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Wage cuts and health at birth: the adverse effects of in utero exposure to economic shocks

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Abstract

We investigate the effects that a major (25%) and unexpected wage cut austerity measure affecting the public sector employees in Romania in 2010 had on the health outcomes at birth. Our findings suggest that children in utero at the time of the austerity announcement had worse health outcomes at birth relative to their unaffected siblings. The most sensitive periods to in utero shocks are the first and the last trimesters of gestation. The main mechanism behind our results seems to be prenatal maternal stress. The negative outcomes at birth appear to have lasting scarring effects up to the age of four.

JEL classification codes: I19, J13, J38, I38

Keywords: austerity measures; public sector wage cut; fetal shock; health at birth; Romania

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1 Introduction

Understanding whether and, if so, how economic downturns affect fetal development is especially relevant in the aftermath of the Great Recession, which caused significant economic disruptions and forced governments to impose harsh austerity measures. While children in utero were not intentionally affected, they might have been among those impacted by the austerity programs. Within the framework of the fetal origin hypothesis put forward by Barker (1990), recent evidence shows that disruptions in prenatal conditions caused by fetal shocks have scarring, life-long consequences (Almond and Currie, 2011; Almond and Mazumder, 2011).[1] Moreover, there is conclusive evidence of a strong correlation between the birth outcomes of mothers and birth outcomes of their children (Currie and Moretti, 2007) suggesting that exogenous shocks to the initial health capital in a generation may also affect future generations and play a significant role in the intergenerational transmission of socio-economic status.

Prior work has found that community-wide disasters and major life events can substantially affect fetal health,[2] but relatively little is known about the effects of shocks induced by economic phenomena. As many governments took actions to eliminate unsustainable budget deficits, public sector wages were frozen in numerous European countries; others implemented wage cut policies.[3] In this paper we exploit the most drastic wage cut austerity measure implemented in Europe, which entailed a 25% cut in the public sector wages and a 15% cut of the social benefits in Romania starting July 1st, 2010. This led to a 60.1 percentage points drop in the public sector wage premium.[4] This unexpected and major wage cut provides an excellent setting to explore the effects of an exogenous income shock on health outcomes at birth.

The effects of economic phenomena on fetal environment are quite difficult to disentangle as their timing is usually diffuse, lack a precise onset date, and may affect fetal health through multiple channels simultaneously (Almond and Currie, 2011). During economic hardship, individuals may reduce expenditures on consumption goods, and nutritional restrictions may affect the unborn child. At the same time, the countercyclical pattern of consumption of health-damaging goods (Ruhm and Black, 2002; Ruhm, 2003) and the decrease of the opportunity cost of health-improving behavior may offset the negative effects and lead to better infant health at birth. In addition, maternal prenatal stress, caused by the financial insecurity entailed by economic shocks, may have either scarring and/or culling effects, leading to an ambiguous net effect on health at birth. Thus, some studies find deteriorating health outcomes at birth (Paxson and Schady, 2005; Burlando, 2014; Lindo, 2011), whereas others find that the effects of improvements in risk-related behavior during pregnancy and maternal selection prevail over the scarring effects, with the net result being an improvement of the health of in utero exposed children Dehejia and Lleras-Muney (2004). Bozzoli and Quintana-Domeque (2013) document the pro-cyclical effects of economic fluctuations in Argentina on the birth outcomes of children, noting that birth weights are sensitive to macroeconomic fluctuations via the nutritional deprivation channel and maternal stress channel. In a recent

[1] Fetal shocks are defined broadly as events that alter the fetal environment and give rise to fetal stressors that may induce developmental adaptations in the unborn child because they signal a change in the predicted postnatal environment (Gluckman and Hanson (2005)).

[2] E.g. civil and military conflicts (Catalano (2003); Mansour and Rees (2012); Valente (2015)), natural disasters (Almond et al. (2007)), terrorist acts (Glynn et al. (2001); Camacho (2008)), pandemics (Almond (2006)) or major life events such as the death of a family member (Black et al., 2016; Persson and Rossin-Slater, 2014).

[3] Wage cuts were implemented in: Romania (25%, 2010), Czech Republic (10%, 2011), Estonia (6%, 2009-2010), Greece (20%, 2012), Ireland (5%, 2010), Hungary (7%, 2008-2010), Latvia (15%, 2009-2010), Lithuania (15%,2009-2010), Portugal (5%, 2011), Slovenia (4%, 2011), Spain (5%, 2010). In Section 2 we discuss that the wage cut austerity policy was not anticipated in Romania.

[4] The public sector wage premium fell from +44.5% in 2009 to -15.6% in 2010 (source: Industrial Relations in Europe 2012 Report, European Commission).

work, Vardardottir (2016) investigates the effects of the 2008 Icelandic financial crisis and finds that worse outcomes at birth are largely due to the maternal stress involving financial insecurity. On the effects of income on pregnancy outcomes, Hoynes et al. (2015) find that increased available income reduces the incidence of low birth weight and increases mean birth weight, most likely due to more prenatal care and less negative health behaviors.

This paper contributes to the limited literature on the impact of (negative) economic shocks on health outcomes at birth by exploring a unique austerity measure, unexpected in its magnitude (25% cut in public sector wages and 15% in all social benefits) and timing (starting with July 1^{st}, 2010, after being first announced on May 7^{th}, 2010). This distinct occurrence eliminates the problems posed by diffuse timing or endogenous income reductions and allows us to pursue a clean identification strategy. While existing studies analyzing in utero exposure to negative economic phenomena use macroeconomic fluctuations (Bozzoli and Quintana-Domeque, 2013) or economic collapse (Vardardottir, 2016) that affected the entire population, the wage cut austerity measure we examine was exclusively focused on the public sector workers. Nominal wage cuts are widely used by firms for short term adjustments of labor costs in both developed and developing economies, especially during economic downturns.[5] Thus, this paper contributes to the understanding of the hidden costs of wage cuts policies.

We use the Romanian Vital Statistics Natality files to analyze employed mothers in the public sector with a child in utero in May 7^{th} 2010. We focus on women already pregnant at the time of the austerity announcement to mitigate any concern related to the change in the composition of families choosing to conceive. Because the austerity shock may have entailed increased psychological stress and because the response of publicly employed mothers with a greater stress may differ in unobserved ways from those with a lower stress response, we will use a mother fixed effects and compare the outcomes at birth for the sibling exposed in utero to the policy shock to the unexposed sibling(s). Moreover, to control for the effects related to the overall state of the economy, as well as for possible birth order effects we also include sibling pairs from mothers not affected by the policy: non-working (housewives) mothers. In this way, we eliminate the time-constant differences between affected and non-affected mothers, while we also deal with seasonality and low statistical power issues when analyzing health outcomes. Hence, in our preferred strategy, we will compare the difference in health at birth outcomes between sibling pairs with one sibling in utero in May 2010 and exposed to the wage cut shock to the difference in sibling pairs with one sibling in utero in May 2010 but not exposed to the policy shock.

Our main findings suggest that children who were in utero at the time of the austerity announcement had significantly worse health outcomes at birth relative to their unexposed siblings: 1.4 percentage points (15.6% relative to the mean) higher probability of low birth weight (birth weight below 2500 grams), they were with an average of 58 grams (1.8% of the mean) smaller, 2.1 percentage points (16.4% of the mean) more likely to be born prematurely, and a shorter gestational length (by 0.2 weeks). They also fare worse when measuring their fetal development using the fetal growth indicator and the small-for-gestational-age indicator by as much as 17% of the mean. These findings are robust to different specifications and tests. Our findings indicate that children exposed to the shock in the first (exposed to both stress and reduced income) and third (exposed to the shock but not to reduced income) trimesters were affected the most. Boys seem to be affected more than girls, although girls are also negatively scarred by the austerity measures. With the data in hand it remains difficult to understand the exact mechanisms that lead to the

[5]For example, Blundell et al. (2014) show that amongst UK workers who stayed in the same job between 2010 and 2011, one third experienced nominal wage freezes or cuts (12% experienced freezes and 21% experienced cuts) and 70% experienced real wage cuts.

changes we observe in our analysis. Our discussion in Section 5 indicates that exposure to a major income shock, especially so late pregnancy, may impact the in utero children through stress. However, we cannot disentangle the direct effect of physiological stress (hormones) from the indirect effect through an increase in health damaging behaviors (smoking and/or drinking). Finally, using complimentary health survey data, we show that, despite some evidence of compensatory parental investments (e.g., vaccination, vitamins use), the negative effects at birth persist until the age of four: children exposed to the in utero shock seem more likely to have chronic diseases and limited activity relative to their unexposed peers.

The remainder of the paper is structured as follows: Section 2 depicts the Romanian context in which the austerity measures were implemented. Section 3 describes the data and the empirical strategy we use. The main results, placebo tests and robustness checks are discussed in Section 4. In Section 5 we discuss the potential mechanisms through which an income shock may affect birth outcomes. Section 6 presents evidence of the medium term effects of the austerity measures on child health and maternal behaviors. Section 7 presents the conclusions.

2 Romanian context

Romania experienced sizable economic and politic insecurity throughout most of its post-communist period.[6] Thus, the international financial crisis that unfolded in the autumn of 2008 was taken lightly and the public opinion showed no signs of distress: the Eurobarometer survey indicated no changes in how the Romanians perceived their overall life and household financial situation, nor significant changes in how they assessed their short-term expectations in the periods preceding the wage cut announcement.[7] Politicians invoked a decoupling of the Romanian economy from the world markets and transmitted an overall confident message in the lead-up period to the presidential elections of December 2009. After being re-elected, the incumbent President declared that "(...) we expect significant growth in the first part of 2010".[8]

In this context, the President's announcement on national TV on May 7^{th}, 2010, that public sector wages and social security benefits would be cut was unexpected and gave rise to widespread social unrest and political dispute. The decision was made by the government and the President after the latest round of negotiations with the IMF and was not preceded by any discussions in the Parliament or with social partners, nor was publicly mentioned as a potential policy. The measures involved a 25% cut in wages for all public sector employees, the revocation of most of their financial and in-kind incentives and a 15% cut in unemployment, cuts in maternity leave benefits and several

[6] Although negative growth rates were replaced by high growth rates beginning in 1999, they were accompanied by high inflation rates and significant public deficit. In 2000, when the GDP growth rate turned positive, the annual inflation rate was over 40%, whereas in 2004, when the GDP growth rate reached a peak of 9%, the inflation rate was still above 10%.

[7] `http://ec.europa.eu/public_opinion/cf/`. The Romanians' personal situations remained largely unchanged prior to the wage cut announcement: about 47% and 44% seem satisfied with their life and household financial situation, respectively, in the autumn 2008, spring 2009 and autumn 2009 waves, followed in the spring 2010 by sharp 11 and 9 point drops, respectively. The spring 2010 wave shows a large drop in how Romanians assess their short-term (12 months) prospects on life, job and household financial situations: the short-term optimism index (as defined by the Eurobarometer) declines by more than 20 points (from values of -7 and -9 in the previous waves to -29) in May 2010.

[8] `http://goo.gl/sMcVEV` (in Romanian). Some political signs recognizing of the deteriorating state of the Romanian economy came in March 2009 when the government initiated discussions with the IMF. After signing a stand-by accord in June 2009, politicians promoted the agreement as an opportunity for state reorganization, but subsequent proposed measures were mild and noncontroversial. Early in 2010, the government adopted a graver attitude toward the worsening economic crisis as the IMF required concrete actions to reduce the significant budget deficit. On March 16th 2010, in front of the Parliament, the Prime Minister presented the anti-crisis measures that were being implemented- all as economic stimulus aimed at improving the business environment and reducing tax evasion.

other social security benefits. They were aimed at re-establishing the budgetary balance agreed to with the IMF. For pregnant women employed in the public sector at the time of the announcement (our treatment group), the austerity policy had a threefold effect: a drop in monthly income due to the wage and benefits cut; a decrease in the annual average wage income, which would lead to a lower child care allowance (calculated as 85% of the average income obtained over the 12 calendar months preceding the birth of the child); and a 15% cut in the recalculated child care allowance to be received after giving birth.

One month after the announcement of the austerity measures, the Finance Minister gave a speech pertaining to the delusional nature of the government's previous statements on the economic status of the country and on the completely unexpected nature of the policy: "As a Finance Minister I am telling you that we could have lied six more months, we could have borrowed for six months, [...] and could have waited six months to see what happened. The fact that what we are doing entails a political risk that nobody imagined a month and a half ago shows a complete responsibility of this government towards the Romanian citizens".[9] He was dismissed shortly after.

The measures were included in a set of legislative projects drafted by the government soon after the President's announcement and forwarded to the Parliament to be adopted through a special procedure that circumvented the regular and lengthy law-making procedures.[10] On June 30th, the President published the laws, which went into effect July 1st, with an initial duration of 6 months, but in January 2011 public sector wages were not restored to their initial level.

Overall, it is safe to assume that the austerity measures were not anticipated in both their unprecedented scope and magnitude or in their timing. In our empirical strategy we will focus on women working in the public sector who were already pregnant at the time of the austerity announcement to mitigate the concern related to the change in the composition of families choosing to conceive. Although the austerity measures were unanticipated, we cannot exclude "written on the wall" effects.[11] The possible selections into fertility will be addressed later in the paper.

Finally, we argue that these austerity measures were especially severe for the women employed in the public sector. In Romania, publicly employed women are concentrated in the health, social services and education sectors and had, even before the austerity measure, lower average wages both relative to the private sector and to other public, male dominated sectors such as local administration and defense.[12] In addition, recent evidence shows that the insecurity coupled with the economic crisis, has worsened the perception of work-related stress in all European countries in general and in Romania, already highly ranked, in particular, making publicly employed women the most affected by the wage cut both in monetary and psychological distress terms (see Vîrgă et al. (2012)).

[9] `http://goo.gl/bJNNYr` (in Romanian)

[10] The Romanian Constitution allows an exception in which the government assumes responsibility for a specific law in front of the Parliament with the law under consideration being adopted by default if the government is not dismissed in the first 3 days by means of an adopted censorship motion. The Parliament can withdraw the trust awarded to the government by adopting a censorship motion, which necessarily means that the Government is dissolved, the law proposed is not adopted and a new Government needs to be invested. After the Government assumed responsibility on the Austerity Laws, a censorship motion was initiated by the opposition parties in the Parliament but because of a tight majority of the governing coalition, the censorship motion was not adopted (though by a very close margin) and the laws were passed in a slightly modified version.

[11] At that time Romania experienced an increase in the unemployment rates in the private sector, which rose from a relatively stable level of about 4% before 2009 to 7.4% in March 2010.

[12] Source: Statistics Romania. This differential gender effect, where the public sector wage cuts affected women significantly more than men due to the structure of public sector employment, was also noted in EuropeanComission (2010).

3 Methods and Data

In our main empirical exercise we use the Vital Statistics Natality (VSN) records for the years 2005 through 2011 as our main dataset. The VSN records cover the entirety of live births with detailed information about the newborn and the socio-economic characteristics of the parents, recorded at the time of the birth: (a) characteristics of the child: date of birth, gender, ethnicity, whether singleton or multiple birth, birth weight and duration of gestation in number of weeks; (b) characteristics of the mother: date of birth, occupational status, education, marital status, county and locality of residence, and mother's fertility history: total number of births, number of children born alive, fetal deaths, month of first prenatal check-up and an indicator for home delivery; (c) characteristics of the father: date of birth and his occupational status.

A key variable for our empirical specification is the mother's occupational status. The VSN records the mother's occupational status using the following categories: employed, entrepreneur, self-employed in agricultural activities, self-employed in non-agricultural activities, unemployed, housewife, retiree, and other situations. However, the employed category does not differentiate between public and private sector of employment. To recover this information, the VSN files are merged with the Population Census conducted in October 2011 to extract information about the mother's occupation and sector of employment at the time of the Census. Of the 1,474,777 children born between 2005 and the 2011 Census, 1,392,249 (or 94.4%) are matched with Census entries. Among those not matched, 19,067 (23.10%) died before the Census (source: Mortality files), while the rest of the children were not living in the same household as their mother (due to, e.g., maternal mortality, international migration of the mother, parental separation).[13] For 1,261,343 children we also have information about the father at the time of the Census.

As the wage cut policy exclusively targeted public sector employees, our main interest is in analyzing children in utero in May 2010 whose mother was employed in the public sector at birth. Due to the data limitations presented earlier, we define as publicly employed the mothers who were registered as employed in the VSN and employed in the public sector at the October 2011 Census. Our main identification strategy is a mother fixed effects framework, where we compare the outcomes at birth for siblings born to the same publicly employed mother at different points in time: the outcomes of child who was in utero at the time of the austerity measure announcement with those of his sibling(s) born after 2005.[14] We recognize the potential endogenous fertility for siblings conceived after May 2010 but we will show that our results do not change when we exclude them from the sample. We consider the public employment status of the mother at the 2010 pregnancy, regardless of her occupational status at the earlier pregnancy. This specification controls for both observed and unobserved characteristics that are constant across births. Moreover, because the austerity shock may have entailed increased psychological stress and because mothers with a greater stress response may differ in unobserved ways from those with a lower stress response, maternal fixed effects will address this potential omitted variable bias. In addition, to control for the possible effects of maternal stress related to the overall state of the economy (and not necessarily the wage cut policy), as well as for possible birth order effects (as we compare outcomes as birth of a latest born to his sibling(s)) we also include sibling pairs from mothers not directly affected by the

[13] This may bias our results if children of the affected mothers are more likely to die as a result of the policy. As it will be clear from the empirical section, this means that we show a lower bound of the true effect of the policy on the outcomes at birth. If because of the policy, some couples decide to separate or the mothers migrated after birth, the effect on birth outcomes is more ambiguous. However, the unmatched mothers appear to be negatively selected relative to the matched mothers, meaning again that our effects are the lower bound of the true ones.

[14] We are not able to identify siblings born before 2005. The average number of children per household in Romania is 1.82 (1.52 among employed and 2.18 among housewives), and the average spacing is approximately 4 years; therefore, our time span likely covers most sibling pairs in which the youngest is born in 2010.

policy. In this way, we eliminate the time-constant differences between affected and non-affected mothers, while we also deal with seasonality and low statistical power issues when analyzing health outcomes in small samples (Currie and Schwandt, 2015). Our preferred category of mothers consists of non-working women (housewives) for two reasons: first, they are out of labour force for a long period of time, not entitled to receive any unemployment or maternity benefits which were also reduced as part of the austerity package; secondly, they are the most numerous group of mothers in Romania. It is worth clarifying that housewife women, as defined by Statistics Romania, are women engaged exclusively in domestic work who does not receive any formal income; they are on average lower-educated, less likely to be married and their children have worse outcomes at birth as compared to the employed mothers (see also Table 1a). In a first instance, we focus on women who were housewives at the 2010 pregnancy and were still housewives at the 2011 Population census. Alternatively, we will also compare the publicly employed mothers with all other mothers, i.e., we include also privately employed mothers who were affected by the austerity (by a 15% cut in maternity benefits and child care allowance) and whose unemployment also increased prior to the austerity measures. Using this comparison group would most likely downward bias our results.

We estimate the following linear regression model in which we compare the difference in health outcomes at birth of sibling pairs with one child in utero at the time of the announcement whose mother was affected by the austerity measure (the public sector employed women) to those with mothers not affected by the measures (housewives women):

$$Outcome_{imt} = \alpha + \beta Public_{im} * Utero2010_{it} + \tau_t + \gamma X_{imt} + \mu_m + \epsilon_{imt} \quad (1)$$

where i indexes a child born to mother m conceived in month-year t.

$Public_{im}$ is an indicator that equals 1 if child i is in the treatment group (mother m is employed at VSN for the 2010pregnancy and employed in the public sector at the Population Census) and 0 if she is in the control group (mother m is housewife at the birth of the pregnancy that was ongoing in May 2010 and at the Population Census). The key coefficient is β, the interaction between Public and an indicator $Utero2010_{it}$, which equals 1 if the child i was in utero in May 7^{th} 2010 and 0 otherwise. τ_t are two-way fixed effects for the year and month of conception. X_{imt} is a vector of time varying control variables for child, maternal and paternal characteristics at the time of birth for child i: child's gender, indicator for hospital delivery, indicators for the trimester of the first prenatal control, birth order indicators, mother's age at birth, indicators for maternal education, indicators for mother's occupational status (as we allow that at the birth of the unexposed sibling, the mother m may have any occupational status), father's age at birth, indicators for his occupational status, and an indicator of whether father's information is missing in the VSN. μ_m are mother fixed effects. We cluster the standard errors at the county-by-urbanity level (42*2 clusters) to allow for correlated errors within county-urbanity (due to for example local labor markets or health care infrastructure).[15]

We analyze the following outcomes that reflect the health at birth of the child: low birth weight indicator (birth weight less than 2500 grams), birth weight in grams, premature delivery (birth occurring earlier than 37 gestational weeks), duration of pregnancy in number of weeks, fetal growth (birth weight divided by duration of pregnancy), and a small-for-gestational-age indicator (weight below the 10th percentile within each gestational age and sex).

We first identify exposure to the wage cut announcement using a binary indicator that is 1 if the child was in utero on May 7^{th} 2010. Because the literature suggests that the effects of in utero shocks may vary according to the stages of gestation, we also explore the fact that at the time of

[15]Similar results are obtained when clustering at locality level (3025 clusters).

the shock children were in different gestational stages. The VSN data contains the gestational age in number of weeks at birth, and thus we are able to infer the gestational age at the date of the austerity announcement.[16] Using this information, we split our sample into the following categories according to their gestational age at May 7^{th}: 1) children in the 1^{st} trimester (up to 13 weeks), who were exposed the longest to the policy: to the announcement shock in early pregnancy and to diminished income later in gestation; (2) children in the 2^{nd} trimester (14-27 weeks), who were unaffected during the 1^{st} trimester, but exposed to policy shock during their 2^{nd} trimester and to both stress and diminished income in late gestation; (3) children in the 3^{rd} trimester (over 28 weeks), exposed only to the announcement shock in late gestation.

Our main working sample are singleton births that were in utero on May 7^{th} 2010, at the time of the austerity announcement, and their sibling(s) born anytime since 2005, to either publicly employed or out of work (housewife) women at the 2010 pregnancy. The way we must define our treatment and control groups entails some restrictions and working assumptions, but our results remain valid when these restrictions are relaxed in various robustness checks. First, we restrict our sample to mothers that do not change their occupational status between the birth of the child who was in utero in May 2010 and the October 2011 Census (a time span of 8 months for those at the end of the 3^{rd} trimester to 18 months for those in the beginning 1^{st} trimester from birth to Census), while we allow them to have any occupational status at the previous births. Second, we assume that employed mothers at the birth of the May 2010 pregnancy do not change their sector of employment (from private to public) before the Census. We argue that this is not a restrictive assumption because after 2009, employment opportunities in the public sector were severely restricted [17] while the rise in female unemployment in the private sector from approximately a steady 4% before 2009 to 7.4% in March 2010 also limits switching from the public to the private sector.[18] Moreover, we expect less job mobility among employed women during the first year after giving birth, when most employed mothers are on maternity leave: Paunescu and Apostu (2012) show that 96.2% of children up to one year old were in the exclusive care of their parents, and employed mothers take at least one year of maternity leave.

In our main working sample, we have 55,136 siblings of whom one was in utero at the time of the wage cut announcement, and belonged to employed women at VSN who were employed in the public sector at the Census or housewife mothers. In addition, there are 12,445 births that are part of sibling pairs belonging to mothers employed at the time of the 2010 birth but no longer occupied at the 2011 Census. Table 1a presents descriptive statistics on the mother's observable characteristics at the birth of the sibling in utero in May 2010. Column 1 shows characteristics of the mothers employed at the time of the austerity measure announcement and employed in the public sector at the Census. Column 2 presents mothers employed at the birth of the May 2010 pregnancy and no longer employed at the Census. Most of these "switcher" mothers declare themselves housewives at the Census. While we cannot infer the sector of employment at birth for these women from the Census, we argue that most of them were privately employed mothers that have chosen to withdraw from the labor market (as will be discussed in more detail in the next section).[19] Ex ante, we expect

[16] Having the gestational age in weeks at the time of the announcement allows us to circumvent the problem of comparing children born in the same month but who were in different developmental stages at the time of the announcement due to different lengths of gestation.

[17] E.g., only one new employee could be hired when there were seven vacancies. The wage cut policy was the chosen alternative over having to reduce significantly the size of the public sector. These employment restrictions in the public sector were in place until the end of 2013.

[18] The increase in female unemployment came almost exclusively from the private sector since, as explained before, the public sector employment was constant over the period.

[19] Because these women were on maternity leave (awarded for two years after the birth of the child), we cannot exclude the possibility that some declared themselves as housewives even if they would be going back to work at the end of their

these mothers to be negatively selected relative to the "non-switchers" mothers. Indeed, relative to mothers in column 1, they are, on average, less educated, younger, have more children, are less likely to be married or from an urban area, and have children with worse outcomes at birth. When we will include these mothers in an extended treatment group, our results will be largely similar, albeit slightly smaller in magnitude. Column 3 presents our control group, housewives at the birth of the 2010 pregnancy who are also housewives at the 2011 Census. Relative to our treatment group, they are, as expected, worse off in terms of all observable characteristics, and their children have worse outcomes at birth. In the last column, we present housewives at the birth of the exposed child who entered the labor market by the time of the Census. They are, on average, better educated, slightly older, and have fewer children; also, the health of their youngest child at birth appears to be slightly better relative to the long term housewives. We will include them in the control group in a robustness exercise. Table 1b presents descriptive statistics for the health outcomes of the children included in our main specification (columns 1 and 3) and of switcher mothers (columns 2 and 4). Children of publicly employed women have better health outcomes at birth than children of housewife mothers. The children of employed women who switch their occupation before the Census have worse outcomes at birth relative to non-switchers, confirming the negative selection we observe at maternal characteristics level. Similarly, the children of housewives at birth who switch their occupational status have slightly better average health outcomes than the children of housewives that do not switch their occupational status.

Finally, in Figure 1 we show the average residual outcomes at birth of the children in the sibling sample, for publicly employed and out of work (housewife) mothers, by their month and year of conception, after accounting for all controls in our main specification.

4 Results

4.1 Main estimates

In Table 2, which shows our main results from equation (1), we observe an across-the-board worsening of the indicators reflecting health at birth of the exposed children: they are 1.4 percentage points (pp) more likely to have low birth weight, or 15.6% relative to the mean (column 1); they have an average birth weight decrease of 58 grams (column 3; 1.8% relative to the mean); they are 2 pp more likely to be delivered prematurely (column 5; 16.4% relative to the mean) and have a shorter duration of pregnancy by approximately 0.2 weeks (column 6; 0.48% relative to the mean). The exposed children have a significantly lower indicator for fetal growth (column 9; 1.3% relative to the mean) and are also significantly more likely to be small for their gestational age (column 11; 17% relative to the mean). In the even columns of the table, we show that the most affected seem to be children in the first and third trimesters at the time of the austerity announcement with the children in late pregnancy particularly affected in terms of the probability of low birth weight (column 2). The effects are not significantly different between the 1st and 3rd trimesters. The effects on the second trimester children are similar for some of the outcomes (birth weight and fetal growth) and smaller and not significant for the rest of the outcomes. This suggests that the developmental periods that are most sensitive to shocks are the first and third trimesters, even though children in late pregnancy were only exposed to the the announcement shock (the reduced income de facto started in August 2010), while children in early gestation in May 2010 were exposed for the longest period to reduced income and potentially to psychological stress entailed by the policy. In Section 5 we discuss the potential mechanisms behind these findings.

leave.

To address the issue of potential endogenous fertility, we exclude the sibling pairs in which the unaffected sibling was conceived after May 2010. The results presented in Appendix Table 11 are very much in line with our main estimates, having the same magnitude and significance. This also suggests that there were most likely no permanent effects on the mother's health that would affect subsequent pregnancies through a biological mechanism.

Previous work has found that fetal stressors may affect the the sex-ratio at birth, via selection in utero. As such, after severe insults, boys are less likely to survive the prenatal period as they are more frail than girls (Catalano and Bruckner, 2005; Catalano et al., 2009; Sanders and Stoecker, 2015). We test whether the austerity measure announcement changes the probability that a live birth is male, but we find no indication that the sex ratio at birth was affected. However, the culling of boys is a very severe form of scarring. A common result in the literature is that boys are typically more affected than girls by in utero insults (Currie and Schwandt, 2015). Given that in our case gender is not endogenous, we estimate the main regression separately by the gender of the child in utero at the time of the austerity.[20] The results, presented in Table 3a and 3b, indicate that both girls and boys are negatively affected by the shock, but the effects for boys seem to come from the lower tail of the birth weight and pregnancy duration distributions because they are significantly more affected when we consider the low birth weight and preterm delivery indicators.

4.2 Changing occupational status

In our main specification, we restrict the treatment group to mothers employed at the birth of the child in utero in May 2010 and employed at the 2011 Census to be able to match their sector of employment. Thus, we are faced with the issue that some (publicly) employed women pregnant at the time of the austerity announcement exited the labor market after birth and thus have a different occupational status in October 2011, as observed in the descriptive statistics. Of all (publicly and privately) employed mothers who were pregnant in May 2010 (and with at least another child born before the announcement), 35% declared a different occupational status at the Census. This group most likely includes a large share of formerly privately employed mothers who had more unstable jobs and were not affected by the wage cut per se but affected by the cut in maternity leave benefits and by the increasing rate of unemployment in the private sector. It can also include formerly publicly employed mothers who decided to exit the labor force due to the wage cut. We expect that the majority come from the private sector because: 1) overall, private sector employment was 5 times larger than public employment in 2010 while, among mothers, only 20% came from the public sector (source: Ministry of Labor and Family) and 2) public sector employment was perceived to be very stable. In Table 4a, we include these mothers in the treatment group, considering them affected by the austerity measures. The results are very close in magnitude and significance to our main results, albeit for most outcomes the point estimates are slightly smaller in absolute value.[21]

It could also be that some (publicly) employed women, pregnant at the time of the austerity announcement, may have: a) decided to exit the labor force before birth due to the wage cut (and declare her status at birth as housewives) or b) have been made redundant (and declare the status at birth as unemployed).[22] We would then include these mothers in the control group when in fact they should be considered treated. We are not worried about b) because an employer does not have

[20] Excluding gender from the main regression does not impact our results.

[21] For the outcome "low birth weight", the effect is larger in magnitude than our main estimates due to the significantly larger share of low birth weight children in the group of switcher employed mothers, as seen in the descriptive statistics.

[22] We also include in the treatment group mothers who were employed at the 2010 birth and who are unemployed at Census, but who we know come from the public sector. Excluding these 200 observations from our estimations does not impact the results.

the right to fire a pregnant woman.[23] However, in the case of a), the selection can go both ways. We will discuss this potential bias in the Mechanism section.

Finally, some housewives at birth re-entered the labor market by the time of the Census, meaning that we do not include them in our main control group. Including them does not significantly alter our main results, as shown in Table 4b.

4.3 Robustness checks

In this section we: 1) include all not directly affected mothers in the control group; 2) check for indirect effects through fathers' sector of employment; and 3) use difference-in-difference without mother fixed effects as an alternative specification.

4.3.1 All mothers as a control group

In Table 5 we expand the control group to include all mothers pregnant in May 2010 (any occupational category other than employed and those working in the private sector at the Census). The results are in the same direction as our main estimates, but smaller in magnitude and for the low birth weight and small-for-gestational-age indicators become insignificant. This is not surprising because the privately employed are affected both by the austerity measures and by the general status of the economy even before May 2010.

4.3.2 Father employed in the public sector

So far, we have controlled for the father's characteristics as declared in the VSN, including his occupational category. However, both the household income and the pregnant woman's psychological stress, can be negatively affected if the father was employed in the public sector. To address the possibility that the austerity shock also has effects via the father's sector of employment, we redefine our treatment to include the sibling pairs for which, at the birth of the 2010 pregnancy, either the mother or the father were employed in the public sector (defined as before). In the control group, we include sibling pairs for which at the birth of the 2010 pregnancy, the mother was a housewife and the father was not employed in the public sector. Table 6 presents the results, which are very much in line with our main results in magnitude and significance. For some outcomes, for instance, the low birth weight indicator, the point estimates are even slightly larger in magnitude. This is because we now have a better measure of affected households (in which either the mother or/and the father were affected by the austerity measures) and, to a lesser extent, because they are a cleaner control group by excluding housewife women with publicly employed husbands (results available).

4.3.3 Difference-in-difference estimates

We start by estimating a difference-in-difference (DD) specification on our main sample of siblings where the mother has the same occupational status at the birth of the 2010 pregnancy and at the Census, irrespective of her occupational status at earlier births. Essentially, we estimate the same specification as in our main exercise but omitting the mother fixed effects. The results are presented in Table 7. While these effects are in the same direction as our main results, for most of the outcomes the magnitudes are reduced substantially (to half or more) and the low birth weight indicator becomes insignificant. These results suggest that a difference-in-difference design might be confounded by unobservable characteristics.

[23] See the Romanian Government Emergency Ordinance no. 96/2003.

Next, we extend the sample to all births occurring between 2005 and 2011, classifying them into treatment and control based on the mother's occupational status at birth and at the Census. In this way, we include first-time mothers and mothers with pairs of siblings in which neither of the children were affected by the austerity announcement. This extension implies stricter assumptions regarding the maintenance of the same occupational status over longer periods of time, most likely inducing some positive selection among the employed women at birth who need to be employed in the public sector at the Census, and a negative selection in the control group (as we have shown that housewives that enter the labor market are positively selected) . The results in Table 8a are in line with the DD effects on the siblings sample with the exception of low birth weight (which is larger) and small-for-gestational-age (which becomes insignificant and is smaller). This may be because of the selections we induce, or because the children of first time mothers, who constitute a significant share of all births, were more affected. When we look only on the sample of first time mothers in Table 8b under the same restrictions, the effects are larger both compared to Tables 7 and 8a, suggesting that these mothers are indeed more affected.

5 Potential mechanisms and discussions

There are three main mechanisms through which the unexpected announcement of the austerity measures (which entailed a cut in a pregnant woman's wage) may affect children's outcomes at birth: (1) selection into motherhood, abortion and changes in labor supply among pregnant women, (2) nutrition, health damaging behaviors and prenatal care, and (3) prenatal maternal stress. Finally, using additional household level data we show some indication of the austerity impact on various household expenditures.

Selection into fertility, abortion and changes in labor supply We address the selection into fertility as a response to the unexpected austerity measures we only include pregnant women at the time of the announcement. In addition, the mother fixed effects framework addresses any time-invariant unobservable characteristics of the women that become mothers.

One concern is that women who are already pregnant may terminate their pregnancy using abortion. Abortion in Romania is available up to 12 gestational weeks meaning that mothers in the 1st trimester on May 2010, could have reacted to the wage cut announcement by having an abortion.[24] However, our main results indicate significant effects for both the children in the 1st and the 3rd trimesters, and for the latter group abortion was no longer possible. Additionally, Bejenariu (2016) shows that women with low socio-economic status are more likely to change their abortion behavior as a response to financial incentives related to childbirth; if indeed worse off (publicly employed) mothers pregnant in the 1st trimester used abortion, this would entail that our estimates are a lower bound of the true effect.

A decrease in wage may also lower the opportunity cost of leisure and induce a shift in the labor supply of pregnant women from full- to part-time employment which would positively influence children's outcomes at birth. A shift to part time employment is unlikely due to the rigidity of the public sector employment in Romania: less than 1% of public sector employees have a part-time contract.[25] Alternatively, women could have changed their sector of employment after the announcement of the wage cut, but as discussed in Section 3, it is unlikely this occurred due to the

[24]We do not have individual data on abortion, but quarterly aggregated data suggests no significant increase in the total number of abortions. Results available.

[25]Source: Romanian Household Budget Survey (RHBS) 2010. Also, pregnant women could respond to the wage cut by an increased rate of absenteeism, thus increasing their leisure time. The RHBS information on absenteeism does not reveal any significant differences between 2010 and 2007-2009 for women employed in the public sector.

particularities of the institutional setting in which hirings in the public sector wwere not possible and unemployment in the private sector was surging. In the most drastic case, women employed in the public sector could have exited the labor force before giving birth (and appear as "housewife" at birth). However, we test whether the number of housewife mothers significantly changes in 2010 for the first born children and for births that signal a problematic pregnancy: early preterm birth (before the 32^{nd} gestational week) and very low birth weight (a birth weight less than 1,500 grams) and find no such effect.[26] As mentioned previously, the selection of mothers who choose to exit the labor market prior to the birth of their child can be either positive or negative. In the latter case we include some worse off, treated mothers, in the control group which would bias our results towards zero. In the case of a positive selection, our results will be over-estimated if these children were better off as a consequence of the austerity relative to their siblings, which is highly unlikely.

Nutrition, health-damaging behaviors and prenatal care A reduced disposable income may lower the quantity or quality of the food intake of the mother which, in turn, may lead to an insufficient nutritional supply to the fetus. Such restrictions may adversely affect fetal development and are often reflected in a higher incidence of low birth weight, preterm delivery and perinatal morbidity (Gluckman and Hanson (2005); Abrams et al. (2000), Fowles (2004)).[27] Bozzoli and Quintana-Domeque (2013) find worsening health outcomes at birth for children exposed in the 1^{st} and 3^{rd} trimesters to negative economic fluctuations in Argentina and argue that the effect for the latter group is via nutritional restrictions. Almond and Mazumder (2011) look at relatively mild forms of nutritional disruptions imposed by Ramadan and find a negative impact on birth weights for children exposed to fasting during the first two trimesters of pregnancy. Almond et al. (2011) show that pregnancies exposed to the Food Stamp Program in the US three months before birth resulted in an increased birth weight.

We find similar effects on birth weight for the 1^{st} and 3^{rd} trimesters, while the low birth weight indicator is only significant for exposure to the announcement shock in late pregnancy. While we can not fully exclude that the nutrition channel is, at least partially, driving our results, especially for children exposed to the shock in early pregnancy (as they were also exposed to reduced income in late gestation), the nutritional channel may be less relevant for the exposure in late pregnancy (the 3^{rd} trimester) unless these pregnant mothers decreased their nutritional intake in anticipation of upcoming diminished income, which seems unlikely. Results at the household level that will be shown below also indicate no significant changes in foodstuff expenditures following the austerity measure announcement. Yet, we can not fully exclude that the nutrition channel is, at least partially, driving our results, especially for children in the first two trimesters of pregnancy.

A decrease in household income may also induce a decrease in the consumption of health-damaging goods, such as cigarettes and alcohol. The medical literature shows this behavior during pregnancy correlates with an increased risk of miscarriage and low birth weight (Floyd et al. (1993)). Dehejia and Lleras-Muney (2004) find significant improvements in infant health outcomes at birth when the child is conceived during times of high unemployment due to the changes in individual behavior of white mothers who significantly reduced smoking and alcohol consumption during pregnancy.

Lastly, a decrease in disposable income may reduce the usage of antenatal medical supervision.

[26] Using RHBS data we observe there is no change after the wage cut announcement in the share of housewives that used to be employed in the prior 12 months.

[27] Nutritional restrictions during the prenatal period are not necessarily reflected in lower birth weights: for example, individuals exposed in utero in early gestation to the Dutch famine did not present lower birth weights but higher rate of incidence of coronary heart diseases, diabetes and obesity as compared to non-exposed individuals (Painter et al. (2005); Roseboom et al. (2011)).

Using our main identification strategy, we do not find any significant effects on probability of having prenatal controls, the timing of the first check-up, or the mode of delivery (in hospital and whether assisted by a doctor) for the women affected by the wage cut policy (see Appendix Table 12).[28] Moreover, in Romania, prenatal care is free of charge and is available to all pregnant women irrespective of their employment status.

Prenatal stress An unexpected and significant income shock may induce psychological distress due to the financial insecurity it entails. Indeed, 2010 survey evidence indicates higher stress levels, particularly related to inadequate wages, among staff in the public vs. the private Romanian sector (Spielberger et al. (2010)). Similarly, the May 2010 Romanian Eurobarometer shows a significant decrease of more than 20 points in the optimism index from relatively stable values before.[29] The psychological stress caused by the austerity shock may influence the fetal development through higher levels of cortisol, a stress hormone that reaches the fetus inducing structural adaptations to accelerate the his maturation and ensuring his survival in a predicted stressful environment (Gluckman and Hanson (2005)). Though these predictive adaptive responses are not necessarily reflected in birth outcomes (but may manifest later), numerous medical studies have identified a direct link between prenatal stress exposure and an increased incidence of preterm delivery and low birth weight or increased risk of a miscarriage (see Mulder et al. (2002), Maconochie et al. (2007), Beydoun and Saftlas (2008) for comprehensive reviews).

In addition, there is a growing interest among economists in quantifying the effects of maternal stress on infant birth outcomes by exploiting instances in which stress is generated by exogenous events. So far, the evidence indicates that early exposure to stress is more likely to harm a child's outcome at birth. Camacho (2008) finds a negative impact of stress induced by land mine explosions on infant birth weight when exposure occurs during the 1^{st} trimester of the pregnancy, while Mansour and Rees (2012) identify a causal relationship between the number of fatalities in an armed conflict that occurs during the 1^{st} trimester of pregnancy and the increased probability of low birth weight. Vardardottir (2016) finds that early in utero exposure to the 2008 Icelandic financial crisis led to worsening health at birth through maternal stress.[30] Bozzoli and Quintana-Domeque (2013) find increased low birth weight incidence due to negative macroeconomic fluctuations for children in the 1^{st} and 3^{rd} trimesters, but they only attribute early exposure effects to maternal stress. On the other hand, Aizer et al. (2015) use cortisol levels during pregnancy in a mother fixed effects strategy and finds no negative effects of maternal prenatal stress on health at birth, although they find significant negative effects on other long term outcomes. Finally, recent evidence on in utero exposure to psychological stress induced by the death of a maternal relative does not seem to have a differential effect across gestational ages (Black et al., 2016; Persson and Rossin-Slater, 2014).

An indirect effect of increased stress may be an increase in the consumption of health damaging goods such as alcohol and tobacco, although there is mixed empirical evidence of a link between job strain and such behaviors (Azagba and Sharaf, 2011). Quintana-Domeque and Ródenas-Serrano (2016) find evidence of a positive relationship between smoking and bomb casualties for childbearing age women, but not for men, which could be a channel for the negative effect of in utero exposure to terrorism on child health at birth. Because information on smoking or drinking habits is not included in our data, additional household level data in the next section will show a negative, albeit

[28] These results also alleviate concerns related to the inclusions of these possible endogenous variables as covariates in all regressions. Our main results do not change when these covariates are not included.

[29] `http://ec.europa.eu/public_opinion/cf/`

[30] Medical evidence shows that prenatal maternal stress could also lead to improved average health outcomes at birth by means of a natural selection mechanism, whereby prenatal maternal stress raises the fitness criterion required to avoid spontaneous abortion (Trivers and Willard (1973)).

not significant, change in alcohol and cigarette expenditures per capita induced by the austerity measures.

Our results indicate a worsening of health outcomes at birth, particularly so for the children exposed to the austerity announcement in the 1st and 3rd trimesters. We have argued above that the children exposed to the shock in late pregnancy were probably less affected by nutritional restrictions, as the actual decrease in income came in effect three months later, when they would have already been born. This, in turn, may indicate that exposure to the shock in late pregnancy, may impact the in utero children trough stress. However, we cannot disentangle the direct effect of physiological stress (hormones) from the indirect effect through an increase in health damaging behaviors (smoking and/or drinking).

Impact of the austerity measures at the household level Finally, in an attempt to understand the impact of teh austerity measures, we use the Romanian Household Budget Survey (RHBS), the main tool of assessing population expenditures and revenues, covering approximately 30,000 households per year and containing detailed income and expenditure information. Because the data are only available at the household level, we compare households that have at least one publicly employed member and households that have no publicly employed members. While these are not households with a pregnant mother, which may behave quite different, we still think it is interesting to evaluate their expenditures just before (January-July 2010) and after (August-December 2010) the austerity measures implementation. The results in Table 9 (Panel A) indicate a significant decrease in household wage related income of 16.7% and in total household income of approximately 7% for households with at least one publicly employed member. Because we only have information at the household level and therefore we also include households with privately employed members in the control group (affected by spillover effects of the austerity measures as we will detail below), the table shows most likely a lower bound of the true effects the austerity impact. Overall, the households affected by the wage cut seem to have no significant changes in food-related (column 3) or alcohol and cigarette (column 4) expenditures, but they have significantly reduce non-food (column 5) and services expenditures (e.g. (column 6). Finally, column (7) seem to indicate that households react to the wage shock by decreasing the (formal) savings by approximately 11.9%. These expenditures reflect the behavior of the average publicly employed households, so we cannot dismiss their effect on pregnant women. Finally, in Panel B, in a similar strategy but for 2009 (as a placebo year), the effects are much smaller and sometimes even move in the opposite direction.

6 Medium-term effects on child health

Given the evidence above, we can conclude that the austerity announcement had a negative impact at birth on children exposed to this shock in utero. However, in light of the latest studies, which tend to show that in utero shocks have long lasting impacts and affect adult outcomes beyond health status, there are still several questions that are interesting to understand. Are these negative outcomes at birth persistent? Can we find any indication in line with the idea of compensatory investments following early-life or in utero shocks? These are very important questions with little evidence at present (Currie and Almond, 2011). Finally, we also provide some evidence on whether the negative income shock (while pregnant) and the likely psychological stress it entails may have affected the mothers' mental health.

In what follows we take advantage of new data, the Romanian Health Interview Survey (SANPOP), collected as part of the 2014 European Health Survey. The survey collects socio-economic information from approximately 18,500 individuals and their households, using three individual

questionnaires (for children under 15 years, for persons aged 15 years and over, and a self-completion questionnaire designed exclusively for persons aged above 15 years) that record information on health status, access to health care services and information on lifestyle. We merge the 740 children born between 2005-2011 (the same period as in the main analysis) with the VSN and Census data used in the main estimations and we find that, among the 323 mothers working in the public sector or housewives at the time they gave birth, 137 (or 18.5%) were pregnant at the time of the austerity announcement.[31]

We employ a simple DD strategy and compare the outcomes for the children in utero on May 7th 2010 with those born before, between the publicly employed mothers and their housewife counterparts.[32] While these results may be informative, due to the nature of the data we need to be cautious in claiming causal effects.

We have a number of variables that could be used as proxies for the child's health and the parents' investment response to the unexpected in-utero shock. Because, a priori, there is no obvious choice, we report estimates for most of these variables in our analysis.[33] In particular we consider whether the child has a chronic disease or limited activity because of a health problem together with information on the child's hospitalization or doctor visits (when not a routine checkup). The survey also asks some questions that could indicate more clearly the parents' post-shock compensating behavior: if the child had a flu vaccination (a parents' preventive investment) or had been administered vitamin supplements.

In column 1 of Table 10, we observe a significant and positive effect on the indicator reflecting a chronic condition, suggesting a lasting negative effect of the in-utero shock. Most of these reported conditions are asthma and diabetes. Interestingly, several studies exploring the in utero exposure to the Dutch famine have found that nutritional restrictions during the prenatal period, although not necessarily reflected in lower birth weights, lead to higher rates of coronary heart diseases, diabetes and obesity compared to non-exposed individuals (Painter et al., 2005; Roseboom et al., 2011).[34] The negative and significant effect on an indicator reflecting the child's limited activity as a result of a health problem during the last half a year (column 2) seems to confirm the lasting negative impact of the in-utero shock. Although the probability of hospitalization and of having a medical check-up (excluding routine checks) are not significantly affected, we observe that exposed children were more likely to have used prescribed drugs (column 5). The variables in columns 6 and 7 may seem to indicate a compensating post-shock behavior: affected children were more likely to have a flu vaccination (as a prevention treatment) and, albeit not significant, to receive vitamins to improve their immune system (not prescribed by a doctor).

Finally, we also check the effects of the austerity measures on the mothers' mental health at the time of the survey, based on their responses to the Patient Health Questionnaire 8 (PHQ-8).[35]

[31] Using the same working assumptions as in the main estimations, among the 328 mothers in our sample, 82 (or 25%) were employed in the public sector at birth. This sample is obtained by merging the SANPOP data with our main data using the child's and mother's day of birth and the child's gender because we do not have the personal identifiers. In Appendix we show summary statistics and a more detailed description of the variables used in this section.

[32] Because of the small sample size, we are not able to use a mother fixed effects specification. However, given that our main results are robust when using a mother fixed effects strategy and a DD, we feel comfortable with the latter. Also, Currie and Almond (2011) discuss some difficulties in interpreting fixed effects models when parents compensate and will underestimate the total effect of the shock.

[33] For some other variables (e.g., subjective health status reported by the mother, dentist visit, surgeon visit, accident related questions) we do not show the results because we do not have enough variation in the data.

[34] At the same time, we may think of a scenario when a compensatory mechanism may determine the affected mother to be more likely to take the child to medical controls, because of their poorer health status at birth. In our regressions, we controls for child health at birth by including indicators for low birth weight and premature delivery, which could mediate the effect.

[35] There are different ways of assessing depression using this scale. In our data we did not have enough variation to

and we observe no significant effect in the mother's depression index for the women pregnant at the time of the announcement relative to those pregnant before (results available).

7 Conclusions

The present study shows that prenatal exposure to economic shocks can influence the birth outcomes of the in utero cohorts. Using a major and unexpected wage cut policy that affected all public sector employees in Romania in 2010, we investigate the effects of negative income shocks on outcomes at birth. Our results suggest that such drastic austerity measures negatively affected child health at birth, with affected children having a lower birth weight, shorter gestational length, greater likelihood of being born prematurely and worse measures of fetal growth. This is especially important because the literature has shown that health at birth has scarring, long-lasting effects on both health and non-health outcomes.

Children who were in the last trimester of gestation at the time of the announcement as well as those who were exposed the longest, starting with the first trimester, appear to be most affected. We argue that that main channel through which the austerity measures affected child health at birth was prenatal maternal stress, but we cannot disentangle the direct effect of physiological stress (hormones) from the indirect effect of an increase in health damaging-behaviors (smoking or drinking).

From a policy perspective, it is important to understand the mechanisms through which such income shocks affect unborn children. If prenatal nutrition, prenatal care or selective abortions would be the main mechanism in place, policymakers could potentially reverse the effect through programs such as food stamps. However, if the main mechanism is primarily a biological response to severe stressors, then there is less opportunity to reverse the policy's impact, and this needs to be taken into consideration when such drastic measures are implemented.

be able to look at severe (in which both the first and second item need to score at least 2, indicating more than half of the time, or other depression with 2 to 4 symptoms, including the first or second item need to be present and score at least 2). In what follows we use the use the "more relaxed" definition of depression: None – Minimal depression (0 to 4); Mild depression (5 to 9); Moderate depression (10 to 14); Moderately severe depression (15 to 19); Severe depression (20 to 24).

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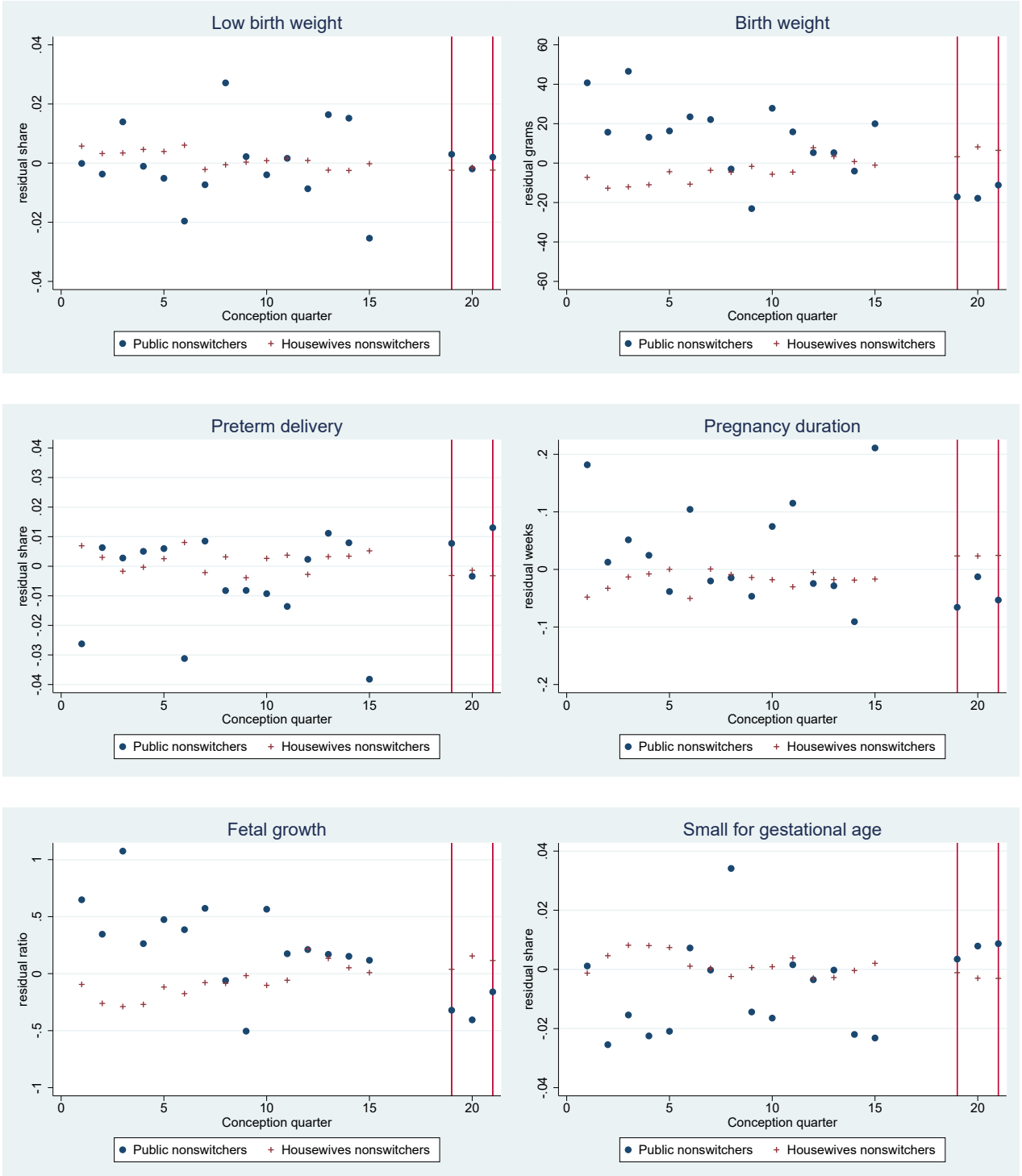
Figure 1: Residual outcomes, sibling sample



Notes: Average residual outcomes at birth in the sibling sample, after controlling for conception month-year, mother fixed effects and all individual covariates from the main regressions. Vertical lines mark children who were in utero at the time of the austerity measures announcement. Siblings conceived between Nobember 2008 and July 2009 are excluded from the graph due to the very small number of observations, caused by the very low probability of mothers conceiving another child in the months after birth.

Table 1: Descriptive statistics for the sibling sample, maternal and child characteristics at the birth of the 2010 pregnancy

(a) Maternal characteristics at the birth of the 2010 pregnancy

	Employed at VSN Public at PC (Public nonswitcher)	Employed at VSN other occup. at PC (Employed switcher)	Housewive at VSN housewife at PC (Housewife nonswitcher)	Housewive at VSN other occup. at PC (Housewive switcher)
High education	0.692	0.294	0.009	0.075
	(0.462)	(0.456)	(0.096)	(0.263)
Medium education	0.262	0.461	0.205	0.318
	(0.440)	(0.498)	(0.404)	(0.466)
Low education	0.046	0.246	0.786	0.607
	(0.211)	(0.431)	(0.410)	(0.488)
Age at birth	30.900	28.716	25.791	26.610
	(3.696)	(4.575)	(5.638)	(5.296)
Number live children	2.142	2.390	3.174	2.918
	(0.649)	(1.254)	(1.875)	(1.734)
Married	0.953	0.896	0.637	0.647
	(0.212)	(0.305)	(0.481)	(0.478)
Urban residence	0.778	0.573	0.263	0.584
	(0.416)	(0.495)	(0.441)	(0.493)
Sample	2839	5862	21222	1862

(b) Child characteristics, all births

	Employed at VSN Public at PC (Public nonswitcher)	Employed at VSN other occup. at PC (Employed switcher)	Housewive at VSN housewife at PC (Housewife nonswitcher)	Housewive at VSN other occup. at PC (Housewive switcher)
Girl	0.493	0.492	0.497	0.484
	(0.500)	(0.500)	(0.500)	(0.500)
Low birth weight	0.037	0.056	0.099	0.090
	(0.188)	(0.231)	(0.299)	(0.286)
Birth weight	3351.004	3297.175	3154.990	3189.288
	(467.611)	(494.752)	(511.590)	(510.810)
Premature delivery	0.088	0.094	0.137	0.137
	(0.283)	(0.292)	(0.344)	(0.344)
Pregnancy duration	38.982	39.019	38.823	38.838
	(1.422)	(1.545)	(1.719)	(1.682)
Fetal growth	85.883	84.392	81.130	81.988
	(11.221)	(11.754)	(12.120)	(12.179)
Small for gest. age	0.055	0.085	0.143	0.128
	(0.228)	(0.278)	(0.350)	(0.335)
Observations	5819	12445	49308	4199

Source: VSN dataset, authors' tabulations.

Table 2: Main results: Mother fixed effects, Public and Housewife Nonswitchers

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.0170		-53.65***		0.0360**		-0.205**		-0.924*		0.0266*
		(0.0118)		(20.10)		(0.0164)		(0.0829)		(0.485)		(0.0141)
inutero_t2_public		0.00625		-53.84**		0.00208		-0.115		-1.084**		0.0233*
		(0.0132)		(21.90)		(0.0167)		(0.0829)		(0.517)		(0.0138)
inutero_t3_public		0.0219**		-68.63***		0.0296*		-0.266***		-1.264**		0.0171
		(0.0103)		(23.57)		(0.0176)		(0.0937)		(0.571)		(0.0158)
inutero_public	0.0145*		-58.03***		0.0217*		-0.190***		-1.080***		0.0227**	
	(0.00804)		(13.73)		(0.0118)		(0.0584)		(0.338)		(0.00963)	
Constant	0.192**	0.192**	2,702***	2,703***	0.710***	0.710***	36.22***	36.23***	74.78***	74.79***	0.127	0.127
	(0.0840)	(0.0839)	(105.3)	(105.3)	(0.0883)	(0.0884)	(0.474)	(0.474)	(2.784)	(2.784)	(0.0960)	(0.0960)
Mean dep var	0.093	0.093	3175.681	3175.681	0.132	0.132	38.840	38.840	81.632	81.632	0.133	0.133
Observations	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136
R-squared	0.581	0.581	0.725	0.725	0.528	0.528	0.555	0.555	0.714	0.714	0.551	0.551

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 3: Effects by the gender of the child in utero in May 2010

(a) Child in utero was a boy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Low birth weight	Low birth weight	Birth weight	Birth weight	Premature delivery	Premature delivery	Pregnancy duration	Pregnancy duration	Fetal growth	Fetal growth	Small for gest. age	Small for gest. age
inutero_t1_public_b		0.0237*		-46.56		0.0460**		-0.203*		-0.699		0.0363
		(0.0127)		(35.43)		(0.0214)		(0.106)		(0.873)		(0.0241)
inutero_t2_public_b		0.00745		-50.20		0.0168		-0.182		-0.860		0.0117
		(0.0165)		(34.20)		(0.0249)		(0.113)		(0.856)		(0.0197)
inutero_t3_public_b		0.0313**		-73.55**		0.0360		-0.273*		-1.348*		0.0282
		(0.0143)		(32.99)		(0.0263)		(0.152)		(0.736)		(0.0200)
inutero_public_b	0.0200**		-55.59**		0.0325*		-0.216***		-0.943*		0.0250*	
	(0.00945)		(21.83)		(0.0165)		(0.0802)		(0.560)		(0.0144)	
Constant	0.192	0.192	2,723***	2,724***	0.473***	0.474***	37.56***	37.56***	72.87***	72.90***	0.147	0.147
	(0.131)	(0.131)	(165.7)	(166.1)	(0.105)	(0.104)	(0.599)	(0.598)	(4.081)	(4.094)	(0.133)	(0.133)
Observations	28,166	28,166	28,166	28,166	28,166	28,166	28,166	28,166	28,166	28,166	28,166	28,166
R-squared	0.575	0.575	0.727	0.727	0.530	0.530	0.561	0.561	0.716	0.716	0.551	0.551

(b) Child in utero was a girl

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Low birth weight	Low birth weight	Birth weight	Birth weight	Premature delivery	Premature delivery	Pregnancy duration	Pregnancy duration	Fetal growth	Fetal growth	Small for gest. age	Small for gest. age
inutero_t1_public		0.0107		-59.23**		0.0250		-0.196*		-1.141*		0.0148
		(0.0177)		(26.25)		(0.0216)		(0.104)		(0.660)		(0.0208)
inutero_t2_public		0.00151		-54.41**		-0.0107		-0.0523		-1.220**		0.0314*
		(0.0161)		(24.93)		(0.0236)		(0.117)		(0.594)		(0.0188)
inutero_t3_public		0.0126		-59.97*		0.0229		-0.253*		-1.094		0.00536
		(0.0171)		(33.97)		(0.0249)		(0.135)		(0.836)		(0.0241)
inutero_public_f	0.00787		-57.66***		0.0112		-0.159**		-1.156***		0.0182	
	(0.00992)		(16.36)		(0.0147)		(0.0778)		(0.389)		(0.0116)	
Constant	0.107	0.107	2,978***	2,978***	0.730***	0.730***	36.67***	36.67***	81.16***	81.16***	0.0718	0.0718
	(0.107)	(0.107)	(154.7)	(154.7)	(0.132)	(0.131)	(0.749)	(0.748)	(3.774)	(3.773)	(0.152)	(0.152)
Observations	27,115	27,115	27,115	27,115	27,115	27,115	27,115	27,115	27,115	27,115	27,115	27,115
R-squared	0.590	0.590	0.723	0.723	0.529	0.529	0.553	0.553	0.711	0.711	0.556	0.556

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 4: Robustness: extending treatment and control groups

(a) Extending treatment group to include public nonswitcher and employed switcher mothers

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
Panel A: Extending treatment group to include public nonswitchers and employed switchers												
inutero_t1_public		0.0244***		-53.66***		0.0321**		-0.213***		-0.945**		0.0195**
		(0.00873)		(15.01)		(0.0128)		(0.0613)		(0.366)		(0.00918)
inutero_t2_public		0.0119		-43.66**		0.00327		-0.0971		-0.896**		0.0217**
		(0.01000)		(17.03)		(0.0133)		(0.0670)		(0.385)		(0.0109)
inutero_t3_public		0.0209**		-41.16***		0.0291**		-0.203***		-0.669*		0.0106
		(0.00828)		(15.28)		(0.0131)		(0.0592)		(0.380)		(0.0124)
inutero_public	0.0189***		-46.47***		0.0210**		-0.169***		-0.847***		0.0177***	
	(0.00618)		(10.18)		(0.00976)		(0.0453)		(0.243)		(0.00662)	
Constant	0.178**	0.178**	2,728***	2,727***	0.705***	0.705***	36.24***	36.24***	75.43***	75.42***	0.112	0.113
	(0.0857)	(0.0856)	(112.9)	(112.9)	(0.0847)	(0.0847)	(0.465)	(0.464)	(2.943)	(2.943)	(0.0947)	(0.0947)
Mean dep var	0.093	0.093	3175.681	3175.681	0.132	0.132	38.840	38.840	81.632	81.632	0.133	0.133
Observations	67,572	67,572	67,572	67,572	67,572	67,572	67,572	67,572	67,572	67,572	67,572	67,572
R-squared	0.581	0.581	0.728	0.728	0.532	0.532	0.559	0.559	0.718	0.718	0.557	0.557

(b) Extending control group to all housewives at birth

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.0156		-49.26**		0.0338**		-0.206***		-0.808*		0.0249*
		(0.0112)		(20.13)		(0.0156)		(0.0770)		(0.480)		(0.0140)
inutero_t2_public		0.00808		-48.24**		-0.00337		-0.103		-0.972*		0.0225*
		(0.0137)		(21.76)		(0.0172)		(0.0834)		(0.508)		(0.0125)
inutero_t3_public		0.0236**		-70.39***		0.0298*		-0.270***		-1.287**		0.0198
		(0.0103)		(23.94)		(0.0168)		(0.0921)		(0.581)		(0.0150)
inutero_public	0.0151*		-54.95***		0.0190		-0.187***		-1.006***		0.0226**	
	(0.00810)		(13.78)		(0.0116)		(0.0557)		(0.342)		(0.00877)	
Constant	0.216***	0.216***	2,677***	2,678***	0.706***	0.707***	35.26***	35.26***	76.41***	76.43***	0.101	0.101
	(0.0727)	(0.0726)	(100.6)	(100.7)	(0.0824)	(0.0824)	(0.460)	(0.459)	(2.660)	(2.661)	(0.0884)	(0.0887)
Mean dep var	0.093	0.093	3175.681	3175.681	0.132	0.132	38.840	38.840	81.632	81.632	0.133	0.133
Observations	67,553	67,553	67,553	67,553	67,553	67,553	67,553	67,553	67,553	67,553	67,553	67,553
R-squared	0.580	0.580	0.724	0.724	0.531	0.531	0.558	0.558	0.712	0.712	0.551	0.551

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 5: Robustness: including all non-publicly employed mothers in the control group

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.00476		-27.90		0.0281*		-0.141*		-0.387		0.0168
		(0.0117)		(20.51)		(0.0142)		(0.0714)		(0.482)		(0.0144)
inutero_t2_public		-0.00242		-28.82		-0.00975		-0.0248		-0.637		0.0151
		(0.0135)		(21.79)		(0.0159)		(0.0834)		(0.509)		(0.0120)
inutero_t3_public		0.0112		-55.46**		0.0232		-0.211**		-1.026*		0.0118
		(0.0101)		(24.72)		(0.0163)		(0.0875)		(0.603)		(0.0146)
inutero_public	0.00392		-36.09**		0.0127		-0.118**		-0.661*		0.0147	
	(0.00832)		(14.22)		(0.0101)		(0.0495)		(0.349)		(0.00909)	
Constant	0.183***	0.183***	2,687***	2,688***	0.639***	0.640***	35.34***	35.35***	76.57***	76.60***	0.0831	0.0834
	(0.0655)	(0.0654)	(93.98)	(93.98)	(0.0865)	(0.0866)	(0.461)	(0.461)	(2.456)	(2.454)	(0.0860)	(0.0861)
Mean dep var	0.093	0.093	3175.681	3175.681	0.132	0.132	38.840	38.840	81.632	81.632	0.133	0.133
Observations	92,379	92,379	92,379	92,379	92,379	92,379	92,379	92,379	92,379	92,379	92,379	92,379
R-squared	0.583	0.583	0.728	0.728	0.539	0.539	0.566	0.566	0.718	0.718	0.557	0.557

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 6: Robustness: Treatment group including publicly employed mothers or fathers; Control group including housewife mothers and non-public fathers.

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.0128		-59.88***		0.0173		-0.156**		-1.164**		0.0282**
		(0.00922)		(18.09)		(0.0139)		(0.0643)		(0.447)		(0.0114)
inutero_t2_public		0.0143		-57.92***		0.00869		-0.140**		-1.168**		0.0223
		(0.0116)		(19.16)		(0.0143)		(0.0659)		(0.451)		(0.0149)
inutero_t3_public		0.0224**		-74.07***		0.0269*		-0.253***		-1.429***		0.0148
		(0.00960)		(19.19)		(0.0159)		(0.0782)		(0.479)		(0.0136)
inutero_public	0.0161**		-63.15***		0.0168		-0.178***		-1.240***		0.0223**	
	(0.00705)		(12.68)		(0.0105)		(0.0464)		(0.313)		(0.00872)	
Constant	0.178**	0.177**	2,747***	2,748***	0.692***	0.692***	36.31***	36.31***	75.80***	75.81***	0.0979	0.0984
	(0.0817)	(0.0816)	(109.7)	(109.7)	(0.0870)	(0.0870)	(0.465)	(0.464)	(2.901)	(2.901)	(0.0981)	(0.0981)
Mean dep var	0.093	0.093	3175.681	3175.681	0.132	0.132	38.840	38.840	81.632	81.632	0.133	0.133
Observations	58,220	58,220	58,220	58,220	58,220	58,220	58,220	58,220	58,220	58,220	58,220	58,220
R-squared	0.581	0.581	0.727	0.727	0.529	0.529	0.557	0.557	0.717	0.717	0.553	0.553

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 7: DD on the same sample as the fixed effects

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.00413		-38.64**		0.0194*		-0.154**		-0.604		0.0242***
		(0.00768)		(15.19)		(0.0115)		(0.0603)		(0.372)		(0.00910)
inutero_t2_public		0.00394		-42.99***		0.00665		-0.106*		-0.840**		0.0184**
		(0.00729)		(15.07)		(0.0119)		(0.0562)		(0.346)		(0.00796)
inutero_t3_public		0.0105		-41.63**		0.0242**		-0.212***		-0.653		0.00613
		(0.00752)		(19.20)		(0.0119)		(0.0619)		(0.461)		(0.00886)
inutero_public	0.00588		-41.11***		0.0161*		-0.153***		-0.706***		0.0169***	
	(0.00602)		(10.05)		(0.00909)		(0.0437)		(0.249)		(0.00629)	
Constant	0.130***	0.130***	2,814***	2,814***	0.366***	0.366***	38.06***	38.06***	74.18***	74.18***	0.196***	0.196***
	(0.0193)	(0.0193)	(37.96)	(37.96)	(0.0501)	(0.0501)	(0.181)	(0.181)	(1.121)	(1.120)	(0.0287)	(0.0288)
Mean dep var	0.093	0.093	3175.681	3175.681	0.132	0.132	38.840	38.840	81.632	81.632	0.133	0.133
Observations	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136
R-squared	0.024	0.024	0.085	0.085	0.032	0.032	0.043	0.043	0.080	0.080	0.020	0.020

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 8: DD on the full sample of births

(a) DD on the full sample of births 2005-2011

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.00437		-28.60***		0.0130*		-0.132***		-0.457**		0.00450
		(0.00418)		(8.918)		(0.00730)		(0.0338)		(0.216)		(0.00469)
inutero_t2_public		0.00731**		-32.55***		0.0147**		-0.0847**		-0.648***		0.00437
		(0.00365)		(8.203)		(0.00719)		(0.0342)		(0.195)		(0.00467)
inutero_t3_public		0.0161***		-41.42***		0.0264***		-0.174***		-0.752***		0.00402
		(0.00401)		(10.44)		(0.00729)		(0.0304)		(0.248)		(0.00484)
inutero_public	0.00858***		-33.12***		0.0169***		-0.122***		-0.605***		0.00431	
	(0.00216)		(5.492)		(0.00528)		(0.0223)		(0.137)		(0.00287)	
Constant	0.141***	0.140***	2,841***	2,842***	0.333***	0.333***	38.06***	38.06***	74.94***	74.95***	0.177***	0.177***
	(0.00950)	(0.00949)	(21.10)	(21.03)	(0.0316)	(0.0315)	(0.124)	(0.123)	(0.584)	(0.583)	(0.0140)	(0.0140)
Observations	530,688	530,688	530,688	530,688	530,688	530,688	530,688	530,688	530,688	530,688	530,688	530,688
R-squared	0.025	0.026	0.069	0.070	0.027	0.028	0.038	0.040	0.066	0.066	0.017	0.017

(b) DD on the full sample of births 2005-2011, first time mothers

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.00322		-27.78**		0.0116		-0.135***		-0.429		0.00613
		(0.00634)		(12.55)		(0.0101)		(0.0463)		(0.292)		(0.00635)
inutero_t2_public		0.0145**		-41.83***		0.0194**		-0.115***		-0.857***		0.00848
		(0.00552)		(10.75)		(0.00855)		(0.0437)		(0.269)		(0.00747)
inutero_t3_public		0.0205***		-40.35***		0.0268***		-0.164***		-0.743**		0.0117
		(0.00626)		(15.11)		(0.00938)		(0.0423)		(0.342)		(0.00705)
inutero_public	0.0124***		-36.33***		0.0186***		-0.134***		-0.676***		0.00868**	
	(0.00312)		(7.108)		(0.00608)		(0.0275)		(0.172)		(0.00416)	
Constant	0.121***	0.120***	2,924***	2,924***	0.305***	0.305***	38.19***	38.19***	76.78***	76.78***	0.161***	0.161***
	(0.0163)	(0.0163)	(28.53)	(28.44)	(0.0378)	(0.0377)	(0.150)	(0.149)	(0.706)	(0.706)	(0.0191)	(0.0191)
Observations	224,838	224,838	224,838	224,838	224,838	224,838	224,838	224,838	224,838	224,838	224,838	224,838
R-squared	0.022	0.022	0.067	0.067	0.022	0.023	0.031	0.032	0.064	0.064	0.019	0.019

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 9: Household income and expenditures pattern

	(1) Log HH wage income	(2) Log HH income	(3) Log Foodstuff expenditures per capita	(4) Log Alcohol & cigarettes exp. per capita	(5) Log Non-foodstuff expenditures per capita	(6) Log Expenditures Services Services	(7) Log (formal) savings
Panel A: 2010							
Public*after	-0.167*	-0.070***	-0.017	-0.043	-0.062**	-0.051*	-0.119**
	(0.095)	(0.022)	(0.018)	(0.080)	(0.030)	(0.028)	(0.057)
after	0.077	0.013	0.219***	0.508***	0.138***	-0.106***	-0.011
	(0.095)	(0.019)	(0.020)	(0.071)	(0.038)	(0.025)	(0.032)
public	2.395***	0.121***	-0.008	-0.015	-0.014	0.032	0.003
	(0.151)	(0.028)	(0.027)	(0.122)	(0.046)	(0.034)	(0.058)
Controls	yes	yes	yes	yes	yes	yes	yes
Observations	14,328	14,328	14,328	14,328	14,328	14,328	14,328
R-squared	0.688	0.587	0.385	0.209	0.186	0.399	0.048
Panel B: 2009							
Public*after	-0.040	-0.049	-0.001	0.023	0.014	0.040	0.002
	(0.060)	(0.030)	(0.016)	(0.082)	(0.041)	(0.029)	(0.050)
after	-0.068	0.073***	0.248***	0.431***	0.216***	-0.107***	0.088**
	(0.072)	(0.019)	(0.017)	(0.071)	(0.044)	(0.029)	(0.040)
public	2.259***	0.175***	0.028	0.004	0.116**	0.125***	-0.014
	(0.127)	(0.036)	(0.024)	(0.125)	(0.052)	(0.042)	(0.086)
Controls	yes	yes	yes	yes	yes	yes	yes
Observations	14,598	14,598	14,598	14,598	14,598	14,598	14,598
R-squared	0.699	0.611	0.371	0.206	0.180	0.369	0.054

Notes: All dependent variables in columns (1)-(7) are in logs. Public = 1 when at least one adult in the household is employed in the public sector, and 0 if no household member is employed in the public sector. The sample does not include households where the head is unemployed. After = 1 for households income/expenditures during June-December of the respective year, and 0, for January-May. 1 USD = 3 RON. Alcohol and cigarettes expenditures are deflated with a specific indicator calculated by the National Bank of Romania to account for inflation and changes in the special excise taxes that apply to alcohol and cigarettes. The sample includes only urban households, as this is our group of interest in the next sections. Controls include: household head gender, education, age, no of kids, household occupational composition, county indicators and indicators for the month for which the income/expenditures are reported. Source: Authors' calculations using 2009-2010 Romanian Household Budget Surveys. Clustered standard errors at the county level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 10: Medium term child health outcomes, DD Public vs Housewives

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Chronic illness	Limited activity	Hospitalization	Doctor visit	Medicine	Vitamins	Vaccination
treated	0.230*	0.0808**	-0.0234	0.271	0.245*	0.134	0.274*
	(0.118)	(0.0358)	(0.0548)	(0.250)	(0.141)	(0.163)	(0.161)
public	-0.0150	0.000716	0.0662	-0.138	-0.0782	-0.128*	0.0495
	(0.0490)	(0.0267)	(0.0503)	(0.134)	(0.0662)	(0.0734)	(0.112)
post	-0.0257	-0.0407	-0.132*	0.317	0.101	-0.0818	0.181
	(0.0720)	(0.0414)	(0.0704)	(0.240)	(0.0947)	(0.175)	(0.165)
Constant	-0.238	0.0195	-0.254*	1.086**	-0.00851	0.154	-0.415
	(0.189)	(0.167)	(0.141)	(0.437)	(0.183)	(0.279)	(0.332)
Observations	323	323	323	234	323	323	323
R-squared	0.372	0.300	0.364	0.462	0.398	0.254	0.424

Notes: All regressions include child gender, child birth year fixed effects, child month of birth fixed effects, low birth weight and preterm delivery indicators, indicators of mother's educational status at the time of the survey, marital status, mother's age at the time of the survey and county of residence fixed effects. Observations are weighted using survey weights. Robust standard errors in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 11: APPENDIX: Robustness: excluding siblings conceived after May 2010

VARIABLES	(1) Low birth weight	(2) Low birth weight	(3) Birth weight	(4) Birth weight	(5) Premature delivery	(6) Premature delivery	(7) Pregnancy duration	(8) Pregnancy duration	(9) Fetal growth	(10) Fetal growth	(11) Small for gest. age	(12) Small for gest. age
inutero_t1_public		0.0173		-53.18**		0.0376**		-0.215***		-0.892*		0.0269*
		(0.0120)		(20.63)		(0.0160)		(0.0814)		(0.495)		(0.0143)
inutero_t2_public		0.00675		-53.71**		0.00135		-0.123		-1.063**		0.0248*
		(0.0132)		(22.07)		(0.0171)		(0.0832)		(0.527)		(0.0135)
inutero_t3_public		0.0188*		-68.48***		0.0273		-0.254**		-1.278**		0.0184
		(0.0102)		(24.05)		(0.0182)		(0.0982)		(0.576)		(0.0164)
inutero_public	0.0139*		-57.74***		0.0214*		-0.192***		-1.065***		0.0237**	
	(0.00818)		(14.18)		(0.0120)		(0.0585)		(0.348)		(0.00999)	
Constant	0.218**	0.217**	2,670***	2,671***	0.740***	0.740***	36.05***	36.06***	74.32***	74.34***	0.145	0.145
	(0.0850)	(0.0850)	(110.7)	(110.7)	(0.0978)	(0.0979)	(0.505)	(0.505)	(2.890)	(2.890)	(0.0976)	(0.0977)
Observations	53,415	53,415	53,415	53,415	53,415	53,415	53,415	53,415	53,415	53,415	53,415	53,415
R-squared	0.581	0.581	0.726	0.726	0.529	0.529	0.554	0.555	0.716	0.716	0.554	0.554

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, assistance at birth, indicators for trimester of first prenatal control, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 12: APPENDIX: Utilization of antenatal medical care

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Prenatal control	Prenatal control	Control in first trim	Control in first trim	Control in second trim	Control in second trim	Control in third trim	Control in third trim	Hospital delivery	Hospital delivery	Doctor at delivery	Doctor at delivery
inutero_t1_public		-0.00677		0.000928		-0.0105		0.00284		0.00379		-0.0109
		(0.0222)		(0.0264)		(0.0220)		(0.00604)		(0.00388)		(0.0119)
inutero_t2_public		-0.0325		-0.0458		0.0167		-0.00338		0.00125		-0.0101
		(0.0301)		(0.0328)		(0.0233)		(0.00740)		(0.00326)		(0.0163)
inutero_t3_public		-0.0391		-0.0205		-0.0176		-0.000894		0.000387		-0.0105
		(0.0341)		(0.0378)		(0.0227)		(0.00798)		(0.00298)		(0.0136)
inutero_public	-0.0255		-0.0224		-0.00261		-0.000511		0.00188		-0.0105	
	(0.0247)		(0.0271)		(0.0179)		(0.00577)		(0.00222)		(0.0127)	
Observations	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136	55,136
R-squared	0.649	0.649	0.610	0.610	0.513	0.513	0.464	0.464	0.555	0.555	0.699	0.699

Notes: All regressions include month-year of conception fixed effects. Individual control include: child gender, birth order, mother's age at birth, indicators for maternal education, indicators for maternal occupational status at birth, marital status, father's age at birth, father's occupational status at birth and an indicator for missing information about the father. Clustered standard errors at the county-urbanity level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$.