Birth during Hyperinflation, Mother Mental Health and Human Capital Accumulation

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Abstract

In episodes of hyperinflation, poor families concentrate expenditures in the pay period and reduce money demand for precautionary reasons to escape the inflation tax. We use rich longitudinal cohort data from a city in Brazil to show that children born in the pay period during hyperinflation attain up to 20% of a standard deviation more years of schooling than children born in the other periods of the month. We also show that mothers who give birth in the pay period are less likely to have mental health problems and are more likely to be working in the future, implying that financial hardship around the delivery date can have lifelong effects on mothers and their children. We also use data for all three million Brazilian students born in 1993 to estimate a positive impact of birth in the pay period on education and find that the effect disappears for the cohort born in 1995, after the end of hyperinflation.

Keywords: Early Child Development; Education, Payday Effect; Hyperinflation

JEL Codes: I25, J13, O15

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1 Introduction

There is a growing literature documenting the relationship between family resources in early childhood and human capital development, with several recent studies showing that policies and shocks that change household resources early in life impact adult outcomes.[1] There is less evidence, however, on the mechanisms driving this relationship, on the importance of shocks that take place soon after birth and on the transmission of early life shocks through middle childhood and early adulthood, sometimes called "the missing middle" (Almond et al. (2018)). Another line of research provides evidence showing that poor families do not usually smooth consumption during the month, increasing spending, consuming more calories and increasing achievement in test scores after receiving pay or transfers.[2]

Hyperinflation episodes are not uncommon in economic history. Hanke and Bostrom (2017) document 58 episodes in the last century and 37 cases since 1990, including the current situation in Venezuela and the Brazilian case in the early 1990s. We hypothesize that hyperinflation exacerbates the lack of consumption smoothing during the month, as poor households decrease their money holdings for precautionary reasons and concentrate most expenditures in the pay period to avoid the inflation tax, which rises enormously during these periods.

In this paper, we use two longitudinal data sets to test whether children born in the pay period during hyperinflation complete more years of schooling than children born in the other periods of the month. We first use rich data from a longitudinal cohort study that follows 2.5 thousand children born in 1993 until they are aged 22 and find that children born in the pay period in 1993 attain more years of schooling than those born in the other days of the month. Children born between 16 and 20 days after the payday period have 12% standard deviation (SD) fewer years of education than those born in the payday period. For the children of low-educated mothers this effect reaches 20%SD,

[1] Almond et al. (2018) review the more recent studies. Research outside economics documents that early life adversity can affect brain structure and have long-term consequences for learning, behavior and emotional health of children over their lifespan, see Phillips and Shonkoff (2000)

[2] See, for instance, Stephens (2003), Gassman-Pines and Bellows (2018) and Cotti et al. (2018).

while no difference is detected for children of more educated mothers. We also show that mothers who give birth in the pay period during hyperinflation are less likely to report mental health problems and more likely to be working eleven years after the birth of the child.

Our main results exclude C-sections, twins and children that went into intensive care right after birth, but the results are robust to the inclusion of those births as well. Since the day of natural birth within a month is arguably random, we interpret our findings as causal effects. We use the cohort data to show that several child and mother health and socioeconomic characteristics at birth are unrelated to the date of birth within a month. We also perform a series of tests to examine the robustness of our results.

We complement the analysis using data from the school census with information on the date of birth and years of schooling of more than three million Brazilian students born in 1993. Since we do not observe whether the child was born through a C-section, we include a full set of day of the month and public holidays indicators to control for any seasonality of births within the month. In this case, we identify the payday effect using the variation in the pay period across months due to weekends and public holidays. The results using the census data also show that children born in the pay period attain more years of schooling than children born in the other periods of the month.

During 1994 an economic plan stabilized inflation in Brazil. We use the sudden end of hyperinflation as a natural experiment to test whether the relationship between birth in the pay period and education observed for the 1993 cohort is due to the effects of hyperinflation. We are unable to detect any payday effects for the cohort of students born in 1995.

Our paper contributes to several strands of the literature. Our main contribution is to the line of studies that examine the effect of shocks on family resources during childhood on future outcomes. Some studies investigate the impact of aggregate economic conditions at the time of birth on adult outcomes, using variation across birth cohorts and regions. An early example is van den Berg et al. (2006) who use Dutch data to show that children born during expansions live longer than those born during recessions. Løken et al. (2012) find that increases in family income during childhood due to the Norwegian oil boom impact child years of education; Adhvaryu et al. (2019) find that a rise in the cocoa price in early life decreases the likelihood of severe mental distress in adulthood and Adhvaryu et al. (2018) document that adverse rainfall in the year of birth decreases grade attainment, but that this effect is attenuated by the receipt of conditional cash transfers. Akee et al. (2018) find that a permanent income shock due to a casino opening impacts children health and personality indicators. A large set of studies examine the short- and long-run impacts of extreme and mild events while *in utero*.[3] Moreover, a growing literature examines the long-term impacts of social transfers on education and health outcomes.[4] We extend these branches of the literature by identifying the impact of financial hardship around the time of birth on human capital in early adulthood, explaining the mechanism driving this effect, and by relying on high-frequency differences in household resources brought about by hyperinflation.[5]

This paper also contributes to the literature that documents the relationship between poverty, maternal mental health and child outcomes.[6] Several studies show that anxiety and depression rates increase during pregnancy and soon after birth and that poor mothers are more likely to be affected. Approximately 10% of postpartum women experience depressive or anxiety disorders in OECD countries, a figure that rises to between 20% and 35% in developing countries such as Brazil (Pawluski et al., 2017). In some cases, postpartum depression can last for several years and have important effects on child outcomes.[7] A negative shock can generate a depression-induced intergenerational poverty trap, as family investments in children have been shown to impact education, health and economic outcomes later in life and are affected by depression.[8] Hu et al. (2019) examine the relationship between credit constraints and depression, while Clark

[3]See Almond et al. (2018) for an excellent review of the recent literature.

[4]See, among others, Hoynes et al. (2016), Aizer et al. (2016) and Dahl and Lochner (2012).

[5]Fitzsimons and Vera-Hernández (2015) use a similar identification strategy by examining how births on weekends impact breastfeeding and, therefore, children's cognitive development.

[6]See Duncan and Brooks-Gunn (1999) for an early review and Reeves and Krause (2019) for a recent survey.

[7]See, for instance, Matijasevich et al. (2015) who also use data from Pelotas and Netsi et al. (2018)).

[8]Cunha et al. (2010) develop and estimate a multistage model of human capital accumulation, and de Quidt and Haushofer (2016) describe the possibility of a depression-induced poverty trap.

and Barazzetta (0175) use a longitudinal study in the UK to show that growing up with a mother with financial problems is associated with worse adolescent cognitive and noncognitive outcomes. We extend this literature by documenting for the first time in the effects of financial hardship around the time of delivery on maternal mental health outcomes in the future and on childrens's human capital accumulation .

A different line of the literature shows that several outcomes of poor families vary within a month depending on the exact timing of receipt of wages and social benefits. Some studies examine consumer behavior around paydays, showing that the poor consume fewer calories in the week before the receipt of benefits (Mastrobuoni and Weinberg, 2009; Stephens, 2003; Hastings and Washington, 2010; Tarasuk et al., 2007). Seligman et al. (2014) show that the risk of hospital admission due to hypoglycemia increases in the last week of the month among the poor population in California. Gassman-Pines and Bellows (2018) and Cotti et al. (2018) find that children test scores also vary within the month depending on the benefit cycle.[9] We contribute to this literature by examining the intergenerational payday effects for the first time. We hypothesize that payday effects are exacerbated in periods of hyperinflation as the inflation tax destroys the liquidity value of money, making poor families even more unable to smooth consumption, since they usually do not have access to inflation-proof payment technologies or to interest-bearing check accounts.[10] Cardoso (1992) estimates that when inflation is 47% per month (as in the case of Brazil in the 1990s), the inflation tax is equivalent to 16% of the monthly income for households that spread their expenditures evenly during the month, as compared to 8% for households that consume half of their income on the day they get paid and the other half evenly during the rest of the month, to pay for transportation, services and perishable goods, for example.

Our paper also relates to the literature that estimates the welfare costs of inflation. There are surprisingly few empirical studies that estimate the impact of inflation on

[9]Carvalho et al. (2016) find that the poor are more present-biased before payday due to liquidity constraints; Gelman et al. (2014) also find excess sensitivity to income around payday for individuals who are credit or liquidity constrained.

[10]See Wen (2015), Sturzenegger (1997) and Erosa and Ventura (2002), for example.

poverty and inequality compared to the rich theoretical literature on this subject.[11] Wen (2015) estimates costs of up to 3% or 4% of consumption when inflation reaches 10% per year, with most of the costs borne by liquidity-constrained cash-poor agents. According to this line of reasoning, when inflation reaches 45% per month, as in the case of the Brazilian hyperinflation, welfare costs will likely be much higher. We innovate by uncovering the long-term social impacts of hyperinflation for the first time in the literature, using the end of hyperinflation in Brazil as a natural experiment to credibly identify its effect on children's human capital accumulation.

The remainder of our paper is as follows. The next section details the institutional background, while section 3 describes the data and the econometric methodology used to estimate the main effects. Section 4 presents our main results, and Section 5 concludes.

2 Institutional Background

We now describe the macroeconomic and pay setting conditions prevailing in Brazil in the early 1990s. Figure 1 describes the evolution of inflation in Brazil between 1991 and 1995. It shows that after a brief decline between January and April of 1991 (as a result of a failed stabilization plan), inflation rose again quickly to reach 25% per month at the end of 1991, remaining around this level until the end of 1992. At the beginning of 1993, however, inflation started to increase continuously again, reaching 45% in June 1994. During the first semester of 1994 a new stabilization plan was launched, which successfully reduced inflation down to low levels uo to the present.

Neri (1995) describes the institutional details of the Brazilian economy during the hyperinflation period. With inflation increasing from 5% per month in 1991 to 45% in 1994, consumers started substituting financial assets for money as a store of value to escape the inflation tax. According to Neri (1995), however, only 10% of the Brazilian adult population had access to interest-bearing bank accounts and among poor families,

[11] See, for instance, Lucas (1980) and Imrohoroglu (1992).

this share was close to zero.[12] Another possibility to avoid the inflation tax is to use credit cards. However, again, only richer individuals held credit cards in Brazil in the early 1990s.[13] The other option would be to stock durable goods, but a higher share of poor families' consumption basket is composed of cash-only perishable goods (Neri, 1995).[14] Therefore, the inflation tax was very regressive.

In terms of pay-setting rules, Brazilian labor legislation mandates that wages and salaries be paid until the fifth working day of the month. If the fifth working day falls on a Saturday, the payment has to be made on the previous Friday. Therefore, in 1993, the maximum allowed payday varied between the fourth and seventh days of the month (because of public holidays). In times of hyperinflation, employers have incentives to delay wage payments until the last permitted day since cash can be invested overnight in investment funds that pay daily interest rates. Labor legislation applies to formal sector workers only, but pay rules in the informal sector tend to closely follow those of the formal sector (Ulyssea, 2010).

The main exceptions to this rule are employers and self-employed workers, whose remuneration is based on their own sales revenues. Nevertheless, it is likely that retail sales also fluctuate within a month in line with the payday during periods of hyperinflation , since workers have incentives to spend their wages right after being paid to avoid the inflation tax; hence earnings of self-employed workers are likely to be affected by paydays as well.

In times of hyperinflation, organized employees can strike in collective action for a reduction in the interval between wage payments as another way to avoid the inflation tax. Data from the monthly household surveys, however, show that in 1993, despite hyperinflation, 79% of all employees received their wages only once a month, while only 21% of them received their pay every two weeks and 10% on a weekly basis. Self-employed and employers comprised 22% of all workers. The fact that a significant share of workers

[12]This happened because banks required a minimum income level to open an account to compensate for the fixed operating costs that could not be passed through prices due to government regulations.

[13]This is because the risk of default varies negatively with income, because there are fixed costs of monitoring and because poor individuals cannot offer any collateral.

[14]Direct exchange of goods (barter economy) was never a common practice in Brazil.

are paid more frequently than monthly means that our estimate of the education premium for children born in the pay period should in fact be interpreted as intention to treat (ITT) effects since our data do not allow us to observe the actual date of pay for the households in our sample.

3 Data

Our data come from two different sources. The first is the "Longitudinal Study of Children Born in Pelotas", which has been used in many influential studies before.[15] Pelotas is a city in the south of Brazil that has a population of approximately 340,000 inhabitants, with 93% of the population living in urban areas. The first wave of the data was collected from daily visits to all five hospitals of the city between January 1st and December 31st, 1993.[16] Only 126 mothers refused to participate in the survey, so 5,249 mothers with born-alive children were interviewed at the baseline on a variety of topics including perinatal health conditions; demographic and socioeconomic status; pregnancy risk factors, such as smoking and drinking habits; health services utilization; and information related to prenatal care and delivery. The newborns were weighed and measured in the first 24 hours after birth. The study has been following the newborns through time, collecting information on their socioeconomic characteristics, health, education and labor market status at different points of their lives. In this paper, we use the data collected at birth and in the 2004, 2008, 2011 and 2015 waves.

The main advantage of the cohort longitudinal data set is its richness, which allows us to examine the plausibility of our identifying assumptions and include a rich set of controls in all econometric specifications. Moreover, we can observe the children at multiple points in time, which permits us to estimate the evolution of the payday effect over the course of their lives, allows for multiple testing and increases the power of the statistical tests. Its main disadvantage is that we observe only one cohort and therefore cannot separate out the payday from the hyperinflation effects. To overcome this limitation we also use

[15] See, for instance, Victora et al. (2008) and Victora et al. (2015).

[16] Around 99% of all deliveries in Pelotas take place in hospitals.

the school census data described below.

Attrition rates in the Pelotas surveys are very low for a longitudinal study of this sort. Approximately 83% of the original cohort members were interviewed again when they were aged 11, 78% of them were found when they were aged 18 and 72% were interviewed when they were aged 22. Below, we show that attrition is not related to the period of birth within a month. Information on the exact date of birth was collected at baseline by the interviewers at the hospital as well as when the children were aged 11 as reported by their mothers. The correlation between these two measures of date of birth is 0.99. In most of our estimations, we use the date of birth reported by the mothers since we believe those data are less prone to measurement error.

Our main results draw on a sample that excludes children born from C-sections, since in those cases the mother could, in principle, choose the date of delivery endogenously by postponing it until after the payday, which would threaten our identification assumption. We also drop twins and the children who went into intensive care soon after birth.

Our main variable of interest is years of schooling. We combine the information on years of schooling obtained from different waves of the cohort data, when the children were aged 11, 15, 18 and 22, following the methodology described in Anderson (2008). In this way, our results provide a statistical test for whether the period of birth has any effect on education, which is robust to concerns of multiple inference, that is, that one null hypothesis is rejected just because we tested many null hypotheses. For each wave, we compute a standardized measure of years of education by subtracting the mean years of schooling in the sample from the education of each adolescent and dividing it by the standard deviation of the omitted group (children born in the pay period). The index is a weighted mean of each standardized education measure, with the weights calculated to maximize the amount of information captured in the index by giving less weight to the measures that are highly correlated. This approach allows us to deal with attrition as well, since we do not need the education attainment information from all the waves for each individual because we can use only the available education measures to construct the education index. We also report the results for each wave separately.

To investigate possible mechanisms linking the day of birth to human capital accumulation, we examine the effects of the day of delivery on maternal mental health. We construct an index that combines information on mothers' mental disorders with working status and whether the mothers took sleeping pills or tranquilizers in the previous month, all measured when the reference child was aged 11. The measure of maternal mental health is obtained using the replies to the Self-Reporting Questionnaire-20 (SRQ-20), which was developed by the World Health Organization as a screening tool for common mental disorders in primary health care settings, especially in developing countries. The instrument consists of 20 yes/no questions about common mental health symptoms such as anxiety, depressive symptoms, and psychosomatic complaints. The SRQ-20 has been used and validated in Brazil.[17] The second measure is the mother's working status at the time of the interview, and the third is whether she was taking sleeping or tranquilizer pills in the month prior to the interview.

Table 1 presents descriptive statistics on our estimation sample. It shows that the date of birth is evenly spread along the 5-day intervals. In terms of education, at aged 11, Brazilian students are supposed to have finished primary education (4 years of education), at 15, secondary (8 years), at 18, high school (11 years) and at 22, college (16 years). In our sample, however, the mean years of education is 3.5, 6.2, 8.5 and 9.9, respectively, as grade repetition is very common in Brazil. Table 1 shows that 23% of children failed the first grade 1 at least once, that the average number of grade one repetitions is 0.36 and that 39% of the children had already failed a grade by the time they were aged 11. In terms of maternal mental health problems, eleven years after the birth of the reference child mothers answered positively for 5.7 out of the 20 SRQ questions on average, 14% were taking pills and 49% were not working.

Figure 2 compares the education attainment of the children born in the pay period with those born on other days of the month, using data from the cohort survey and adding 95% confidence intervals around the means. The figure shows that children born in the pay period attain approximately 8%SD more years of education than the sample

[17]See de Jesus Mari and Williams (1986).

mean, while children born between 11 and 15 days after the pay period have almost 10%SD fewer years of education than the mean. The mean years of education approach the sample mean for those born more than 25 days after the pay period, as they are born closer to the pay period again.

Figure 3 compares the index of maternal mental health problems eleven years after giving birth for mothers who delivered in the pay period with that of mother who gave birth in the other periods of the month, using data from the cohort survey and adding 95% confidence intervals around the means. The figure shows that mothers who gave birth in pay period are 7%SD less likely to have mental health problems than the sample mean, while those who delivered between 11 and 15 days after the pay period are 5%SD more likely to suffer problems than the mean.

We complement the cohort longitudinal data with the school census, using information on date of birth, gender and grade level currently attended for the universe of Brazilian students (approximately 3 million students in each cohort). The school census started collecting data at the individual level since 2007, when the children of the 1993 cohort were 14 years old. We use these data to estimate the impact of birth in the pay period on education for Brazil as a whole and to estimate its effect for the cohort born in 1995, after inflation was controlled by a successful stabilization plan.[18]

The main advantage of using the school census is that it has data on education achievement for all Brazilian students, covering all municipalities. Its main drawback is that it only has information on date of birth, race and gender so that it is not possible to control for other mother/child characteristics. In particular, the census does not contain information about mother education or whether the child was born through a C-section. Unfortunately, it is not possible to match the students in the survey data with the same students in the census data.

The bottom panel of Table 1 presents descriptive statistics of the variables available in the census data. It shows that the date of birth is evenly spread along the 5-day

[18]We do not use the schooling information after the age 14 because many adolescents leave school after this age so that the sample of continuing students becomes highly selected.

intervals, that average years of schooling is 7.45 when the children are aged 14 and that approximately half of the children are boys; the observations are equally split between those born in 1993 and in 1995.

4 Empirical Strategy

The aim of this paper is to estimate the causal effect of birth in the pay period on long-run human capital accumulation. To do that, we first use the cohort longitudinal data from children born in 1993 in the city of Pelotas. Because of the limited sample size in the cohort survey, we group the dates of birth in five-day intervals and estimate the following reduced-form equation:

$$Educ_{it} = \alpha + \sum_{t=2}^{6} \gamma_t D_t + \delta_m + \lambda_w + X_{it}\beta + \varepsilon_{it}, \tag{1}$$

where $Educ_{it}$ is number of years of education of children i, born on date t; D_t indexes a vector of dummies accounting for birth in each five-day interval, defined based on the difference between the child's date of birth and the last day of the pay period. The omitted group represents the days in the pay period, and our parameters of interest are the γ_t, which measure the differential impacts of birth in each of the five-day intervals with respect to birth in the pay period. Also, δ_m represents month of birth effects, λ_w represents day-of-the-week effects; the vector X_{it} is composed of several child and mother characteristics observed at birth; and ε_{it} is a random shock.

Equation 1 controls for month-of-birth dummies to capture seasonal effects that may impact education and be correlated with socioeconomic characteristics, such as minimum age requirements to enter school in the first grade. By including the month dummies as controls, our identification strategy is rather different from previous models that have used variation across months or quarters of birth to identify the effect of education on wages.[19] Our main identification assumption is that there are no systematic unobserved differences between mother/child pairs who give birth/are born during the pay period and

[19] See Angrist and Keueger (1991), for example.

their counterparts who give birth/are born on other days of the month. This assumption would fail, for instance, if there were selection on unobservables driving deliveries on specific days of the month. Since we are excluding the cases of C-section delivery, this would occur, for instance, if there were *in utero* shocks in specific periods of the month that led some mothers to self-select into preterm delivery.

While this is essentially a nontestable assumption, we can subject it to several "plausibility" tests. We first test whether the main variables of interest (the periods of birth) are correlated with several child/mother characteristics observed at birth. If there is unobserved selection of births on specific days of the month, we would expect to see correlations between mother, gestation and/or child characteristics, such as education, gestational length or childbirth conditions and birth on specific days of the month.

The variables we use to test the balance of the sample across weeks of the month are mother characteristics such as age, race, body mass index, weight gain, education, household composition, family income, number of previous children and number of previous pregnancies. With respect to the gestational period, we examine the correlation between date of birth and measures of the number of prenatal visits, labor force participation, labor induction and indicators of gestational risk such as whether the mother had hypertension, diabetes, urinary infections, other problems, anemia, risk of abortion, whether she was hospitalized and whether she drank alcohol or smoked during pregnancy. With respect to baby birth conditions, we consider the gender, gestational age (using the measure of Dubowitz et al. (1970)), birth length and weight, cephalic perimeters and whether the child was born with low birth weight or preterm.

Table 2 summarizes these variables and reports the p-value of a joint test that each of the five interval dummies is zero in a separate regression for each variable that does not include month dummies or any additional controls. Column (1) reports the mean of each variable, column (2) reports the standard deviations, column (3) reports the F-statistics of the joint test and column (4) reports the p-values. The results of columns (3) and (4) show that the tests fails to reject the null in all cases. None of the interval dummies are jointly statistically different from zero at the 10% level. Moreover, the last row of the table reports the result of a joint test that each of the 5 interval dummies is zero in all regressions, which does not reject the null with a p-value equal to 0.86. This result is very reassuring and is important evidence supporting our identifying assumption.

5 Results

5.1 Cohort Data

We begin with the results using the longitudinal cohort data for the city of Pelotas. Table 3 presents the results of estimating equation 1 relating education attainment with the date of birth intervals in 1993. The regressions control for all child and mother birth characteristics described in Table 2, plus hospital, month and day of the week fixed effects. The first column shows that children born in the other periods of the month attain less years of education than those born in the pay period, with the magnitude of the estimated coefficient peaking 16 to 20 days after the pay period. Since the education index is measured in units of a standard deviation (SD) of the omitted group (birth in the pay period), the magnitude of the estimated coefficient means that birth in the period between 16 and 20 days after the pay period results in 12%SD less education as compared to birth in the pay period.

The results of the second column, which uses the sample of the poorer households only (those with per capita income lower than the median at birth), show that the magnitudes of the estimated coefficients are larger, implying a 17%SD reduction in years of schooling for those born between 16 and 20 days after the pay period. Column (3) presents the results for the sample of households whose income was higher then the median when the reference child was born, which show, as expected, that none of the estimated coefficients are statistically different from zero, since richer families are able to avoid the inflation tax by using inflation-proof technologies. Column (4) estimates the same regression for the sample of low-educated mothers only (less then six years of schooling, the sample median at 1993), and shows even stronger effects, peaking at 25%SD for the children born between 11 and 15 days after the pay period. Column 5 shows that none of the effects are statistically different from zero in the sample of more educated mothers.

Table 13 presents results from estimating the main equation separately for each wave of the survey. The table shows that all of the date-of-birth dummies attract negative coefficients at all ages and that the dummies identifying birth between 11 and 15, 16 and 20 and more than 20 days after the pay period are higher in magnitude and statistically significant more often. In the last wave, when the children are aged 22 and most have finished their formal education, birth between 16 and 20 days after the pay period leads to a reduction in completed years of schooling of 14%SD.

To shed more light on the transmission of early childhood shocks to early adulthood (the "missing middle"), we examine the impacts of birth in the pay period on children's grade repetition patterns in their first few years at school by constructing an index composed by variables indicating whether the child repeated grade one, the number of times she repeated this grade and whether she had failed any grades by age 11.[20] Table 4 shows that children born between 11 and 15 days after the end of the pay period are 15%SD more likely to fail initial grades than children born in the pay period. Columns (2) and (4) show that the impact on grade repetition is much larger for poorer and less educated mothers, respectively, reaching approximately 25%SD for those families. Columns (3) and (5) show that there is no impact of birth in the pay period on grade repetition for richer and more educated mothers, as one would expect. Table 15 in the appendix show that children born between 11 and 15 days after the pay period are more likely than those born in the pay period to fail at their first grade at school, to fail this grade more than once and also to fail any grade by the age 11. It appears, therefore, that one of the channels by which early shocks are transmitted to adult outcomes is through school readiness, that is reflected in grade repetition.

[20] Gomes-Neto and Hanushek (1994) examines the causes and consequences of grade repetition in Brazil.

5.2 Mechanisms

We now investigate the underlying mechanisms that may explain the striking payday effects on education we have uncovered so far, drawing on the emerging literature in economics about depression and intergenerational mobility.[21] Table 5 examines the impact of giving birth in the pay period on the index of maternal mental health problems eleven years after birth, which combines the results of the Self-Reporting Questionnaire screening device described in Section 3 with information on working status and usage of sleeping pills or tranquilizers.

Column (1) shows that mothers who gave birth in the pay period are less likely to have mental health problems 11 years later, even after we control for all variables of Table 2, plus hospital, month and day-of-the-week birth fixed effects. The magnitude of the estimated coefficients peaks at 11%SD for mothers who delivered between 16 and 20 days after the pay period. In the remainder of the table we examine the heterogeneity of the payday effect on maternal mental health problems with respect to family income and human capital. In columns (2) and (4) we show that the magnitude of the effects is much higher in the sample of poorer and less educated mothers, reaching 15%SD and 19%SD, respectively. Columns (3) and (5) estimate the same regression on the samples of richer and more educated mothers, respectively, and show that none of the estimated coefficients on the date-of-birth dummies are statistically significant, as expected.

Table 14 in the appendix shows the impact of hyperinflation separately for each measure of maternal mental health problems. It shows that all of the date-of-birth dummies attract positive coefficients (that indicate more problems) and that the dummy identifying birth more than 20 days after the pay period is a statistically significant determinant of maternal mental disorders, while the dummy indicating birth between 11 and 15 days is the best predictor that the mother will not be working fifteen years after the birth of her child, leading to a reduction in the working probability of 14%SD compared to the mothers who gave birth in the pay period.

[21] See de Quidt and Haushofer (2016) and Reeves and Krause (2019).

5.3 Census Data

We now use the school census data to examine the external validity of the effects we have uncovered in the city of Pelotas. The school census data do not have information on either delivery methods or mother education, and therefore, we have to use data from all students born in 1993. Moreover, since the individual-level information only became available in the 2007 census, our estimating sample is composed of students observed when they are aged 14.

We start by replicating the results obtained with the cohort survey using the census data for the city of Pelotas, and then, we examine the effect of birth in the pay period for the generation born in 1995 (after the end of hyperinflation). We then extend the analysis to all Brazilian students. In column (1) of Table 6, we use the information from the second wave of the cohort study, carried out when the children were aged 15, to reestimate the effect of birth in the pay period on years of education. The estimated coefficients are negative and most are statistically significant, reaching 19%SD for those born between 11 and 15 days after the pay period. Next, column (2) uses the sample of students drawn from the school census who we born in 1993 in the city of Pelotas and still live in the city, for the sake of comparison with the cohort surveys. The estimates are all negative and similar to those obtained through the cohort study. Column (3) includes in the estimation the students born in Pelotas, but who left the city before they were aged 14; the results show that the estimates are still negative and statistically significant for those born between 11 and 15 days and more than 20 days after the pay period, although the magnitudes of the estimated effects are smaller than those obtained in column (2).

In 1993, the Brazilian economy was in the midst of a hyperinflation process, which arguably is the force driving the striking payday effects that we have uncovered so far, as households concentrate expenditure on goods and services in the pay period to escape the inflation tax and reduce demand for money, which was the only source of savings available to poor household to self-insure against idiosyncratic shocks associated with giving birth, for instance. During 1994, an economic stabilization plan was launched (the "real plan") that successfully controlled inflation. The end of hyperinflation offers an excellent opportunity for us to examine whether the payday effects that we have estimated so far are due to the effects of hyperinflation. In column (4), we use the sample of students born in 1995 in the city of Pelotas, and none of the estimated coefficients are statistically different from zero. We are therefore unable to detect any effects of birth in the pay period on education after inflation was controlled for.

Table 7 extends the analysis to all Brazilian students. Column (1) shows that birth after the pay period is associated with lower levels of education, especially for children born more than 20 days after the last allowed payday. Column (2) includes a complete set of calendar-day-of-the-month effects to control for seasonality effects that may affect date of birth and be associated with years of schooling, such as delivery through C-sections, for example. When we insert these dummies in the regression, we are in fact using the variations in the distance from pay periods of the same calendar day across months due to public holidays and weekends to identify the effect of each group of days. The magnitudes of the estimated coefficients increase (in absolute value) in column (2), which means that the results in column (1) are not being overestimated by cyclical effects within the month. The estimates show that children born more than 20 days after the pay period attain 3%SD less education at age 14 than those born in the pay period. In column (3), we use the sample students born in 1995 and the estimates show that none of the coefficients are statistically different from zero, as in the case of Pelotas.

5.4 Robustness

In Table 8, we present additional results using the main specification to examine whether the main result is robust to alternative sample definitions and changes in the set of control variables. Column (1) displays the results of a regression that does not include any controls, showing that the magnitudes of the estimated coefficients actually increase in absolute value, as compared to those in Table 3. Column (2) includes only the month and day-of-the-week fixed effects and the results show that the estimated date of birth effects increase even more. Column (3) reproduces the main specification of Table 3, but includes births through C-sections. The estimated coefficients decline in absolute size but remain statistically significant for births between 16 and 20 days after the pay period. Column (4) reports estimates including births through C-sections, babies who went into intensive care and twins with similar results.

In Table 9, we compare the estimates of the main equation produced using the measure of date of birth reported by the mothers when the children were aged 11 with another measure that was collected by the interviewers at the hospitals where the mother gave birth. Column (1) shows that using this alternative measure of date of birth does not qualitatively change the main results, reported in column (2) as a benchmark.

In Table 10, we examine whether the dummies accounting for the five-day intervals predict the probability of C-sections using the complete cohort survey data. In column (1), we do not include any controls, the estimates show that none of the coefficients associated with the date- of-birth intervals is statistically different from zero and the F-test on whether all coefficients are jointly equal to zero cannot reject the null, with a p-value of 0.84. In column (2), we include all the controls from Table 2, and the coefficient on birth between 16 and 20 days after the pay period is now marginally significant at 10%, but the F-test also fails to reject the null, with a p-value of 0.35. This result means that the probability of birth through C-sections is not related to the pay period.

In Table 11 we examine whether the dummies accounting for the five-day intervals predict the probability of attrition at age 11, when we collect our preferred measure of the date of birth and the first measure of years of schooling.[22] In column (1), we do not include any controls and the estimates show that none of the coefficients associated with the date of birth intervals is statistically different from zero and the F-test that all coefficients are jointly equal to zero cannot reject the null with a p-value of 0.21. In column (2), we include all the controls from Table 2 and the coefficient on birth more than 20 days after the pay period is now marginally significant at 10%, but the F-test also fails to reject the null, with a p-value of 0.24.

In Table 12, we examine whether the dummies accounting for the five-day intervals predict the probability of infant mortality in the first year of life. Only 2% of the children

[22] Attrition rate was 16% in the cohort survey.

born alive died before the age of 1. In column (1) we do not include any controls, the estimates show that none of the coefficients associated with the date-of-birth intervals is statistically different from zero, and the F-test of whether all coefficients are jointly equal to zero cannot reject the null, with a p-value of 0.89. In column (2), we include all the controls from Table 2, and the F-test also fails to reject the null, with a p-value of 0.63.

6 Conclusions

This paper shows that children born in the pay period during hyperinflation in Brazil attain more years of education over the course of their lives than those born in the other periods of the month. We also show that mothers who give birth in the pay period are less likely to have mental health problems eleven years after birth and are more likely to be working. These effects are stronger for low-educated mothers and those who were poorer at the time of birth.

The results of the paper show for the first time in the literature that financial hardship around the time of birth can have important long-term effects for both mothers and their children. In terms of policy recommendations, it is important to protect poor mothers from financial problems at the time of delivery and to identify and provide help for mothers who become depressed after giving birth, since they may become unable to invest in their children, who will then face developmental problems that may create an intergenerational depression-induced poverty trap.

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7 Tables

Table 1: Descriptive Statistics

	Mean	Std. Dev.	Count
Pelotas Survey Data			
Pay Days	0.173	0.378	2662
1-5 Days After PD	0.164	0.370	2662
6-10 Days After PD	0.166	0.372	2662
11-15 Days After PD	0.166	0.372	2662
16-20 Days After PD	0.183	0.387	2662
>20 Days After PD	0.148	0.355	2662
Education Index	-0.026	0.945	2662
Education Age 11	3.479	1.118	2660
Education Age 15	6.268	1.773	2547
Education Age 18	8.474	2.229	2363
Education Age 22	9.892	2.832	2160
Mother Problems Index	0.031	0.641	2647
Mental Health Disorders	5.744	4.531	2636
Mother Takes Pills	0.138	0.344	2647
Mother Does Not Work	0.489	0.500	2645
Grade Repetition Index	0.000	0.944	2637
Repeated Grade 1	0.233	0.423	2636
No. Grade 1 Repetitions	0.362	0.794	2636
Repeated Any Grade	0.392	0.488	2637
School Census Data			
Pay Days	0.194	0.395	6,647,903
1-5 Days After PD	0.164	0.370	6,647,903
6-10 Days After PD	0.165	0.371	6,647,903
11-15 Days After PD	0.167	0.373	6,647,903
16-20 Days After PD	0.161	0.368	6,647,903
>20 Days After PD	0.149	0.356	6,647,903
Education Age 14	5.730	1.504	6,647,903
Education Index	0.000	0.995	6,647,903
Male	0.507	0.500	6,647,903
Born in 1995	0.505	0.500	6,647,903

Note: Survey sample consists of children born in 1993 in the city of Pelotas, interviewed when they were 11 years old, excluding children born by C-sections, twins and children placed in intensive care after delivery. Census sample consists of children born in 1993 and in 1995 attending school when they were age 14 in all Brazilian municipalities.

Table 2: Balance

	Mean	Std.Dev.	F-stat	p-val
	(1)	(2)	(3)	(4)
Mother Characteristics				
Mother Age	25.49	6.22	0.32	0.90
Mother White	0.77	0.42	0.89	0.49
Mother BMI	22.59	3.62	1.16	0.33
Mother Weight Gain	11.26	5.57	0.28	0.92
Mother Education	6.39	3.16	0.60	0.70
Lives with Partner	0.88	0.33	0.89	0.49
Lives with Parents	0.27	0.45	1.09	0.36
Number of Previous Kids	1.19	1.45	0.37	0.87
Number of Pregnancies	2.47	1.76	0.22	0.95
Household Income per Capita	564	757	0.78	0.57
Pregnancy				
Worked	0.35	0.48	0.51	0.77
Number of Ante-natal Visits	7.58	3.25	1.03	0.40
Hypertension	0.13	0.33	1.06	0.38
Diabetes	0.02	0.14	0.31	0.91
Urinary Infections	0.33	0.47	1.08	0.37
Hospitalized	0.06	0.24	0.40	0.85
Anemia	0.47	0.50	1.34	0.24
Other Problems	0.08	0.28	1.14	0.34
Abortion Risk	0.13	0.33	0.97	0.44
Smoked	0.35	0.48	0.45	0.81
Drank Alcohol	0.05	0.22	0.26	0.93
Induced Labor	0.39	0.49	0.56	0.73
Child Characteristics				
Male	0.49	0.50	1.33	0.25
Birth Size	48.93	2.18	0.45	0.81
Cephalic perimeter	34.58	1.45	1.42	0.21
Birth Weight	3.18	0.47	1.13	0.34
Dubowitz Score	53.56	4.96	0.87	0.50
Thoracic perimeter	33.35	1.93	0.69	0.63
Low Weight	0.07	0.26	1.11	0.35
Prematurity Dubowitz	0.06	0.23	0.88	0.49
Overall test	.	.	126.8	0.86

Note: Columns (1) and (2) report mean and standard deviation of each variable. Columns (3) and (4) report F-Stats and p-value of the joint null that the coefficients of the five interval dummies are each equal to zero in all equations. N: 2662

Table 3: Birth in the Pay Period and Years of Education

	(1)	(2)	(3)	(4)	(5)
	All	Poor	Rich	Low Ed	High Ed
1-5 Days After PP	-0.098*	-0.167**	-0.009	-0.112	-0.059
	(0.052)	(0.078)	(0.067)	(0.082)	(0.067)
6-10 Days After PP	-0.073	-0.123	-0.035	-0.162*	0.001
	(0.052)	(0.081)	(0.068)	(0.084)	(0.067)
11-15 Days After PP	-0.114**	-0.155*	-0.073	-0.252***	0.004
	(0.053)	(0.079)	(0.071)	(0.082)	(0.067)
16-20 Days After PP	-0.121**	-0.172**	-0.084	-0.199***	-0.021
	(0.050)	(0.077)	(0.066)	(0.075)	(0.068)
>20 Days After PP	-0.093*	-0.142*	-0.019	-0.152*	-0.037
	(0.053)	(0.079)	(0.071)	(0.082)	(0.068)
Observations	2662	1346	1316	1230	1432
R^2	0.361	0.310	0.343	0.298	0.311

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of an index of standardized measures of years of schooling on five indicators of the timing of birth with respect to the last day of the pay period (PP) in each month. Column (1) reports the results of a regression that uses the complete sample, column (2) uses the sample of poorer families only (with per capita household income lower than median), column (3) uses the sample of richer families only (with per capita household income lower than median), column (4) uses the sample of low education mothers only (education lower than median) and column (5) uses the sample of high education mothers only (education higher than median). All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. All columns exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 4: Grade Repetition

	(1)	(2)	(3)	(4)	(5)
	All	Poor	Rich	Low Ed	High Ed
1-5 Days After PP	0.056	0.164*	-0.086	0.088	0.020
	(0.055)	(0.088)	(0.066)	(0.096)	(0.064)
6-10 Days After PP	0.071	0.134	0.018	0.146	0.044
	(0.058)	(0.090)	(0.073)	(0.095)	(0.068)
11-15 Days After PP	0.151***	0.254***	0.043	0.236**	0.096
	(0.058)	(0.089)	(0.072)	(0.092)	(0.072)
16-20 Days After PP	0.080	0.167*	0.007	0.136	0.008
	(0.054)	(0.085)	(0.068)	(0.089)	(0.066)
>20 Days After PP	0.076	0.139	-0.019	0.095	0.080
	(0.060)	(0.095)	(0.073)	(0.099)	(0.070)
Observations	2637	1333	1304	1220	1417
R^2	0.229	0.213	0.194	0.199	0.189

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of an index of standardized measures of grade repetition on five indicators of the timing of birth with respect to the last day of the pay period (PP) in each month. Column (1) reports the results of a regression that uses the whole sample, column (2) uses the sample of poorer families only (with per capita household income lower than median), column (3) uses the sample of richer families only (with per capita household income lower than median), column (4) uses the sample of low education mothers only (education lower than median) and column (5) uses the sample of high education mothers only (education higher than median). All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. All columns exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 5: Mother Mental Health Problems

	(1)	(2)	(3)	(4)	(5)
	All	Poor	Rich	Low Ed	High Ed
1-5 Days After PP	0.024	0.099	-0.050	0.129**	-0.059
	(0.041)	(0.061)	(0.058)	(0.065)	(0.055)
6-10 Days After PP	0.073*	0.095	0.066	0.105*	0.053
	(0.041)	(0.060)	(0.055)	(0.061)	(0.056)
11-15 Days After PP	0.102**	0.136**	0.078	0.165***	0.060
	(0.040)	(0.060)	(0.057)	(0.060)	(0.056)
16-20 Days After PP	0.113***	0.148**	0.091	0.191***	0.062
	(0.041)	(0.060)	(0.058)	(0.062)	(0.055)
>20 Days After PP	0.079*	0.147**	0.017	0.142**	0.019
	(0.043)	(0.065)	(0.058)	(0.068)	(0.056)
Observations	2647	1338	1309	1223	1424
R^2	0.132	0.127	0.140	0.130	0.112

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of an index of standardized measures of mother mental health problems on five indicators of the timing of birth with respect to the last day of the pay period (PP) in each month. Column (1) reports the results of a regression that uses the complete sample, column (2) uses the sample of poorer families only (income lower than median), column (3) uses the sample of richer families only (income higher than median), column (4) uses the sample of low education mothers (education lower than median) and column (5) uses the results for high education mothers (education higher than median). All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. All columns exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 6: School Census - Pelotas

	(1)	(2)	(3)	(4)
	Survey 93	Census 93	Census 93	Census 95
1-5 Days After PP	-0.057	-0.031	-0.010	-0.021
	(0.052)	(0.049)	(0.044)	(0.049)
6-10 Days After PP	-0.120**	-0.048	-0.049	-0.032
	(0.054)	(0.051)	(0.045)	(0.048)
11-15 Days After PP	-0.189***	-0.156***	-0.114**	0.065
	(0.055)	(0.052)	(0.046)	(0.045)
16-20 Days After PP	-0.084	-0.033	-0.013	-0.031
	(0.051)	(0.048)	(0.043)	(0.048)
>20 Days After PP	-0.131**	-0.124**	-0.097**	-0.004
	(0.054)	(0.052)	(0.047)	(0.050)
Observations	4253	4718	5808	5877
R^2	0.016	0.023	0.022	0.023

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of standardized years of schooling on five indicators of the timing of birth with respect to the last day of the pay period (PP) in each month. Column (1) uses the sample of children born in 1993 and interviewed by the Pelotas cohort survey when they were aged 15, column (2) uses the sample of children born in 1993 in Pelotas and observed in the 2007 school census still living in Pelotas, column (3) uses the sample of all children born in Pelotas in 1993 and observed in the 2007 school census, column (4) uses the sample of all children born in Pelotas in 1995 and observed in the 2009 school census. All columns include fixed effects for month of birth, day of the week and gender. Robust standard errors are in parentheses. ***p<0.001, **p<0.05, *p<0.10.

Table 7: School Census - Brazil

	1993	1993	1995
1-5 Days After PP	-0.005***	-0.001	0.006
	(0.002)	(0.005)	(0.006)
6-10 Days After PP	-0.003	-0.007	0.002
	(0.002)	(0.008)	(0.008)
11-15 Days After PP	-0.003	-0.012	0.001
	(0.002)	(0.009)	(0.010)
16-20 Days After PP	-0.007***	-0.018	0.010
	(0.002)	(0.011)	(0.013)
>20 Days After PP	-0.010***	-0.032***	0.020
	(0.002)	(0.012)	(0.014)
Observations	3288102	3288102	3359860
R^2	0.046	0.046	0.045

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of standardized years of schooling on five indicators of the timing of birth with respect to the last day of the pay period (PP) in each month using data from the school census for all Brazilian students. Columns (1) and (2) use the sample of students born in 1993 and observed in the 2007 School Census and column (3) uses the sample of students born in 1995 and observed in the 2009 School Census. All columns include fixed effects for gender, month, day of the week and public holidays. Columns (2) and (3) additionally include day of the month fixed effects. Robust standard errors are in parentheses. ***p<0.001, **p<0.05, *p<0.10.

Table 8: Robustness Tests

	(1)	(2)	(3)	(4)
1-5 Days After PP	-0.082	-0.075	-0.074*	-0.072*
	(0.061)	(0.062)	(0.041)	(0.041)
6-10 Days After PP	-0.101*	-0.102	-0.067	-0.065
	(0.061)	(0.062)	(0.043)	(0.043)
11-15 Days After PP	-0.163**	-0.183***	-0.077*	-0.072*
	(0.064)	(0.064)	(0.043)	(0.042)
16-20 Days After PP	-0.139**	-0.137**	-0.102**	-0.094**
	(0.060)	(0.061)	(0.041)	(0.040)
>20 Days After PP	-0.100	-0.110*	-0.074*	-0.069
	(0.064)	(0.064)	(0.043)	(0.043)
Controls	No	No	Yes	Yes
Month/Week Dummies	No	Yes	Yes	Yes
Include C-Sections	No	No	Yes	Yes
Include IC/Twins	No	No	No	Yes
Observations	2662	2662	3832	3939
R^2	0.023	0.003	0.372	0.374

Note: Each column reports estimated coefficients of Ordinary Least Squares regressions of an index of standardized measures of years of schooling on five indicators of the timing of birth with respect to the last day of the pay period (PP) in each month. Column (1) excludes all control variables of Table 3, column (2) includes only the month and day of the week indicators, column (3) includes all control variables and the children born through cesarean sections and column (4) includes all control variables and the children born through cesarean sections, placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 9: Different Measures of Date of Birth

	(1)	(2)
1-5 Days After PD	-0.128**	-0.098*
	(0.053)	(0.052)
6-10 Days After PD	-0.111**	-0.073
	(0.052)	(0.052)
11-15 Days After PD	-0.124**	-0.114**
	(0.052)	(0.053)
16-20 Days After PD	-0.151***	-0.121**
	(0.050)	(0.050)
>20 Days After PD	-0.134**	-0.093*
	(0.052)	(0.053)
Observations	2662	2662
R^2	0.362	0.361

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of the index of standardized measures of years of schooling on indicators of the timing of birth with respect to the maximum allowed payday in each month. Column (1) reports the results of a regression that uses date of birth collected by the interviewers at birth and column (2) uses date of birth reported by the mothers of the members of the cohort. All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. All columns exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 10: C-sections

	(1)	(2)
1-5 Days After PD	-0.018	-0.023
	(0.022)	(0.023)
6-10 Days After PD	-0.026	-0.027
	(0.022)	(0.023)
11-15 Days After PD	-0.020	-0.030
	(0.022)	(0.023)
16-20 Days After PD	-0.025	-0.042*
	(0.022)	(0.023)
>20 Days After PD	-0.012	0.003
	(0.022)	(0.024)
F-test (p-value)	0.84	0.35
Observations	4997	3831
R^2	0.000	0.193

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of attrition on indicators of the timing of birth with respect to the maximum allowed payday in each month. Attrition is defined by the children not interviewed in the second wave of the survey in 2004. Columns (1) does not include controls and column (2) includes all controls of Table 3. All columns exclude children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 11: Attrition

	(1)	(2)
1-5 Days After PD	0.032	0.024
	(0.020)	(0.022)
6-10 Days After PD	0.025	0.025
	(0.020)	(0.021)
11-15 Days After PD	-0.009	-0.010
	(0.019)	(0.021)
16-20 Days After PD	0.005	0.004
	(0.019)	(0.020)
>20 Days After PD	0.029	0.038*
	(0.021)	(0.023)
F-test (p-value)	0.21	0.24
Observations	3522	3073
R^2	0.002	0.030

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of the birth through c-section on indicators of the timing of birth with respect to the maximum allowed payday in each month. Columns (1) does not include controls and column (2) includes all controls of Table 3. All columns exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 12: Infant Mortality

	(1)	(2)
1-5 Days After PD	0.006	0.001
	(0.006)	(0.005)
6-10 Days After PD	0.003	0.007
	(0.006)	(0.006)
11-15 Days After PD	0.003	0.006
	(0.006)	(0.006)
16-20 Days After PD	0.001	-0.001
	(0.005)	(0.004)
>20 Days After PD	0.006	0.005
	(0.006)	(0.006)
F-test (p-value)	0.89	0.63
Observations	3522	3073
R^2	0.000	0.037

Note: Each column reports estimated coefficients of an Ordinary Least Squares regression of infant mortality on indicators of the timing of birth with respect to the maximum allowed payday in each month. Columns (1) does not include controls and column (2) includes all controls of Table 3. All columns exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 13: Main Results by Age

	(1)	(2)	(3)	(4)
	Age 11	Age 15	Age 18	Age 22
1-5 Days After PD	-0.067	-0.138**	-0.062	-0.172**
	(0.057)	(0.059)	(0.062)	(0.068)
6-10 Days After PD	-0.058	-0.074	-0.011	-0.105
	(0.058)	(0.061)	(0.063)	(0.064)
11-15 Days After PD	-0.113*	-0.126**	-0.089	-0.112*
	(0.059)	(0.061)	(0.065)	(0.065)
16-20 Days After PD	-0.108*	-0.111*	-0.069	-0.136**
	(0.056)	(0.057)	(0.063)	(0.063)
>20 Days After PD	-0.137**	-0.127**	-0.045	-0.042
	(0.061)	(0.063)	(0.064)	(0.066)
Observations	2660	2547	2363	2160
R^2	0.304	0.302	0.310	0.327

Note: Entries are estimated coefficients of OLS regressions of normalized years of schooling on indicators of timing of birth with respect to the maximum allowed payday in each month. Column (1) uses data from the first wave of the survey (2004), column (2) uses data from the second wave of the survey (2008), column (3) uses data from the third wave of the survey (2011) and column (4) uses data from the fourth wave of the survey (2015). All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

Table 14: Mother Mental Health Problems

	(1)	(2)	(3)
	Mental Disorders	Took Pills	Does not Work
1-5 Days After PD	0.012	0.016	0.063
	(0.050)	(0.023)	(0.067)
6-10 Days After PD	0.082	0.035	0.084
	(0.052)	(0.023)	(0.067)
11-15 Days After PD	0.073	0.026	0.139**
	(0.050)	(0.022)	(0.067)
16-20 Days After PD	0.051	0.021	0.087
	(0.049)	(0.022)	(0.065)
>20 Days After PD	0.105**	0.012	0.007
	(0.053)	(0.023)	(0.068)
Observations	2636	2647	2645
R^2	0.114	0.049	0.107

Note: Entries are estimated coefficients of OLS regressions using different dependent variables on indicators of timing of birth with respect to the maximum allowed payday in each month. Column (1) uses the continuous measure of mother mental health disorders as dependent variable, column (2) uses a dichotomous variable indicating consumption of sleeping pills or tranquilizers in the previous month and column (3) uses the dichotomous variable indicating that the mother did not work. All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

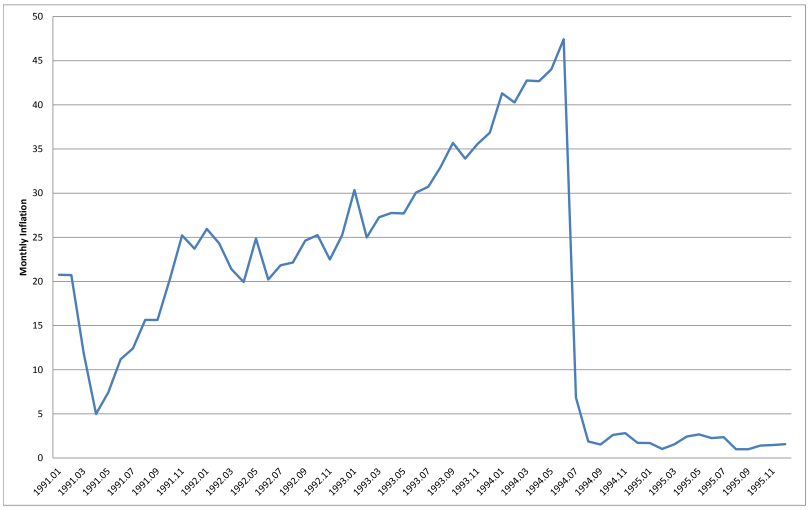
Table 15: Grade Repetition - Different Measures

	(1)	(2)	(3)
	Failed Grade 1	No. Failures Grade 1	Failed any Grade
1-5 Days After PP	0.012	0.041	0.084
	(0.064)	(0.066)	(0.063)
6-10 Days After PP	0.063	0.101	0.066
	(0.065)	(0.073)	(0.064)
11-15 Days After PP	0.167**	0.153**	0.142**
	(0.067)	(0.071)	(0.063)
16-20 Days After PP	0.052	0.080	0.097
	(0.063)	(0.066)	(0.060)
>20 Days After PP	0.109	0.172**	0.028
	(0.068)	(0.075)	(0.065)
Observations	2636	2636	2637
R^2	0.163	0.163	0.214

Note: Entries are estimated coefficients of OLS regressions using different dependent variables on indicators of timing of birth with respect to the maximum allowed payday in each month. Column (1) uses a dichotomous variable indicating failure at grade 1 as dependent variable, column (2) uses the number of grade 1 repetitions and column (3) uses a dichotomous variable indicating failure at any grade by age 11. All columns include fixed effects for month of birth, day of the week, hospital of birth, plus all controls of Table 1. Robust standard errors are in parentheses. ***p<0.01, **p<0.05, *p<0.10.

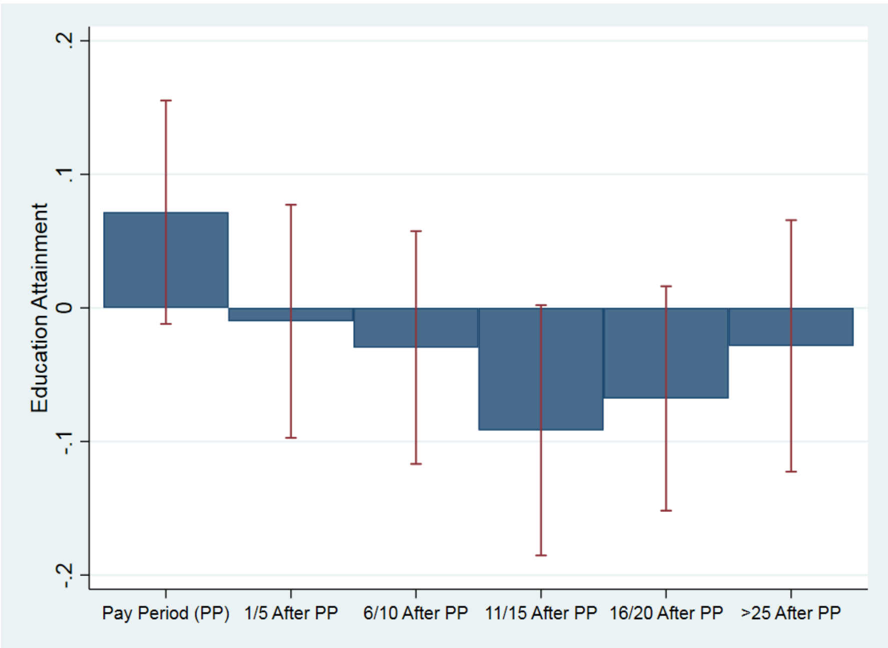
8 Figures

Figure 1: Inflation in Brazil



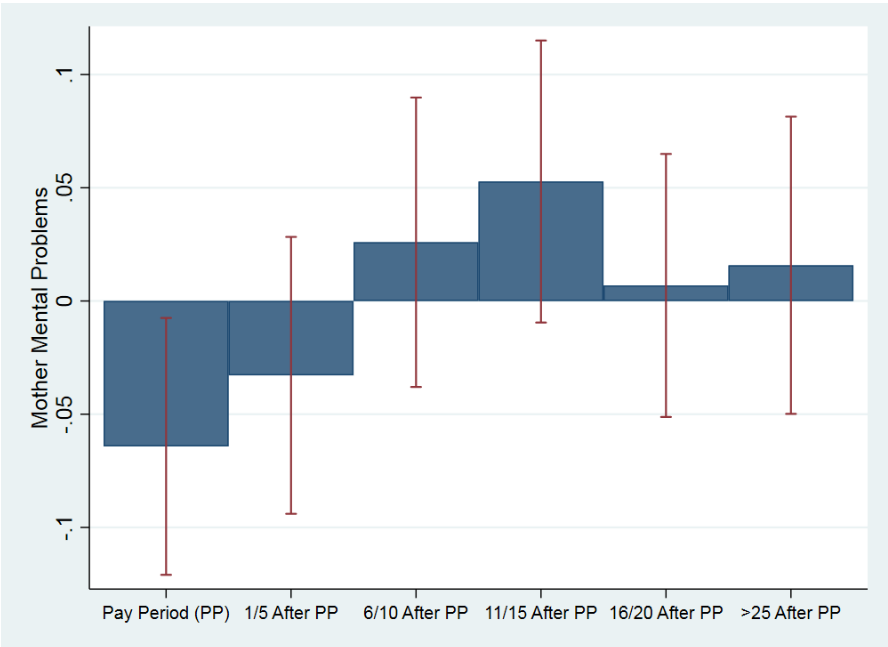
Notes: Monthly Inflation in Brazil. Source: National Census Bureau-IBGE

Figure 2: Birth in the Pay Period and Education Attainment



Notes: Education attainment is an index of standardized measures of years of schooling at various ages (see text). Data exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Source: Pelotas Cohort Study

Figure 3: Birth in the Pay Period and Mother Mental Health Problems



Notes: Mother mental health problems is an index of standardized measures of mental disorders, not working and taking sleeping pills or tranquilizers. Data exclude children born through cesarean sections, children placed in intensive care after delivery and twins. Source: Pelotas Cohort Study