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

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Abstract

This paper presents novel evidence about the effects of a permanent, large-scale, and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*. Using fifteen years of administrative records linked at the individual level, I describe how the program affected individuals' transition to adulthood in three key dimensions: education, fertility, and labor market. The main findings can be grouped into three. First, the program leads to a 2.2-years delay in women's age at first birth, explained mostly by a fall in teenage or early-life births. Second, the program leads women to a 1.8-years anticipated entry to the formal labor market, which is not explained by a reduction in higher-education participation. The effects on labor market outcomes across all margins are null for men. Third, changes in transitions to adulthood have lasting consequences. By the time they are last observed, treated women have more accumulated experience and earnings in the formal labor market and have fewer children than women who did not participate in the program. I provide suggestive evidence that avoiding early-life births is a necessary condition for observing positive effects on labor market outcomes, whereas education seems to act as a multiplier. Overall, this evidence suggests that cash transfers may be viable policies to improve women's future life trajectories and contribute to reducing the labor market gender gap.

1 Introduction

Worldwide, governments spend billions of dollars on social safety net (SSN) policies aimed at vulnerable households with children, with cash transfers being one of its key components. Because they represent sizable investments, affect multiple generations, and trigger ethical debates about who are the deserving beneficiaries, cash transfers are typically a controversial topic, especially when the focus is on the children that grew up as beneficiaries. The policy debate circles around two broad views. On the one hand, some argue that cash transfers could be beneficial for children's life trajectories as they reduce child poverty, improve economic security, and connect vulnerable individuals to the labor force. On the other hand, others argue that they could be inefficient or even hurtful for long-run upward mobility, especially when provided without work requirements for the adults.[1]

The academic literature has contributed to this debate with abundant but still incomplete empirical evidence. This literature can be organized into two snapshots. The first snapshot, focuses on outcomes measured at ages 0-5, and illustrates that cash transfers generally improve children's early life health, nutrition, and education outcomes across different settings (see e.g., Almond et al. 2011; Hoynes and Schanzenbach 2018 or Bastagli et al. 2016; Fiszbein et al. 2009). The second, focuses on these children's young adulthood outcomes (i.e., around the age of 30) and shows promising but still mixed evidence about effects on labor market outcomes (Barr et al., 2022; Araujo and Macours, 2021; Price and Song, 2018) and longevity (Bailey et al., 2024; Aizer et al., 2016). However, there is still a missing piece: the literature has yet to thoroughly describe how cash transfers affect individuals life trajectories in between these two snapshots, a period when a series of critical decisions with long-lasting consequences are made.

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 of people who benefited from it during their childhood. I focus on three dimensions: education, fertility, and labor market decisions. 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 which overlaps with what sociologists and psychologists usually refer to as "transition to adulthood" or "emerging adulthood" (Settersten Jr et al., 2008; Arnett, 2000).

Studying how anti-poverty policies affect individuals' transition to adulthood is relevant both from an academic and a policy perspective. From an academic perspective,

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[1]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.

adulthood is a distinct and socially recognized stage of life defined by, among other markers, the culmination of education cycles, labor market participation, residential independence, marriage, and fertility decisions. Until recently, "adolescence" was the term used to describe the life stage between childhood and adulthood. However, in the last fifty years, the idea of a uniform adolescence is becoming socially and economically inexact (Settersten Jr et al., 2008). As sizable household income shocks, cash transfers could strongly affect this transition, with long run consequences. From a policy perspective, understanding 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.

PANES/AFAM-PE consists of a cash transfer that represents between 30% and 50% of the self-reported pre-program income. It was implemented in 2005 and remains in place until today as the most generous anti-poverty program in the country's history, accounting for 0.4% of the Uruguayan GDP and reaching more than 10% of households (Manacorda et al., 2011). *PANES/AFAM-PE* provides short-term financial assistance to socio-economically disadvantaged households but it also aims to encourage medium- and long-term human capital accumulation for a more permanent transition out of poverty. Eligibility to participate in the program is based on a poverty score. Participants who are above a certain (unknown) threshold are deemed eligible, whereas participants who are below the threshold are ineligible. Upon acceptance, households are required to satisfy some conditions, such as school enrollment and attendance and health check-ups but these were not enforced until eight years after the program was implemented. Uruguay constitutes an interesting case of study since it is a middle-high income country with a relatively high human development index, but with some lagging indicators, especially in terms of fertility and secondary education completion. This is presumably a more general context compared to other small-scale, context-specific interventions such as the more traditional Conditional Cash Transfer programs (CCTs), many of which which are carried out under very specific conditions (e.g., temporary interventions, implemented in extremely poor rural areas, conducted sometimes by local or international NGOs).

To identify the causal effects of the program, I use a Regression Discontinuity Design (RDD) that exploits the sharp change in the probability of participating in *PANES/AFAM-PE* just at the eligibility threshold. Intuitively, this approach compares 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 (Thistlethwaite and Campbell, 1960; Hahn et al., 2001; Imbens and Lemieux, 2008; Calonico et al., 2019). The permanent nature of the program implies that households could have filed multiple applications since the program was established, and therefore can have multiple values in their poverty scores and application resolutions. To address

this, I follow the approach in Jepsen et al. (2016) and use eligibility based on the score obtained in the *first* household application as an instrument for ever being treated during the childhood or teenage years.

To conduct the empirical analysis, I assembled an exhaustive individual-level, multidimensional, and longitudinal dataset that covers the universe of program applicants. This is key to overcome one of the main empirical challenges in studying how anti-poverty policies shape individuals transitions to adulthood, which is the need for multidimensional and longitudinal data. This dataset is built on a series of administrative records that can be merged at the individual level through an individual identification number. This allows me to track individuals' application and participation in *PANES/AFAM-PE*, as well as education, fertility, and labor market outcomes for about fifteen years of their lives. The main sample of analysis consists of more than 279,000 individuals who were younger than eighteen years old when their parents first applied to the program, and at least nineteen years old in December, 2021, the moment when I last observe them.

The main empirical findings can be grouped into two. In the first part of the paper, I summarize the effects of *PANES/AFAM-PE* on individuals' transition to adulthood on three variables: age of first child, age of first employment spell, and participation in the higher-education system. Regarding fertility decisions, the program significantly affects individuals' age of their first child. My preferred estimate - i.e., from the fuzzy RDD specification (τ_{frd}) - indicates that women postpone the timing of their first birth by 2.2 years. This effect is statistically significant at traditional levels (p-val<0.001), it remains significant when adjusted by multiple hypotheses testing, and can be compared to an average of 19.9 years old in the reference group ($\bar{Y}^c$). In this case, the reduced form effect - i.e., without re-scaling by the change in the probability of treatment (τ_{srd}) - is -0.512. For men, the effect goes in the opposite direction and indicates an anticipation of 1.1 years ($\tau_{srd} = -0.339$, $\bar{Y}^c = 22.7$). While statistically significant (p-val=0.018), this estimate is not robust to multiple hypothesis testing adjustments and should be taken with a grain of salt due to data limitations concerning the availability of father's information on birth certificates.[2] Regarding labor market participation, women anticipate their entry to the labor market by about 1.8 years (with $\tau_{frd} = -0.421$, and $\bar{Y}^c = 20.8$). This effect is statistcaily significant both when considered individually and when adjusted for multiple hypothesis testing. For men, the effects are null and precisely estimated. Finally, women's anticipated entry to the labor market is not explained by a reduction in higher education enrollment. If anything, the program induced some women to enroll in the tertiary education system. All together, these findings suggest that women's anticipated labor market participation or higher-education enrollment comes from time that otherwise

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[2]Fathers' information on birth records only started to be systematically collected in 2010 with the introduction of electronic certificates, and even in the best years, only 50% of the father's individual identification number is reported.

would have been dedicated to working at home as the primary childcare provider.

In the second part of the paper I exploit the richness of the data and provide additional evidence on when, for how long, and why do the effects take place. Regarding the timing, the bulk of the effects are observed during women's late-teens and early-twenties. In particular, women's delayed births are explained by a substantial reduction in the probability of a teenage birth ($\tau_{frd} = 16p.p.$, $\tau_{srd} = 6p.p.$, $\bar{Y}^c = 31.5\%$), while the anticipated entry to the labor market is driven by positive effects on employment that are strongest around the age of 21 ($\tau_{frd} = 45.1p.p.$, $\tau_{srd} = 8.5p.p.$, $\bar{Y}^c = 32.5\%$). For both outcomes, which capture extensive margin responses, the effects start to fade-out at the age of 25 and differences are null when measured at the age of 30. In terms of persistence, changes in life trajectories induced by the program have lasting consequences. When last observed in the data, women who participated in the program have accumulated on average almost three more years of experience in the labor market, earned 80% more labor income, and have 0.3 fewer children with respect to the comparison group. Finally, I conduct a series of exploratory analyses that suggest that stronger responses in labor market outcomes are both mediated by changes in fertility and secondary education outcomes. More specifically, I provide suggestive evidence that avoiding early-life births is a necessary condition to observe a better performance in the labor market, whereas education seems to act more as a multiplier of the effect. In general, these results are consistent with the idea that labor market participation and motherhood are strong substitutes and that returns to human capital investment depend on fertility decisions, particularly for women.

The magnitude of these effects is sizable and illustrates that, although extremely costly, cash transfers can be a powerful policy instrument to improve children's lives and limit intergenerational transmission of poverty. For instance, in percentage terms, the reduction in teenage births caused by *PANES/AFAM-PE* is equivalent to the fall in Uruguay's teenage fertility rate between 1960 and 2020. Cash transfers also seem to be relatively more effective compared to other policy changes that reduced teenage fertility around the same period, such as abortion legalization (Cabella and Velázquez, 2022) or a large-scale intervention that granted access to subdermal contraceptive implants (Ceni et al., 2021). The direction of the 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. By the time they are last observed, women have doubled their experience and earnings in the labor market compared to non-participating women and the differential is on average already 40% larger than the amount transferred to the households by the program. This is a critical finding to assess whether cash transfers are worth to be implemented. A stronger involvement with the formal labor market will result in additional tax revenues and will help offset the revenue losses due to negative effects on parental labor supply documented

in Bergolo and Cruces (2021). The direction of these effects is also consistent with some of the recent evidence for the US (Bailey et al., 2024, 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).

The main contribution of this paper is to the literature that studies how anti-poverty policies affect children throughout their lives. As previously discussed, there is well-documented evidence about positive early-life (0-5 years old) 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; Bosch and Manacorda, 2012; Fiszbein et al., 2009). On the other hand, there is also an incipient but inconclusive literature that focuses on later-life outcomes (27-30 years old). This recent literature provides 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; Parker and Vogl, 2023), Nicaragua's *Red de Proteccion Social* (Barham et al., 2018), and Colombia's *Familias en Accion* (Attanasio et al., 2021).[3]. However, 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 in a unified setting. Furthermore, I do so using longitudinal, high-quality, individual-level data from a middle-high income country.

From an academic perspective, there are several reasons why studying how cash transfers affects individuals transitions to adulthood is important. In the first place, because it helps bridge the existing evidence on early- and late-life outcomes. Aizer et al. (2022) discuss in detail why short-run and long-run results are not enough to improve the design of social safety nets and they highlight the need for a more dynamic approach. While they discuss this in the context of the U.S., these arguments can be easily extended to a broader context. In particular, they note that recent findings in the literature of in-utero shocks (see e.g., Almond et al. 2018 for a discussion) or early childhood interventions (e.g., Chetty et al. 2011) illustrate that the dynamic of the effects of different shocks or interventions is not necessarily linear. For instance, the results reported in this paper show that if one focuses on the outcomes measured at the age of 30, one could reasonably conclude that the program did not affect women's labor market participation or decision to become a mother. However, this would completely miss the anticipated entry to the labor market and the postponement in age of first birth, two of the key effects of the program. On the contrary, if one would focus only on outcomes measured at the age of 17, the conclusion would be that women are less likely to become mothers and that there are only small effects on employment. Hence, focusing either on the very early- or later-in-life effects, or extrapolating linearly from one to the other might lead to inaccurate

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[3]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

conclusions. To my knowledge, this is the first paper to characterize how an anti-poverty policy affects children's future life trajectories in the context of a cash transfer program with such dynamic perspective. Furthermore, I do this by focusing on a period of their lives when they make a series of critical decisions.

The second contribution is that I am able to study these transitions in a unified setting, using high-quality, individual-level, multidimensional administrative records. This presents two advantages. First, measuring multiple outcomes at different points in time is important for decisions that are closely inter-temporally correlated such as employment, education, and fertility. As opposed to a uni-dimensional static analysis, my setting allows me to provide a better answer to whether, when, for how long, and why a cash transfer program has effects. In turn, this might also help to figure out why cash assistance programs have different effects in different settings (Aizer et al., 2022). Second, it also allows for some methodological improvements with respect to the related literature, which has usually been affected by important data or research design limitations (e.g., aggregated data, staggered roll-out as source of exogenous variation, imperfect matching at individual level, attrition in survey data, among others).[4] 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.

The last main contribution is to provide an additional set of results to a literature that is still incipient and have yield some mixed results. In particular, some papers have shown evidence of positive effects of the EITC (Barr et al., 2022) or PROGRESA (Araujo and Macours, 2021; Parker and Vogl, 2023) on early adulthood labor market outcomes. However, the only evidence that is built on an RCT suggests that cash assistance might have null long-term effects (Price and Song, 2018). My paper provides a new data point and shows that a cash assistance program in the context of a middle-high income country can have large and lasting effects on women's education, fertility, and labor market outcomes.

The paper also relates to three other broad strands of literature. First, 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), 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), and in particular the relation between teenage fertility, education, and labor market prospects (Kearney

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[4]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.

and Levine 2014b, 2012). My paper contributes to this literature by providing complementary evidence that highlights how fertility decisions, particularly during the critical late-teens and early twenties, might have long-lasting consequences in terms of labor market participation, experience, and earnings. Furthermore, this paper illustrates how cash transfer policies can be useful in reducing labor market gender gaps, even when they are not specifically designed for this purpose. Second, to the literature in Demography that analyzes the increase in the mean age of first birth that has affected rich countries since the 1970s (i.e., *postponement transition*) and, more recently, Latin-American countries (Rosero-Bixby et al., 2009). This transition has been explained by several factors, such as trends in modern contraceptives use or 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). 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 also relates to the literature on the role of household income on children's outcomes. The bulk of the empirical literature has found that early childhood conditions 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, the main definitions of the treatment and outcomes of interest, and the main sample used in the analysis. In Section 5, I describe the main features of the Regression Discontinuity approach used to estimate the causal effects of the program. Section 6, reports the main results from the empirical analysis. Finally, Section 7 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).[5] 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 second-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.[6] 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 social relief program in response to the economic downturn that affected most Latin American countries in the early 2000s, substantially expanded in 2007, and remains in place until today.[7] 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 phase started in 2005 under the name of *PANES* and continued until 2007. The second phase, *AFAM-PE*, started immediately after and remains in place until today. The program was widely publicized and rapidly became the largest anti-poverty program in the country's history (Manacorda et al., 2011). In terms of its size, *PANES/AFAM-PE* is estimated to cost 0.4% of Uruguayan GDP. As a reference, the United States typically spends

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

[6]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.

[7]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%.

around 0.67% of its GDP on family benefits.[8] The size of the program is also comparable to other programs in Latin American countries such as *PROGRESA-Oportunidades* (Mexico) and *Bolsa Familia* (Brazil).

The main component of *PANES* was a cash transfer targeted at the poorest 150,000 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 to help break the intergenerational transmission 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 between 30% and 50% of the average self-reported household income in the application forms.[11]

Between 2005 and 2007, more than 180,000 different households (17.6% of all households in the country) applied to *PANES/AFAM-PE*. Eligibility was determined based on a proxy-means test. Households were visited by program officials who conducted a thorough interview to evaluate their socio-economic and material conditions. This information was used to compute a poverty score (z), which consists of the predicted probability of being below a critical per-capita income level.[12] Households with a poverty score z above a certain region-specific 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 June, 2013.

On January 1st, 2008 *PANES* was expanded and re-branded into *AFAM-PE* with the goal of increasing the program's coverage. The program's main components - i.e., eligibility criteria and type of benefits and conditionalities - remained the same and the transition between the two phases was straightforward. Most *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 more lenient eligibility requirements (i.e., a lower eligibility threshold). Appendix A discusses

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[8]https://data.oecd.org/socialexp/family-benefits-public-spending.htm

[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]Because it was self-reported, 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 XX and in Manacorda et al., 2011; Amarante et al., 2016.

additional details on the institutional background of *PANES/AFAM-PE*.

2.3 Existing studies of the effects of *PANES/AFAM-PE* on adults

There are several studies that analyze how *PANES/AFAM-PE* affects the life of the adults that collected the transfer. In terms of labor market outcomes, Bergolo and Cruces (2021) find that the program led to a 13% reduction in adults' formal employment, which can be explained both by a reduction in labor supply but also by a shift to the informal labor market. There is also a series of papers that analyze how the program affected fertility, birth outcomes, and household structures. For instance, Amarante et al. (2016) find that the program increased birth-weight of the newborns, and that these improvements were associated to an improved nutrition. In terms of household structure, Parada (2020) find that the program did not affect adult women's fertility decisions, but it led to more stability in pre-program marital status (i.e., participant women were more likely to remain single if they were single, or married if they were married). The program also seems to have improved women's self-perceived agency in decisions about household expenditure (Bergolo and Galván, 2018), but it does not seem to change actual power structures within the household (Parada, 2020). Finally, some other papers have focused on how cash transfers might have affected individuals preferences and attitudes. Manacorda et al. (2011) finds that the program increased support for the current government and trust in the President and other institutions implementing the program, while Nicolau (2020) find that the program might have slightly increased some dimensions of stigma.

3 Conceptual Framework

Due to a combination of income and substitutions effects, cash transfers may trigger a series of behavioral responses among all household members and across several margins. In addition, decisions on education, fertility, and labor market participation are also inter-temporarily linked, might vary depending on market imperfections such as information frictions, or could be different in collective household frameworks. In this section, I motivate the research hypotheses by broadly discussing how cash transfers may affect individuals who benefited from the program in their childhood. The list of mechanisms discussed in this section is not intended to be exhaustive, but to provide an overview of the main mechanisms that have been discussed in the related literature.

Income and substitution effects: Cash transfers can trigger a series of responses associated with income and substitution effects. On the one hand, income effects lead to an increased demand for normal goods (e.g., leisure or education/human capital) due to

the additional financial resources disposed by the households (see e.g., Todd and Wolpin 2006, 2008; Keane and Wolpin 2010).[13] Cash transfers are particularly important for poor households in the presence of credit constraints. They might allow some children to enroll and remain at school (e.g., through purchases of books, clothing, transportation, etc.), or enable access to modern contraceptives methods.[14] On the other hand, cash transfers can also affect household decisions through a substitution effect when the benefit is contingent on specific behaviors. For instance, education requirements reduce the opportunity cost of schooling, and make it more attractive compared to any other non-education-related activities (e.g., Parker and Todd, 2017). A cross-substitution effect could also affect children's education enrollment if parents reduce their labor supply and spend more time with their children (e.g., Martinelli and Parker, 2008). This increase in supervised time could also affect fertility outcomes since it reduces the chances of engaging in risky behaviors.

Dynamic effects: When individuals make decisions that have consequences for multiple time periods, the set of potential behavioral responses becomes more complex. The reduction in the marginal cost of schooling today, not only increases *current* investment in education but it also affects decisions in *subsequent* periods through changes in future wages (i.e., "future human capital effect" in Black et al. 2008). For instance, if future wages are higher, some individuals will decide to enter the labor market earlier instead of accumulating more education (Behrman et al., 2011) or becoming a parent (e.g., Duncan and Hoffman, 1990; Wolfe et al., 2001). This is particularly relevant for women for whom fertility, education, and labor market participation are more likely to be mutually exclusive. In addition, in a dynamic framework, fertility decisions are also affected by age-increasing biological or psychological costs and pregnancy risks (Schmidt et al., 2012; Gustafsson, 2001). Another example of these complexities is the dynamic complementarities framework in Cunha and Heckman (2007) where today's education investments increase future returns to education, leading to additional years of schooling to the detriment of non-education activities, such as labor market participation and childbearing.

Besides the direct effects of cash transfers on education, fertility, and labor market decisions, these decisions might also have direct effects on each other. For instance, additional education could affect fertility decisions if there is a trade-off between quality and quantity of children (Becker and Lewis, 1973); if it improves the ability to predict the benefits of delaying childbearing (i.e., "current human capital effect" in Black et al., 2008); if it improves access to contraceptives, family planning and health care services

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[13]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.

[14]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)

(Kearney and Levine, 2009; Bailey, 2006; Lundberg and Plotnick, 1995); or by changing women's empowerment, attitudes, and values toward maternity, just to name a few.[15],[16] 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: Cash transfers might also affect households' decisions through mechanisms other than the standard income and substitution effects. For instance, in collective household decision models (e.g., Chiappori, 1988, 1992; Browning and Chiappori, 1998) changes in the distribution of income between household members can lead to changes in members' bargaining power (Martinelli and Parker, 2003, 2008; Attanasio et al., 2012), which in turn might change consumption patterns (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 (Jensen, 2010) and fertility decisions (Kearney and Levine, 2014a). For instance, by participating in a government program parents could be more exposed to highly educated professionals and change their perceptions about their children's opportunities and required investments, leading to a stronger attachment to 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 parents who reduce their labor market participation. Regarding fertility decisions, improved perceptions about the chances of being successful or achieving certain levels of socio-economic status that is only possible if there is no teenage birth could lead to delays in the age at which women give birth. Finally, alternative mechanisms such as a changes in household financial stress (Gershoff et al., 2007; Yeung et al., 2002; Conger et al., 1993); improved children's health outcomes (e.g., Currie, 2009); or changes in social interactions and peer effects (e.g. Bobonis and Finan, 2009; Lalive and Cattaneo, 2009) might also play a role.

In sum, the related theoretical literature provides mostly unambiguous predictions about the early-life effects of cash transfers on children's education, fertility, and labor market decisions. More specifically, cash transfers are expected to reduce teenage births, increase education enrollment, and reduce children's labor market participation. How-

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[15]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

[16]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.

ever, 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 approach. In the end, the effects of cash transfers on children's transition to adulthood is mostly an empirical matter.

4   Data, Sample, and Outcomes of Interest

The analysis of the effects of cash transfers on the transition to adulthood is highly data demanding for two reasons. First, because it requires information on a large number of individual characteristics: adulthood is defined by a series of markers across several dimensions such as education, fertility, and labor market participation, among the most important ones. Second, because transitions are a dynamic phenomenon by nature. Hence, its study requires longitudinal information. The data I use in this paper satisfy these two requirements. First, I compiled administrative records from different government 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 it is based on administrative records, I can observe all relevant outcomes at different ages (15-30 years old), and therefore describe the full trajectory of the effects. 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, Treatment, and Outcome Variables

***PANES/AFAM-PE* records: Application and participation variables.** These records are used to construct all application- and participation-related variables. They were provided by the Ministry of Social Development, and correspond to the universe of applications to *PANES/AFAM-PE* between April 2005 and December 2019. This baseline information is collected in the thorough interview conducted by program officials to assess the socio-economic and material conditions of the applicants. Since its collected before participation, this should be considered as baseline information. Among other variables, these records include information on city, date of application, poverty score, resolution, participation history, housing conditions (e.g., materials, access to sanitation, appliances, etc.), education, employment status, and other personal characteristics for each household member. The total number of application forms included in the raw participation data is 747,204, corresponding to 1,476,696 unique individuals.[17]

Birth Records: Fertility outcomes Birth records were provided by the Ministy of

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

Public Health and are used to measure fertility outcomes. They cover the universe of births in Uruguay between 2005 and 2021, including individual-level information such as birth date, birth weight, gestation weeks, and mother and father identification information. Estimates for men's fertility should be taken with a grain of salt since this information is only collected starting in 2010, and even in the best years about 50% of the information is missing. With these records, I construct three groups of outcomes. First, I use age of first child as a summary measure to describe fertility-related changes in transitions to adulthood. Second, for the dynamic analysis, I consider two variables which I define for every possible age $\gamma \in [15, 33]$: having a child and number of children by age γ. Finally, I define two analogous variables measured when individuals are last observed. All the outcome variables are 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.[18]

Secondary and tertiary education administrative records: Education outcomes.
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*. I use these records to measure education outcomes. Secondary education records contain yearly information for the universe of students enrolled in public schools in 2006-2012, 2014, 2017, and 2018. Information from the National Council of Technical and Professional Education contains vocational and technical public school enrollment for the same period.[19] The information provided by *Universidad de la Republica* consists of a list of all people enrolled at the University between 2005 and 2020, and the enrollment year.[20]

Due to data limitations, and because it is easier to compare across levels and institutions, education outcomes refer exclusively to enrollment.[21] More specifically, for secondary education, I define the following variables: number of different grades enrolled

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

[19]Careers offered by technical and vocational schools can be classified into middle school, high school, and tertiary careers, based on enrollment requirements. 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.

[20]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.

[21]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.

in middle school (1 to 3), high school (1 to 3), total (1 to 6), and maximum grade (1 to 6). 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.

Labor histories: Labor market outcomes. I use labor histories provided by the Social Security Agency and the Ministry of Labor to construct the labor market outcomes that cover the universe of workers for the period 2005-2022. Labor histories contain monthly individual-level information on wages, sector, type of employment, and employers' industry sector for the universe of registered employees. While these records only provide information about the formal labor market, it is important to note that the informal sector in Uruguay is relatively small (i.e., 17% of total employment). To analyze the effect of *PANES/AFAM-PE* on labor market outcomes, I use a similar structure to the fertility outcomes. First, a summary variable that captures how individuals' transition to adulthood changed in the labor market dimension represented by the age at which someone entered the labor market for at least four consecutive months. Second, a binary variable that captures if someone was employed at a given age γ. Third, a series of cumulative variables that measure the total number of months employed and cumulative earnings by a given age γ. Finally, two additional variables that measure employment and earnings in the last year that someone was observed.

4.2 Sample of Interest

The empirical analysis in this paper is restricted to individuals that were younger than eighteen years old when their parents applied to *PANES/AFAM-PE* for the first time, and had at least nineteen years old in December, 2021, when fertility outcomes are last available. In addition, the analysis is restricted to individuals from households who applied to the program for the first time between 2005-2007, which represent 70% of the sample.[22] Henceforth, I refer to this sample as the *full sample*. The analysis of the dynamic effects requires additional age-restrictions. For instance, to estimate effects measured at the age of 23, the sample should be restricted to people who had at least 23 years old in December, 2021. This applies to each estimate corresponding to an age γ. I refer to this as the *dynamic sample*. The different restrictions depending on the time at which an outcome is measured introduces concerns about trajectories of effects being explained by

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[22]The reason for this is to ensure comparability of people around the threshold in a context where the threshold is becoming more lenient. With this restriction, I compare applicants that were subject to a unique region-specific threshold.

compositional changes. In Appendix E, I present additional estimates based on a *balanced sample* that rule out this possibility. In particular, the balanced sample is comprised of people who had already turned 29 by December, 2021 for whom I can estimate the full age-by-age effects in the 15-29 years old period, keeping the composition constant. To maximize statistical power, my preferred estimates are based either in the *full* or the *dynamic* sample.

Columns (1) and (2) in Table 1 describe the main characteristics of *full sample*. In column (1), the description corresponds to 279,031 individuals across the full support of the poverty score z, while in column (2) I focus on the subset of 70,402 individuals who are within 5p.p. of the eligibility threshold based on the score obtained in their first household application. Columns (3) and (4) focus on the *balanced sample*. In terms of individual characteristics, the average individual in the *full sample* had 10 years old at the moment their parents first applied to the program, are last observed at around the age of 26, and half of them are women. Individuals are typically included in about 3 application forms throughout the period. By construction, individuals in the *balanced sample* are older and had 15.8 years old at the moment of the first application and 32 when they are last observed. Both for the *full* and the *balanced* sample, these characteristics remain the same when focusing on a neighborhood of 5p.p. of the eligibility threshold. Regarding the characteristics of the reference form, when considering the full support of z in the *full sample*, we observe that these individuals are around 16p.p. above the eligibility threshold, with 75% of the reference forms corresponding to accepted applications, and 27.5% coming from applications in the capital city. When focusing on the applications closer to the threhold, the characteristics are different by construction: the average individual is just at the eligibility threshold and only 53% of these first-time applications were accepted. Furthermore, only 15.5% of the application forms correspond to applications in the capital city. Reference form characteristics are similar when considering the *balanced sample*. Finally, in terms of household level characteristics, 45% of the households in the *full sample* across the full support of z are single-parent households with an average size of 5 people, and an average age of household members of 21. Households heads are low-educated with years of education barely above completed primary school, only 64.5% of them employed, and with a household self-reported income of $209, which is substantially below the poverty line. Characteristics of the households remain relatively similar when focusing on close applications, and also for the *balanced 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).[23]

In practice, the implementation of the eligibility rule worked well: most ineligible applications were rejected and most eligible applications were accepted. It is worth noting however, that the acceptance rate for some ineligible forms that were extremely close to the threshold (i.e., closer than 0.0015, or 0.15p.p.) was abnormally high. This is most likely due to precision issues in the raw data, which in some cases was provided as a string variable with only 4 decimal places. For this reason, in what follows, I always exclude application forms within a radius of 0.15p.p. of the eligibility threshold. For robustness, I present additional estimates varying the radius of the donut, and show that estimates are not sensitive to the specific choice of the radius. Appendix D, and in particular Figure D.1 provide further details on the eligibility rule from the perspective of the program administrator (i.e., where the unit of analysis is an application form).

If this was a one-time program to which individuals can apply only once and receive just one decision on acceptance based on a unique observation of their poverty score, one could implement a very simple sharp RDD by comparing individuals just above and just below the eligibility threshold. However, *PANES/AFAM-PE* has been in place uninterruptedly since 2005 and households might have applied to it multiple times, introducing two challenges for the RDD setup. The first one is which value of the running variable to chose. The second is how to address the possibility of endogenous sorting around the eligibility threshold induced by re-applications. This is particularly important when the characteristics of the household that determine whether to file a re-application are also correlated with the outcomes of interest.[24] To address these challenges, I follow the approach proposed by Jepsen et al. (2016) who show that in settings with multiple applications one can implement a fuzzy RDD where: 1) the running variable is defined based on the score obtained in the *first application*n, and 2) the endogenous variable can be defined as being *ever treated*. The intuition for this design is that the score obtained in the first application is presumably less likely to be affected by manipulation and avoids the

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[23]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: $\mathbb{E}[Y(1) - Y(0)|Z = z] = \lim_{z \downarrow 0} \mathbb{E}[Y|Z = z] - \lim_{z \uparrow 0} \mathbb{E}[Y|Z = z]$.

[24]Endogenous sorting 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).

issue of selection into re-applications.[25] Hence, the RDD will be based on the following variables:

Exogenous variable: eligibility based on the score of the first application form (D^{1st}): I define the *first application* as the earliest application 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.[26] Hence, D^{1st} is a binary variable that takes the value of 1 if the score obtained in the first application is larger than a region-specific eligibility threshold 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 into *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 at earlier ages. As a robustness test, I present estimates based on two analogous continuous treatment variables: 1) years in the program and 2) 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, captured by the term τ_{FRD}, is based on the following specification:

$$Y_i = \mu + \tau_{FRD}T_i + \beta_1 Z_i^{1st} + \beta_2 Z_i^{1st}T_i + \mathbf{X_i} + \mathbf{\Lambda} + u_i \tag{1}$$

where Y_i is the outcome of interest (e.g. age of the first birth, age of first employment spell, etc.) for individual i, (Z_i^{1st}) is the centered value of the poverty score obtained in the first application, T_i is the endogenous treatment variable, $\mathbf{X_i}$ is a series of individual-level baseline characteristics, and $\mathbf{\Lambda}$ represents year of birth, year of application, and region FE. Because T_i and Y_i are endogenous, T_i is instrumented using D_i^{1st} based on the following first-stage equation, where D_i^{1st} is an indicator variable for eligibility in the first application ($Z_i^{1st} > 0$):

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

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[25] Jepsen et al. (2016) analyze the effects of GED scores on employment and earnings. 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 challenge is that re-takers can be different from non-re-takers in ways that are also correlated with the outcomes. 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.

[26] 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. 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 $f_{h_1,A}$.

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. The estimation procedure follows Calonico et al. (2014), who provide robust standard errors and confidence intervals. The bandwidth 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. In Appendix E.1 I present specification curves containing all possible combinations of optimization algorithms, kernel functions, and polynomial degrees and show that the results are not driven by these specific technical choices. In all cases, standard errors are clustered at the household level.

The described strategy provides an estimate that should be interpreted as a local average treatment effect. In addition, I will also report estimates corresponding to the reduced-form effects, as well as the corresponding figures for the main outcome variables. These reduced form estimates, captured by the term τ_{SRD}, are based on the following sharp RD specification:

$$Y_i = \mu + \tau_{SRD} \mathbb{1}(Z_i^{1st} > 0) + \beta_1 Z_i^{1st} + \beta_2 Z_i^{1st} \mathbb{1}(Z_i^{1st} > 0) + u_i \tag{3}$$

Finally, it is important to note that, compared to sharp RDs, fuzzy RDs 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 lower than 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, falsification, and balance tests typical in these settings. Second, I present a series of estimates that characterize the main changes in individuals' transition to adulthood. In particular, I focus on three variables: age of first child, age of first employment spell, and enrollment in tertiary education system. Because of strong heterogeneous responses between men and women, all results are estimated and reported separately for each group. Third, I dig into anatomy of the changes in the transition and I analyze when, for how long, and why the reported effects took place.

One concern about the use *age of an event* as the main outcome of interest is that this variable is only defined for people who actually experienced the event. In Section

6.3, I show that the program did not change the probability of becoming a parent or ever participating in the formal labor market. Therefore, selection should not be a critical concern when discussing effects of the program on age first child or employment spell.

6.1 Validity of the RDD Design

Figure 1 summarizes a series of empirical tests performed to validate the use of a RDD as the identification strategy. Panels a. and b. focus on first stage results. Panel a. reports the share of 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 *full sample*. The figure is restricted to the optimal MSE bandwidth (1.2p.p.) based on Calonico et al. (2019) and uses 20 quantile-spaced bins at each side of the threshold. The figure shows an abrupt change in the probability of ever participating in the program just at the eligibility threshold. The discontinuity is sizable - 23.1p.p. corresponding to more than 30% change with respect to the ineligible individuals - and statistically significant - $p - value \leq 0.001$ -. In Appendix D, I provide additional first-stage estimates that use alternative definitions of the endogenous variable such as number of months in the program, total amount collected, amount per month, as well as estimates for men and women separately. As a reference, the discontinuity at the threshold can also be interpreted as an average change of 20-23 months in treatment exposure (comparable to an average baseline exposure of 56 in the reference group, i.e., ineligible based on the score obtained in the first application), or an additional $4,500-$4,900 collected in benefits from the program (comparable to a baseline average of $8,012).

To illustrate that the first stage estimates average differences in treatment intensity from across the distribution, panel b. in Figure 1 shows the shifts in treatment intensity induced by the instrument, as in Rose and Shem-Tov (2021). Each bin represents the local change in the probability of collecting $D \geq d$ from the cash transfer program during the whole period. For instance, the bin at $d = 10,000$ shows how much more likely it is for people just above the threshold to receive $10,000 or more in *PANES/AFAM-PE* benefits compared to those just below the threshold. Given that differences are statistically significant for all values reported ranging from $1,000 to $30,000, one can conclude that the proposed instrument provides substantial variation in the intensity of exposure to the program.

Figure 1, panels c and d, provide indirect tests about the validity of the continuity assumption. Panel c. depicts a summary test for systematic differences in baseline characteristics just at the threshold. More specifically, I compute the predicted eligibility score based on a probit model that uses first-time eligibility ($\mathbb{1}(Z_i^{1st} > 0)$) as the dependent variable and all other observed baseline characteristics as the independent variables. The fact that there is no discontinuity in predicted eligibility at the threshold ($\tau_{SRD} = -0.006$,

$p - value = 0.268$) can be interpreted as a signal of no systematic discontinuities in the baseline variables. Analogous estimates for each baseline variable considered individually are provided in Table 2, and lead to the same conclusion. Panel d. provides a formal test of continuity of the running variable based on Cattaneo et al. (2018) and McCrary (2008). Intuitively, if some people have the ability to manipulate the running variable, there should be an excess of mass just above the eligibility threshold. If there are some specific individual characteristics that explain the ability to manipulate the running variable, which are also correlated to the outcome of interest, this could introduce concerns about the bias of the RDD estimate. In the case of *PANES/AFAM-PE* this test provides no evidence to reject the null hypothesis of continuity ($p-$value=0.990).

Finally, Appendix D also reports additional validation tests using predicted outcomes based on baseline characteristics as the dependent variable as a dimension-reduction exercise (Londoño-Vélez et al., 2020). In line with panels a. through d., the evidence reported in the appendix suggests that there are no other individual baseline characteristics that change discontinuously just at the threshold, except for the probability of treatment. Overall, the evidence discussed in this section provides strong and reassuring evidence that supports the use of a RDD as the identification strategy.

6.2 Summary of Effects on Transitions to Adulthood

In this section, I analyze how *PANES/AFAM-PE* impacted individuals' transition to adulthood using three main outcomes: 1) individuals' age at first birth, 2) enrollment in higher education system, and 3) age of first employment spell. In addition, to summarize these alternative margins of response into a single measure, I create a composite index that aims to characterize individuals' transition to adulthood as more "market-oriented" as opposed to "stay-at-home oriented". This index takes the value of 1 if a person did not have an early-life birth (i.e., before the age of 20) *and* had either an early entry to the labor market (again, before the age of 20) or was enrolled in the higher education system.

Figures 2 and 3 illustrate the reduced form effects for women and men separately. In all cases, the figure is restricted to the optimal bandwidth obtained by minimizing the mean squared error (MSERD), as suggested in Cattaneo et al. (2019). The local averages (represented by blue and red bins) are computed using 20 quantile-spaced bins. The local polynomial regression is estimated using a linear fit, a triangular kernel function, and including all baseline covariates discussed in section 5.[27] In addition, each figure reports the τ_{SRD} estimate based on Equation 3, the bandwidth used for estimation, as well as the p-value of the discontinuity test, based on robust inference.

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[27]It is worth noting that I choose the number of bins arbitrarily, but following the advice for visual inference in RDD settings discussed in Korting et al. (2023). While the optimal number of bins suggested by the *rdrobust* command in most cases is about 5 bins to each side of the threshold, I choose an arbitrarily higher number (x4) to avoid concerns on over-smoothing.

Figure 2, panel a. shows a strong discontinuity in women's age of first birth just at the eligibility threshold for those who were eligible based on the score obtained their first application to the program (+0.51 years). This abrupt jump is statistically significant at traditional levels ($p-value < 0.001$). Panel b. shows somewhat muted effects in terms of the probability of ever being enrolled to the higher-education system. The discontinuity is estimated in +2.18p.p., it is borderline statistically insignificant at 10% ($p-value = 0.119$), and not so visually compelling. Panel c. reports the analogous result for the labor market dimension. The reduced form figure shows a sharp change in the age at which women had their first employment spell of -0.39 years, and statistically significant ($p-value = 0.001$). Finally, panel d. reports the summary measure, which combines the information used in panels a. through c., In this case, the figure illustrates that the value of the market-oriented index shifts significantly just at the eligibility threshold. The jump is of about 8.40p.p. and statistically significant ($p-value < 0.001$). Figure 3 reports the same estimates but for men. In general, the program does not seem to have affected men's transition to adulthood, except for a reduction in the age at which they become fathers. While this estimate is statistically significant ($p-value = 0.018$), it must be taken with a grain of salt due to important limitations on the availability of father's identifying information as discussed in Section 4.

Table 3 reports the main results obtained in the econometric analysis. Panel a. describes the local average treatment effects (LATE) obtained with the 2SLS estimation. As discussed in Section 5, this is the preferred specification and essentially re-scales the reduced-form effect depicted in Figures 2 and 3 by the size of the first stage. Local average treatment effects are useful to understand the expected effects when changing from non-treated to treated status, for the sub-group of compliers (as discussed in Section 5). However, the 2SLS model used to estimate equation 1 assumes a linear relationship between the probability of treatment and the outcome of interest, which has some limitations. In particular, this assumption implies that each percentage point increase in the probability of treatment should have the same effect on the outcome of interest. While this is a natural starting point because of its simplicity, it might be too restrictive. Hence, tables that report the 2SLS estimates also report the reduced-form and first-stage estimates in panels b. and c., respectively.

The main Fuzzy RD results can be described as follows. First, column (1) in panel a. shows that participating in the program increases women's age of first birth by 2.2 years. This effect is statistically significant when considered individually ($p-value < 0.001$), but also when adjusted by multiple hypothesis testing. τ_{FRD} estimates in columns (2) through (4) indicate effects of +10.9p.p. for enrollment in higher education ($p-value = 0.107$), -1.72 for age of first employment spell ($p-value = 0.001$), and +41.95 ($p-value < 0.001$) for the market-oriented index. For men, the effects are null, except for age of first birth where the estimated LATE is 1.07 ($p-value = 0.017$).

Interpreting the magnitude of the effects is not straightforward. For instance, consider the average value of the outcome variable for the reference group, defined as ineligible individuals based on the score obtained in the first application, who are close to the threshold ($\bar{Y}^c$). The 2.2-year delay in women's age of the first birth obtained in the Fuzzy RD estimate accounts for 11.2% of the average in the reference group, whereas the -1.7-year anticipated entry to the labor market represents a reduction in 8.27% in the age of the first employment spell. In addition, the τ_{FRD} estimate for the market oriented index is 1.5 times larger than the average for the comparison group, and the point estimate for the enrollment in higher education variable is about twice as large as the average observed for the reference group. As previously discussed, the magnitude of these effects relies on the 2SLS linearity assumption, which might somewhat exaggerate the effect. Hence, a more conservative approximation can be obtained by looking at the effect-sizes in the reduced form specification. Despite this more conservative approach, which can be thought of as a lower bound, the effect size of all estimates are still large. For the age-related variables, the reduced form estimates indicate a 2.6% increase in women's age of first birth and a 8.3% increase in age of the first employment spell. For enrollment in higher education and the market oriented index, the magnitudes are 19.6% and 38.4%, respectively. Hence, while under different assumptions, one could compute different effects sizes, the overall conclusion is still that even in the most conservative scenario, *PANES/AFAM-PE* had sizable effects on women's lives.

Regardless the specifics on the magnitude of the effects, the evidence reported here reliably suggests that *PANES/AFAM-PE* changes the timing of two key events in women's lives, while not inducing such major changes for men. More specifically, women delayed the age at which they have their first child and anticipate their entry to the labor market by about the same time. For men, if anything, there is a slight anticipation in the age of their first child, but this estimate is subject to some data limitations. In general, changes in women's transition to adulthood are consistent with a more market-oriented transition to adulthood as opposed to a more stay-at-home one. The results on education outcomes are consistent with this story, but are not as strong. One possible reason is that tertiary education is not a relevant margin of decision for these extremely disadvantaged households. Another potential explanation could be the presence of heterogeneous responses by age at the time of the treatment. The estimates reported so far are based on a pooled sample of people who were treated before the age of 18, but started to be treated at different points in their lives. For the late-treated individuals, perhaps it was too late for the program to have any effects. As discussed later in this section, this is the most likely explanation.

The findings on fertility postponement are consistent with the incipient but scarce literature on the long-run effects of cash transfers. 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 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, but they do not provide later-life estimates. While insightful, none of these works have addressed changes in fertility, education, and labor market outcomes in a unified dynamic setting. As I discuss in detail in Section 6.3 this is key to unveil the processes and mechanisms behind these major changes in the timing of events.

Robutsness tests: The results reported in this section are robust to a series of tests discussed in detail in Appendix E. First, I report estimates based on alternative specifications that do not include the baseline covariates or region, year of birth, and year of application fixed effects. Second, I report estimates based on alternative definitions of the endogenous variables. In particular, I replicate the main results but using the total amount of cash collected and the number of years in the program before turning 18 as endogenous variables. In addition, I also report estimates based on different age thresholds, i.e., being treated before 16, 14, and 12 years old. Third, I replicate the analysis on the sub-sample of children that were more affected by the changes in the enforcement environment of the conditionalities. More specifically, I focus on the sub-sample of children that were younger than 16 years old when the government decided to start enforcing education enrollment. Fourth, I use the balanced sample of individuals who were at least 29 years old when they are last observed (December 31, 2021). Fifth, for each main outcome and group I present four series of specification tests: a) falsification tests using placebo eligibility thresholds, b) alternative estimates with arbitrary selection of bandwidth, c) specification curves combining all possible estimation decisions (kernel function, degree of the polynomial, and optimization algorithm), d) different donut radii. In general, the robustness tests confirm that the main results are not driven by specific choices in how to implement the RDD, the use of control variables, or specific sample-selection criteria.

Exploratory evidence on differential effects by timing of exposure: The robustness tests discussed in Appendix E, not only are reassuring about the validity of the estimates discussed so far, but they also reveal some interesting patterns related to the timing of the intervention and the strength of the effects. There are two suggestive findings that are worth mentioning.

First, and especially for fertility decisions, the effects seem to be stronger for girls who started their participation in the program at earlier ages. For instance, women who turned 16 years old after the enforcement of education conditionalities, i.e. girls treated at younger ages, show stronger delays in the age of their first birth. Furthermore, when considering different age-thresholds for the definition of the endogenous variable, there is a clear negative gradient in the treatment effects consistent with the idea that early-treated

girls were more responsive that late-treated girls. For instance, the effects on age of first birth are the largest when the treatment variable is defined as being treated before the age of 12, compared to analogous definitions for ages 14, 16, and 18.

While these results are consistent with the idea that treatment at earlier ages might induce stronger responses, it is hard to draw more definite conclusions. First, because of the strong correlation between the age at which children were treated and the time they spent in the program. This makes impossible to distinguish whether differential effects are explained by an early intervention (e.g., in the spirit of Heckman 2006; Cunha and Heckman 2007) or by the intensity of exposure. Second, because the first stage becomes weaker the earlier the household applied to the program and the youngest the child. Intuitively, children that applied at very young ages have more opportunities to re-apply to the program and being accepted into it before turning 18 years old. This leads to scores obtained in the first application being less informative of the probability of treatment compared to older kids, who have less chances of re-application. While one could look directly at the reduced form estimates to avoid this issue, they become less insightful as the share of people who eventually entered the program, even with ineligible first application scores, becomes larger.

The second suggestive finding is that effects on education outcomes are stronger when cohorts are affected by a stronger enforcement of the education requirements. In particular, for children who were younger than 16 years old when the administration started to enforce education enrollment and attendance, the effects on higher-education enrollment are much higher, and statistically significant at 1% level. Again, this requires a cautious interpretation given that the differential effects in favor of younger children could also be explained by these children being treated at earlier ages, or higher intensity of treatment.

6.3   Digging into the changes in the transitions

In the previous section, I presented a summary of effects of PANES/AFAM-PE on individuals' transition to adulthood showing that women anticipated their entry to the labor market and postponed the age of their first child as a response to the program. However, it is still unclear when did the changes happen, whether they just represent a change in the timing of events with no longer-term consequences, and also why did this happen. In this section, I exploit the richness of the data to answer these questions.

Timing of the Events: Dynamic Analysis

To understand more precisely the changes in the timing of the events, I exploit the longitudinal nature of the data and estimate the effects of the program age by age. The data available for university enrollment only includes information on the year of first en-

rollment, but does not have any additional information on activity, academic progress, or graduation. This prevents me from replicating the dynamic analysis in this specific dimension.

Figure 4 summarizes the dynamics of the effects of *PANES/AFAM-PE* on fertility and labor market participation decisions. In Panel a, I display the effects of the program on the probability of having had a child measured at different ages (γ). These are represented in the x-axis and range from 15 to 33 years. The y-axis shows the estimated effect measured in percentage points (p.p.). These estimates are based on equation 1. In this case, the dependent variable takes the value of 1 if an individual has had a child at a given age, and 0 otherwise. For instance, the coefficient reported for $\gamma = 25$ corresponds to the effect of *PANES/AFAM-PE* on the probability that an individual has had their first child at or before the age of 25. The effects are estimated separately for men and women (including 90% robust confidence intervals), with effects on men depicted in blue and effects on women depicted in red. Estimates in panel a. are conducted using the *dynamic sample*. This means that estimates for a given age γ are restricted to individuals who are γ years old or older. Appendix E reports the same results for a balanced sample of individuals who were at least 29 years old in December, 2021, so that composition remains constant across ages. The results remain unchanged, indicating that the dynamic patterns observed in the figure are not driven by composition changes in the *dynamic sample*.

The program's effects on women's age-by-age fertility display a clear u-shaped pattern with negative effects being strongest during the early-twenties. More specifically the bulk of the effects seems to build up between the ages of 15 and 19, and they remain negative, strong, and statistically significant until around the age of 25. At this age, the effects start to fade out, such that by the age of 30 there are no longer signs of negative effects of the program on fertility decisions.

Two conclusions can be drawn from this pattern. First, the fertility postponement discussed in section 6.2 is explained mostly by a reduction of births that in the absence of the program would have happened during teenage or early-twenties years. Appendix E reports the age-by-age estimates in detail. For instance, there is a strong and negative effect of *PANES/AFAM-PE* on the probability of having a teenage pregnancy (τ_{FRD} =22p.p., $\tau_{SRD} = 4.60$p.p., $\bar{Y}^c = 32.9$). This is also illustrated in Panel b. of Figure 4, which depicts the cumulative distribution function (CDF) for the age of first birth in the *balanced sample*, conditional on having a child by the age of 29, separately for people who are just above the threshold (eligible) and people who are just below the threshold (ineligible). The differences in the distribution of the age of first birth are striking and illustrated by the rightward shift in the CDF corresponding to eligible individuals.

The magnitude of the changes in early-life births is statistically significant and also economically relevant. For instance, the 54.9% reduction in the probability of having a child by the age of 20 implied by the τ_{FRD} estimate is already larger in percentage terms

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to the reduction observed in Uruguay's adolescent fertility rate between 1960 and 2020, which changed from 5% to 3%. Compared to other policy interventions carried out in Uruguay, the effects of *PANES/AFAM-PE* are also 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 that the effects of an expansion of *Familias en Accion* on teenage pregnancies measured at age 18 was -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).

The second main conclusion that can be drawn from this dynamic analysis is that the effects of *PANES/AFAM-PE* on fertility seem to be associated mostly with a change in the preferred timing of the events, rather than by actual changes in overall preferences for having children, at least in the extensive margin. Of course, delaying the age of the first child might lead to a reduction in total fertility rate given that women have now less time to achieve their desired number of children. I will provide additional evidence about the effects of the program on the number of children in the next section, with the corresponding caveat that I am only able to observe women until the age of 33 at most. Therefore, they might have not concluded their reproductive cycle yet.

In panels c. and d. of Figure 4, I replicate the analysis for employment-related outcomes. In particular, the main outcome of interest in panel c. is a dummy variable that takes the value of 1 if an individual had an employment spell of at least four consecutive months at a given age γ. 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 employment at any of the ages considered in the analysis. On the contrary, estimates for women provide substantial, robust evidence of positive effects on employment that start as early as around 17-18 years old, continue growing until the age of 22 and then remain relatively stable until mid-twenties. After that, the effects start to attenuate, becoming null when measured at around the age of 30s. These differences between men and women's responses to the program at late-teens and early-twenties are large and, generally, statistically significant. It is also important to note here, that the dynamics of the effects on women's employment decisions follows the same, but oppositely-signed pattern observed for women's fertility outcomes. I will come back to this when I discuss the potential mechanisms in play to explain women's improved performance in

the labor market.

One of the key advantages of having access to longitudinal data is to provide several estimates of the effect of a certain intervention, on the same variable, at different points of the same individual's life. The results discussed in this section illustrate that static analyses might be misleading. For the two dimensions considered, focusing on only one data point at a given age would have led to very different conclusions depending on the specific time at which the outcomes were measured. On the one hand, if one focuses on outcomes measured at early twenties, one would say that the program had strong positive effects on women's employment and reduced their probability of becoming mothers. On the other hand, if one focuses on outcomes measured at the age of 30, the conclusion would be that the program was ineffective. In both cases, the critical changes in the timing of the events would have remained hidden. While this is an extreme example, it helps showing that the timing at which an outcome is measured is a non-trivial decision. As discussed in Aizer et al. (2022), longer-term analyses necessarily need to be complemented by more exhaustive descriptions of the trajectories of the effects to fully understand how different policies might affect people's lives, and why.

Do changes in transitions have long-lasting consequences?

The fade-out of the effects of *PANES/AFAM-PE* on fertility and employment extensive margin decisions close to the age of 30 might raise concerns that the program only changed the timing of events that would have happened anyway. While mere changes in the timing of the events might already have long-term consequences, in this section, I report additional evidence on whether and how *PANES/AFAM-PE* affected longer-term outcomes. For simplicity, I restrict the analysis to women, who are the only ones affected by the program. For completeness, estimates on men's outcomes are reported in Appendix E.

Table 4 reports the main longer-term estimates. Columns (1) and (2) concentrate on fertility outcomes, with column (1) reporting effects on the probability of having a child, and column (2) on the number of children. The remaining columns focus on labor market outcomes. More specifically, column (3) reports the effect on the probability of ever having an employment spell in the formal labor market. Columns (4) and (5) measure the effects on earnings and employment status during the last full year in which people are observed. Columns (6) and (7) focus on cumulative variables and report effects on total life earnings and number of months employed.

For fertility-related outcomes, the results based on the τ_{FRD} reported in panel a. indicate that the program significantly reduced the number of children women had by an average of 0.304 children (with $\tau_{SRD} = 0.066$, $\bar{Y}^c = 0.99$). The effect on the likelihood of having had any children was not statistically significant. Both together, these result seem to suggest that women might have reacted in the intensive margin of fertility, rather than in the extensive margin. However, the interpretation of these results must be done

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cautiously given that the sample is comprised of women who are, at most, 33 years old when they are last observed, and they still have 10+ years to complete their reproductive cycles. While one could be confident in saying that the program did not affect women's preferences for becoming a mother, we still cannot rule out that the negative longer-term effects on the number of children are simply a mechanical response to start having children at later ages. Unfortunately, this question cannot be addressed currently, and we still need to wait for at least 10 years before drawing stronger conclusions on fertility preferences regarding the number of children.

In terms of labor market outcomes, the program did not change the probability of ever having an employment spell in the formal labor market. However, the program significantly increased the likelihood of being employed ($\tau_{FRD} = 34.9 p.p.$, $\tau_{SRD} = 6.8 p.p.$, $\bar{Y}^c = 39.5$) and boosted last-year earnings ($\tau_{FRD} = 2,857$, $\tau_{SRD} = 567$, $\bar{Y}^c = 3.4$) when women are last observed. In addition, this improved performance in labor market outcomes is also observed when considering the effects on the total number of months employed ($\tau_{FRD} = 37.1$, $\tau_{SRD} = 7.3$, $\bar{Y}^c = 33.7$), and cumulative earnings. As a reference, the total estimated additional earnings due to participating in *PANES/AFAM-PE* is USD 18,723 (with $\tau_{SRD} = 6,840$, $\bar{Y}^c = 22,010$), which is 50% larger than the average total amount of benefits collected by households in the program.

The positive effects on longer-term employment outcomes reported in Table 4 might seem at first glance contradictory with the late-twenties fade-out reported in the previous section. However, it is important to note that Table 4 pools all the individuals in the sample, whereas estimates in the baseline dynamic analysis are restricted to people who are older than a given age (e.g. at the age of 30, estimates correspond to individuals who are 30 years old or older). Hence, the positive effects reported in this section measured when women are last observed, might be explained by: 1) women who have not reached the fade-out stage, or 2) by younger women being more responsive to the program, as suggested in Section 6.2. In any case, the longer cumulative experience and earnings on the labor market do not depend on this distinction, and we can confidently conclude that the program indeed induced longer-term improvements in women's labor market performance.

The fact that women respond more strongly to this type of policy intervention compared to men is not rare for the recent related literature. For instance, for 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 typically stronger for women. In the context of PROGRESA, both Araujo and Macours (2021) and Parker and Vogl (2018) find more pronounced effects on women, although in some cases, the differences are not statistically significant. One recent piece of evidence that 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, this contrasting pattern is not so strong, and 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.

Mechanisms: how does *PANES/AFAM-PE* improves women's labor market outcomes?

As discussed in Section 3, there are several ways in which cash transfers might affect people's fertility, education, and labor market decisions. In this section, I provide evidence on some specific mechanisms that could explain why women who received *PANES/AFAM-PE* improved their labor market outcomes. I focus specifically on labor market outcomes because of their strong correlation with poverty and mobility, which are key to understand whether cash transfers are fulfilling their longer-run goals. It is important to note that the evidence reported in this section should be interpreted only as exploratory since there is no additional source of exogenous variation to identify precisely the causal chain of mechanisms. However, the richness of the data allows me to provide a series of facts that are consistent with a story where both fertility and education explain the presence and the magnitude of women's improvements in the labor market.

1) Effects on fertility play a major role: Figure 4 illustrated the trajectory of the effects measured for ages 15 to 33 for fertility and labor market outcomes separately. For exposition purposes, Figure 5 puts women's estimates reported in in panels a. and b. of Figure 4 together into the same figure. When comparing the age profiles of the effects for each outcome a clear pattern emerges, showing that effects on fertility and labor market outcomes are strongly and inversely correlated. More precisely, when the effects on fertility outcomes are trending downwards, effects on employment outcomes are trending upwards. Then, there is a period in which both remain more or less stable, but precisely at the age of 25 they both jointly start to fade-out.

These patterns are consistent with the idea that women's fertility and labor market participation are mutually exclusive, or at least strongly substitutes. However, by themselves, they do not inform about the direction of the causality. An additional result that might shed some light on this is the consistently null effects observed for men across specifications and outcomes. If cash transfers had direct effects on labor market outcomes, which in turn explain negative effects on fertility, one should expect at least a moderate effect of *PANES/AFAM-PE* on men's labor market participation, experience, or earnings. However, the effects for men are always null. This suggests that the direction of the causality is more likely to be from changes in fertility to changes in employment, and not the other way around. Hence, one plausible hypothesis is that *PANES/AFAM-PE* is improving young women's labor market participation through a postponement of

early-life births.

This finding is in line with studies in Demography that discuss 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).

2) Effects on education also play a role: Table 5 shows information on the effect of the program on women's secondary education enrollment. The information currently available on secondary education outcomes is incomplete and only spans until 2018, with some missing data in between. Hence, these results must be interpreted with caution. Columns (1) through (4) focus on enrollment outcomes: 1) ever enrolled in secondary education institutions, 2) ever enrolled in middle school, 3) ever enrolled in high-school, and 4) number of years enrolled in secondary education. Overall, the results indicate null effects on the extensive margin decisions, but some slightly positive effects on the total number of years enrolled in the education system ($\tau_{FRD} - 0.60$, $\tau_{SRD} = 0.12$, $\bar{Y}^C = 2.68$). Given the lack of information on students' performance, columns (5) to (8) must be thought of as the best attempt to report effects on proxy variables to academic progress. In particular, I report effects on the number of different grades, by level, in which a woman was enrolled, and also the effects on the maximum grade in which she was enrolled. The results suggest some weak positive effects on academic progress when measured as the maximum grade enrolled ($\tau_{FRD} - 0.54$, $\tau_{SRD} = 0.11$, $\bar{Y}^C = 3.8$) or the different grades of high-school enrollment ($\tau_{FRD} - 0.36$, $\tau_{SRD} = 0.07$, $\bar{Y}^C = 1.9$). Even in this context of weak and attenuated effects, women's responses are substantially different compared to men's. As reported in Appendix E, men do not seem to show any change in secondary education related variables as a response to participating in the program.

While weaker and perhaps not as conclusive, the effects reported on women's enrollment in secondary education and academic progress are consistent with the trajectories observed for fertility and labor market outcomes. First, more time spent 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).

3) Reduction in early-life births seems to be a necessary condition, whereas improvements in education levels act as a multiplier: One way to test whether the effects on education and fertility decisions are explaining the improved performance in women's labor market outcomes is to analyze the correlations between these potential effects, in the spirit of mediation analyses. In this section, I show how different the effects on labor market outcomes are, conditioning on women's ex-post early-life pregnancies and education level. The fact that the program affected both fertility and education decisions requires these results to be interpreted as correlational evidence, but they still provides some helpful insights to understand what mechanisms could be at play.

Figure 6 summarizes the main results. Each panel in the figure corresponds to the standardized Fuzzy RD effects (τ_{FRD}) for different labor market outcomes, including anticipated entry to the labor market (measured as the opposite value of age of first employment spell), employment when last observed, total number of months worked, and life-time earnings. Within each panel, I report estimates for three groups of women: 1) women that had early-life births (i.e., before the age of 20), 2) women who did not have a early-life birth and were not enrolled in high-school, 3) women who did not have an early-life birth and were enrolled into high-school. The pattern of the effects shows a clear positive gradient for these three groups. First, the effects on labor market outcomes are consistently null for women who gave birth before the age of 20. Second, both less and more educated women show improved performances in the labor market as a result of participating in *PANES/AFAM-PE*. Third, within women who did not have early-life births, the program seem to induce stronger positive effects on more educated women. The only exception is when looking at total cumulative earnings. In this case, one plausible explanation is that women who were induced to be more educated as a result of the program are being compared with women who were intrinsically more likely to be high-educated, even when not participating in the program. Hence, effects on earnings are more likely to be relatively smaller given that now they are being compared to a better-performing group.

Although with some limitations, as illustrated by the potential issues in the comparison of the effects on cumulative income, the correlational analysis reported here suggests two hypotheses. First, not having an early-life birth seems to be a necessary condition to observed positive effects of *PANES/AFAM-PE* on women's labor market outcomes. Second, conditional on not having an early-life birth, education also seems to play a role. More specifically, more educated women seem to be more positively affected by the program in terms of labor market outcomes. In Appendix E, I report additional evidence

consistent with this hypothesis by replicating the longer-term results reported in Table 4 separately for women with and without early-life births, and for low and high education levels. For completeness I also report how effects on fertility decisions differ by education level.

7 Conclusions

Worldwide, governments spend billions of dollars on social safety net (SSN) policies to reduce poverty and inequality but the academic literature has yet to provide detailed evidence on their dynamic and long-term effects. This paper presents novel evidence of how a large-scale and government-implemented cash transfer program, the Uruguayan *PANES/AFAM-PE*, affected indidivuals' transition to adulthood for people who benefited from it during their childhood. The main findings can be grouped into three. First, the program leads to a 2.2-years delay in women's age at first birth, explained mostly by a fall in teenage or early-life births. Second, the program leads women to a 1.8-years anticipated entry to the formal labor market, which is not explained by a reduction in higher-education participation. The effects on labor market outcomes across all margins are null for men. Third, changes in transitions to adulthood have lasting consequences. By the time they are last observed, treated women have more accumulated experience and earnings in the formal labor market and have fewer children than women who did not participate in the program. I provide suggestive evidence that avoiding early-life births is a necessary condition for observing positive effects on labor market outcomes, whereas education seems to act as a multiplier.

This paper contributes to two main strands of literature. First, it contributes to our understanding of how social safety nets affect people's lives and shows the importance of including a more exhaustive and dynamic perspective to its study (Aizer et al., 2022). The analysis of the age-by-age trajectory of the effects provides a clear example of how a static analyses might be misleading. In particular, I show that opposite conclusions can be drawn from analyzing the effects on the same outcomes but measured at different ages, and how this static approach misses critical changes more related to the timing of certain decisions. This finding resembles the discussion of non-linear trajectories of effects in Almond et al. (2018); Chetty et al. (2011) and highlights that this issue in not necessarily unique of early childhood interventions. Furthermore, the existence of these non-linear patterns of effects is one of the main reasons that justifies the study of the "missing middle" (Almond et al., 2018). In the literature of early childhood interventions, this missing middle refers to the years between early childhood and adulthood in terms of developmental trajectories. My paper provides novel evidence about a different "missing middle".

In addition, this paper presents some methodological improvements with respect to

the existing literature. To my knowledge, this is the first paper that analyzes the effects of a cash transfer program on three significant margins of response (fertility, education, labor market) with a dynamic perspective (i.e., from late teens to adulthood) in a unified setting, using high-quality administrative records with information that varies at the individual-level both for the outcome and participation variables. 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.

Second, this paper also contributes to the literature on gender inequality in the labor market (Altonji and Blank, 1999, Blau and Kahn, 2017, Olivetti and Petrongolo, 2016 Waldfogel, 1998 Kleven et al., 2019; ?), and in particular the relation between teenage fertility, education, and labor market prospects (Kearney and Levine 2014b, 2012). In particular, it provides complementary evidence that highlights how fertility decisions, in particular during the critical adolescent ages, might have long-lasting consequences in terms of labor market participation, experience, earnings, and overall stability.

The evidence reported in this paper has implications for the design, implementation, and evaluation of cash transfer policies. For instance, by the time they are last observed, women have doubled their experience and earnings in the labor market compared to non-participating women and the differential is on average already 40% larger than the amount transferred to the households by the program. This is a critical finding to assess whether cash transfers are worth to be implemented. A stronger involvement with the formal labor market will result in additional tax revenues and will help offset the revenue losses due to negative effects on parental labor supply documented in e.g., Bergolo and Cruces (2021). Another policy implication is that cash transfers may play a key role in reducing labor market gender gaps, even when they are not specifically designed for this purpose. For instance, they could 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 contexts, such as the Uruguayan, where the motherhood penalty is larger for low-income mothers (Querejeta 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 plays a key role. Given that the motherhood penalty explains a sizable share of the labor market gender gap, policies that promote changes in the timing of events such as a postponement of early-life births might be particularly effective in making labor market outcomes more equitable (Bratti, 2015; Miller, 2011).

At this point, it is important to briefly discuss three key questions that remain unanswered, but are still extremely relevant for the study of this topic. The first one corresponds to the potential inter-generational effects of cash transfers on welfare participation (in the spirit of the welfare culture argument). 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 2019 and prevent me of providing precise estimates on this, yet. However, this constitutes a major avenue for future research.

The second question regards to the main mechanisms behind changes in fertility and education decisions during the teenage years. As discussed in Section 3, there are several potential mechanisms that could explain the negative effects of *PANES/AFAM-PE* on teenage pregnancies. For instance, changes could be due to an increased demand for contraceptives due to the income effect, which in turn tend to reduce teenage pregnancies Kearney and Levine, 2009; Bailey, 2006; Lundberg and Plotnick, 1995. In addition, it is important to consider how *PANES/AFAM-PE* might have interacted with other public institutions or policy changes. For instance, access to the cash transfer program might have connected individuals better to the public health network (Ceni et al., 2021), or to institutions providing access to abortion (Cabella and Velázquez, 2022). Additional explanations relate to improvements in household climate that reduce the benefits of forming an independent household, increase in supervised time due to reductions in parental labor supply (Bergolo and Cruces, 2021), or changes in expectations about future wages (Araujo and Macours, 2021).

Finally, it is still early enough to provide more definite answers on the long-run effects on women's fertility preferences, with its corresponding welfare implications. While the postponement of fertility has improved women's labor market outcomes, delays in fertility might entail higher expected pecuniary and psychological costs of pregnancies (Schmidt et al., 2012; Gustafsson, 2001). In addition, one must consider how changes in the timing of the first birth might affect the realization of desired fertility plans. In particular, 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 results discussed in this paper show that postponed fertility comes from early-life births and therefore the associated costs are not as significant as they would be if the delay corresponded to later ages. Furthermore, such early-life pregnancies also have some additional non-pecuniary costs

that must be considered in an overall welfare assessment, since they are typically reported as unwanted pregnancies. 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. This has strong implications for the design of the early care education system or parental leave policies.

Tables

Table 1: Descriptive Statistics: Individual Characteristics

	Full Sample		Balanced Sample	
	Full Support (1)	Opt. Bandwidth (2)	Full Support (3)	Opt. Bandwidth (4)
a. Individual Characteristics				
Female (%)	49.56	49.98	49.71	50.79
	(50.00)	(50.00)	(50.00)	(49.99)
Age of 1st application	10.00	10.26	15.29	15.38
	(4.25)	(4.27)	(1.52)	(1.50)
Age at 31 Dec. 2021	26.18	26.40	31.49	31.54
	(4.24)	(4.26)	(1.51)	(1.50)
Number of app. forms	3.29	3.09	3.30	3.02
	(1.39)	(1.33)	(1.44)	(1.39)
b. Reference Form				
Std. Score	0.16	-0.00	0.15	-0.00
	(0.22)	(0.03)	(0.22)	(0.03)
App. Accepted (%)	75.82	52.87	73.57	50.74
	(42.82)	(49.92)	(44.10)	(50.00)
Capital City (%)	27.42	15.47	27.33	15.75
	(44.61)	(36.16)	(44.57)	(36.43)
c. Household characteristics (ref. form)				
Single Parent (%)	45.27	50.07	47.67	53.20
	(49.78)	(50.00)	(49.95)	(49.90)
Number of members	4.96	4.29	5.11	4.34
	(2.03)	(1.87)	(2.06)	(1.90)
Avg. age	21.14	23.37	23.47	26.11
	(7.48)	(8.28)	(7.50)	(8.08)
Household head: Employed (%)	64.55	63.85	65.29	64.25
	(47.84)	(48.04)	(47.61)	(47.93)
Household Head: Ed. years	6.28	6.57	6.10	6.31
	(2.35)	(2.42)	(2.39)	(2.45)
Household: income	209.11	233.26	222.92	241.24
	(193.69)	(197.08)	(200.95)	(200.34)
Observations	279,024	70,401	80,817	21,787

Notes: Table 1 reports a series of descriptive statistics for the *full sample* and *balanced sample* as defined in Section 4.2. Columns (1) and (2) are focused on individuals that were younger than eighteen years old when their parents applied to *PANES/AFAM-PE* for the first time, and had at least nineteen years old in December, 2021. Columns (3) and (4) are based on the *balanced sample* used for the robustness analysis that keeps the composition of the sample constant across estimates (i.e., at least 30 years old in December, 2021). Columns (1) and (3) report statistics that describe individuals across the full support of the running variable, while columns (2) and (4) report statistics corresponding to individuals that are within 5p.p. of the eligibility threshold. 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: Continuity at Baseline

	Ineligible Intercept	Eligible Intercept	Difference (2) - (1)	p-value Robust	Anderson q-value
	(1)	(2)	(3)	(4)	(5)
Predicted Eligibility	0.62	0.61	-0.006	0.264	1.000
Female	48.12	47.31	-0.804	0.314	1.000
Number of Apps.	2.74	2.76	0.020	0.740	1.000
HHH - Employed	63.13	63.82	0.696	0.553	1.000
HHH - Years of Educ.	7.23	7.02	-0.206	0.033	0.199
HHH - Income	152.59	152.51	-0.077	0.901	1.000
HH - Avg. Age	19.60	20.84	1.244	0.000	0.002
HH - Baseline App. Before Sep.2005	6.05	5.13	-0.926	0.446	1.000
HH - Number of people	4.00	3.98	-0.020	0.886	1.000
HH - Single Parent	56.14	53.97	-2.171	0.235	1.000
HH - Mother's Age when First born	22.39	22.52	0.124	0.576	1.000

Notes: This table reports estimates on the continuity of baseline covariates at the eligibility threshold. Estimates are based on the reduced form specification included in equation 3, where each row represents a different dependent variable. The model is estimated including all fixed effects but no additional covariates. Column (1) depicts the intercept for the local linear regression fitted in the ineligible side of the centered threshold (i.e., $z < 0$), while column (2) does the same for the local linear regression fitted in the eligible side of the threshold. Column (3) is simply the difference between columns (1) and (2). Columns (4) and (5) report simple, and multiple-hypothesis-adjusted p-values. RDD estimates are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial. Standard errors, reported in parentheses, are robust and clustered at the household level.

Table 3: Summary of Effects on Transitions to Adulthood

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	2.209***	10.923	-1.723***	41.952***	-1.067**	0.035	-0.260	12.027
	(0.572)	(6.950)	(0.536)	(10.118)	(0.451)	(3.614)	(0.385)	(9.921)
Robust p-value	0.000	0.107	0.001	0.000	0.017	0.866	0.468	0.277
Adj. Robust Q-value	0.001	0.094	0.003	0.001	0.022	0.481	0.251	0.207
Mean Baseline Outcome	19.87	11.12	20.83	29.57	22.73	5.11	19.71	48.37
Effect Size (%)	11.12%	98.23%	-8.27%	141.89%	-4.70%	0.69%	-1.32%	24.87%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.513***	2.182	-0.400***	8.389***	-0.339**	0.009	-0.071	2.977
	(0.129)	(1.383)	(0.122)	(1.940)	(0.142)	(0.911)	(0.105)	(2.454)
Robust p-value	0.000	0.119	0.001	0.000	0.018	0.866	0.465	0.282
Mean Baseline Outcome	19.87	11.12	20.83	29.57	22.73	5.11	19.71	48.37
Effect Size (%)	2.58%	19.62%	-1.92%	28.37%	-1.49%	0.17%	-0.36%	6.15%
c. First Stage								
Elig. 1st. App.	0.232***	0.200***	0.232***	0.200***	0.318***	0.252***	0.274***	0.247***
	(0.013)	(0.014)	(0.015)	(0.014)	(0.017)	(0.014)	(0.014)	(0.017)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.71	0.74	0.69	0.74	0.61	0.71	0.70	0.72
Effect Size (%)	32.74%	26.98%	33.44%	27.00%	52.45%	35.25%	39.26%	34.58%
Selection of Bandwidth:								
Opt. Bandwidth	[0.031]	[0.020]	[0.027]	[0.020]	[0.049]	[0.022]	[0.027]	[0.015]
Effective Obs.	10,164	10,843	9,306	10,961	7,222	12,163	10,685	8,424

Notes: Robust and household-level-clustered standard errors are in parentheses (Calonico et al., 2014) Statistical significance is computed based on the robust p-value and **, *, and indicate significance at 1, 5, and 10, respectively. Panel a. reports the Fuzzy RDD estimates (τ_{FRD}) based on 2SLS estimation of equation 1. Panel b. depicts the reduced form effects (τ_{SRD}) based on a Sharp RDD design based on equation 3. Panel c. reports the first-stage estimates based on equation 2. Each panel includes the corresponding τ estimate, the robust and clustered standard errors, the individual p-value, and a p-value adjusted by multiple hypotheses based on Anderson (2008). In addition, the table also reports the mean baseline outcome, computed as the average value of the outcome variable for people included in the regression who were in the ineligible side of the threshold (i.e. $z < 0$). The last two rows of the table report the optimal bandwidth used for estimation, and the effective number of observations. Each column reports the results from a separate local linear regression on different outcomes and population. Columns (1) through (4) correspond to estimates on women, whereas columns (5) through (8) correspond to men. Columns (1) and (5) report the effect of the program on a continuous variable that measures the age in which the individual had their first child. Columns (2) and (6) report the effects on enrollment in higher-education. This variable takes the value of 100 for people who ever enrolled in the largest public university or in tertiary-level technical education programs, and 0 otherwise. Columns (3) and (7) report estimates for age of the first employment spell. Employment spells are considered only if they are comprised of four consecutive months of work in the same firm, to avoid including temporary jobs such as summer jobs. Finally, columns (4) and (8) report the effect on a composite index of the three variables that aims to measure transitions that are more market-oriented as opposed to stay-at-home/home care-oriented. It takes the value of 100 if the individual did not have an early-life birth (i.e., before the age of 20) *and* had an early entry to the labor market (before the age of 20) or got enrolled in a tertiary-education institution. RDD estimates are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial.

Table 4: Summary of Effects: Lasting Consequences

	Fertility		Labor Market				
	Had a Child (1)	Number of Children (2)	Employed (Ever) (3)	Employed (Last Observed) (4)	Yearly Earnings (Last Observed) (5)	Months Employed (Cumulative) (6)	Total Earnings (Cumulative) (7)
a. Fuzzy RDD Estimate							
Ever Treated Before 18	-9.073	-0.304**	8.237	34.489***	2.857**	37.123***	18.723***
	(7.122)	(0.124)	(9.318)	(11.316)	(1.311)	(10.218)	(6.877)
Robust p-value	0.266	0.022	0.361	0.002	0.023	0.000	0.007
Adj. Robust Q-value	0.098	0.019	0.116	0.007	0.019	0.001	0.012
Mean Baseline Outcome	57.98	0.99	61.92	39.54	3.36	33.68	22.01
Effect Size (%)	-15.65%	-30.83%	13.30%	87.23%	85.05%	110.23%	85.05%
b. Sharp RDD Estimate							
Elig. 1st. App.	-1.915	-0.066**	1.658	6.844***	0.567**	7.319***	3.729***
	(1.500)	(0.027)	(1.874)	(2.167)	(0.255)	(1.919)	(1.338)
Robust p-value	0.272	0.020	0.378	0.002	0.025	0.000	0.007
Mean Baseline Outcome	57.98	0.99	61.92	39.54	3.36	33.68	22.01
Effect Size (%)	-3.30%	-6.74%	2.68%	17.31%	16.87%	21.73%	16.94%
c. First Stage							
Elig. 1st. App.	0.211***	0.219***	0.201***	0.198***	0.198***	0.197***	0.199***
	(0.011)	(0.010)	(0.013)	(0.015)	(0.015)	(0.017)	(0.014)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.74	0.73	0.74	0.74	0.74	0.74	0.74
Effect Size (%)	28.69%	29.78%	27.15%	26.86%	26.91%	26.67%	26.90%
Selection of Bandwidth:							
Opt. Bandwidth	[0.027]	[0.034]	[0.021]	[0.019]	[0.018]	[0.015]	[0.019]
Effective Obs.	14,879	19,532	11,440	10,115	10,030	8,304	10,499

Notes: Robust and household-level-clustered standard errors are in parentheses (Calonico et al., 2014). Statistical significance is computed based on the robust p-value and **, *, and indicate significance at 1, 5, and 10, respectively. Panel a. reports the Fuzzy RDD estimates (τ_{FRD}) based on 2SLS estimation of equation 1. Panel b. depicts the reduced form effects (τ_{SRD}) based on a Sharp RDD design based on equation 3. Panel c. reports the first-stage estimates based on equation 2. Each panel includes the corresponding τ estimate, the robust and clustered standard errors, the individual p-value, and a p-value adjusted by multiple hypotheses based on Anderson (2008). In addition, the table also reports the mean baseline outcome, computed as the average value of the outcome variable for people included in the regression who were in the ineligible side of the threshold (i.e. $z < 0$). The last two rows of the table report the optimal bandwidth used for estimation, and the effective number of observations. Each column reports the results from a separate local linear regression on different women's outcomes. Columns (1) and (2) correspond to estimates on women's fertility outcomes where column (1) depicts effects on the probability of having a child and column (2) focus on the number of children. Both variables are measured at last observed (December, 2021). Columns (3) through (7) depict the effects of the program on labor market outcomes. Column (5) reports estimates on a variable that takes the value of 100 if the woman ever had an employment spell. Columns (4) estimates the effects on employment measured at the last full year when a woman was last observed. This variable takes the value of 100 if the woman was employed and 0 otherwise. Column (4) measures the yearly earnings at the same moment as column (3). In this case, yearly labor market earnings are expressed in thousands of PPP, Jan. 2008, dollars. Finally, columns (7) and (8) focus on cumulative outcomes. Column (7) reports the effects on a variable that counts the total number of months in which a woman has been employed throughout her life, whereas column (7) does the same for cumulative earnings, expressed in thousands of PPP, Jan. 2008, dollars. RDD estimates are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial.

Table 5: Effects on Secondary Education Outcomes - Women

	Enrollment				Academic Progress			
	Ever Enrolled in Secondary (1)	Ever Enrolled in Middle School (2)	Ever Enrolled in High School (3)	Years Enrolled in Secondary (4)	Number of $\neq$ Grades in Middle School (5)	Number of $\neq$ Grades in High School (6)	Number of $\neq$ Grades in Secondary (7)	Max. Grade Enrolled (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	-1.082	-1.710	11.874	0.598*	0.219	0.359*	0.602*	0.541*
	(7.933)	(8.305)	(8.877)	(0.272)	(0.153)	(0.198)	(0.306)	(0.274)
Robust p-value	0.775	0.775	0.220	0.064	0.178	0.098	0.072	0.053
Adj. Robust Q-value	0.410	0.410	0.238	0.238	0.238	0.238	0.238	0.238
Mean Baseline Outcome	85.79	75.87	54.46	2.68	1.22	0.70	1.90	3.80
Effect Size (%)	-1.26%	-2.25%	21.80%	22.32%	17.98%	51.52%	31.63%	14.25%
b. Sharp RDD Estimate								
Elig. 1st. App.	-0.214	-0.340	2.470	0.124*	0.047	0.072	0.123*	0.108**
	(1.571)	(1.649)	(1.836)	(0.056)	(0.032)	(0.039)	(0.061)	(0.054)
Robust p-value	0.778	0.779	0.232	0.068	0.179	0.108	0.079	0.049
Mean Baseline Outcome	85.79	75.87	54.46	2.68	1.22	0.70	1.90	3.80
Effect Size (%)	-0.25%	-0.45%	4.54%	4.62%	3.84%	10.37%	6.44%	2.84%
c. First Stage								
Elig. 1st. App.	0.198***	0.199***	0.208***	0.207***	0.214***	0.201***	0.203***	0.200***
	(0.016)	(0.015)	(0.012)	(0.012)	(0.011)	(0.013)	(0.013)	(0.010)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.74	0.74	0.74	0.74	0.73	0.74	0.74	0.76
Effect Size (%)	26.70%	26.84%	28.16%	28.03%	29.08%	27.13%	27.51%	26.34%
Selection of Bandwidth:								
Opt. Bandwidth	[0.016]	[0.017]	[0.024]	[0.024]	[0.029]	[0.021]	[0.022]	[0.038]
Effective Obs.	8,874	9,325	13,545	13,203	16,286	11,418	12,221	16,201

Notes: Robust and household-level-clustered standard errors are in parentheses (Calonico et al., 2014). Statistical significance is computed based on the robust p-value and **, *, and indicate significance at 1, 5, and 10, respectively. Panel a. reports the Fuzzy RDD estimates (τ_{FRD}) based on 2SLS estimation of equation 1. Panel b. depicts the reduced form effects (τ_{SRD}) based on a Sharp RDD design based on equation 3. Panel c. reports the first-stage estimates based on equation 2. Each panel includes the corresponding τ estimate, the robust and clustered standard errors, the individual p-value, and a p-value adjusted by multiple hypotheses based on Anderson (2008). In addition, the table also reports the mean baseline outcome, computed as the average value of the outcome variable for people included in the regression who were in the ineligible side of the threshold (i.e. $z < 0$). The last two rows of the table report the optimal bandwidth used for estimation, and the effective number of observations. Each column reports the results from a separate local linear regression on different women's outcomes. Columns (1) through (2) focus on women's secondary enrollment decisions. In particular, column (1) depicts the effects of the program on a variable that takes the value of 100 if the woman was ever enrolled into any grade of secondary education, either in the traditional sector or vocational/technical schools. Columns (2) and (3) do the same but separate the secondary education level into middle school (grades 1-3) and high school (grades 4-6). Column (4) reports estimates on a variable that adds the number of years in which women were enrolled into secondary education programs. Columns (5) through (8) report estimates on proxies of academic progress. Column (5) takes values between 0 and 3 and counts the number of different grades that women were enrolled to in middle-school. Column (6) does the same for high-school grades. The variable used for Column (7) takes values between 0 and 6 and is defined as the sum of the variables used in columns (5) and (6). Finally, column (8) takes values between 0-6 and indicates the maximum grade in which women were enrolled in the secondary education system. RDD estimates are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial.
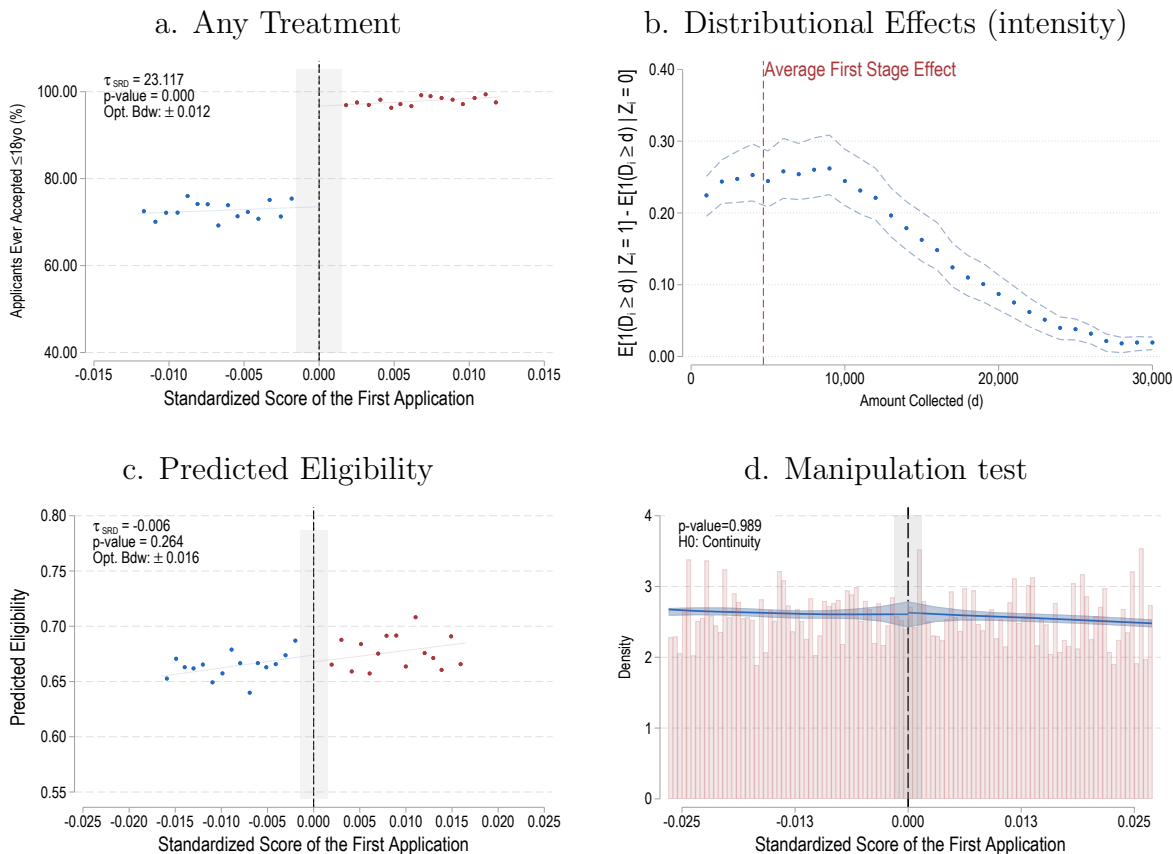
Figures

Figure 1: First Stage and Validity of the RDD

a. Any Treatment

b. Distributional Effects (intensity)

c. Predicted Eligibility

d. Manipulation test

Notes: This figure reports a series of tests on the validity of the RDD design. Panel a. depicts 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 *full sample* as defined in Section 5. Following Calonico et al. (2019) the optimal bandwidth is selected by minimizing the mean squared error (MSERD) and based on rdrobust default options: local linear regressions using triangular weights. Average acceptance rates are grouped in 20 quantile-spaced bins at each side of the threshold. 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 optimal bandwidth, and the continuity test p-value. Robust standard errors are clustered at the household level. Panel b. illustrates that the first stage estimates average differences in treatment intensity from across the distribution as in Rose and Shem-Tov (2021). Each bin represents the local change in the probability of collecting $D \geq d$ from the cash transfer program during the whole period. This estimate is obtained replicating equation 2 on values ranging from 1,000 to 30,000 of the following variable: $E[1(D_i \geq d)|Z_i = 1] - E[1(D_i \geq d)|Z_i = 0]$. These estimates are based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial. Panel c provides an indirect tests about the validity of the continuity assumption. In the y-axis I report the local average of a predicted eligibility score. This score is computed based on a probit model that uses first-time eligibility ($\mathbb{1}(Z_i^{1st} > 0)$) as the dependent variable and all other observed baseline characteristics reported in Table 1 as the independent variables. All methodological decisions regarding optimal bandwidth selection, kernel function, selection of bins, and reported statistics explained for the construction of the figure included in panel a., apply here too. Panel d. provides an illustration of a continuity test of Z_i^{1st} at the eligibility threshold as proposed by Cattaneo et al. (2018). Parameter selection is based on the default options in the *rddensity* Stata command and the p-value of the continuity test is provided as a note in the upper-left corner. The sample used corresponds to the *full sample* as defined in in Section 5.

Figure 2: Summary of Reduced Form Estimates - Women

a. Age of First Birth

$\tau_{SRD} = 0.513$
p-value = 0.000
Opt. Bdw: ± 0.031

b. Ever Enrolled in Higher Education

$\tau_{SRD} = 2.182$
p-value = 0.119
Opt. Bdw: ± 0.020

c. Age of First Employment Spell

$\tau_{SRD} = -0.400$
p-value = 0.001
Opt. Bdw: ± 0.027

d. Market-oriented summary Index

$\tau_{SRD} = 8.389$
p-value = 0.000
Opt. Bdw: ± 0.020
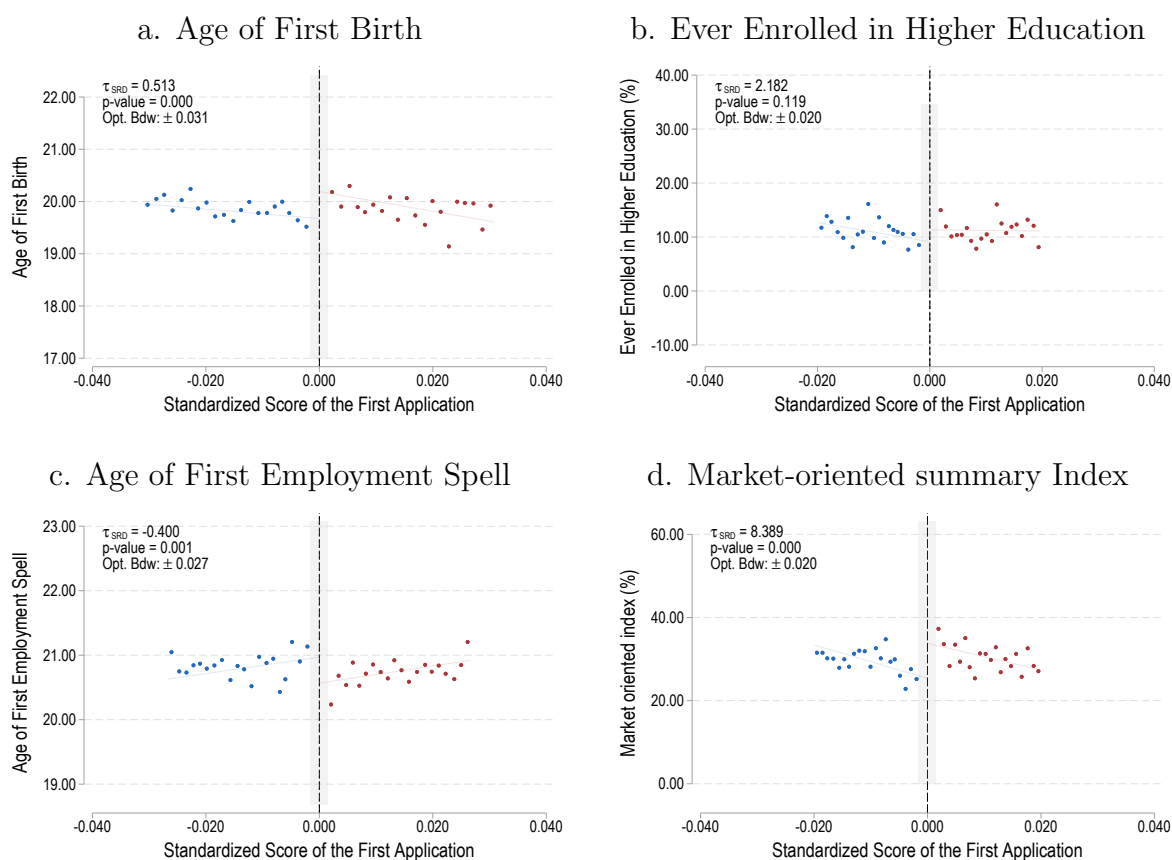
Notes: Panels a. through d. depict the reduced form effects on women's four main outcome variables defined in Section 6.2. Panel a. corresponds to estimates of the effect of the program on a continuous variable that measures the age in which the individual had their first child. Panel b. reports the effects on enrollment in higher-education. This variable takes the value of 100 for people who ever enrolled in the largest public university or in tertiary-level technical education programs, and 0 otherwise. Panel c. reports estimates for age of the first employment spell. Employment spells are considered only if they are comprised of four consecutive months of work in the same firm, to avoid including temporary jobs such as summer jobs. Finally, panel d. reports the effect on a composite index of the three variables that aims to measure transitions that are more market-oriented as opposed to stay-at-home/home care-oriented. The optimal bandwidth used in each figure is computed in the Fuzzy RDD estimation reported in Table 3. For the other elements in the figure, default options are selected: local linear regressions and triangular weights. Average outcomes are computed within each one of the 20 quantile-spaced bins at each side of the threshold. In addition, the figure reports the point estimates of the local differences just at the threshold (τ_{SRD}), the optimal bandwidth, and the continuity test p-value.

Figure 3: Summary of Reduced Form Estimates - Men

a. Age of First Birth

$\tau_{SRD} = -0.339$
p-value = 0.018
Opt. Bdw: ± 0.049

b. Ever Enrolled in Higher Education

$\tau_{SRD} = 0.009$
p-value = 0.866
Opt. Bdw: ± 0.022

c. Age of First Employment Spell

$\tau_{SRD} = -0.071$
p-value = 0.465
Opt. Bdw: ± 0.027

d. Market-oriented summary Index

$\tau_{SRD} = 2.977$
p-value = 0.282
Opt. Bdw: ± 0.015

Notes: Panels a. through d. depict the reduced form effects on men's four main outcome variables defined in Section 6.2. Panel a. corresponds to estimates of the effect of the program on a continuous variable that measures the age in which the individual had their first child. Panel b. reports the effects on enrollment in higher-education. This variable takes the value of 100 for people who ever enrolled in the largest public university or in tertiary-level technical education programs, and 0 otherwise. Panel c. reports estimates for age of the first employment spell. Employment spells are considered only if they are comprised of four consecutive months of work in the same firm, to avoid including temporary jobs such as summer jobs. Finally, panel d. reports the effect on a composite index of the three variables that aims to measure transitions that are more market-oriented as opposed to stay-at-home/home care-oriented. The optimal bandwidth used in each figure is computed in the Fuzzy RDD estimation reported in Table 3. For the other elements in the figure, default options are selected: local linear regressions and triangular weights. Average outcomes are computed within each one of the 20 quantile-spaced bins at each side of the threshold. In addition, the figure reports the point estimates of the local differences just at the threshold (τ_{SRD}), the optimal bandwidth, and the continuity test p-value.

Figure 4: Changes in the Timing of the Events: Age by Age Effects
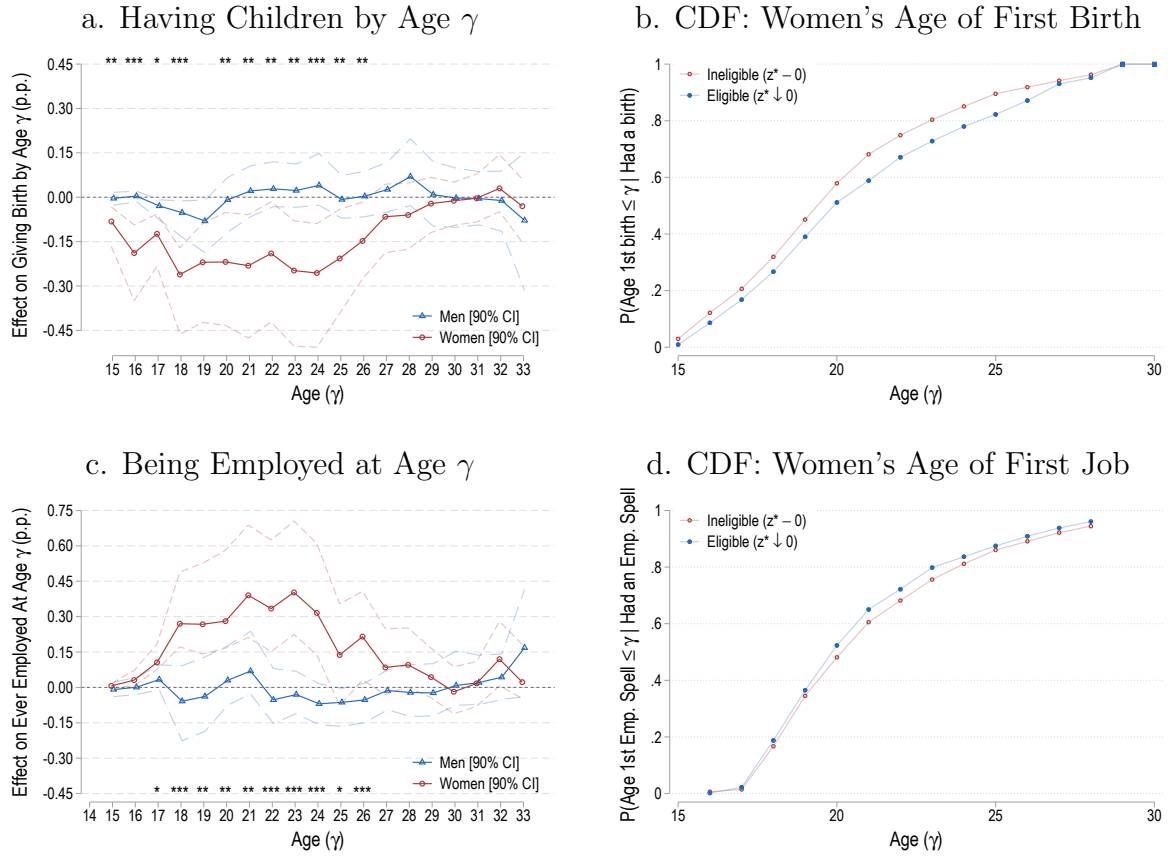
Notes: Panels a. through d. summarize the dynamics of the effects of *PANES/AFAM-PE* on fertility and labor market participation decisions. In Panel a, I report the τ_{FRD} estimates of *PANES/AFAM-PE* on the probability of having had a child measured by different ages (γ) represented in the x-axis and ranging between 15 and 33 years old. The y-axis shows the estimated effect measured in percentage points (p.p.). These estimates are based on equation 1 and are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial. The dependent variable takes the value of 1 if an individual has had a child at a given age, and 0 otherwise. For instance, the coefficient reported for $\gamma = 25$ corresponds to the effect of *PANES/AFAM-PE* on the probability that an individual has had their first child at or before the age of 25. The effects are estimated separately for men and women (including 90% robust confidence intervals), with effects on men depicted in blue and effects on women depicted in red. Estimates are conducted using the *dynamic sample*. This means that estimates for a given age γ are restricted to individuals who are γ years old or older. In panel c. I report the same estimates but using a dummy variable that takes the value of 1 an individual had a four-month employment spell at a given age. Panels b. and d. depict the cumulative distribution function for women's ages of first birth and first employment spell, respectively. CDFs are reported separately for eligible and ineligible individuals. Each value is obtained as the intercept of the fitted polynomial regression at each side of the threshold for the reduced form estimates (equation 3) conducted separately at each age. Hence, these results must be interpreted as reduced form effects. Since these figures represent the CDFs, women included in panel b. estimates are restricted to women who have had a child by age 29. Analogously, women included in panel d. analysis are restricted to those who by the age of 29 had had an employment spell.

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Figure 5: Correlation Between Changes in Fertility and Employment Decisions
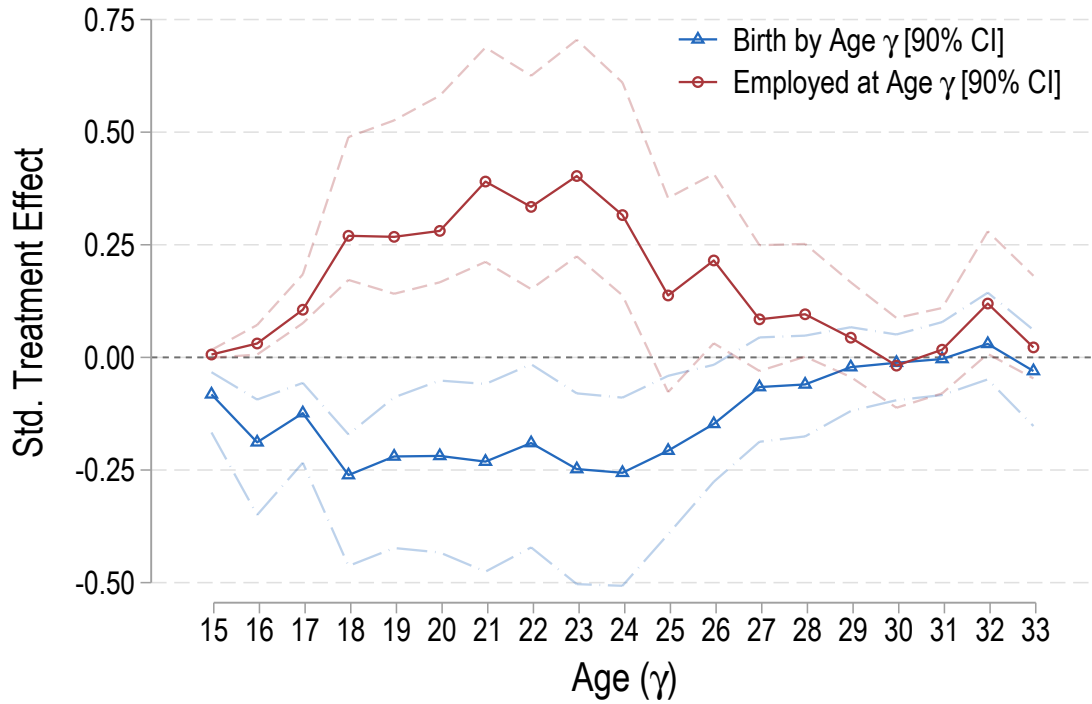
Notes: This figure combines women's dynamic effects depicted in panels a. and c. of Figure 4 for exposition purposes. On the one hand, depicted in blue, I report the τ_{FRD} estimates of *PANES/AFAM-PE* on the probability of having had a child measured by different ages (γ) represented in the x-axis and ranging between 15 and 33 years old. The y-axis shows the estimated effect measured in percentage points (p.p.). These estimates are based on equation 1 and are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial. The dependent variable takes the value of 1 if an individual has had a child at a given age, and 0 otherwise. For instance, the coefficient reported for $\gamma = 25$ corresponds to the effect of *PANES/AFAM-PE* on the probability that an individual has had their first child at or before the age of 25. Estimates are conducted using the *dynamic sample*. This means that estimates for a given age γ are restricted to individuals who are γ years old or older. On the other hand, depicted in red, I report the same estimates but using a dummy variable that takes the value of 1 an individual had a four-month employment spell at a given age.

Figure 6: Labor Market Effects By Education and Fertility
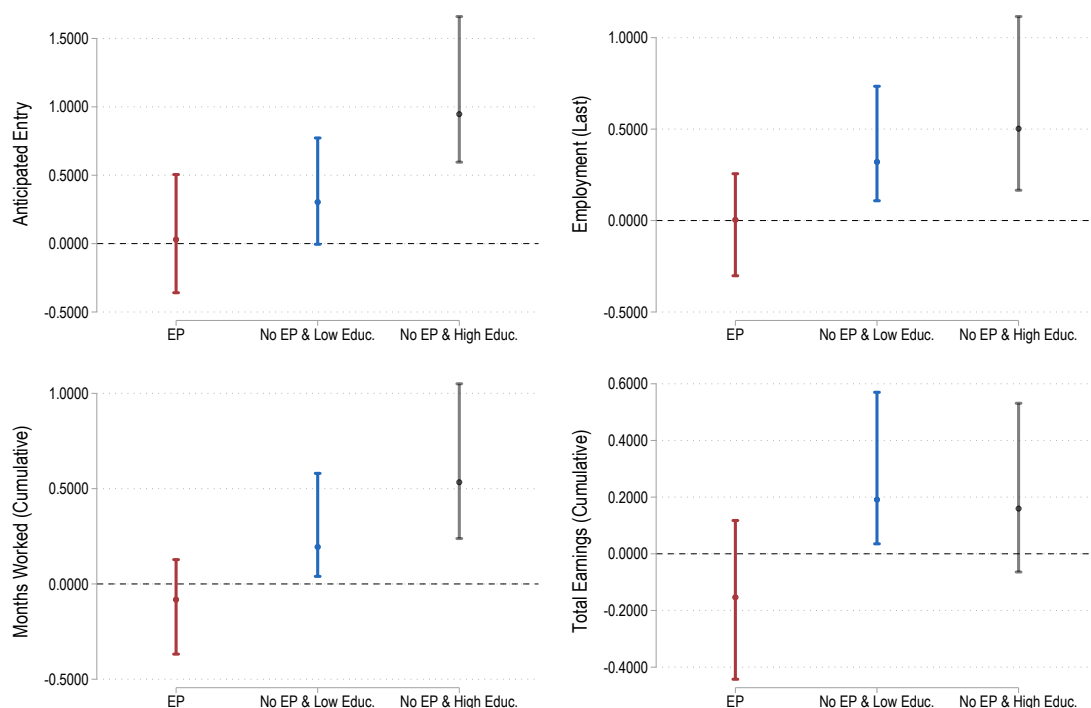
Notes: Each panel in the figure corresponds to the standardized Fuzzy RD effects (τ_{FRD}) for different women's labor market outcomes. Panel a. corresponds to *minus* the age of the first employment spell, as defined in column (3), Table 3. Panels b, c, and d, correspond to variables reported in columns 4, 6, and 7 in Table 4. Within each panel, I report estimates for three groups of women: 1) in red, women that had early-life births (i.e., before the age of 20), 2) in blue, women who did not have a early-life birth and were not enrolled in high-school, 3) in gray, women who did not have an early-life birth and were enrolled into high-school. Statistical significance and confidence intervals are computed based on the robust estimators. Estimates are conducted on the *full sample* and are based on 2SLS estimation of equation 1 and are obtained based on the following setup: MSERD optimal bandwidth, triangular kernel function, linear local polynomial.

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Online Appendix

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

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A Further Details on the Institutional Background

Uruguay's Background and Comparison to Other Countries

In this section, I compare Uruguay's socioeconomic indicators relative to various Latin American countries and other OECD countries. Compared to other Latin American countries, Uruguay has a GDP PPP of $23,585, only below Chile ($25,526) and Argentina ($23,290). In terms of Human Development Index (HDI) Uruguay's is in the group of countries classified as having a very-high index (0.816). It ranks lower than Chile (0.849) but higher than most other Latin American nations. However, it is still has a HDI that is substantially lower when compared to other developed countries, such as Sweden (0.933) and the United States (0.925).

In terms of tax revenue as a percentage of GDP, Uruguay (29.17%) has the largest share among Latin American countries, indicating a relatively high state capacity and a strong presence of the public sector in the economy. This is even more striking, when considering that the share of tax revenues in Uruguay is similar to in Spain's (34.43%) and higher than in the United States (24.33%)

Despite doing relatively well in terms of GDP, HDI, and state capacity, Uruguay's lagging indicators with respect to other developed countries are clearly exposed when considering lower secondary education completion rates and adolescent fertility. First, the completion rate for lower secondary education in Uruguay is low: 56.83%. While this is comparable to Argentina (57.16%), it is significantly lower than Chile (79.60%), and even more compared to OECD average (84.24%) and countries like Sweden (89.63%) and the United States (96.03%). Second, Uruguay's adolescent fertility rate (58.24 per 1000) is lower than Colombia (74.14) and Mexico (59.45), but higher than Chile (46.14) and Costa Rica (52.12). When compared to OECD countries, differences are much larger with rates being drastically higher than in Spain (7.49), Sweden (5.64), and the United States (18.56). The poor performance across these indicators is even more worrisome considering the relatively high GDP.

Overall, Uruguay can be considered as a relatively advanced Latin American country but lags behind OECD and other developed nations across key indicators related to education and fertility outcomes.

PANES: Plan de Asistencia Nacional a la Emergencia Social

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.

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.[28]

The base cash transfer consisted of around USD 133.[29] In addition, a complementary transfer that ranged between USD 29 and USD 78 was provided to households with under-age children (70% of the participant households). Overall, the cash transfer represented between 30-50% 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 187,727, corresponding to 679,077 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

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

[29]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.

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
- 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).

AFAM-PE: Asignaciones Familiares - Plan de Equidad

AFAM-PE is the name given to *PANES* after its expansion and re-branding in January 2008. While *PANES* was conceived as a temporary program, *AFAM-PE* expanded *PANES*' benefits to a larger share of the population and was permanently established as one of the main components of the social safety net.

In practice, *AFAM-PE* was implemented as an expansion of *PANES*. The total number of applications – until December, 2017 - was 679,477, corresponding to about 1,487,920 different individuals. This represents a substantial increase compared to the population covered by *PANES* both because the program increased its coverage, but also because it was in place for longer. 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 an eligibility requirement, while PANES did not include any restriction in this regard. The second was the change in the eligibility threshold which became more lenient. Finally, the formula used to define the transfer amount was also 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 June, 2013 the government started to require household to present proof that they were actually fulfilling the requirements. Just as an example, in June, 2013 more than 30,000 children were dismissed 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.

Figure A.1: Description of the Program

PHASE I (PANES) April 2005 – December 2007	TRANSITION	PHASE II (AFAM-PE) January 2008 – present
CONDITIONS TO BE ACCEPTED: 1. Income test 2. Predicted income score	Applicants from PANES (accepted and rejected) that met the new conditions were automatically enrolled in AFAM-PE	**CONDITIONS TO BE ACCEPTED:** 1. Income test 2. Predicted income score (More generous threshold than PANES) 3. Presence of children under 18 in the household
TRANSFER AMOUNT: USD 133 + complement (May, 2006, 70% of HH) between USD 29 and USD 78 Transfer to HH with 3 kids = USD 211		**TRANSFER AMOUNT:** $57 * (\#Kids\ 0\ to\ 17)^{0.6} + \$24 * (Kids\ High\ School)^{0.6}$ Transfer to HH with 3 young kids = 110.19 Transfer to HH with 3 HS kids = 156.59
CONDITIONS TO STAY: 1. Monthly income check – enforced 2. Education and health checks requirement - not enforced		**CONDITIONS TO STAY:** 1. Monthly income check – enforced 2. Education and health checks requirement – enforced since 2013

Total application forms: 187,639
Accepted: 102,367 (54.6%)
Rejected: 85,272 (45.4%)

Total application forms: 559,565 by Dec. 2017
Accepted: 499,277 (75.8%)
Rejected: 135,445 (24.2%)

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

B Data Construction and Methodological Decisions

PANES/AFAM-PE records

Application and participation records contain form-level information about successful and unsuccessful applications to PANES/AFAM-PE between April 2005 and December 2019. Information comes from a detailed socio-demographic questionnaire implemented by program officials to applicant households. The information collected in this questionnaire was used to compute the poverty score that defines eligibility to participate in the program. The following Table describes the raw variables included in these administrative records.

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

PANES participation records contain information of 187,727 application forms. Of these, 102,436 were accepted applications and 85,291 were rejected. In *AFAM-PE*, the total number of application forms is 679,477. This also includes households that were transferred automatically from *PANES* to *AFAM-PE*. Of the 679,477 *AFAM-PE* application forms, 480,517 (70.7%) were accepted and 198,960 (29.3%) were rejected. *AFAM-PE* individual level data contains information of 1,487,920 individuals

In the process of cleaning the data some application forms were discarded due to different reasons. First, XXX application forms in *PANES* 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 *AFAM-PE*, I exclude XXX forms for similar reasons. In addition, there are 29,101 duplicated *AFAM-PE* forms that show up both in the accepted and rejected applications datasets. Second, XXX 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 XXX forms. As to identical application forms with different identification numbers and rejection reason, I collapse the reasons and then drop XXX duplicated forms. Finally, for the remaining same day applications I drop XXX forms keeping the application form with the largest score and another XXX forms that have multiple applications the same day with different form number but the same score. The resulting number of forms is 827,144 and the corresponding number of unique individuals is 1,558,972.

Birth Records

The birth certificates records come from the Ministry of Health and include the universe of births in the country (851,232) between 2003 and 2021. 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 weight and gestation weeks. The number of mothers included in the dataset is 531,695 and 57.3% of them only gave birth to a single child, 30.2% gave birth two children, 9.0% gave birth three children and the remaining 3.5% 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.[30]

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 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 the Ministry of Labor and contain information about registered employment between 2005-2023. 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.

Household Identifiers for *PANES* Data

Unlike *AFAM-PE* data, *PANES* data only contains application form and personal identifiers, but it does not include a household identification number. This is key informatino to detect which households applied multiple times to the program, and for the definition of the first application form 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 *PANES* household identifier based on the following procedure that establishes a set of rules to define when application forms correspond to the same household.

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 *PANES* application.

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[30]Complete and up-to-date information is expected in the second semester of 2024

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. I define 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 *PANES*.

After identifying same-household applications in *PANES*, the next step is to match the *PANES* household identifiers with *AFAM-PE* household identifiers. To do this, I merge the participation data of the two phases using the unique national identification number. For every individual included in the *AFAM-PE* data, I observe a list of *PANES* household identifiers where the national id number was included. 64.32% of all the national ID numbers observed in the pooled data correspond to *AFAM-PE* data only, while 35.68% show up both in *PANES* and *AFAM-PE* data. 99.66% of the national ID numbers that are included both in *PANES* and *AFAM-PE* data are associated to a unique household in *PANES* data. This is reasonable since *PANES* only lasted two years. 27.62% of the individuals show up only in *PANES* data. 99.66% of them are associated to a unique household.

The key challenge to link *AFAM-PE* and *PANES* households IDs correspond to cases where different individuals in the same *AFAM-PE* household can be linked to different phase *PANES* 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 *AFAM-PE*). For this 1.31% of the households I implemented the following rules:

1. For households in *AFAM-PE* that merge with multiple households in *PANES*, I

assign the match to the household match that appears the most (of the total number of households in *AFAM-PE* 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

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:

Table B.1: Description of Baseline Covariates

Group	Variable
Household Characteristics	Region fixed effects
	Number of household members
	Single parent household
	Adults' age when first born
	Average age of household members
	Number of children
Household Head Characteristics	Educ. years
	Employment status
	Income reported head
	Total reported household income
Individual Characteristics	Number of application forms
	Year of birth fixed effects
	Age at first application

C Further Results

Descriptive Statistics

Table C.1: Descriptive Statistics: Outcome Variables

	Women				Men			
	Full Sample		Balanced Sample		Full Sample		Balanced Sample	
	Full Support (1)	Opt. Bandwidth (2)	Full Support (3)	Opt. Bandwidth (4)	Full Support (5)	Opt. Bandwidth (6)	Full Support (7)	Opt. Bandwidth (8)
a. Fertility Outcomes								
Age of 1st. Birth	19.49	19.85	20.70	21.07	22.47	22.64	24.03	24.10
	(3.18)	(3.36)	(3.61)	(3.73)	(3.37)	(3.37)	(3.34)	(3.31)
Ever had a birth (%)	61.91	58.26	82.05	79.36	23.91	24.68	41.07	42.47
	(48.56)	(49.31)	(38.38)	(40.47)	(42.65)	(43.12)	(49.20)	(49.43)
Total births	1.09	1.00	1.67	1.53	0.31	0.33	0.58	0.60
	(1.12)	(1.08)	(1.23)	(1.17)	(0.63)	(0.64)	(0.81)	(0.81)
b. Education Outcomes								
Number of $\neq$ grades (Middle school)	1.14	1.21	0.51	0.51	0.85	0.93	0.38	0.39
	(1.08)	(1.11)	(0.77)	(0.77)	(0.99)	(1.04)	(0.68)	(0.69)
Number of $\neq$ grades (High school)	0.55	0.70	0.53	0.66	0.27	0.36	0.26	0.33
	(0.87)	(0.94)	(0.91)	(0.98)	(0.64)	(0.72)	(0.66)	(0.74)
Max. grade enrolled	3.41	3.77	3.82	4.12	2.82	3.11	3.26	3.53
	(1.70)	(1.67)	(1.69)	(1.63)	(1.61)	(1.63)	(1.67)	(1.65)
Ever enrolled tertiary educ. (%)	7.75	11.09	7.24	10.18	3.74	5.34	3.56	4.92
	(26.74)	(31.40)	(25.91)	(30.23)	(18.97)	(22.48)	(18.54)	(21.62)
c. Labor Market Outcomes								
Age of 1st. Employment Spell	20.77	20.85	21.59	21.65	19.74	19.73	20.05	20.05
	(2.91)	(2.93)	(3.47)	(3.47)	(2.38)	(2.38)	(2.79)	(2.80)
Employed (last observed, %)	35.27	38.43	40.85	44.19	41.81	45.03	46.44	49.45
	(47.78)	(48.64)	(49.16)	(49.66)	(49.32)	(49.75)	(49.87)	(50.00)
Anual Income (PPP USD) (last observed)	2750.00	3231.95	3932.14	4501.44	3873.90	4402.22	5161.39	5766.61
	(5053.04)	(5591.27)	(6356.69)	(6861.88)	(6279.60)	(6858.78)	(7654.29)	(8229.79)
Cumulative months worked	29.19	32.78	52.34	56.75	38.44	42.22	67.50	72.22
	(39.93)	(42.38)	(51.80)	(53.49)	(45.88)	(47.97)	(57.52)	(58.81)
Cumulative labor income earned	21770.41	30476.24	43418.43	64415.97	31018.05	35358.89	56751.71	62850.54
	(644114.53)	(1275389.06)	(1194120.01)	(2273675.22)	(49196.17)	(53872.19)	(68172.67)	(72919.71)
Observations	138,272	35,189	40,173	11,066	140,752	35,212	40,644	10,721

Notes:

Figure C.1: Distribution of Age at 31 December, 2021
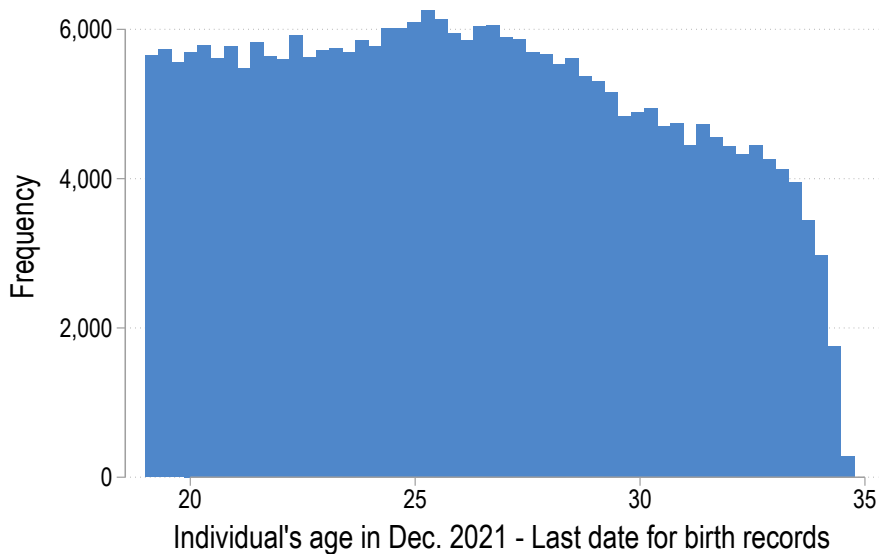
Notes:

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
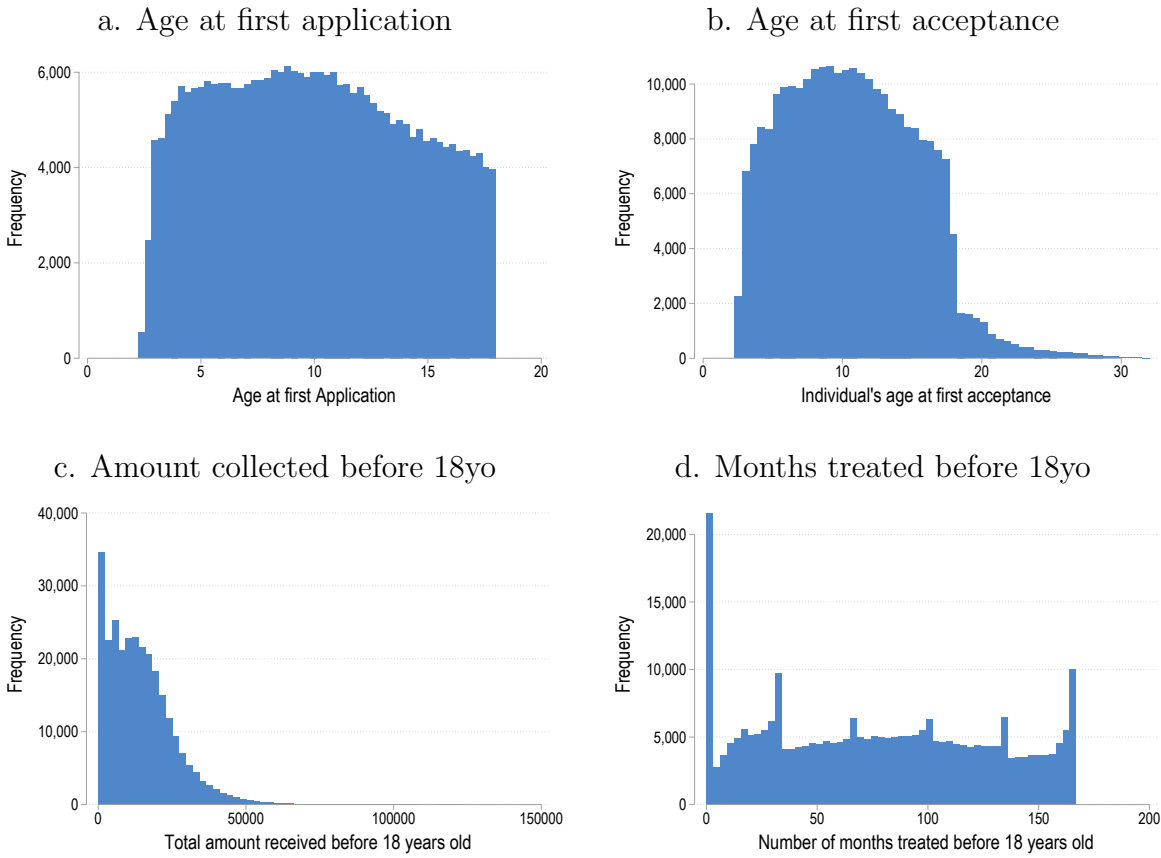
Notes:

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 XX 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 XX 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.

Figure D.1: Relation Between Application Form Eligibility and Resoultion

a. Full Support

b. Optimal Bandwidth
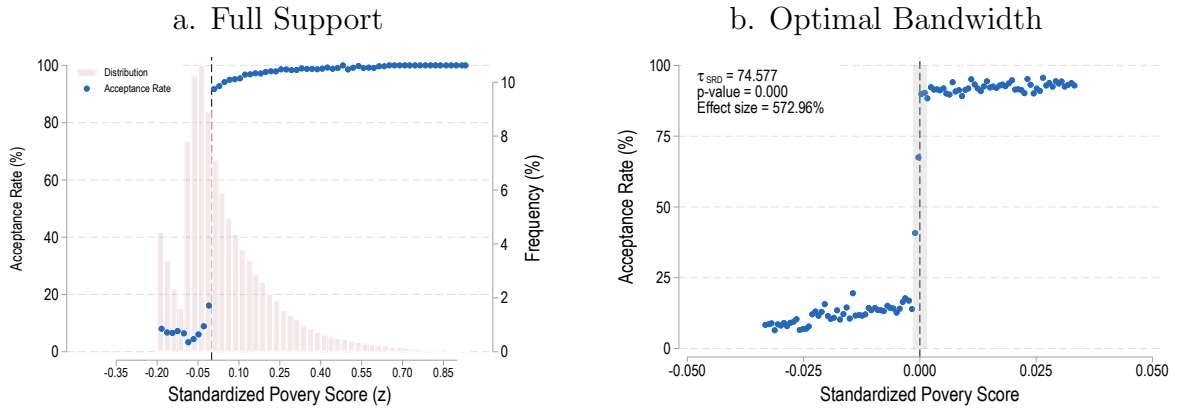
Notes:This figure reports the share of application forms that were accepted as a function of the standardized poverty score (z) for the forms corresponding to individuals in the *main sample* as defined in Section 4.2.. Each observation used to construct this figure corresponds to an application form. Panel a. reports this relation for the full support of z. Negative values of z indicate that the application does not meet the eligibility requirements, while positive values 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. Local linear polynomials and a triangular kernel function are used to approximate the population conditional expectation functions. Panel b. focuses on application forms that are located within an optimal bandwidth. Following Calonico, Cattaneo, Farrell, and Titiunik (2018) the optimal bandwidth is selected by minimizing the mean squared error (MSE). Average acceptance rates are grouped in 10 equally-spaced bins at each side of the threshold. Bins are size-weighted and local linear polynomials and a triangular kernel function are used to approximate the population conditional expectation functions. 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 continuity test p-value, and the effect size expressed as a percent of the share of applications accepted for the ineligible group within the bandwidth depicted. These estimates are based on *rdrobust* default options: local linear regressions using triangular weights. Robust standard errors are clustered at the household level. Reported estimates and figures are based on specifications that do not include any covariates.

Figure D.2: First Stage and Distribution of the Poverty Score for the Full Support

a. First Stage
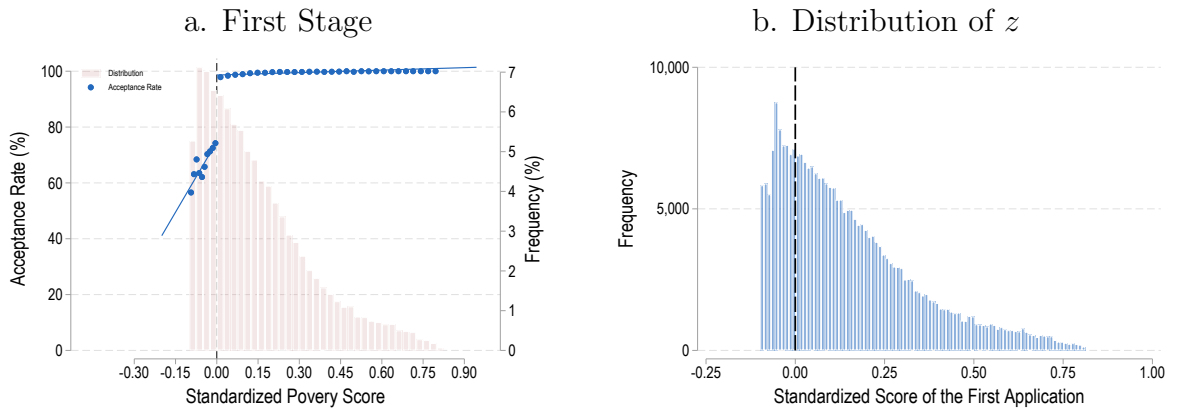
b. Distribution of z

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 in bars of 2p.p. width.

Figure D.3: Robustness Tests: First Stage

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
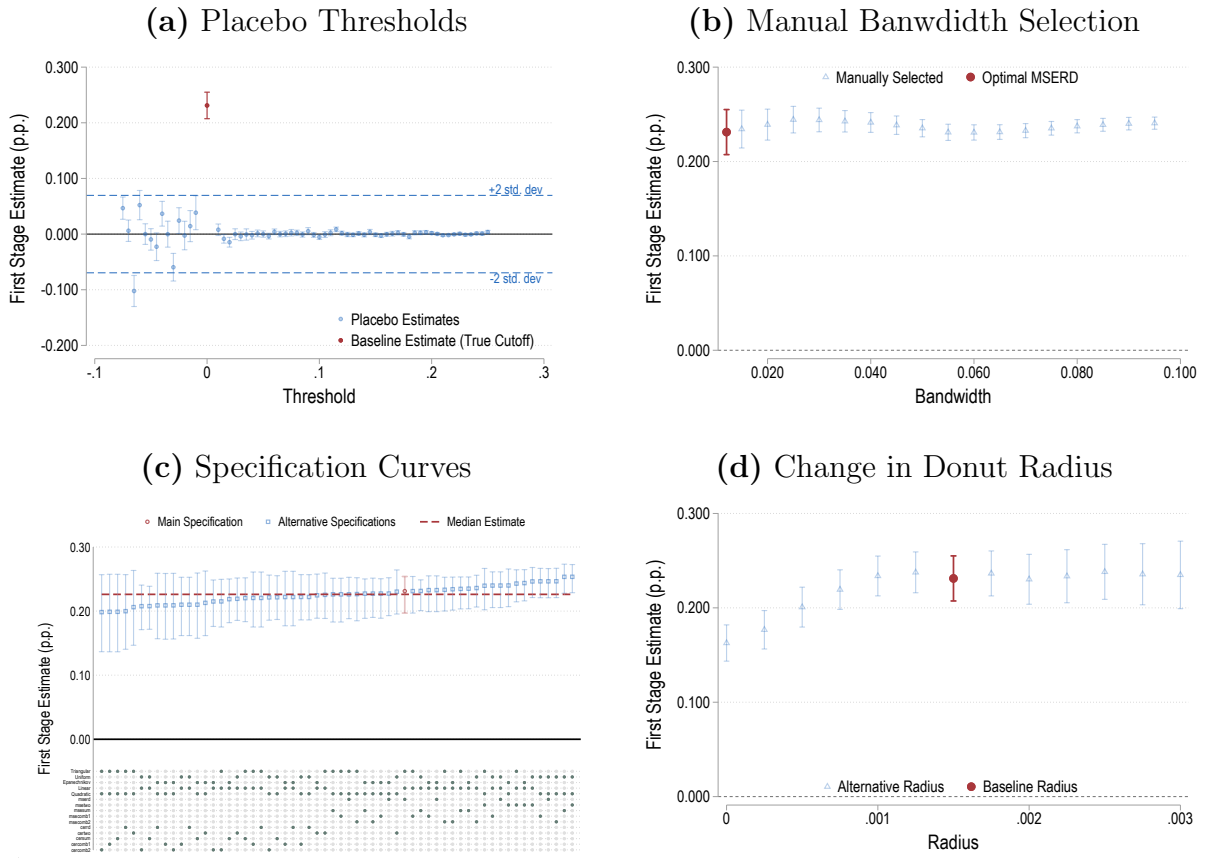
Notes:

Table D.1: First Stage Intensity Measures

	Men				Women			
	Ever Treated Before 18yo (1)	Months Treated Before 18yo (2)	Amount Received Before 18yo (PPP USD) (3)	Amount Received Before 18yo (PPP USD per month) (4)	Ever Treated Before 18yo (5)	Months Treated Before 18yo (6)	Amount Received Before 18yo (PPP USD) (7)	Amount Received Before 18yo (PPP USD per month) (8)
a. First Stage Estimate								
Eligible 1st. App	25.500***	23.034***	4,577.937***	32.949***	21.004***	20.322***	4,899.738***	46.534***
	(1.723)	(2.390)	(394.222)	(4.106)	(1.922)	(2.210)	(424.580)	(5.261)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	71.66	56.36	8012.48	142.16	73.76	55.12	8139.55	145.45
Effect Size (%)	35.59%	40.87%	57.14%	23.18%	28.47%	36.87%	60.20%	31.99%
Selection of Bandwidth:								
Opt. Bandwidth	[0.016]	[0.016]	[0.016]	[0.019]	[0.013]	[0.017]	[0.016]	[0.016]
Effective Obs.	10,248	9,881	10,318	10,897	8,174	10,841	10,005	9,255

Notes:

Table D.2: Falsification Tests on Predicted Outcomes - Women

	Had a child (1)	Age of First birth (2)	Number of births (3)	Ever enrolled tertiary (4)	Had an employment apell (5)	Age of First emp. spell (6)	Months Employed (7)	Total Labor Inc. Earned (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	-0.025	0.003	-0.001	-0.003	-0.000	0.001	-0.002	1.358
	(0.035)	(0.003)	(0.000)	(0.022)	(0.000)	(0.001)	(0.018)	(26.447)
Robust p-value	0.425	0.341	0.348	0.857	0.128	0.289	0.958	0.941
Mean Baseline Outcome	53.84	19.77	0.94	13.01	0.63	20.49	34.39	23327.75
Effect Size (%)	-0.05%	0.01%	-0.07%	-0.02%	-0.02%	0.00%	-0.01%	0.01%
b. Sharp RDD Estimate								
Elig. 1st. App.	-0.483	0.048	-0.013	-0.054	-0.003	0.011	-0.036	26.414
	(0.676)	(0.054)	(0.016)	(0.429)	(0.002)	(0.011)	(0.355)	(513.871)
Robust p-value	0.439	0.350	0.360	0.862	0.140	0.312	0.962	0.943
Mean Baseline Outcome	53.84	19.77	0.94	13.01	0.63	20.49	34.39	23327.75
Effect Size (%)	-0.90%	0.24%	-1.42%	-0.41%	-0.44%	0.05%	-0.10%	0.11%
c. First Stage								
Elig. 1st. App.	19.397***	18.884***	19.342***	19.466***	19.449***	19.408***	19.536***	19.449***
	(1.697)	(1.932)	(1.731)	(1.670)	(1.677)	(1.693)	(1.645)	(1.677)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	74.13	74.44	73.93	74.10	74.10	74.12	74.12	74.10
Effect Size (%)	26.17%	25.37%	26.16%	26.27%	26.25%	26.18%	26.36%	26.25%
Selection of Bandwidth:								
Opt. Bandwidth	[0.016]	[0.013]	[0.015]	[0.016]	[0.016]	[0.016]	[0.016]	[0.016]
Effective Obs.	8,556	7,147	8,299	8,676	8,650	8,570	8,879	8,650

Notes:

Table D.3: Falsification Tests on Predicted Outcomes - Men

	Had a child (1)	Age of First birth (2)	Number of births (3)	Ever enrolled tertiary (4)	Had an employment apell (5)	Age of First emp. spell (6)	Months Employed (7)	Total Labor Inc. Earned (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	-0.005	-0.000	-0.000	0.004	-0.000	0.000	-0.009	-6.155
	(0.008)	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)	(0.009)	(10.757)
Robust p-value	0.408	0.835	0.445	0.406	0.185	0.730	0.244	0.425
Mean Baseline Outcome	24.40	21.35	0.32	7.24	0.68	19.50	45.88	38593.94
Effect Size (%)	-0.02%	-0.00%	-0.02%	0.05%	-0.02%	0.00%	-0.02%	-0.02%
b. Sharp RDD Estimate								
Elig. 1st. App.	-0.136	-0.002	-0.002	0.088	-0.004	0.001	-0.218	-153.338
	(0.199)	(0.011)	(0.003)	(0.081)	(0.003)	(0.006)	(0.231)	(269.145)
Robust p-value	0.403	0.833	0.439	0.399	0.185	0.731	0.249	0.429
Mean Baseline Outcome	24.40	21.35	0.32	7.24	0.68	19.50	45.88	38593.94
Effect Size (%)	-0.56%	-0.01%	-0.50%	1.21%	-0.52%	0.01%	-0.47%	-0.40%
c. First Stage								
Elig. 1st. App.	25.632***	24.833***	25.852***	24.370***	24.686***	25.081***	24.832***	24.913***
	(1.325)	(0.802)	(1.254)	(0.722)	(1.917)	(1.582)	(1.761)	(1.698)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	71.89	68.97	71.76	68.52	71.24	71.35	71.48	71.34
Effect Size (%)	35.65%	36.01%	36.03%	35.57%	34.65%	35.15%	34.74%	34.92%
Selection of Bandwidth:								
Opt. Bandwidth	[0.024]	[0.052]	[0.026]	[0.061]	[0.014]	[0.018]	[0.016]	[0.017]
Effective Obs.	13,426	31,250	14,602	37,643	7,787	10,223	8,812	9,231

Notes:

E   Further Results

E.1   Robustness Tests

Table E.1: Summary of Effects - Estimates Without Covariates

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	1.511***	16.656**	-1.475***	38.051***	-1.341*	2.399	-0.349	5.339
	(0.606)	(7.737)	(0.460)	(10.548)	(0.647)	(2.116)	(0.387)	(7.131)
Robust p-value	0.008	0.032	0.001	0.000	0.054	0.329	0.343	0.488
Adj. Robust Q-value	0.017	0.042	0.004	0.001	0.058	0.173	0.173	0.224
Mean Baseline Outcome	19.86	10.54	20.86	28.63	22.80	5.50	19.71	48.40
Effect Size (%)	7.61%	158.01%	-7.07%	132.88%	-5.88%	43.65%	-1.77%	11.03%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.361***	3.561**	-0.360***	8.271***	-0.434*	0.604	-0.098	1.379
	(0.139)	(1.617)	(0.112)	(2.170)	(0.212)	(0.531)	(0.109)	(1.840)
Robust p-value	0.007	0.031	0.001	0.000	0.054	0.330	0.336	0.483
Mean Baseline Outcome	19.86	10.54	20.86	28.63	22.80	5.50	19.71	48.40
Effect Size (%)	1.82%	33.78%	-1.72%	28.88%	-1.91%	10.98%	-0.50%	2.85%
c. First Stage								
Elig. 1st. App.	0.239***	0.214***	0.244***	0.217***	0.324***	0.252***	0.281***	0.258***
	(0.014)	(0.018)	(0.014)	(0.016)	(0.024)	(0.008)	(0.016)	(0.014)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.70	0.74	0.69	0.74	0.63	0.70	0.70	0.72
Effect Size (%)	34.03%	28.97%	35.49%	29.40%	51.76%	35.95%	39.87%	35.99%
Selection of Bandwidth:								
Opt. Bandwidth	[0.031]	[0.014]	[0.032]	[0.016]	[0.031]	[0.048]	[0.025]	[0.022]
Effective Obs.	12,087	9,108	12,791	10,206	5,179	32,396	11,005	14,160

Notes:

Table E.2: Summary of Effects - Estimates Without Fixed Effects

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	2.379***	14.636*	-1.931***	51.602***	-1.082**	2.815	-0.250	7.994
	(0.630)	(7.805)	(0.604)	(12.409)	(0.480)	(2.610)	(0.336)	(8.254)
Robust p-value	0.000	0.058	0.001	0.000	0.025	0.453	0.458	0.329
Adj. Robust Q-value	0.001	0.053	0.003	0.001	0.033	0.208	0.208	0.197
Mean Baseline Outcome	19.86	10.83	20.81	29.13	22.75	5.44	19.70	48.95
Effect Size (%)	11.98%	135.20%	-9.28%	177.16%	-4.76%	51.73%	-1.27%	16.33%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.533***	2.895*	-0.429***	10.136***	-0.347**	0.720	-0.068	2.012
	(0.136)	(1.528)	(0.131)	(2.290)	(0.153)	(0.667)	(0.091)	(2.079)
Robust p-value	0.000	0.063	0.001	0.000	0.026	0.436	0.447	0.328
Mean Baseline Outcome	19.86	10.83	20.81	29.13	22.75	5.44	19.70	48.95
Effect Size (%)	2.68%	26.74%	-2.06%	34.80%	-1.53%	13.23%	-0.35%	4.11%
c. First Stage								
Elig. 1st. App.	0.224***	0.198***	0.222***	0.196***	0.321***	0.256***	0.272***	0.252***
	(0.014)	(0.016)	(0.016)	(0.017)	(0.019)	(0.010)	(0.013)	(0.015)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.71	0.74	0.70	0.74	0.61	0.71	0.70	0.72
Effect Size (%)	31.54%	26.72%	31.96%	26.50%	52.47%	36.05%	39.18%	35.18%
Selection of Bandwidth:								
Opt. Bandwidth	[0.029]	[0.017]	[0.024]	[0.016]	[0.044]	[0.036]	[0.033]	[0.019]
Effective Obs.	9,399	9,277	8,460	8,574	6,426	20,863	13,398	10,843

Notes:

Table E.3: Summary of Effects - Years Treated Before 18yo

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Months Treated Before 18	0.382***	1.655	-0.392***	6.530***	-0.237*	0.167	-0.109	1.463
	(0.098)	(1.046)	(0.118)	(1.639)	(0.122)	(0.513)	(0.118)	(1.350)
Robust p-value	0.000	0.114	0.001	0.000	0.054	0.822	0.362	0.304
Adj. Robust Q-value	0.001	0.122	0.003	0.001	0.073	0.446	0.224	0.224
Mean Baseline Outcome	19.87	10.94	20.81	29.28	22.80	5.23	19.70	48.92
Effect Size (%)	1.92%	15.13%	-1.88%	22.30%	-1.04%	3.20%	-0.55%	2.99%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.512***	2.347	-0.595***	9.328***	-0.345**	0.271	-0.146	2.318
	(0.129)	(1.473)	(0.169)	(2.181)	(0.176)	(0.832)	(0.155)	(2.144)
Robust p-value	0.000	0.115	0.001	0.000	0.048	0.823	0.364	0.304
Mean Baseline Outcome	19.87	10.94	20.81	29.28	22.80	5.23	19.70	48.92
Effect Size (%)	2.58%	21.45%	-2.86%	31.86%	-1.51%	5.18%	-0.74%	4.74%
c. First Stage								
Elig. 1st. App.	1.347***	1.423***	1.517***	1.428***	1.454***	1.620***	1.338***	1.593***
	(0.085)	(0.125)	(0.146)	(0.132)	(0.111)	(0.098)	(0.142)	(0.120)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	3.73	4.63	3.74	4.65	2.94	4.63	4.05	4.60
Effect Size (%)	36.10%	30.75%	40.59%	30.69%	49.50%	35.01%	33.03%	34.65%
Selection of Bandwidth:								
Opt. Bandwidth	[0.031]	[0.018]	[0.017]	[0.017]	[0.034]	[0.025]	[0.016]	[0.018]
Effective Obs.	10,220	9,788	5,717	9,106	4,855	14,175	6,230	10,266

Notes:

Table E.4: Summary of Effects - Amount Collected (S.D.) Treated Before 18yo

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Months Treated Before 18	1.160***	3.131	-0.943***	12.704***	-0.860**	1.890	-0.164	5.643
	(0.300)	(2.210)	(0.293)	(3.354)	(0.426)	(1.726)	(0.243)	(4.108)
Robust p-value	0.000	0.189	0.001	0.000	0.049	0.399	0.477	0.123
Adj. Robust Q-value	0.001	0.145	0.003	0.001	0.066	0.246	0.246	0.110
Mean Baseline Outcome	19.84	11.60	20.84	29.82	22.79	5.46	19.71	49.31
Effect Size (%)	5.85%	26.98%	-4.53%	42.61%	-3.77%	34.63%	-0.83%	11.44%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.567***	1.328	-0.402***	5.445***	-0.345**	0.723	-0.062	2.159
	(0.142)	(0.932)	(0.122)	(1.396)	(0.170)	(0.658)	(0.092)	(1.562)
Robust p-value	0.000	0.176	0.001	0.000	0.046	0.394	0.476	0.117
Mean Baseline Outcome	19.84	11.60	20.84	29.82	22.79	5.46	19.71	49.31
Effect Size (%)	2.86%	11.44%	-1.93%	18.26%	-1.51%	13.24%	-0.31%	4.38%
c. First Stage								
Elig. 1st. App.	0.489***	0.424***	0.426***	0.429***	0.401***	0.382***	0.378***	0.383***
	(0.029)	(0.018)	(0.026)	(0.019)	(0.034)	(0.017)	(0.021)	(0.019)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.65	0.69	0.57	0.70	0.49	0.66	0.61	0.68
Effect Size (%)	74.67%	61.59%	74.05%	61.51%	82.20%	57.69%	62.35%	56.62%
Selection of Bandwidth:								
Opt. Bandwidth	[0.026]	[0.037]	[0.027]	[0.033]	[0.036]	[0.036]	[0.033]	[0.030]
Effective Obs.	8,591	20,976	9,289	18,817	5,160	21,243	13,215	17,303

Notes:

Table E.5: Summary of Effects - Affected by Conditionality Enforcement (Turned 16 after June, 2013)

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	4.922**	50.963**	-1.451	105.403***	-2.385	18.267	-0.254	26.485
	(2.342)	(22.452)	(1.194)	(34.898)	(2.304)	(12.345)	(0.837)	(23.383)
Robust *p*-value	0.045	0.011	0.188	0.001	0.234	0.170	0.804	0.282
Adj. Robust *Q*-value	0.099	0.041	0.232	0.009	0.243	0.232	0.432	0.253
Mean Baseline Outcome	18.38	11.59	19.99	28.07	20.06	5.43	19.47	42.44
Effect Size (%)	26.78%	439.64%	-7.25%	375.44%	-11.89%	336.65%	-1.30%	62.40%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.387**	5.001***	-0.166	10.341***	-0.206	2.292	-0.030	3.322
	(0.164)	(2.002)	(0.135)	(2.864)	(0.187)	(1.494)	(0.100)	(2.931)
Robust *p*-value	0.019	0.008	0.174	0.000	0.193	0.159	0.793	0.285
Mean Baseline Outcome	18.38	11.59	19.99	28.07	20.06	5.43	19.47	42.44
Effect Size (%)	2.11%	43.14%	-0.83%	36.84%	-1.03%	42.24%	-0.16%	7.83%
c. First Stage								
Elig. 1st. App.	0.079***	0.098***	0.114***	0.098***	0.087***	0.125***	0.120***	0.125***
	(0.016)	(0.017)	(0.020)	(0.018)	(0.024)	(0.017)	(0.014)	(0.017)
Robust *p*-value	0.000	0.000	0.000	0.000	0.002	0.000	0.000	0.000
Mean Baseline Outcome	0.92	0.88	0.86	0.88	0.85	0.86	0.86	0.86
Effect Size (%)	8.58%	11.20%	13.29%	11.20%	10.12%	14.64%	13.98%	14.65%
Selection of Bandwidth:								
Opt. Bandwidth	[0.033]	[0.021]	[0.033]	[0.021]	[0.070]	[0.021]	[0.043]	[0.022]
Effective Obs.	3,228	5,234	3,764	5,230	2,132	5,490	6,382	5,572

Notes:

Table E.6: Summary of Effects - Balanced Sample

	Women				Men			
	Age of First Birth (1)	Ever Enrolled in Higher Educ. (2)	Age of First Employment (3)	Market-oriented Index (4)	Age of First Birth (5)	Ever Enrolled in Higher Educ. (6)	Age of First Employment (7)	Market-oriented Index (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	1.655***	0.420	-0.813**	10.173**	-1.322**	-3.585*	-0.205	-1.457
	(0.471)	(3.367)	(0.389)	(4.844)	(0.527)	(2.509)	(0.320)	(5.400)
Robust *p*-value	0.000	0.952	0.041	0.036	0.013	0.080	0.422	0.954
Adj. Robust *Q*-value	0.001	0.558	0.066	0.066	0.048	0.090	0.268	0.558
Mean Baseline Outcome	21.18	10.86	21.63	30.63	24.28	5.11	20.01	54.89
Effect Size (%)	7.81%	3.87%	-3.76%	33.21%	-5.44%	-70.12%	-1.03%	-2.66%
b. Sharp RDD Estimate								
Elig. 1st. App.	0.720***	0.189	-0.367**	4.572**	-0.647**	-1.796*	-0.101	-0.729
	(0.200)	(1.517)	(0.174)	(2.167)	(0.254)	(1.255)	(0.157)	(2.701)
Robust *p*-value	0.000	0.949	0.045	0.040	0.012	0.078	0.423	0.953
Mean Baseline Outcome	21.18	10.86	21.63	30.63	24.28	5.11	20.01	54.89
Effect Size (%)	3.40%	1.74%	-1.70%	14.92%	-2.67%	-35.14%	-0.50%	-1.33%
c. First Stage								
Elig. 1st. App.	0.435***	0.451***	0.451***	0.449***	0.490***	0.501***	0.492***	0.500***
	(0.021)	(0.018)	(0.019)	(0.017)	(0.031)	(0.021)	(0.020)	(0.020)
Robust *p*-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.44	0.44	0.42	0.43	0.43	0.43	0.44	0.43
Effect Size (%)	98.45%	103.30%	106.76%	104.39%	112.81%	115.90%	113.06%	115.78%
Selection of Bandwidth:								
Opt. Bandwidth	[0.044]	[0.046]	[0.050]	[0.048]	[0.039]	[0.034]	[0.043]	[0.037]
Effective Obs.	6,088	8,130	7,021	8,594	2,960	6,158	6,242	6,610

Notes:

Table E.7: Timing of Changes: Birth by Age γ

	Men					Women					
	Estimate (1)	Std. Error (2)	Eff. Size (%) (3)	Opt. Bdw. (4)	Effective Observations (5)	Estimate (6)	Std. Error (7)	Eff. Size (%) (8)	Opt. Bdw. (9)	Effective Observations (10)	p-val Difference (11)
15	-0.004	(0.013)	-304.55	0.018	8,035	-0.082**	(0.041)	-230.49	0.020	8,928	0.026
16	0.004	(0.011)	133.14	0.035	17,749	-0.189***	(0.078)	-203.36	0.018	8,493	0.003
17	-0.029**	(0.020)	-312.87	0.022	11,474	-0.124***	(0.054)	-80.95	0.037	19,773	0.053
18	-0.052**	(0.037)	-234.15	0.016	8,816	-0.261***	(0.089)	-111.43	0.024	13,127	0.007
19	-0.080*	(0.054)	-186.60	0.014	7,544	-0.220**	(0.102)	-69.63	0.023	12,745	0.135
20	-0.009	(0.056)	-12.07	0.018	9,493	-0.219**	(0.116)	-54.87	0.021	10,622	0.042
21	0.021	(0.052)	20.24	0.024	12,013	-0.231**	(0.127)	-48.94	0.019	9,030	0.023
22	0.028	(0.046)	19.18	0.037	17,444	-0.190*	(0.124)	-35.97	0.021	9,199	0.040
23	0.023	(0.044)	11.88	0.052	22,801	-0.248**	(0.129)	-42.97	0.020	8,285	0.013
24	0.040	(0.053)	16.94	0.040	15,417	-0.256**	(0.127)	-41.34	0.021	7,649	0.008
25	-0.008	(0.044)	-2.80	0.067	24,710	-0.207**	(0.107)	-31.30	0.023	7,830	0.033
26	0.004	(0.047)	1.17	0.062	19,880	-0.148*	(0.079)	-21.44	0.032	9,669	0.043
27	0.026	(0.051)	7.86	0.056	15,546	-0.066	(0.070)	-9.26	0.033	8,526	0.193
28	0.070	(0.069)	19.52	0.034	7,423	-0.060	(0.068)	-8.15	0.031	6,593	0.111
29	0.009	(0.066)	2.28	0.036	6,501	-0.022	(0.057)	-2.86	0.042	7,429	0.669
30	-0.003	(0.061)	-0.67	0.045	6,448	-0.012	(0.044)	-1.56	0.059	8,825	0.883
31	-0.004	(0.055)	-1.00	0.055	6,270	-0.004	(0.049)	-0.45	0.049	5,147	0.990
32	-0.011	(0.062)	-2.51	0.053	3,817	0.030	(0.059)	3.76	0.036	2,516	0.561
33	-0.078	(0.142)	-16.56	0.025	926	-0.030	(0.065)	-3.78	0.054	2,036	0.706

Notes:

Table E.8: Timing of Changes: Employed at Age γ

	Men					Women					
	Estimate (1)	Std. Error (2)	Eff. Size (%) (3)	Opt. Bdw. (4)	Effective Observations (5)	Estimate (6)	Std. Error (7)	Eff. Size (%) (8)	Opt. Bdw. (9)	Effective Observations (10)	p-val Difference (11)
15	-0.009	(0.019)	-145.35	0.021	9,770	0.006	(0.005)	2,082.78	0.041	19,239	0.344
16	0.001	(0.022)	3.92	0.024	11,889	0.031*	(0.020)	566.58	0.017	8,358	0.216
17	0.033	(0.032)	92.80	0.023	12,366	0.106***	(0.033)	698.36	0.020	10,199	0.061
18	-0.058	(0.097)	-22.36	0.017	9,400	0.270***	(0.096)	213.53	0.018	9,835	0.004
19	-0.038	(0.096)	-10.73	0.019	10,341	0.268***	(0.117)	120.76	0.018	9,658	0.017
20	0.030	(0.077)	7.39	0.027	14,410	0.281***	(0.126)	101.40	0.018	9,324	0.035
21	0.069	(0.081)	15.31	0.025	12,553	0.390***	(0.145)	120.19	0.015	7,336	0.023
22	-0.053	(0.071)	-11.02	0.034	15,917	0.334***	(0.144)	94.75	0.017	7,455	0.004
23	-0.030	(0.054)	-6.06	0.058	26,724	0.403***	(0.146)	106.17	0.017	6,844	0.001
24	-0.070	(0.053)	-13.62	0.060	24,829	0.316***	(0.144)	78.28	0.019	6,827	0.002
25	-0.063*	(0.049)	-12.24	0.069	25,246	0.137	(0.131)	33.29	0.021	6,806	0.078
26	-0.053	(0.051)	-10.19	0.061	19,628	0.215*	(0.114)	50.42	0.023	6,738	0.008
27	-0.013	(0.051)	-2.52	0.064	17,391	0.085	(0.085)	19.11	0.028	7,280	0.248
28	-0.022	(0.067)	-4.24	0.041	9,093	0.096*	(0.076)	21.41	0.036	7,787	0.158
29	-0.023	(0.067)	-4.54	0.037	6,624	0.044	(0.064)	9.58	0.047	8,298	0.386
30	0.008	(0.070)	1.51	0.038	5,442	-0.018	(0.061)	-3.83	0.052	7,539	0.731
31	0.020	(0.064)	4.04	0.044	4,799	0.017	(0.058)	3.71	0.053	5,649	0.961
32	0.043	(0.059)	8.88	0.060	4,498	0.120*	(0.083)	26.45	0.033	2,290	0.359
33	0.168	(0.138)	34.63	0.026	998	0.022	(0.069)	4.58	0.072	2,742	0.238

Notes:

Table E.9: Summary of Effects: Lasting Consequences - Men

	Fertility		Labor Market				
	Had a Child (1)	Number of Children (2)	Employed (Ever) (3)	Employed (Last Observed) (4)	Yearly Earnings (Last Observed) (5)	Months Employed (Cumulative) (6)	Total Earnings (Cumulative) (7)
a. Fuzzy RDD Estimate							
Ever Treated Before 18	-1.168	0.030	-1.214	12.432*	1.227*	3.727	1.181
	(4.365)	(0.098)	(4.747)	(7.684)	(0.860)	(6.818)	(4.143)
Robust p-value	0.971	0.767	0.656	0.066	0.083	0.423	0.449
Adj. Robust Q-value	1.000	1.000	1.000	0.410	0.410	1.000	1.000
Mean Baseline Outcome	24.91	0.33	70.41	46.02	4.61	43.93	32.38
Effect Size (%)	-4.69%	9.17%	-1.72%	27.01%	26.65%	8.49%	3.65%
b. Sharp RDD Estimate							
Elig. 1st. App.	-0.298	0.008	-0.310	3.134*	0.316*	0.935	0.302
	(1.115)	(0.024)	(1.211)	(1.933)	(0.221)	(1.709)	(1.059)
Robust p-value	0.967	0.766	0.660	0.065	0.080	0.422	0.445
Mean Baseline Outcome	24.91	0.33	70.41	46.02	4.61	43.93	32.38
Effect Size (%)	-1.20%	2.30%	-0.44%	6.81%	6.86%	2.13%	0.93%
c. First Stage							
Elig. 1st. App.	0.255***	0.250***	0.255***	0.252***	0.258***	0.251***	0.256***
	(0.009)	(0.014)	(0.009)	(0.013)	(0.011)	(0.014)	(0.010)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.71	0.72	0.71	0.72	0.72	0.72	0.71
Effect Size (%)	35.91%	34.96%	35.91%	35.24%	35.98%	35.05%	35.90%
Selection of Bandwidth:							
Opt. Bandwidth	[0.038]	[0.020]	[0.038]	[0.022]	[0.028]	[0.021]	[0.033]
Effective Obs.	22,245	11,456	22,234	12,281	16,396	11,650	18,888

Notes:

Table E.10: Effects on Secondary Education Outcomes - Men

| | Enrollment | | | | Academic Progress | | | |
	Ever Enrolled in Secondary (1)	Ever Enrolled in Middle School (2)	Ever Enrolled in High School (3)	Years Enrolled in Secondary (4)	Number of ≠ Grades in Middle School (5)	Number of ≠ Grades in High School (6)	Number of ≠ Grades in Secondary (7)	Max. Grade Enrolled (8)
a. Fuzzy RDD Estimate								
Ever Treated Before 18	6.451	10.052	16.816	0.206	0.221	0.120	0.376	0.262
	(5.324)	(6.599)	(9.601)	(0.248)	(0.157)	(0.121)	(0.191)	(0.370)
Robust p-value	0.270	0.106	0.121	0.561	0.262	0.658	0.115	0.516
Adj. Robust Q-value	0.477	0.477	0.477	0.669	0.477	0.699	0.477	0.669
Mean Baseline Outcome	78.36	69.95	40.09	2.27	0.92	0.33	1.26	3.06
Effect Size (%)	8.23%	14.37%	41.94%	9.06%	24.08%	36.80%	29.91%	8.55%
b. Sharp RDD Estimate								
Elig. 1st. App.	1.650	2.502	4.171	0.052	0.055	0.030	0.097	0.056
	(1.364)	(1.643)	(2.368)	(0.063)	(0.039)	(0.030)	(0.049)	(0.079)
Robust p-value	0.271	0.108	0.121	0.560	0.261	0.661	0.114	0.524
Mean Baseline Outcome	78.36	69.95	40.09	2.27	0.92	0.33	1.26	3.06
Effect Size (%)	2.11%	3.58%	10.40%	2.29%	6.05%	9.19%	7.68%	1.82%
c. First Stage								
Elig. 1st. App.	0.256***	0.249***	0.248***	0.252***	0.251***	0.250***	0.257***	0.213***
	(0.012)	(0.015)	(0.017)	(0.013)	(0.014)	(0.015)	(0.012)	(0.017)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.72	0.71	0.71	0.72	0.72	0.72	0.72	0.76
Effect Size (%)	35.58%	34.82%	34.78%	35.24%	35.08%	34.87%	35.75%	28.18%
Selection of Bandwidth:								
Opt. Bandwidth	[0.025]	[0.019]	[0.016]	[0.022]	[0.021]	[0.019]	[0.026]	[0.023]
Effective Obs.	14,106	10,464	8,992	12,513	11,726	10,824	14,656	7,871

Notes:

Table E.11: Effects on Labor Market Outcomes by Early Pregnancy

| | Early Pregnancy | | | | | No Early Pregnancy | | | | |
	Age of First Emp. Spell (1)	Employed (Last Observed) (2)	Yearly Earnings (Last Observed) (3)	Months Employed (Cumulative) (4)	Total Earnings (Cumulative) (5)	Age of First Emp. Spell (6)	Employed (Last Observed) (7)	Yearly Earnings (Last Observed) (8)	Months Employed (Cumulative) (9)	Total Earnings (Cumulative) (10)
a. Fuzzy RDD Estimate										
Ever Treated Before 18	-1.565***	21.341***	1.544**	18.158***	9.312**	-0.093	0.194	-1.201	-3.333	-4.779
	(0.442)	(7.009)	(0.792)	(6.128)	(4.689)	(0.673)	(6.117)	(0.807)	(4.587)	(4.320)
Robust p-value	0.000	0.001	0.041	0.002	0.034	0.781	0.892	0.146	0.425	0.340
Adj. Robust Q-value	0.001	0.005	0.061	0.006	0.061	0.532	0.555	0.139	0.322	0.321
Mean Baseline Outcome	20.05	44.29	4.24	41.25	28.93	20.78	38.23	2.92	33.37	21.00
Effect Size (%)	-7.81%	48.19%	36.41%	44.02%	32.19%	-0.45%	0.51%	-41.19%	-9.99%	-22.76%
b. Sharp RDD Estimate										
Elig. 1st. App.	-0.410***	5.140***	0.380**	4.354***	2.248**	-0.019	0.035	-0.212	-0.606	-0.852
	(0.113)	(1.667)	(0.194)	(1.450)	(1.125)	(0.135)	(1.112)	(0.142)	(0.834)	(0.770)
Robust p-value	0.000	0.001	0.042	0.003	0.035	0.784	0.892	0.151	0.428	0.345
Mean Baseline Outcome	20.05	44.29	4.24	41.25	28.93	20.78	38.23	2.92	33.37	21.00
Effect Size (%)	-2.04%	11.60%	8.96%	10.55%	7.77%	-0.09%	0.09%	-7.28%	-1.82%	-4.06%
c. First Stage										
Elig. 1st. App.	0.262***	0.241***	0.246***	0.240***	0.241***	0.200***	0.182***	0.177***	0.182***	0.178***
	(0.016)	(0.012)	(0.011)	(0.013)	(0.012)	(0.014)	(0.007)	(0.008)	(0.007)	(0.008)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.68	0.71	0.71	0.71	0.71	0.74	0.75	0.76	0.75	0.76
Effect Size (%)	38.77%	33.97%	34.68%	33.96%	34.00%	27.11%	24.33%	23.28%	24.33%	23.55%
Selection of Bandwidth:										
Opt. Bandwidth	[0.018]	[0.019]	[0.022]	[0.018]	[0.019]	[0.049]	[0.116]	[0.074]	[0.117]	[0.079]
Effective Obs.	9,934	16,233	19,521	15,301	16,534	8,201	27,696	19,940	27,830	21,044

Notes:

Table E.12: Effects on Labor Market Outcomes by Education Level

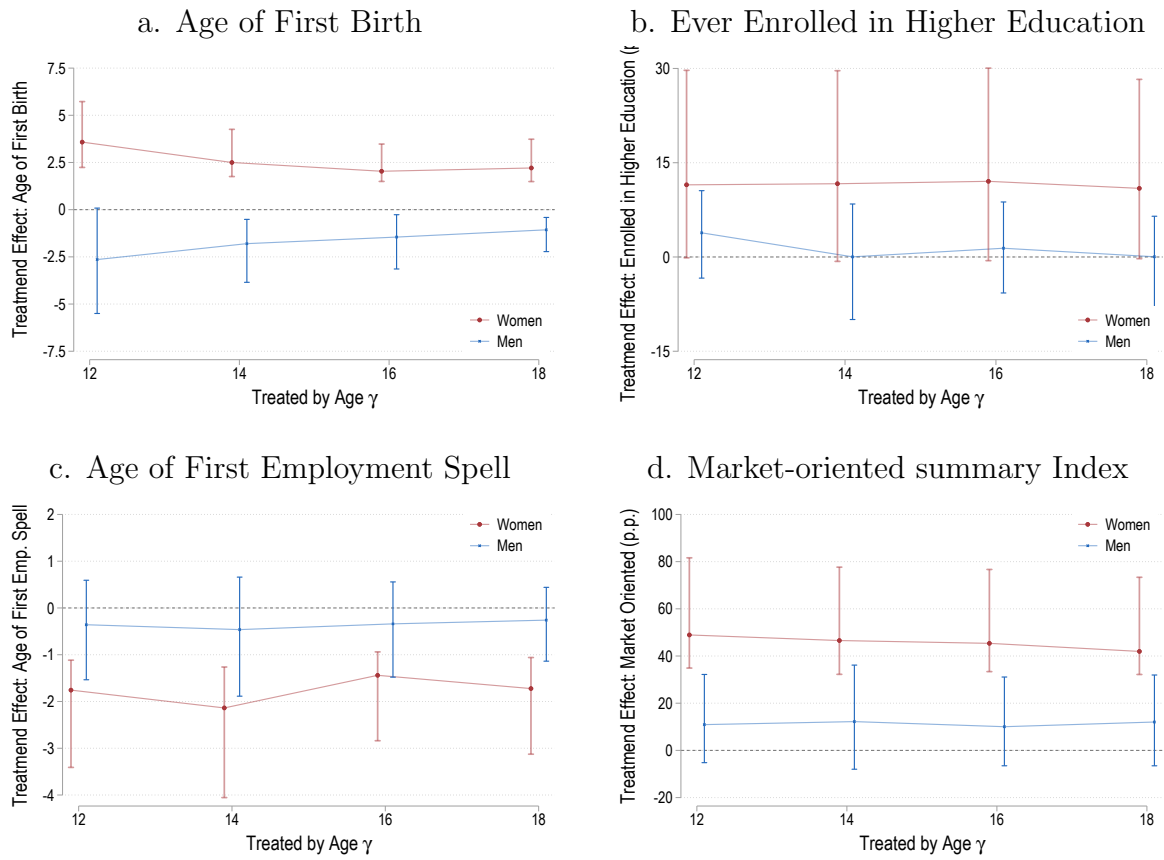|	Less Educated					More Educated				
	Age of First Emp. Spell	Employed (Last Observed)	Yearly Earnings (Last Observed)	Months Employed (Cumulative)	Total Earnings (Cumulative)	Age of First Emp. Spell	Employed (Last Observed)	Yearly Earnings (Last Observed)	Months Employed (Cumulative)	Total Earnings (Cumulative)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. Fuzzy RDD Estimate										
Ever Treated Before 18	-0.573	23.866***	2.534***	10.129**	7.607**	-2.342***	9.218	-0.125	19.457**	5.905
	(0.438)	(7.577)	(0.788)	(5.057)	(3.816)	(0.636)	(9.950)	(1.144)	(8.416)	(6.164)
Robust p-value	0.191	0.001	0.001	0.022	0.021	0.000	0.182	0.996	0.010	0.237
Adj. Robust Q-value	0.106	0.004	0.004	0.026	0.026	0.001	0.106	0.314	0.018	0.118
Mean Baseline Outcome	20.17	37.52	3.20	35.82	23.74	20.33	53.26	5.60	45.10	34.40
Effect Size (%)	-2.84%	63.61%	79.06%	28.28%	32.05%	-11.52%	17.31%	-2.23%	43.14%	17.16%
b. Sharp RDD Estimate										
Elig. 1st. App.	-0.137	5.072***	0.545***	2.242**	1.689**	-0.615***	2.311	-0.032	4.852**	1.511
	(0.105)	(1.584)	(0.168)	(1.114)	(0.843)	(0.158)	(2.488)	(0.298)	(2.079)	(1.578)
Robust p-value	0.196	0.001	0.001	0.022	0.020	0.000	0.187	0.998	0.011	0.242
Mean Baseline Outcome	20.17	37.52	3.20	35.82	23.74	20.33	53.26	5.60	45.10	34.40
Effect Size (%)	-0.68%	13.52%	17.01%	6.26%	7.11%	-3.02%	4.34%	-0.58%	10.76%	4.39%
c. First Stage										
Elig. 1st. App.	0.239***	0.212***	0.215***	0.221***	0.222***	0.262***	0.251***	0.260***	0.249***	0.256***
	(0.012)	(0.011)	(0.011)	(0.009)	(0.009)	(0.024)	(0.018)	(0.015)	(0.019)	(0.016)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.71	0.74	0.74	0.74	0.74	0.67	0.71	0.70	0.70	0.71
Effect Size (%)	33.62%	28.75%	29.15%	29.98%	30.16%	39.10%	35.47%	36.98%	35.49%	36.22%
Selection of Bandwidth:										
Opt. Bandwidth	[0.024]	[0.020]	[0.022]	[0.026]	[0.027]	[0.020]	[0.021]	[0.028]	[0.020]	[0.024]
Effective Obs.	11,757	14,965	16,412	19,747	20,475	4,977	7,547	10,469	7,034	8,988

Notes:

Table E.13: Effects on Labor Market Outcomes by Early Birth and Education Level

	Early Birth or Less Educated					No Early Birth and More Educated				
	Age of First Emp. Spell	Employed (Last Observed)	Yearly Earnings (Last Observed)	Months Employed (Cumulative)	Total Earnings (Cumulative)	Age of First Emp. Spell	Employed (Last Observed)	Yearly Earnings (Last Observed)	Months Employed (Cumulative)	Total Earnings (Cumulative)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
a. Fuzzy RDD Estimate										
Ever Treated Before 18	-0.821	17.414***	2.259***	11.071**	8.708**	-2.255***	25.036**	0.134	25.790***	6.450
	(0.489)	(6.838)	(0.796)	(5.795)	(4.505)	(0.671)	(12.319)	(1.235)	(10.231)	(6.070)
Robust p-value	0.109	0.006	0.002	0.032	0.034	0.000	0.027	0.763	0.009	0.197
Adj. Robust Q-value	0.052	0.017	0.010	0.031	0.031	0.001	0.031	0.181	0.019	0.071
Mean Baseline Outcome	20.22	38.42	3.30	36.27	24.27	20.24	54.28	5.79	45.83	35.14
Effect Size (%)	-4.06%	45.32%	68.42%	30.52%	35.88%	-11.14%	46.12%	2.31%	56.28%	18.36%
b. Sharp RDD Estimate										
Elig. 1st. App.	-0.189	3.742***	0.482***	2.365**	1.861**	-0.631***	6.680**	0.036	6.951***	1.719
	(0.112)	(1.461)	(0.169)	(1.234)	(0.958)	(0.179)	(3.202)	(0.331)	(2.693)	(1.616)
Robust p-value	0.119	0.007	0.002	0.034	0.036	0.000	0.024	0.763	0.008	0.195
Mean Baseline Outcome	20.22	38.42	3.30	36.27	24.27	20.24	54.28	5.79	45.83	35.14
Effect Size (%)	-0.94%	9.74%	14.60%	6.52%	7.67%	-3.12%	12.31%	0.62%	15.17%	4.89%
c. First Stage										
Elig. 1st. App.	0.230***	0.215***	0.213***	0.214***	0.214***	0.280***	0.267***	0.268***	0.270***	0.267***
	(0.013)	(0.010)	(0.011)	(0.011)	(0.011)	(0.029)	(0.023)	(0.016)	(0.025)	(0.016)
Robust p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Mean Baseline Outcome	0.71	0.74	0.74	0.74	0.74	0.66	0.70	0.70	0.70	0.70
Effect Size (%)	32.59%	29.11%	28.88%	28.90%	28.91%	42.14%	37.95%	38.10%	38.44%	38.07%
Selection of Bandwidth:										
Opt. Bandwidth	[0.021]	[0.021]	[0.021]	[0.021]	[0.021]	[0.018]	[0.017]	[0.028]	[0.016]	[0.027]
Effective Obs.	10,658	17,350	16,679	16,714	16,730	3,778	5,038	9,025	4,754	8,553

Notes:

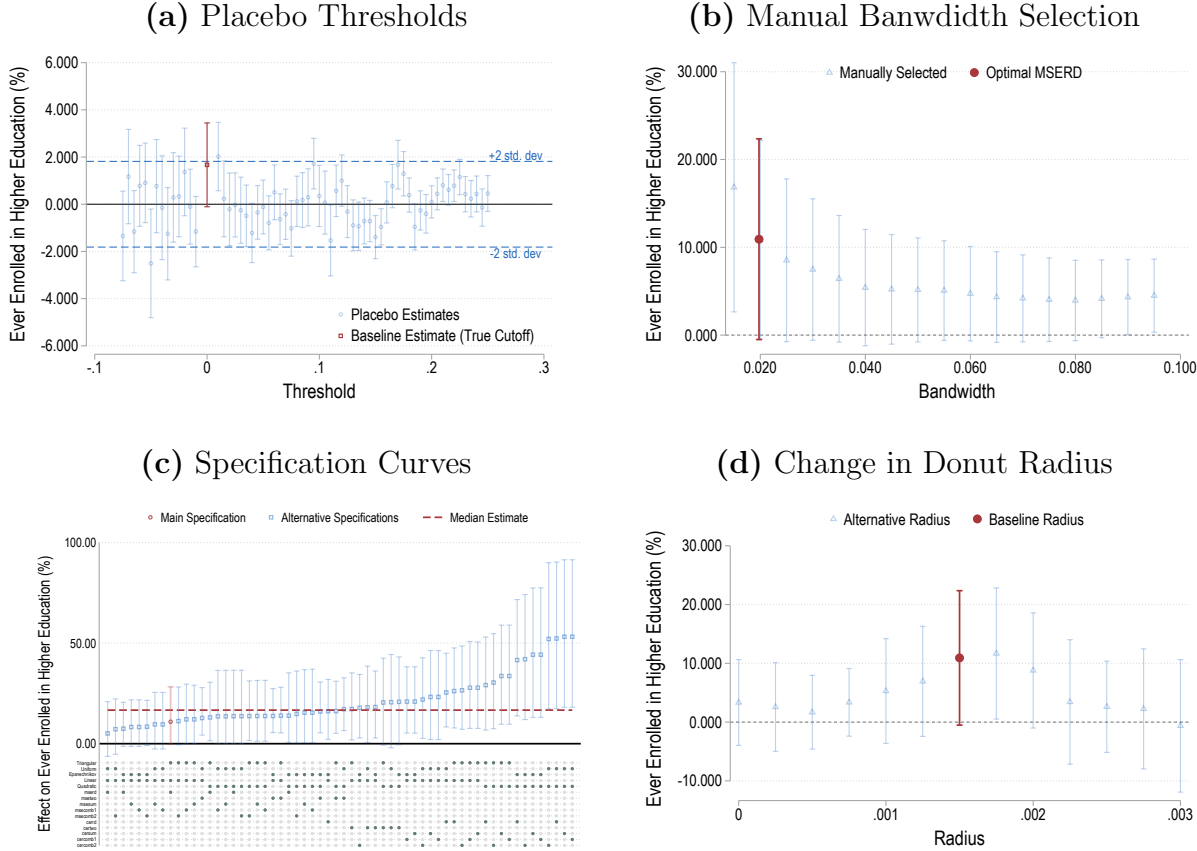Figure E.1: Summary of Effects - Alternative Ages of Treatment

a. Age of First Birth

b. Ever Enrolled in Higher Education

c. Age of First Employment Spell

d. Market-oriented summary Index

Notes:

Figure E.2: Robustness Tests: Treatment Effects on Women's Age of First Birth

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
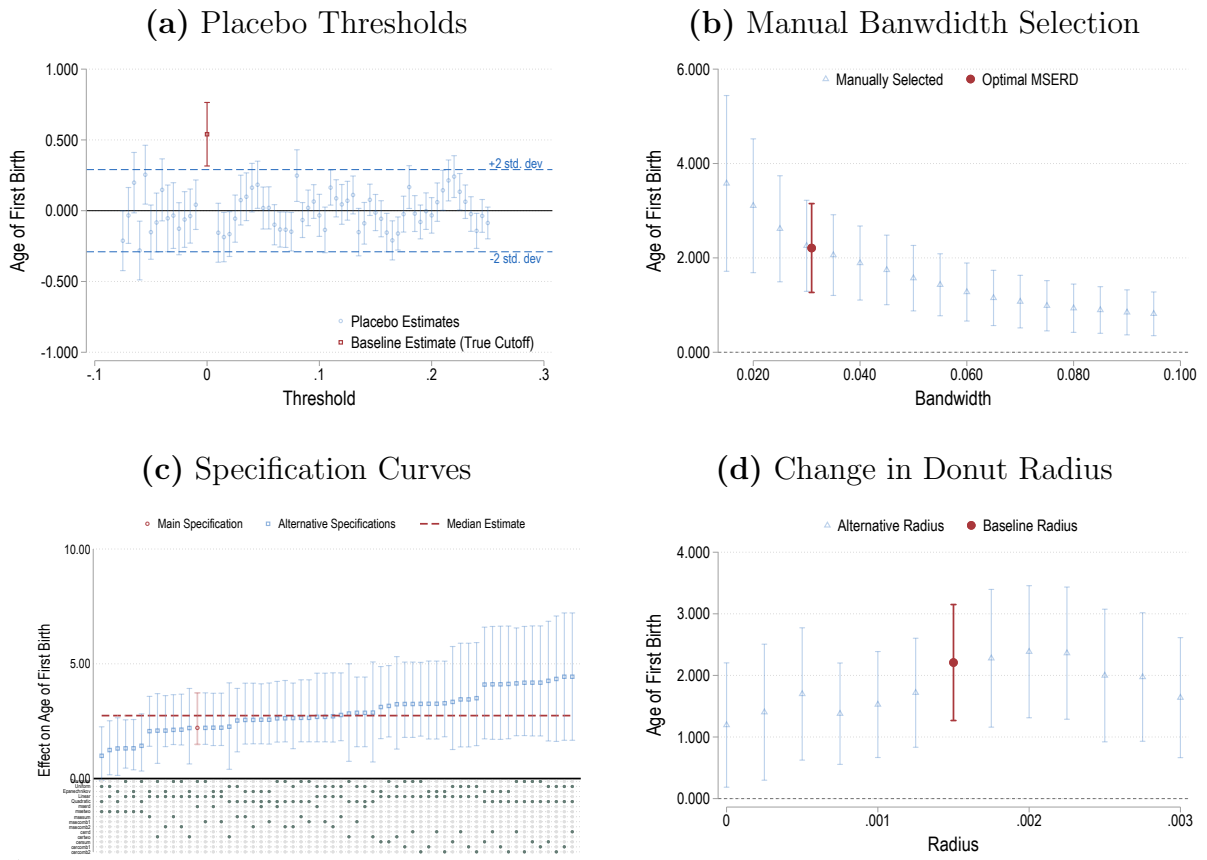
Notes:

Figure E.3: Robustness Tests: Treatment Effects on Women's Enrollment in Tertiary Education

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius

Notes:

Figure E.4: Robustness Tests: Treatment Effects on Women's Age of First Employment Spell

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
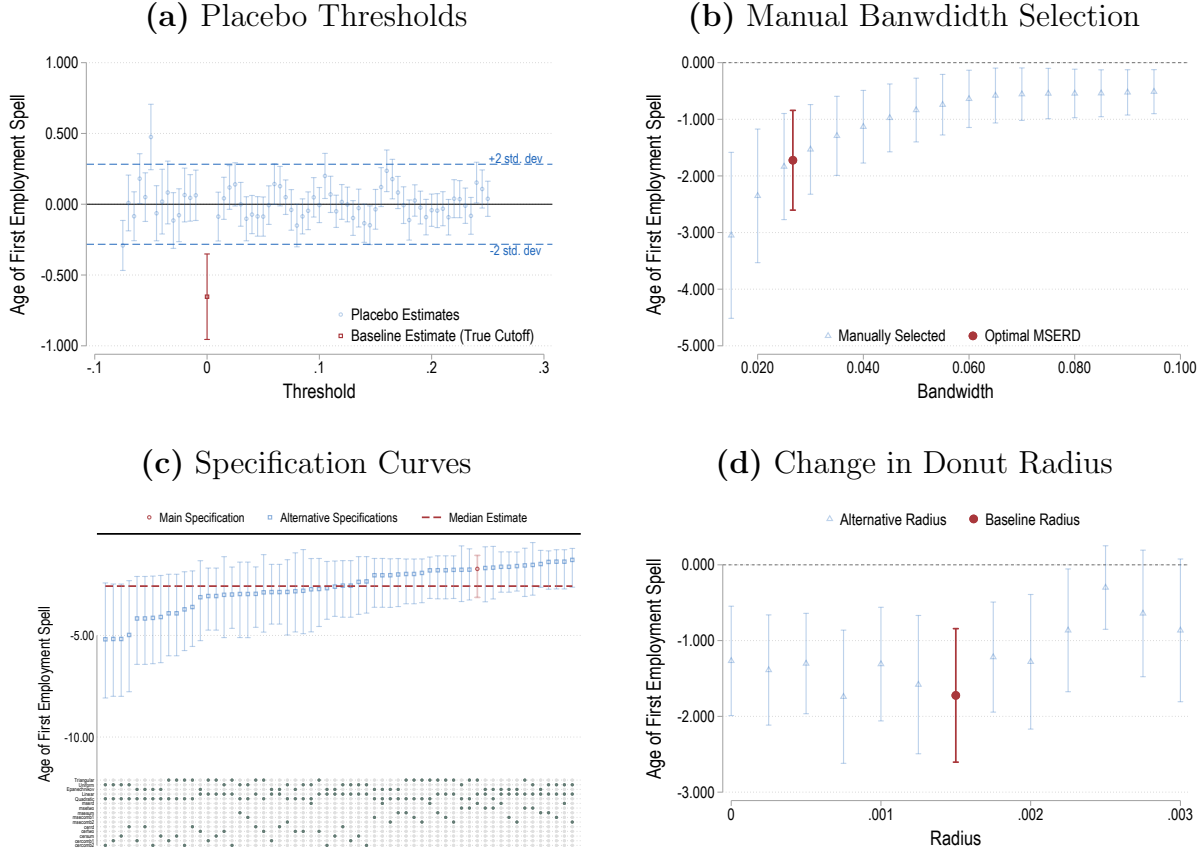
Notes:

Figure E.5: Robustness Tests: Treatment Effects on Women's Market-Oriented Transition Index

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
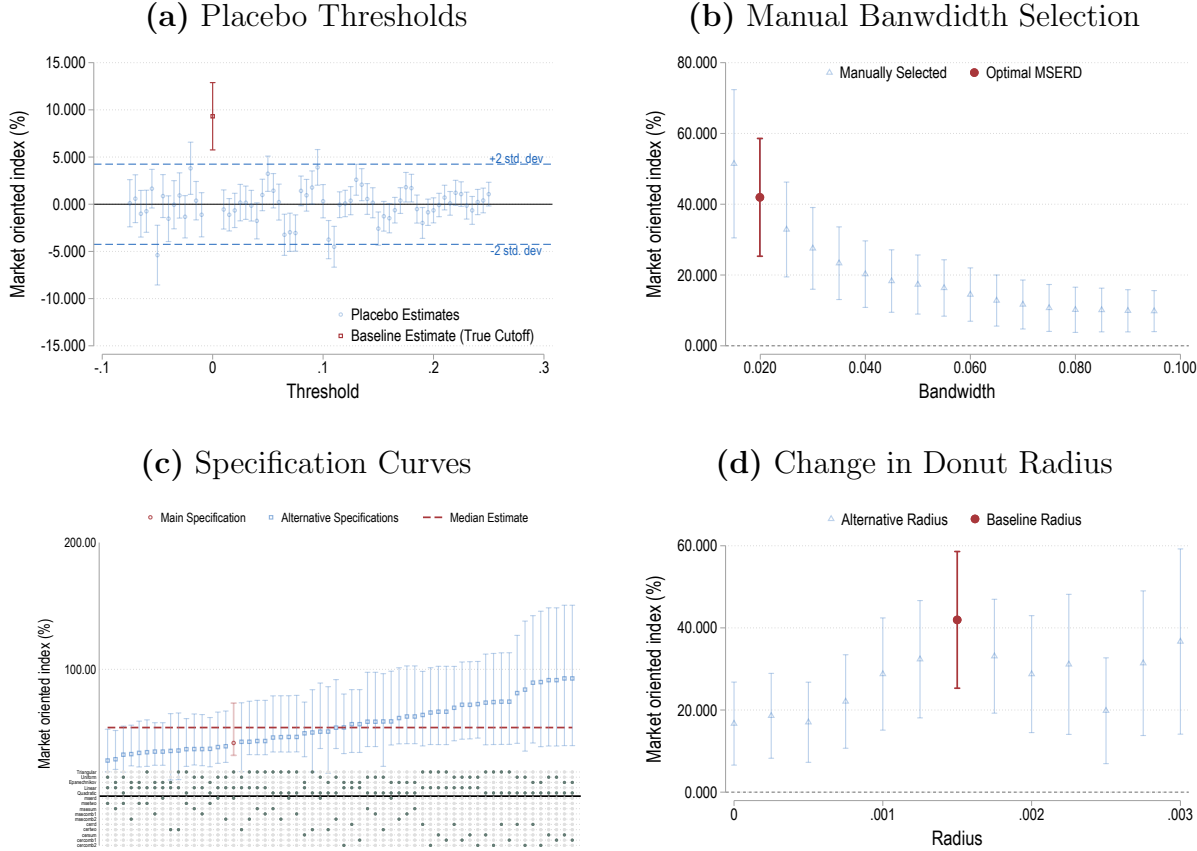
Notes:

Figure E.6: Robustness Tests: Treatment Effects on Men's Age of First Birth

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
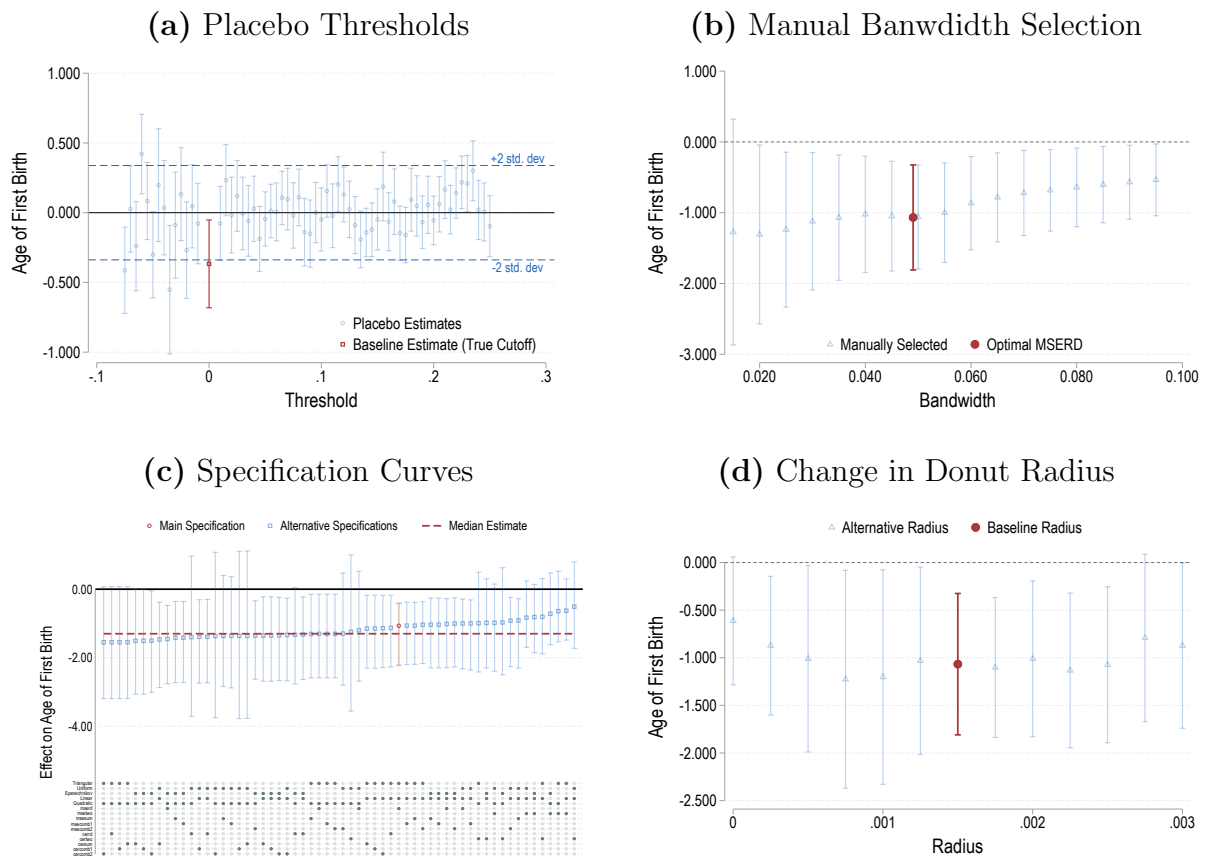
Notes:

Figure E.7: Robustness Tests: Treatment Effects on Men's Enrollment in Tertiary Education

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
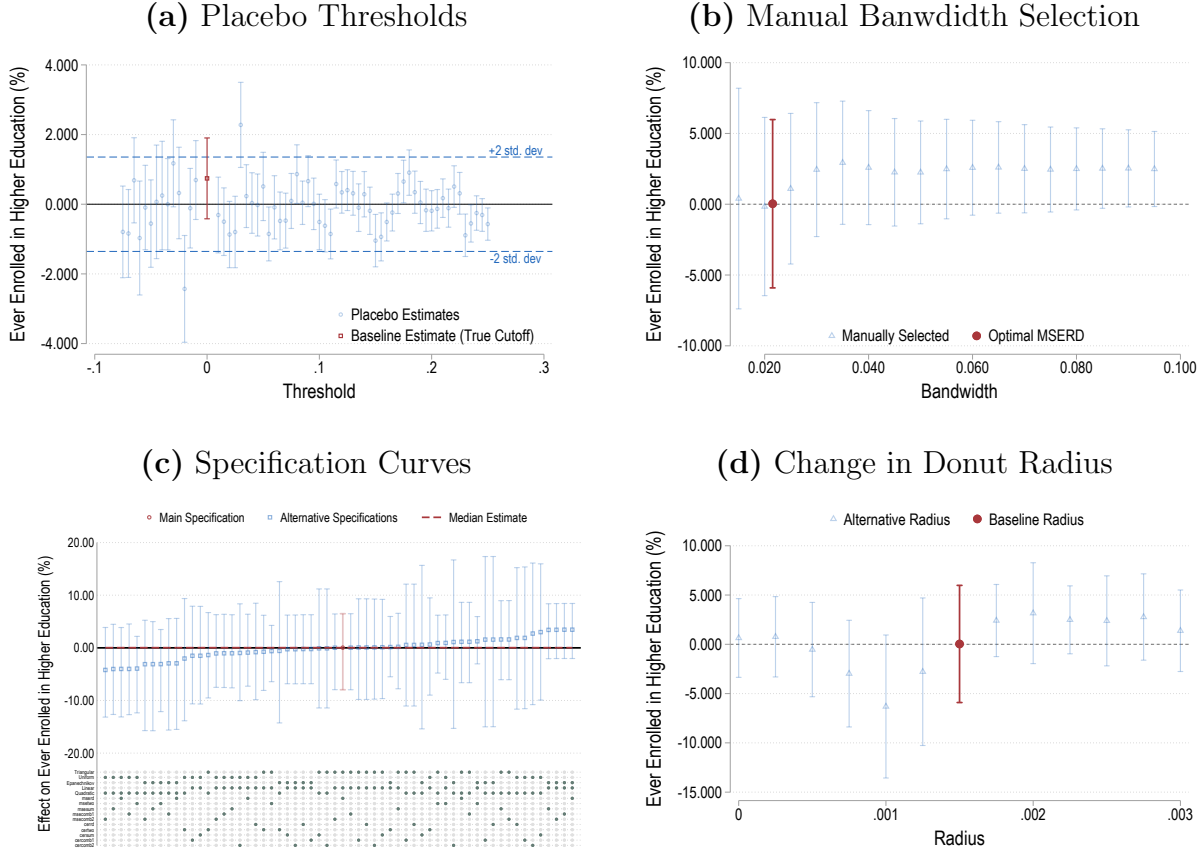
Notes:

Figure E.8: Robustness Tests: Treatment Effects on Men's Age of First Employment Spell

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
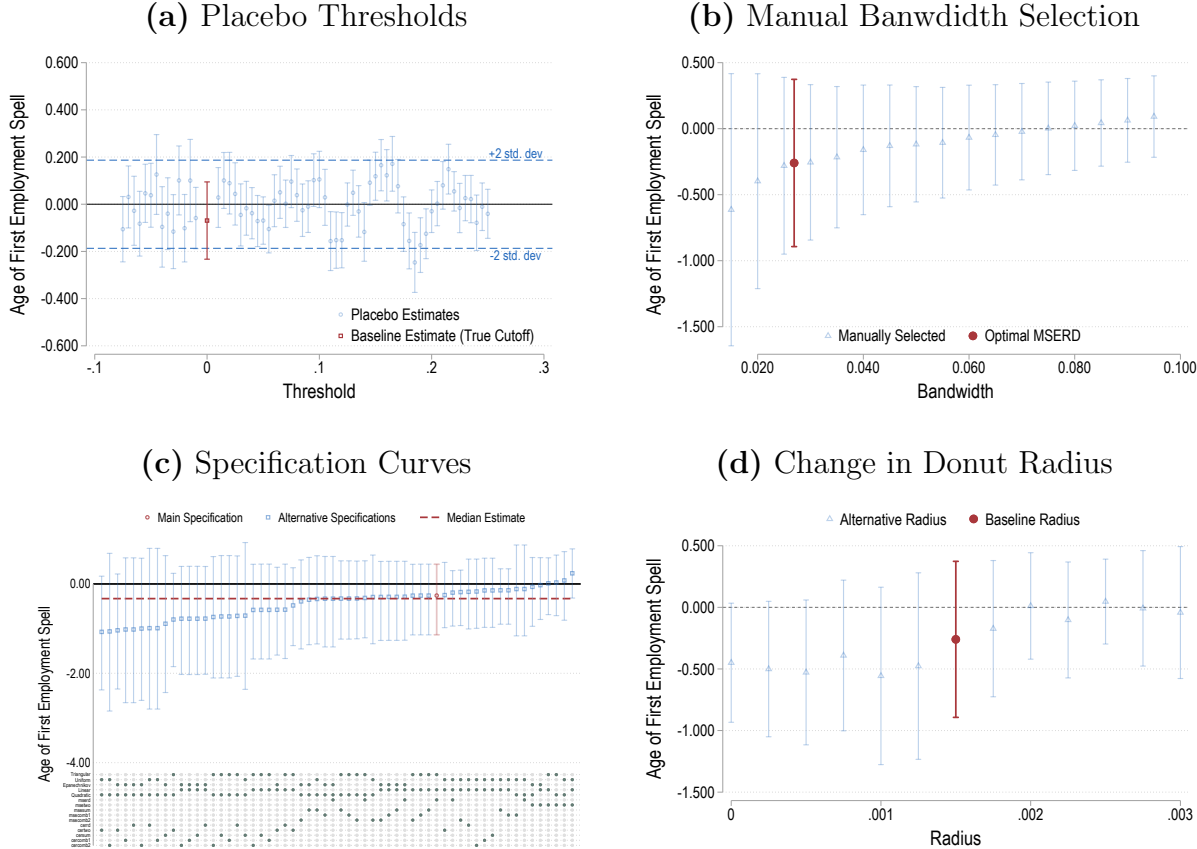
Notes:

Figure E.9: Robustness Tests: Treatment Effects on Men's Market-Oriented Transition Index

(a) Placebo Thresholds

(b) Manual Banwdidth Selection

(c) Specification Curves

(d) Change in Donut Radius
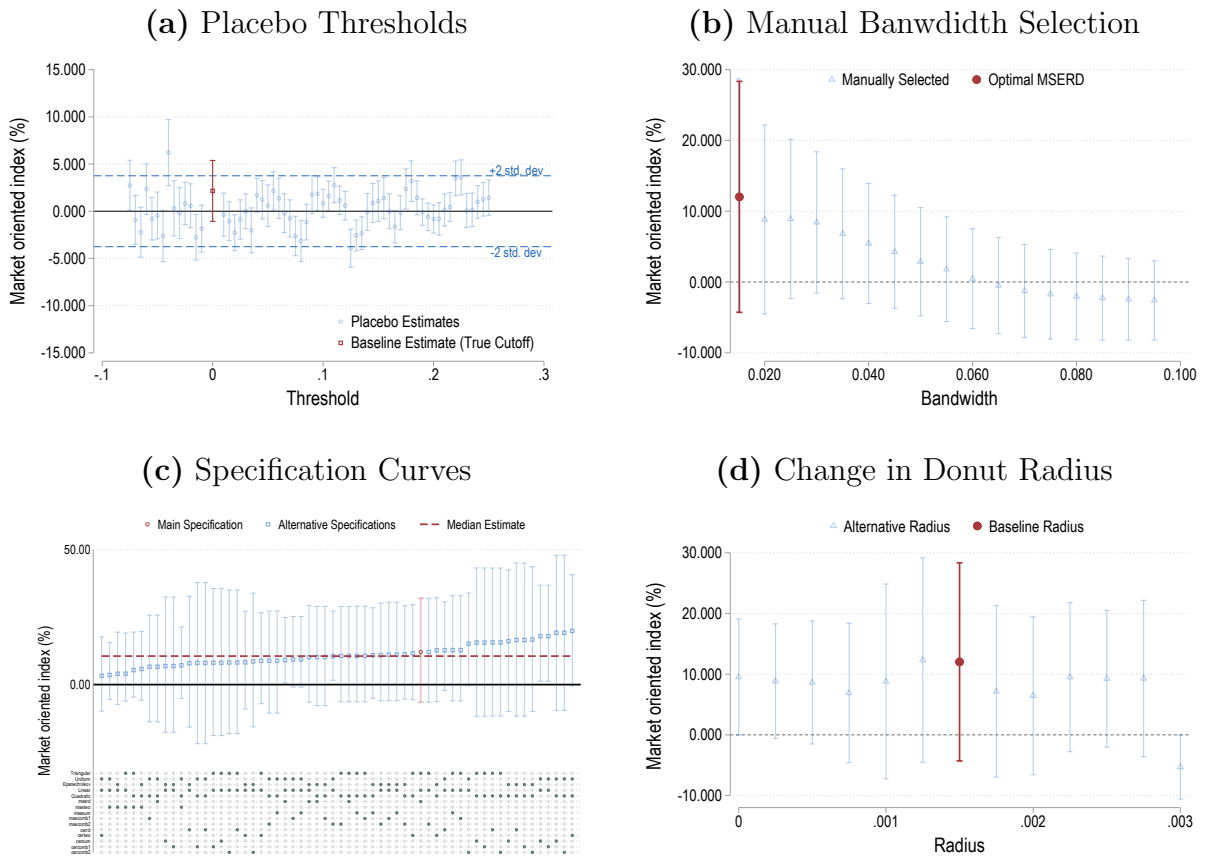
Notes:

Figure E.10: Changes in the Timing of the Events: Age by Age Effects - Balanced Sample

a. Having Children by Age γ

b.Being Employed at Age γ

Notes: