	3,760	4,789
Parameter Selection:								
Pol. Degree	1	1	1	1	1	1	1	1
MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.8: LATE Effects on Education, by Grade

	Secondary Education: Grade					
	Middle School			High School		
	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}	6^{th}
			a. Dep. Var.: Dummy Variable			
Ever Treated	0.008	0.022	0.037	0.093***	0.059**	0.047**
	(0.036)	(0.026)	(0.027)	(0.030)	(0.026)	(0.021)
Robust p-value	0.952	0.304	0.108	0.003	0.024	0.030
Effect Size (%)	1.83%	5.29%	9.63%	32.07%	28.83%	32.36%
Bwd.	[0.045;0.045]	[0.083;0.083]	[0.071;0.071]	[0.045;0.045]	[0.046;0.046]	[0.048;0.048]
Observations	27,191	58,416	57,025	38,176	40,356	43,208
Parameter Selection:						
Pol. Degree	1	1	1	1	1	1
Bwd. Method	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.9: LATE Effects on Education, by Grade - Additional Results

	Total Grades Enrolled		Grade Enrolled	
	Middle School	High School	Minimum	Maximum
	a. Dep. Var.: Dummy Variable			
Ever Treated	0.018	0.187***	0.224**	0.355**
	(0.061)	(0.061)	(0.095)	(0.147)
Robust p-value	0.868	0.002	0.019	0.018
Effect Size (%)	1.92%	30.39%	16.22%	9.49%
Bwd.	[0.044;0.044]	[0.043;0.043]	[0.045;0.045]	[0.045;0.045]
Observations	39,016	38,625	39,957	25,712
Parameter Selection:				
Pol. Degree	1	1	1	1
Bwd. Method	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.10: LATE Effects on Education, by Grade - Male

	Secondary Education: Grade					
	Middle School			High School		
	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}	6^{th}
	a. Dep. Var.: Dummy Variable					
Ever Treated	0.023	0.020	0.034	0.092***	0.030	0.021
	(0.049)	(0.045)	(0.035)	(0.028)	(0.027)	(0.018)
Robust p-value	0.711	0.784	0.368	0.001	0.290	0.251
Effect Size (%)	5.66%	6.14%	11.21%	42.35%	21.23%	22.70%
Bwd.	[0.040;0.040]	[0.048;0.048]	[0.074;0.074]	[0.082;0.082]	[0.064;0.064]	[0.088;0.088]
Observations	13,078	17,663	30,007	35,062	28,873	39,056
Parameter Selection:						
Pol. Degree	1	1	1	1	1	1
Bwd. Method	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.11: LATE Effects on Education, by Grade - Additional Results - Male

	Total Grades Enrolled		Grade Enrolled	
	Middle School	High School	Minimum	Maximum
	a. Dep. Var.: Dummy Variable			
Ever Treated	0.047	0.139**	0.136	0.324
	(0.077)	(0.056)	(0.118)	(0.203)
Robust p-value	0.569	0.014	0.328	0.157
Effect Size (%)	5.58%	31.68%	11.87%	9.63%
Bwd.	[0.056;0.056]	[0.080;0.080]	[0.052;0.052]	[0.059;0.059]
Observations	25,737	35,708	23,350	15,315
Parameter Selection:				
Pol. Degree	1	1	1	1
Bwd. Method	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.12: LATE Effects on Education, by Grade - Female

	Secondary Education: Grade					
	Middle School			High School		
	1^{st}	2^{nd}	3^{rd}	4^{th}	5^{th}	6^{th}
	a. Dep. Var.: Dummy Variable					
Ever Treated	0.012	0.024	0.071	0.082*	0.101**	0.064
	(0.046)	(0.053)	(0.058)	(0.053)	(0.045)	(0.038)
Robust p-value	0.653	0.513	0.163	0.084	0.012	0.119
Effect Size (%)	2.17%	4.92%	15.73%	21.56%	36.78%	32.29%
Bwd.	[0.076;0.076]	[0.061;0.061]	[0.050;0.050]	[0.049;0.049]	[0.048;0.048]	[0.048;0.048]
Observations	22,016	20,975	18,925	20,372	21,314	21,801
Parameter Selection:						
Pol. Degree	1	1	1	1	1	1
Bwd. Method	MSERD	MSERD	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

Notes:

Table F.13: LATE Effects on Education, by Grade - Additional Results - Female

	Total Grades Enrolled		Grade Enrolled	
	Middle School	High School	Minimum	Maximum
	a. Dep. Var.: Dummy Variable			
Ever Treated	0.012	0.226**	0.303**	0.366**
	(0.096)	(0.101)	(0.144)	(0.196)
Robust p-value	0.998	0.021	0.019	0.047
Effect Size (%)	1.14%	27.75%	18.63%	9.06%
Bwd.	[0.045;0.045]	[0.047;0.047]	[0.052;0.052]	[0.049;0.049]
Observations	20,255	21,180	23,860	15,919
Parameter Selection:				
Pol. Degree	1	1	1	1
Bwd. Method	MSERD	MSERD	MSERD	MSERD
Kernel	Triangular	Triangular	Triangular	Triangular

Notes:

G Further Results - Dynamic Effects

Table G.1: Full LATE Effects, by Age - Fertility

	Women			
	Estimate	Std. Error	Robust p-value	Eff. Size (%)
At or before age				
14	0.00	(0.007)	0.495	50.36%
15	-0.01	(0.014)	0.509	-30.14%
16	-0.04	(0.026)	0.150	-47.77%
17	-0.05*	(0.027)	0.066	-35.60%
18	-0.09**	(0.037)	0.011	-44.41%
19	-0.03	(0.040)	0.405	-10.80%
20	-0.04	(0.045)	0.247	-11.84%
21	-0.05	(0.043)	0.220	-10.65%
22	-0.02	(0.039)	0.534	-3.69%
23	-0.05	(0.042)	0.193	-8.52%
24	-0.06	(0.040)	0.148	-9.49%
25	-0.04	(0.041)	0.284	-6.72%
26	0.00	(0.041)	0.853	0.46%
27	0.03	(0.030)	0.404	4.12%
28	0.05	(0.040)	0.206	6.75%
29	0.06	(0.042)	0.119	7.62%
30	0.04	(0.045)	0.288	5.62%

Notes:

Table G.2: Full LATE Effects, by Age - Number of Births

	Women			
	Estimate	Std. Error	Robust p-value	Eff. Size (%)
At or before age				
14	0.00	(0.007)	0.580	37.39%
15	-0.01	(0.015)	0.418	-37.14%
16	-0.04*	(0.026)	0.096	-53.38%
17	-0.08**	(0.032)	0.015	-51.78%
18	-0.12***	(0.045)	0.007	-51.24%
19	-0.06	(0.051)	0.251	-16.45%
20	-0.09	(0.062)	0.105	-20.49%
21	-0.08	(0.065)	0.166	-14.38%
22	-0.10	(0.071)	0.125	-14.05%
23	-0.14*	(0.078)	0.067	-16.86%
24	-0.17**	(0.079)	0.031	-18.60%
25	-0.14	(0.087)	0.120	-13.27%
26	-0.02	(0.095)	0.799	-2.21%
27	0.03	(0.086)	0.952	2.37%
28	0.02	(0.099)	0.878	1.72%
29	0.00	(0.109)	0.980	0.10%
30	-0.04	(0.114)	0.635	-2.78%

Notes:

Table G.3: Full LATE Effects, by Age - Secondary Education Enrollment

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
12	0.04	(0.042)	0.480	9.48%	0.05	(0.045)	0.240	9.50%	-0.01	0.878
13	0.10**	(0.044)	0.043	15.60%	-0.04	(0.041)	0.521	-5.49%	0.14	0.019
14	0.05	(0.042)	0.305	6.59%	-0.02	(0.038)	0.523	-2.21%	0.07	0.212
15	0.03	(0.034)	0.630	3.27%	-0.05	(0.036)	0.155	-5.68%	0.08	0.121
16	0.03	(0.035)	0.456	3.69%	0.01	(0.037)	0.710	1.65%	0.02	0.750
17	0.04	(0.035)	0.348	4.56%	-0.01	(0.044)	0.842	-1.02%	0.05	0.413
18	0.04	(0.032)	0.248	4.92%	-0.00	(0.035)	0.874	-0.56%	0.04	0.365
19	0.04	(0.032)	0.276	5.46%	0.00	(0.034)	0.817	0.59%	0.04	0.432
20	0.03	(0.034)	0.409	4.34%	0.00	(0.034)	0.825	0.37%	0.03	0.543
21	0.03	(0.029)	0.244	4.58%	0.02	(0.035)	0.522	2.41%	0.01	0.746
22	0.03	(0.036)	0.575	3.52%	0.02	(0.036)	0.579	2.06%	0.01	0.862
23	-0.00	(0.031)	0.964	-0.13%	0.03	(0.038)	0.386	3.67%	-0.03	0.550

Notes:

Table G.4: Full LATE Effects, by Age - Years Enrolled in Secondary Education

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
12	0.03	(0.042)	0.636	6.34%	0.06	(0.042)	0.124	11.37%	-0.03	0.594
13	0.08	(0.073)	0.344	8.76%	0.02	(0.068)	0.665	2.00%	0.06	0.562
14	0.04	(0.098)	0.809	3.22%	0.02	(0.103)	0.953	1.23%	0.03	0.858
15	0.04	(0.119)	0.967	2.17%	0.01	(0.126)	0.826	0.62%	0.03	0.877
16	0.20	(0.138)	0.212	9.45%	0.08	(0.156)	0.787	3.10%	0.13	0.541
17	0.32	(0.176)	0.113	13.66%	0.12	(0.190)	0.735	4.55%	0.20	0.444
18	0.33**	(0.146)	0.033	13.72%	-0.01	(0.172)	0.782	-0.41%	0.34	0.130
19	0.36**	(0.154)	0.026	14.40%	-0.00	(0.180)	0.777	-0.14%	0.37	0.120
20	0.41**	(0.165)	0.019	15.94%	0.01	(0.187)	0.859	0.42%	0.40	0.107
21	0.37**	(0.171)	0.035	14.16%	0.06	(0.191)	0.916	1.82%	0.31	0.220
22	0.34*	(0.169)	0.058	12.90%	0.17	(0.195)	0.486	5.60%	0.16	0.525
23	0.22	(0.168)	0.233	8.69%	0.16	(0.197)	0.489	5.35%	0.06	0.819

Notes:

Table G.5: Full LATE Effects, by Age - Tertiary Education Enrollment

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
17	0.00	(0.006)	0.445	65.56%	-0.01	(0.013)	0.226	-95.53%	0.02	0.204
18	0.00	(0.014)	0.802	1.61%	0.03	(0.025)	0.333	34.08%	-0.03	0.371
19	-0.01	(0.017)	0.564	-19.81%	0.02	(0.031)	0.657	16.10%	-0.03	0.458
20	-0.01	(0.018)	0.815	-12.32%	-0.01	(0.031)	0.566	-9.22%	0.00	0.926
21	-0.01	(0.018)	0.693	-22.95%	0.04	(0.030)	0.395	32.29%	-0.05	0.142
22	-0.02	(0.016)	0.244	-34.18%	0.01	(0.030)	0.850	11.15%	-0.03	0.313
23	-0.01	(0.015)	0.499	-14.64%	0.02	(0.032)	0.644	18.57%	-0.03	0.387

Notes:

Table G.6: Full LATE Effects, by Age - Labor Market Participation

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
14	-0.01*	(0.007)	0.089	-264.62%	0.00	(0.001)	0.673	551.98%	-0.01	0.128
15	0.00	(0.012)	0.835	6.79%	0.00	(0.011)	0.968	31.33%	-0.00	0.973
16	-0.01	(0.020)	0.932	-21.85%	0.02	(0.018)	0.315	124.69%	-0.03	0.301
17	-0.04	(0.041)	0.253	-20.55%	0.08*	(0.043)	0.061	70.16%	-0.13	0.029
18	-0.03	(0.046)	0.568	-8.54%	0.13**	(0.055)	0.019	56.38%	-0.16	0.027
19	0.00	(0.050)	0.794	0.47%	0.14**	(0.063)	0.031	42.26%	-0.13	0.094
20	0.01	(0.053)	0.635	2.48%	0.09	(0.067)	0.191	21.10%	-0.07	0.392
21	0.05	(0.057)	0.251	9.25%	0.09	(0.066)	0.170	19.43%	-0.04	0.657
22	0.05	(0.053)	0.283	7.75%	0.11*	(0.061)	0.065	20.77%	-0.06	0.443
23	0.01	(0.047)	0.549	2.25%	0.07	(0.058)	0.142	12.43%	-0.06	0.443
24	0.03	(0.045)	0.551	4.09%	0.07	(0.057)	0.128	10.89%	-0.04	0.581
25	0.03	(0.039)	0.531	4.49%	0.06	(0.055)	0.145	9.44%	-0.03	0.653
26	0.02	(0.046)	0.679	2.70%	0.08	(0.055)	0.130	11.85%	-0.06	0.410
27	0.03	(0.040)	0.416	4.60%	0.05	(0.044)	0.275	8.23%	-0.02	0.697
28	0.01	(0.035)	0.690	2.04%	0.03	(0.036)	0.667	4.35%	-0.02	0.761
29	0.02	(0.031)	0.538	2.55%	0.03	(0.052)	0.462	5.04%	-0.02	0.781

Notes:

Table G.7: Full LATE Effects, by Age - Months Worked

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
14	-0.09**	(0.057)	0.038	-241.33%	0.01	(0.016)	0.545	336.73%	-0.11	0.066
15	-0.12	(0.117)	0.363	-72.07%	0.10	(0.091)	0.325	220.06%	-0.21	0.148
16	-0.07	(0.254)	0.855	-15.82%	0.33	(0.202)	0.126	165.45%	-0.39	0.224
17	-0.40	(0.462)	0.470	-15.74%	0.84**	(0.432)	0.041	62.82%	-1.24	0.049
18	-0.70	(0.747)	0.447	-12.57%	1.62**	(0.808)	0.041	49.73%	-2.31	0.036
19	-0.85	(1.125)	0.554	-9.23%	2.46*	(1.254)	0.053	42.92%	-3.31	0.049
20	-0.23	(1.651)	0.910	-1.73%	3.77**	(1.782)	0.032	42.92%	-4.01	0.099
21	0.75	(2.238)	0.551	4.14%	5.70***	(2.236)	0.009	47.07%	-4.95	0.118
22	0.68	(2.497)	0.627	3.02%	8.17***	(2.543)	0.001	51.80%	-7.49	0.036
23	-1.30	(2.755)	0.845	-4.72%	8.20***	(2.874)	0.002	41.68%	-9.50	0.017
24	-3.22	(2.973)	0.497	-9.97%	9.62***	(3.258)	0.002	40.79%	-12.83	0.004
25	-2.28	(3.248)	0.695	-6.21%	7.27**	(3.635)	0.027	26.58%	-9.55	0.050
26	-3.21	(3.628)	0.602	-7.90%	8.85**	(4.035)	0.021	28.74%	-12.06	0.026
27	-0.99	(4.264)	0.910	-2.27%	8.28**	(4.284)	0.046	24.54%	-9.27	0.125
28	-3.15	(3.825)	0.436	-6.85%	3.15	(3.490)	0.313	8.71%	-6.30	0.224
29	-4.70	(3.665)	0.263	-9.77%	5.85	(5.237)	0.215	15.48%	-10.55	0.099

Notes:

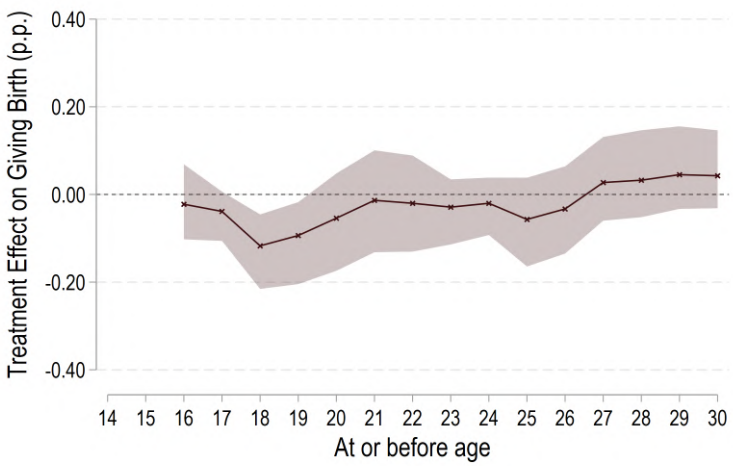
Table G.8: Full LATE Effects, by Age - Cumulative Earnings

	Men				Women				Difference	
	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Estimate	Std. Error	Robust p-value	Eff. Size (%)	Difference	p-value
At or before age										
14	-6.01	(7.337)	0.372	-589.17%	0.00	(0.000)	0.287	.%	-6.01	0.413
15	-34.79*	(26.397)	0.084	-213.85%	7.45	(5.129)	0.229	681.17%	-42.24	0.116
16	-29.73	(54.712)	0.672	-44.58%	44.77	(34.243)	0.233	327.51%	-74.50	0.248
17	44.15	(116.573)	0.658	24.90%	131.92	(78.517)	0.105	206.29%	-87.77	0.532
18	-269.27	(266.888)	0.385	-21.72%	279.92	(209.305)	0.171	51.35%	-549.20	0.105
19	-426.76	(488.612)	0.519	-14.18%	449.92	(449.912)	0.290	30.53%	-876.68	0.187
20	-714.34	(852.110)	0.487	-13.20%	703.24	(757.479)	0.387	24.89%	-1,417.58	0.214
21	-164.29	(1,366.722)	0.922	-1.94%	1,466.08	(1,176.824)	0.209	31.98%	-1,630.36	0.366
22	290.61	(1,956.242)	0.712	2.43%	2,814.94*	(1,549.091)	0.067	42.03%	-2,524.33	0.312
23	-566.84	(2,254.611)	0.976	-3.60%	4,525.43**	(1,872.898)	0.013	49.11%	-5,092.28	0.082
24	-2,795.76	(2,705.134)	0.408	-14.07%	3,982.05*	(2,222.201)	0.063	33.14%	-6,777.81	0.053
25	-5,227.96	(3,215.875)	0.152	-21.77%	5,475.80**	(2,701.818)	0.037	36.68%	-10703.75	0.011
26	-4,194.83	(3,507.755)	0.304	-14.73%	3,450.08	(3,254.366)	0.260	19.22%	-7,644.90	0.110
27	-6,492.47	(4,201.744)	0.167	-20.10%	4,708.14	(3,765.284)	0.191	22.63%	-11200.61	0.047
28	-5,552.64	(5,254.163)	0.415	-15.64%	3,065.95	(3,921.318)	0.399	13.25%	-8,618.59	0.189
29	-5,254.05	(4,384.429)	0.233	-13.86%	828.51	(3,469.457)	0.838	3.29%	-6,082.56	0.277
30	-4,724.94	(4,758.804)	0.516	-11.92%	-580.26	(5,426.674)	0.985	-2.19%	-4,144.68	0.566

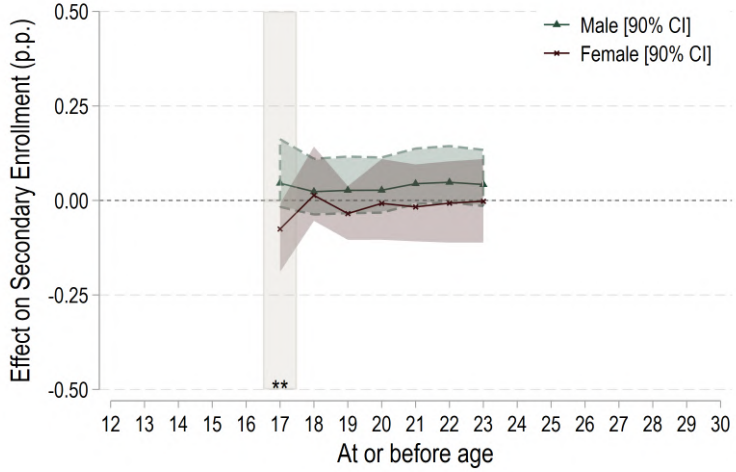
Notes:

Figure G.1: Dynamic Effects, by outcome and gender - Binary Variable - Balanced Sample

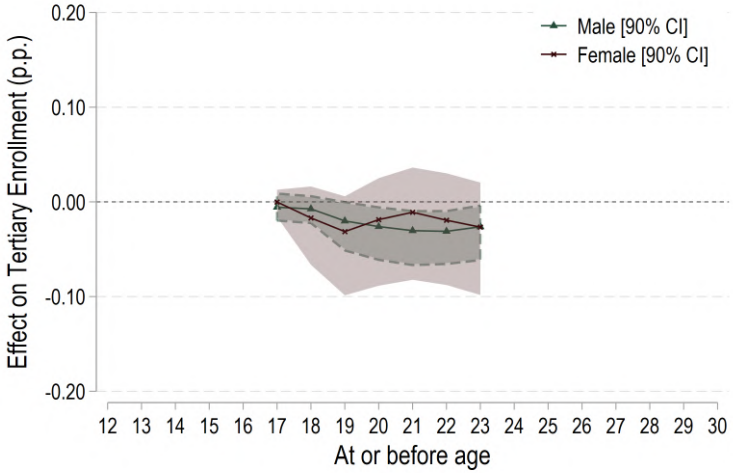
a. Fertility



b. Secondary Education



c. Tertiary Education



d. Labor Market Participation

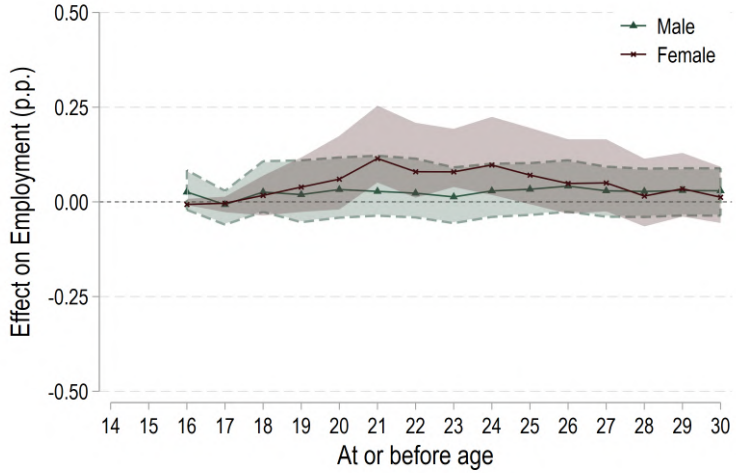
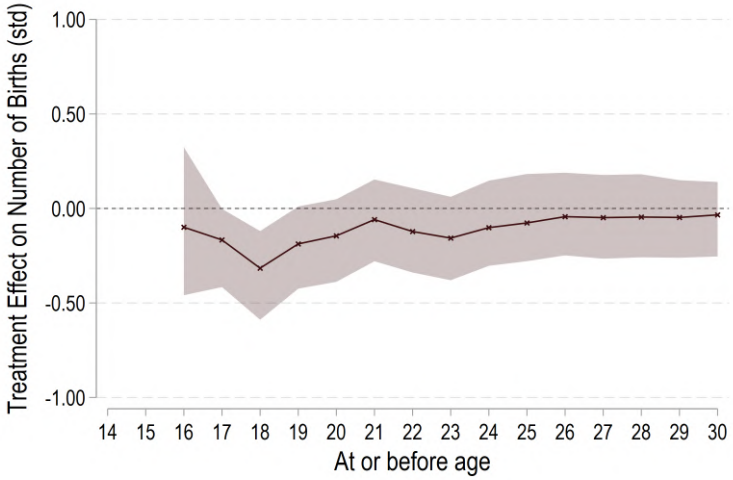
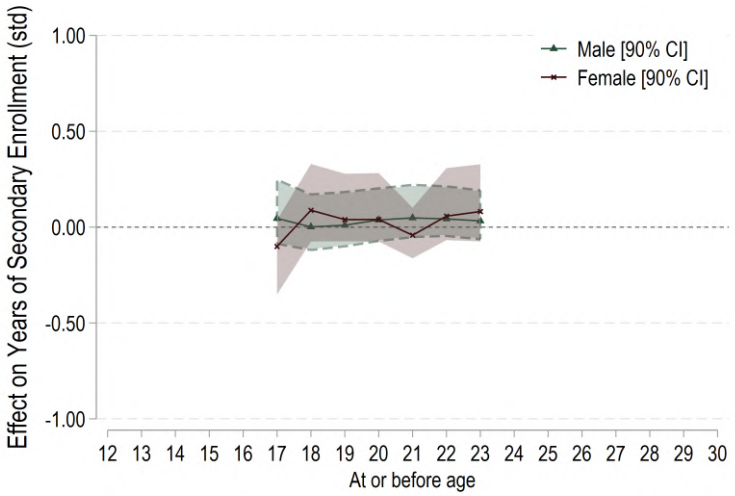


Figure G.2: Dynamic Effects, by outcome and gender - Continuous Variable - Balanced Sample

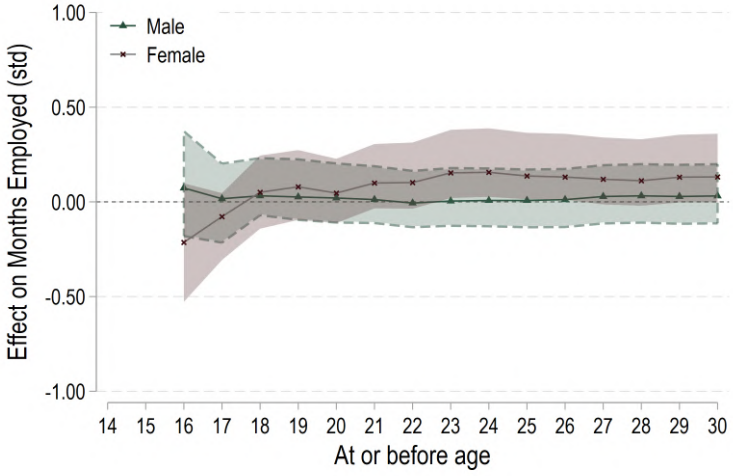
a. Fertility



b. Secondary Education



c. Cumulative Months Worked



c. Cumulative Earnings

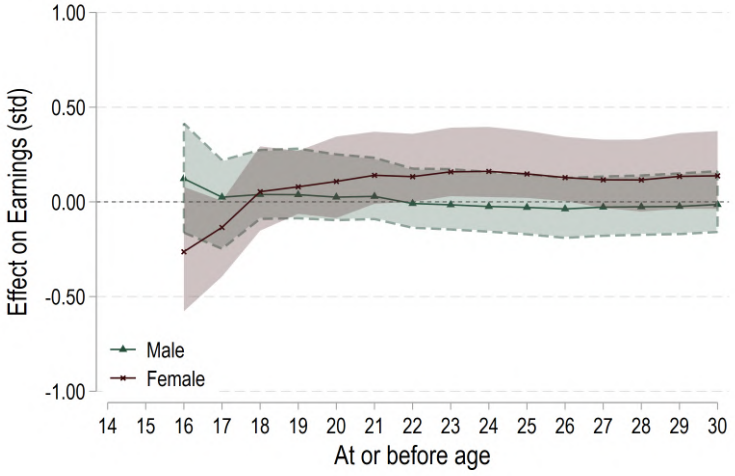
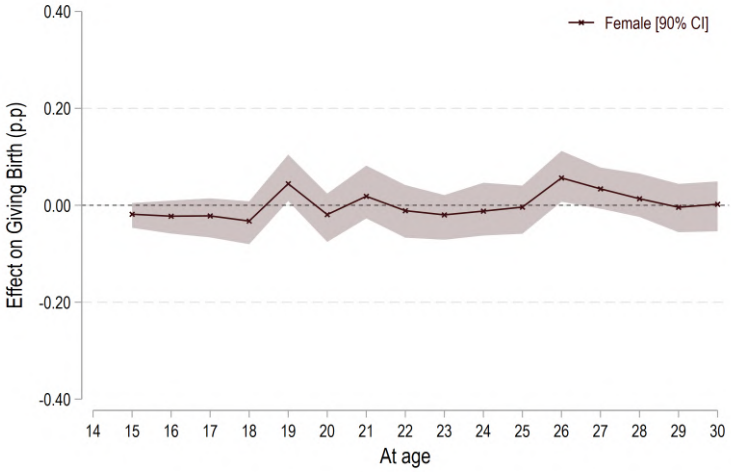
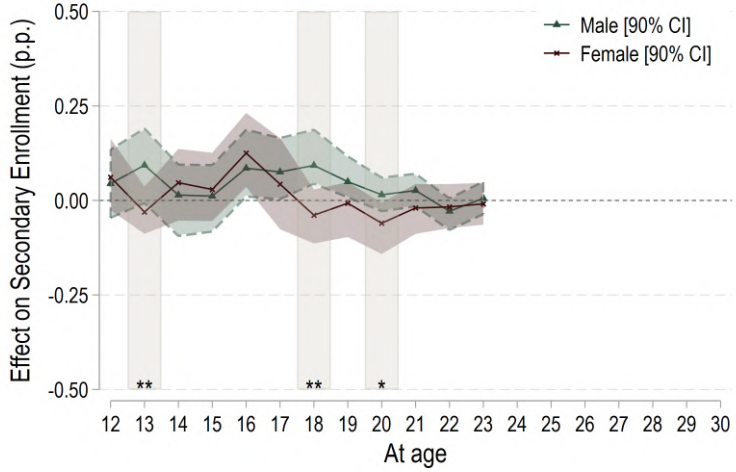


Figure G.3: Dynamic Effects, by outcome and gender - Estimate at age

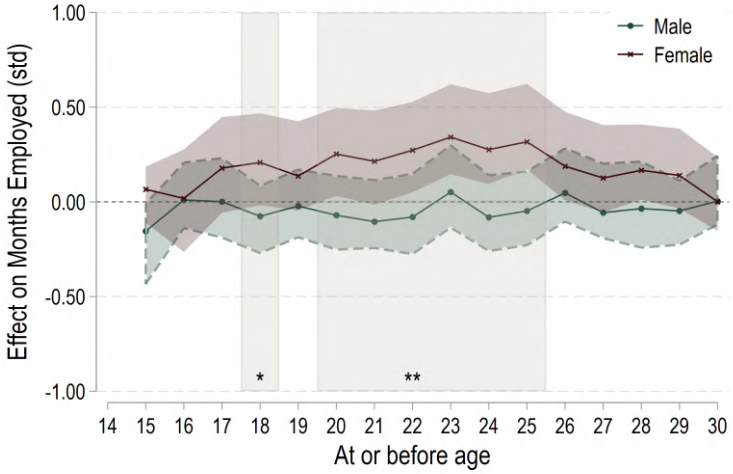
a. Fertility



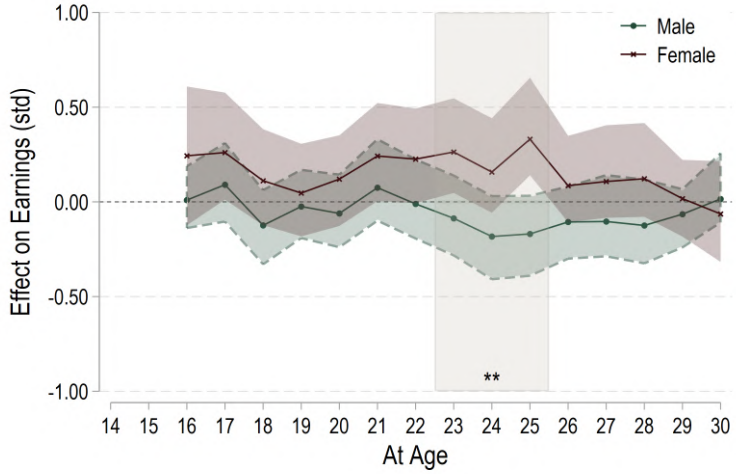
b. Secondary Education



c. Labor Market Participation



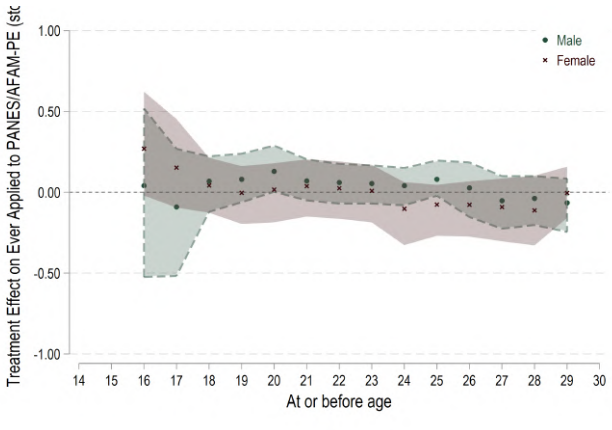
d. Earnings



H Further Results on Welfare Application and Participation

Figure H.1: Dynamic Effects, by outcome and gender - Welfare Participation

a. Applied



b. Accepted

