**ECONOMIC STUDIES**

**DEPARTMENT OF ECONOMICS**
**SCHOOL OF BUSINESS, ECONOMICS AND LAW**
**UNIVERSITY OF GOTHENBURG**
**220**

# Determinants of Health Capital at Birth: Evidence from Policy Interventions

**Simona Bejenariu**

UNIVERSITY OF GOTHENBURG

ISBN 978-91-85169-95-5 (printed)
ISBN 978-91-85169-96-2 (pdf)
ISSN 1651-4289 print
ISSN 1651-4297 online

Printed in Sweden,
Ineko 2015

*To Valentin and our families.*

# Acknowledgments

During the last five years, I thought many times about the moment I would write these Acknowledgments. I thought it would be marked by a feeling of accomplishment and joy. But it is a time of reflection, of looking back at all the opportunities I was given, and most of all of being truly grateful for all the help I received to getting here. Being a PhD student meant more than just working towards a degree, it was an extremely valuable life experience, that I am grateful to have received. Standing at this milestone, I am both happy and sad it is so close to finishing.

I am deeply grateful to my supervisors, Andreea Mitruţ and Randi Hjalmarsson. Andreea became more than supervisor to me, she is a mentor and a very close friend. She generously shared her knowledge and experience, and taught me most of what I know about research. Her constant encouragement, optimism and warmth gave me a feeling of comfort and trust that meant so much to me during these years and helped me overcome the occasional hold-backs. I am grateful to her for so many things that I cannot even express them all. I am also extremely grateful to Randi, who motivated me to strive and do better, guided and prepared me for my next steps after finishing this PhD. She read every version of my single authored paper and never seemed tired of giving me excellent feedback and suggestions. I felt like I was learning to ride a bike without the training wheels, while knowing that I am watched over and guided by someone who cared. I am grateful for all her support. I have been extremely fortunate to have them both as role models!

This thesis benefited greatly from the comments of Peter Nilsson from Stockholm University and Hans Grönqvist from Uppsala University who were my opponents at the Licentiate and Final seminars. Their insightful comments helped me improve the papers a great deal.

I would like to thank all my colleagues at the Department of Economics at the University of Gothenburg for the stimulating and supportive environment they created. I am especially grateful to Yonas Alem, Kristian Bolin, Dick Durevall, Lennart Flood, Lennart Hjalmarsson, Olof Johansson-Stenman, Per Krusell, Katarina Nordblom, Ola Olsson, Amrish Patel, Oleg Shchetnin, Eyerusalem Siba, Johan Stennek, Michele Valsecchi and Conny Wollbrant, who offered excellent courses and mentoring. Måns Söderbom was an inspiring teacher, who gave me time and again valuable academic advice. Simon Felgendreher, Verena Kurz, Efi Kyriakopoulou and Andrea Martinangeli became good friends of mine who made coming to work (and after work!) a pleasure! I also thank Åsa Adin, Åsa Löfgren, Eva-Lena Neth-Johansson, Selma Oliveira, Ann-Christin Räätäri Nyström and Jeanette Saldjoughi for their great administrative support.

My peers played an important part in this experience. Sied Hassen, Marcela Jaime, Reda Moursli, Diem Van Nguyen, Emil Persson, Remidius Ruhinduka, Joakim Ruist, Anja Tolonen, Hang Yin and Xiao-Bing Zhang - thank you all for your friendship! I will always cherish the memory of you showing up for my surprise $25^{th}$ birthday! A very special thanks goes to Oana Borcan, one of my dearest friends ever. We shared the joys and sorrows of this PhD, but also recipes, the latest news from home, memories and hopes. Thank you for being there for me!

Luckily for me, as I have never been much of an "workaholic", I encountered many great people that became our very good friends. Lavinia, Costel, Vlad, Elena, Cri, Cesar, Diana, Ruxi, Alin, Irina, Mihai B., Mihai C., Daniel, Dani, Claudia, Laurentiu, Thomas, Ioannis, Vasiliki, Iosif and Maria, thanks to you I have so many nice memories when I look back at all these years!

I dedicate this thesis to my family. My mother, father and brother have always been close to my heart. This thesis meant that we missed birthdays and other important moments in each other's lives, but their love shone through and the distance faded away. The hundreds of hours of Skype calls were of immense help, and I cannot express my gratitude for their constant support. Vă mulţumesc! Above all, I owe what I've accomplished to Valentin, my husband-to-be. He has been my greatest support and his unconditional love made me feel I could never fail. Nearly six years ago we were two kids who packed their bags in a car and drove off together. We didn't know where our journey would lead, we still don't, but I am sure that by each other's side, the road ahead is bright and we will make the best of all that is to come. Îţi mulţumesc pentru tot, te iubesc!

Simona Bejenariu
Göteborg, April 2015

# Contents

**Acknowledgments** **iii**

**1 Introduction** **1**

1.1 Early childhood and human capital formation . . . . . . . . . . . 1
1.2 Romanian context . . . . . . . . . . . . . . . . . . . . . . . . . 3
1.3 Summary of the thesis . . . . . . . . . . . . . . . . . . . . . . . 4
Bibliography . . . . . . . . . . . . . . . . . . . . . . . . . . . . 6

**2 Austerity Measures and Infant Health. Lessons from an Unexpected Wage Cut** **11**

2.1 Introduction . . . . . . . . . . . . . . . . . . . . . . . . . . . . 12
2.2 Background and data . . . . . . . . . . . . . . . . . . . . . . . 15
2.3 Identification and main results . . . . . . . . . . . . . . . . . . 22
2.4 Potential mechanisms . . . . . . . . . . . . . . . . . . . . . . . 28
2.5 Further evidence of selection in utero . . . . . . . . . . . . . . . 33
2.6 Further sensitivity checks . . . . . . . . . . . . . . . . . . . . . 36
2.7 Conclusions . . . . . . . . . . . . . . . . . . . . . . . . . . . . 38
Bibliography . . . . . . . . . . . . . . . . . . . . . . . . . . . . 39

**3 Bridging the Gap for Roma Women. The Effects of a Health Mediation Program on Roma Prenatal Care and Child Health at Birth** **71**

3.1 Introduction . . . . . . . . . . . . . . . . . . . . . . . . . . . . 72
3.2 The Roma Health Mediators Program . . . . . . . . . . . . . . . 76
3.3 Data and methodology . . . . . . . . . . . . . . . . . . . . . . 78
3.4 Results . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 84

3.5 Robustness tests . . . . . . . . . . . . . . . . . . . . . . . . . 89
3.6 Further evidence using survey data . . . . . . . . . . . . . . . . . . 92
3.7 Potential Mechanisms and Discussion . . . . . . . . . . . . . . . 94
3.8 Conclusions . . . . . . . . . . . . . . . . . . . . . . . . . . . 97
Bibliography . . . . . . . . . . . . . . . . . . . . . . . . . . . 99

**4 The Effects of Financial Incentives on Fertility and Early Investments in Child Health 131**
4.1 Introduction . . . . . . . . . . . . . . . . . . . . . . . . . . . 132
4.2 Background . . . . . . . . . . . . . . . . . . . . . . . . . . . 135
4.3 Data . . . . . . . . . . . . . . . . . . . . . . . . . . . . . . 147
4.4 Identification strategy . . . . . . . . . . . . . . . . . . . . . . 153
4.5 Main results . . . . . . . . . . . . . . . . . . . . . . . . . . . 157
4.6 Robustness checks . . . . . . . . . . . . . . . . . . . . . . . . 164
4.7 Heterogeneity . . . . . . . . . . . . . . . . . . . . . . . . . . 171
4.8 Discussion and conclusions . . . . . . . . . . . . . . . . . . . . 172
Bibliography . . . . . . . . . . . . . . . . . . . . . . . . . . . 175

# Chapter 1

# Introduction

## 1.1 Early childhood and human capital formation

*"Childhood shows the man as morning shows the day"* wrote John Milton over three centuries ago. Indeed, mounting evidence in economics and other domains suggests that early childhood environment can have persistent impacts on numerous later life outcomes, primarily mediated through the accumulation of human capital ( [Currie and Almond, 2011]). One of the most salient components of this environment is health status during infancy and childhood that plays a crucial role in the formation of human capital and explains a significant share of the variation in several important non-health adult outcomes. Birth weight, a widely used measure of the child's endowment at birth, is a strong predictor not only of health status in adulthood, but also educational attainment, labor market participation and income, whereas low birth weight, the condition of being born with a weight below 2,500 grams, has significant negative long-term effects on the aforementioned outcomes ( [Currie and Hyson, 1999, Black et al., 2007]). Moreover, there is conclusive evidence of a strong correlation between the birth weight of mothers and the birth weight of their children, particularly for poorer women, and this plays a significant role in the intergenerational transmission of income and socio-economic status ( [Currie and Moretti, 2007, Currie, 2009]).

But early life circumstances also refer to events that occurred prior to birth, in the prenatal period. In medicine, the fetal origins hypothesis, put forward by Barker ( [Barker, 1990]), postulates that numerous adult diseases have their origins in the prenatal period, when the fetus adapts to the environmental cues and to the predicted postnatal environment. This hypothesis has since been explored in economics, and it was shown that in utero conditions, and especially adverse fetal shocks, can have significant deleterious effects both in the short run as well as in the long run on human capital measures that also extend beyond health outcomes.

As such, birth weight has been found to be negatively affected by nutritional deprivation during pregnancy caused by, for example, Ramadan observance ( [Almond and Mazumder, 2011]) or financial hardship in economic downturns ( [Bozzoli and Quintana-Domeque, 2014] ). Maternal exposure during pregnancy to psychological stress, such as that induced by armed conflict ( [Camacho, 2008]) or distressing economic news ( [Carlson, 2015]), also lowers birth weight, and similar effects have been shown as a consequence of prenatal exposure to environmental pollutants ( [Currie and Schmieder, 2009]). However, these deleterious effects at birth are conditional on the child being carried to term. Significant fetal insults can have such a large negative effect that the least fit fetuses are miscarried, and the culling mechanism leads to better average health status in the surviving cohort. This selection in utero phenomenon has been documented in response severely stressful events, such as civil conflicts ( [Valente, 2015]). However, the effects of fetal insults may remain latent until adulthood and affect non-health outcomes. For example, [Almond et al., 2009] finds that exposure while in utero to the radioactive fallout from Chernobyl led to worse performance in secondary school, but did not affect heath status. Very importantly, it seems that that early life shocks occur more frequently and have larger impacts on children from poorer families ( [Case et al., 2002]).

This body of literature that shows that early life environment, as the sum of pre and post birth influences, has a large effect on the accumulation of human capital has then important policy implications. Policies and remediation programs could be designed to reduce the incidence of shocks or adverse effects that impact child health, either before or after birth. Reducing inequalities in child health in general, and in health at birth in particular, could be an effective means of increasing the equality of opportunity and improving adult outcomes and breaking the intergenerational transmission of poor outcomes. It is thus important to understand in greater depth the determinants of early child health, and how social and economic policies directly or indirectly affect this essential part of the human capital stock. The work in this thesis contributes to this strand of the literature by investigating further the determinants of early child health and the effects on health at birth of specific policies and programs, in the Romanian context.

Early life environment is also implicitly related to the fertility decisions of parents, and the parental investment response to the previously discusses shocks. The seminal work of Becker ( [Becker, 1960]) outlines the trade-off between quantity and quality of children and analyzes how the demand for fertility is affected by the cost of children, and the relationship between income and fertility. Parental investments during early childhood may compensate or reinforce the effects of early shocks; [Hsin, 2012] finds using a siblings sample that less educated mothers devote more total time to heavier birth weight children, whereas better educated mothers devote more total time to lower birth weight children, with the compensating effects being much larger than the reinforcing effects. The last chapter of this thesis investigates the role of financial incentives on fertility, reproductive behavior and early investments in child health by exploiting a major change in the maternity leave benefits policy in Romania.

## 1.2 Romanian context

Romania, European Union's seventh largest member state by population size but second to last by GDP per capita, provides excellent opportunities to study the determinants of early life environment and fertility. After several decades under a communist regime that enforced drastic pro-natalist measures, which included an abortion ban and penalties for childless couples, the country transitioned to democracy and liberalized fertility choices, by re-legalizing abortion and the use of fertility control methods. The regime shift led to a very large drop in the total fertility rate, from 2.30 in the late 1980s to 1.30 just a decade later, and the highest abortion rates in Europe. These spiked in 1990 to a staggering 3152 abortions per 1000 live births in 1990 and then steadily decreased to roughly 1000 abortions per 1000 births in 2000 and halved again by 2010, but remained double relative to the European Union average. Family policies were updated to accommodate the new market economy, and suffered several substantial changes over the last decades. While the scope of benefits for families increased up to 2010, Romania experienced decreasing fertility rates and negative population growth.

In terms of child health at birth, although significant progress has been registered, Romania still registers high low birth weight rates and infant death rates relative to the European and US averages. The average low birth weight rate in Romania over the last two decades has been 9%, compared to an EU average of 6.5%. For infant mortality, the rate per 1000 live births decreased steadily from over 20 in the early 1990s to little over 10 in 2010, whereas the average rate in the other European Union countries rate decreased over the same period from roughly 10 to 4. These statistics indicate that there is a large scope both for direct and indirect interventions that target child health at birth and other components of the early live environment.

Despite the increasing checks and balances that are being placed on the political system, the Government, which holds the executive power, frequently intervenes in the legislative process by de facto introducing new laws or changing the content of already active ones through Emergency Ordinances. As such, over the last decades the major laws governing health, education or social assistance changed radically and suddenly several times, without parliamentary consensus or prior consultations with the civil society. These provide excellent sources of quasi-natural experiments induced by unexpected policy changes, that are decided upon and implemented over very short periods of time, which most often do not provide the individuals the opportunity to adjust their behavior prior to the change.

## 1.3 Summary of the thesis

This thesis contains three empirical papers that explore the socio-economic determinants of early child health, using quasi-natural experiments induced by policy changes. The first paper investigates the effects of income shocks during pregnancy on the health at birth of children in utero at the time of the shock and finds evidence that the selection effects of economic shocks can be larger than the scarring effects. The second paper evaluates the effects of a public health program targeted at a very disadvantaged ethnic minority that provided information to increase the health status of women and children; we show that information provision increases the take-up of prenatal care, but may be insufficient to improve children's health outcomes at birth. The third paper assesses the effects of financial incentives, in the form of maternity leave benefits, on fertility behaviors and early investments in child heath. We find effects in line with the Becker model of fertility ( [Becker, 1991]) and the trade-off between quantity and quality of children.

**Paper I**, "*Austerity Measures and Infant Health. Lessons from an Unexpected Wage Cut Policy*" (with Andreea Mitrut), analyzes the effects of an exogenous income shock during pregnancy on health at birth of children in utero. We use the quasi-experimental setting created by a major (25%) and unexpected wage cut austerity measure that affected all public sector employees in Romania in 2010. We use all registered births in Romania over the period 2007-2010 in a double difference design, where we use out of the labor force mothers as the control group. Our main findings indicate an overall improvement in health at birth, measured as the probability of low birth weight, for boys exposed to the shock in early gestation, whereas there are no effects on girls of any gestational age. Additionally, we find a decreased sex ratio at birth among early exposed children. These findings are consistent with the selection in utero theory hypothesizing that maternal exposure to a significant shock early in gestation preponderantly selects against frail male fetuses, with healthier survivors being carried to term. This is the first economic study to find evidence consistent with selection in utero induced by economic shocks.

In **Paper II**, "*Bridging the Gap for Roma Women: The Effects of a Health Mediation Program on Roma Prenatal Care and Child Health*" ( with Andreea Mitrut), we investigate the effects of a large public health program, targeted at a highly disadvantaged ethnic group, on maternal and child health. Roma, Europe's largest minority, face poverty, social exclusion and life-long inequalities, despite the intensified efforts to alleviate their plight. The Roma Health Mediation program was designed as a large-scale public health program, aiming to improve the health status of pregnant Roma women and their children with the help of trained Roma health mediators from the local community. Mostly through home visits, the mediators provided information and basic health education, and facilitated the communication between Roma ethnics and medical practitioners. Using unique register data from Romania, we exploit the spatial and temporal variation in the implementation dates of the program at the locality level to identify the causal effects of the Roma Health Mediation program on prenatal care take-up rates and child health at birth for Roma ethnics. We find that the program had a very large impact on the take-up rates of prenatal care services, which increase with time since implementation. Despite the large improvements in prenatal care take-up rates, we find no changes in the health outcomes at birth of Roma children, in line with previous literature on the limited effect of prenatal care in non-problematic pregnancies on health at birth. However, we do find evidence of a decreased number of stillbirths and infant deaths at the locality level after the program implementation, but due to data limitations, it is unknown whether these were Roma ethnics.

**Paper III**, "*The Effects of Financial Incentives on Fertility and Early Investments in Child Health*" identifies the impact of financial incentives on fertility behavior and early child outcomes using an unexpected change in the way maternity leave benefits were awarded. The change entailed the switch from proportional (equal to 85% of the mother's pre-birth earnings) to fixed benefits, with the level of the fixed benefits larger than the wage income of most employed women. Using data from the Romanian Reproductive Health Survey collected one and a half years after the policy change announcement, I explore the entire spectrum of individual level decisions related to fertility: decision to conceive, decision to carry the pregnancy to term, and several important outcomes conditional on live birth (maternal behavior during pregnancy, child health at birth and early investments in child health). I employ a double difference identification design in which employed women are the treatment group and out of the labor force women are the control group. Although marginally insignificant, the main findings suggest that the substantial increase in the financial incentives led to an increase in conception rates and a decrease in the probability of abortion, especially for women from poorer households, who benefited more from the policy change. Employed mothers who benefited from the change appear to have worse prenatal behaviors but have children with better health outcomes at birth. Employed mothers who were disadvantaged by the change make more investments in child health.

# Bibliography

[Almond et al., 2009] Almond, D., Edlund, L., and Palme, M. (2009). Chernobyl's subclinical legacy: Prenatal exposure to radioactive fallout and school outcomes in Sweden. *The Quarterly Journal of Economics*, 124(4):1729–1772.

[Almond and Mazumder, 2011] Almond, D. and Mazumder, B. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. *American Economic Journal-Applied Economics*, 3(4):56.

[Barker, 1990] Barker, D. J. (1990). The fetal and infant origins of adult disease. *BMJ: British Medical Journal*, 301(6761):1111.

[Becker, 1960] Becker, G. S. (1960). An economic analysis of fertility. In *Demographic and economic change in developed countries*, pages 209–240. Columbia University Press.

[Becker, 1991] Becker, G. S. (1991). *A treatise on the family*. Harvard University Press, Cambridge, Mass.

[Black et al., 2007] Black, S. E., Devereux, P. J., and Salvanes, K. G. (2007). From the cradle to the labor market? The effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, 122(1):409–439.

[Bozzoli and Quintana-Domeque, 2014] Bozzoli, C. and Quintana-Domeque, C. (2014). The weight of the crisis: Evidence from newborns in Argentina. *Review of Economics and Statistics*, 96(3):550–562.

[Camacho, 2008] Camacho, A. (2008). Stress and birth weight: evidence from terrorist attacks. *The American Economic Review*, pages 511–515.

[Carlson, 2015] Carlson, K. (2015). Fear itself: The effects of distressing economic news on birth outcomes. *Journal of Health Economics*, 41(0):117 – 132.

[Case et al., 2002] Case, A., Lubotsky, D., and Paxson, C. (2002). Economic status and health in childhood: The origins of the gradient. *American Economic Review*, 92(5):1308–1334.

[Currie, 2009] Currie, J. (2009). Healthy, wealthy, and wise: Socioeconomic status, poor health in childhood, and human capital development. *Journal of Economic Literature*, 47(1):87–122.

[Currie and Almond, 2011] Currie, J. and Almond, D. (2011). Chapter 15 — Human capital development before age five. volume 4, Part B of *Handbook of Labor Economics*, pages 1315 – 1486. Elsevier.

[Currie and Hyson, 1999] Currie, J. and Hyson, R. (1999). Is the impact of health shocks cushioned by socioeconomic status? The case of low birthweight. *American Economic Review*, 89(2):245–250.

[Currie and Moretti, 2007] Currie, J. and Moretti, E. (2007). Biology as destiny? Short and long run determinants of intergenerational transmission of birth weight. *Journal of Labor Economics*, 25(2):pp. 231–264.

[Currie and Schmieder, 2009] Currie, J. and Schmieder, J. F. (2009). Fetal exposures to toxic releases and infant health. *American Economic Review*, 99(2):177–83.

[Hsin, 2012] Hsin, A. (2012). Is biology destiny? Birth weight and differential parental treatment. *Demography*, 49(4):1385–1405.

[Valente, 2015] Valente, C. (2015). Civil conflict, gender-specific fetal loss, and selection: A new test of the Trivers-Willard hypothesis. *Journal of Health Economics*, 39(0):31–50.

# Paper I

# Chapter 2

## Austerity Measures and Infant Health. Lessons from an Unexpected Wage Cut

Simona Bejenariu* Andreea Mitrut†

**Abstract**
We investigate the effects on the health at birth of children exposed in utero to a major (25%) and unexpected wage cut austerity measure that affected all public sector employees in Romania in 2010. Our findings suggest an overall improvement in health at birth for boys exposed to the shock in early gestation and a decreased sex ratio at birth among early exposed children. These findings seem consistent with the selection in utero theory hypothesizing that maternal exposure to a significant shock early in gestation preponderantly selects against frail male fetuses.

**JEL classification codes:** I19, J13, J38, I38
**Keywords:** austerity; fetal shock; health at birth; selection in utero; Romania

---

*Department of Economics, University of Gothenburg. simona.bejenariu@economics.gu.se
†Department of Economics, University of Gothenburg. andreea.mitrut@economics.gu.se; Corresponding author. Mailing address: Department of Economics, University of Gothenburg, Box 640, SE 40530, Sweden. Mitrut gratefully acknowledges support from Jan Wallanders and Tom Hedelius Fond. We thank Marcus Eliason, Lennart Flood, Mikael Lindahl, Peter Nilsson, Katarina Nordblom, Henry Ohlsson, Ola Olsson, Olof Johansson-Stenman, Climent Quintana-Domeque, Matthias Sutter, Måns Söderbom, and participants at various workshops and seminars.

## 2.1 Introduction

(How) Are the unborn children affected by austerity measures? While children in utero are not the intended target of the recent austerity programs, they may be negatively affected as governments across many countries take action to eliminate unsustainable budget deficits. Within the framework of the fetal origin hypothesis put forward by [Barker, 1990], recent evidence shows that, indeed, disruptions in prenatal conditions, caused by fetal shocks,[1] have scarring, life-long consequences (see [Almond and Currie, 2011, Almond and Mazumder, 2011]). While prior work has found that extreme events can substantially affect fetal health,[2] little is still known about the effects of shocks induced by economic phenomena. Understanding whether and 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. Public sector wages were frozen in numerous European countries, while others implemented wage cut policies.[3] In this paper we exploit the most drastic wage cut austerity measure implemented in Europe, entailing a 25% cut in wages and in all the additional benefits for all public sector employees in Romania starting July 1st, 2010. This led to a drop of 60.1 percentage points 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, in general, quite difficult to disentangle as their timing is usually diffuse, lacking a precise onset date, and they 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-

[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, as 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]) and pandemics ( [Almond, 2006]).

[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). We discuss in Section 2 that the wage cut austerity policy was most likely not anticipated in Romania.

[4] The public sector wage premium fell from +44.5% in 2009 to -15.6% in 2010 (a loss of 60.1 percentage points) (source: Industrial Relations in Europe 2012 Report, European Commission).

damaging goods and the decrease of the opportunity cost of health-improving behaviour may offset the negative effects and lead to better infant heath 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 of economic shocks on health at birth, depending on a wide array of factors. Thus, some studies find evidence of deteriorating health outcomes at birth ( [Bozzoli and Quintana-Domeque, 2013, Paxson and Schady, 2005, Burlando, 2010, Lindo, 2011]), whereas others find that the effects of improvements in risk-related behavior during pregnancy and maternal selection prevail over the scarring effects, the net result being an improvement of the health of in utero exposed children ( [Dehejia and Lleras-Muney, 2004]). Additionally, the sex-ratio at birth has also been found to respond to economic circumstances ( [Catalano and Bruckner, 2005, Catalano, 2003, Catalano et al., 2009]). [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 during the third trimester of pregnancy via the nutritional deprivations channel and during the first trimester of pregnancy via the maternal stress channel. [Almond et al., 2011] look at the effect of the Food Stamps Program in the US as a positive shock in utero and find improvements in health outcomes at birth.

However, these effects observed at birth and/or later on in life are, in fact, conditioned on the fetus surviving the pre-birth period. Medical literature finds that significant prenatal maternal stress, especially during early gestation, may induce a selective mortality of the least fit fetuses through increased miscarriages. This process, known as selection in utero, may yield a positive selection of those that are carried to term, visible in an improvement in the health outcomes of the affected cohort, with weak male fetuses significantly more affected than female fetuses ( [Hobel et al., 1999, Catalano et al., 2009]).

This paper contributes to the literature on the impacts of (negative) economic shocks on the health outcomes at birth by exploring a unique austerity measure, unexpected in its magnitude (25% cut in wages and in all benefits) and timing (to start with July $1^{st}$, 2010, after being firstly announced on May $7^{th}$, 2010). The distinct occurrence of the shock eliminates the problem posed by diffuse timing or endogenous income reductions and allows us to pursue a clean identification strategy to infer the causal effects of an income shock on birth outcomes of children exposed in utero. Our findings indicate that maternal exposure to economic insecurity and income loss may lead to what appears to be selection in utero. While some evidence of selection in utero induced by economic fluctuations is provided in epidemiology and demography (e.g., [Catalano et al., 2010], [Catalano, 2003]), this is the first study to show consistent evidence that unexpected austerity measures (entailing large income cuts) experienced during early pregnancy may lead to fewer, but apparently healthier boys at birth.[5]

Our main empirical strategy to assess the impact of the unexpected income shock on health outcomes at birth is a difference-in-difference (DD) specification. We use the Romanian Vital Statistics Natality files containing the universe of births for the period 2007-2010 and compare outcomes at birth for children in utero at the time of the policy announcement belongings to mothers employed in the public sector and housewife (or alternatively, privately employed) mothers in 2010, relative to earlier years. We will focus on women 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. Unfortunately, we are not able to clearly disentangle between the impact of announcement per se and the wage cut two months after.

Our main findings suggest an overall improvement in health at birth as measured by a 1.4 percentage point (pp) decrease (13% of the mean) in the probability of low birth weight of children exposed to the shock during their $1^{st}$ trimester of gestation. We find significant improvements in health at birth exclusively for boys and not for girls, driven by significant effects for those who have been exposed to the shock starting with very early developmental stages ($1^{st}$ trimester), a decrease of 2.9 pp (29% of the mean) in the probability of low birth weight. This effect is particularly large for boys belonging to highly educated mothers. We also find indications of a decreased sex-ratio at birth of about 3.3 percentage points (6.7% of the mean) for the same sub-sample of children. Our results hold to a wide series of falsification and robustness tests, including a mother's fixed effects specification.

Using complimentary datasets, we investigate the potential mechanisms through which the austerity measures affected health at birth and find evidence which seem to indicate that selection in utero due to maternal prenatal exposure to the policy shock resulted in a healthier but smaller cohort of boys. Yet, we cannot fully un-

[5]Within economics, [Valente, 2015] documents selection in utero following a civil conflict in Nepal, while [Nilsson, 2014] finds that boys exposed early in utero to an increase in the availability of alcohol in Sweden are more likely to be spontaneously aborted.

derstand whether the fetal stressors are related directly to stress per se (through increase in cortisol level) and/or indirectly, through higher intake of alcohol or smoking consumption. Overall, our findings are consistent with the medical literature that has established that weaker males are more vulnerable to adverse conditions in utero and that maternal prenatal stressors raises the fitness criterion of children in utero. The remainder of the paper is organised as follows: Section 2.2 depicts the Romanian context in which the policy change occurred, and presents the data we are using. Section 2.3 describes the empirical strategy, and presents the main results, followed by several sensitivity checks. In Section 2.4 we discuss the potential mechanisms through which an income shock may affect birth outcomes and further test these mechanisms in Section 2.5. Section 2.6 contains a series of further robustness checks that support our main results. Section 2.7 concludes.

## 2.2 Background and data

### 2.2.1 The 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 in Romania: politicians invoked a decoupling of the Romanian economy from the world markets, and the public opinion was moderate in its expectations: the autumn 2008 Euro-barometer showed that more than half of respondents anticipated no change or even an improvement in the general economic situation of Romania, with the same attitude prevailing in the 2009 waves of the survey.[7]

The first political signs of the recognition 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. Moreover, the political class transmitted an overall confident message in the lead-up period to the presidential elections of

[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 annual growth rate reached a peak of almost 9%, the annual inflation rate was still above 10%.

[7] http://ec.europa.eu/public_opinion/cf/

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 the 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, involving 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, maternity leave benefits and several other social security benefits, were aimed at re-establishing the budgetary balance agreed to with the IMF. Thus, 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 monthly income drop due to the wage and benefits cut; a decrease in the annual average wage income which would lead to a lower (forthcoming) 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 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 happens. 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

[8] `http://goo.gl/sMcVEV` (in Romanian). 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. As such, on March 16th, 2010, the Prime Minister presented in front of the Parliament the anti-crisis measures that were being implemented, all as economic stimulus, aimed at improving the business environment and reducing tax evasion.

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

law making procedures.[10] On June 30th, the President promulgated the laws, which came in effect July 1st, with an initial duration of 6 months, but in fact in January 2011 public sector wages were not restored to their initial level.[11]

Overall, it is safe to assume that the austerity measures were not anticipated, in both their unprecedented scope and magnitude, or their timing. In our empirical strategy we will focus on women working in the public sector, 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. Even though the austerity measures were unanticipated, we cannot exclude "written on the wall".[12] The possible selections into fertility will be addressed later in the paper.

The public sector wage cuts affect females significantly more than men due to the structure of the public sector employment.[13]. In Romania, the publicly employed women are concentrated in 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.[14] 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 ranked high, in particular, making the publicly employed women the most affected by the wage cut, both in monetary and psychological distress terms (see [Vîrgă et al., 2012]).

[10]The Romanian Constitution allows, as an exception, that 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]It is important to distinguish between a permanent and a temporary wage cut: transitory changes in wages have no effect on lifetime income or on total fertility though they may affect the timing of fertility, while a permanent wage cut has an ambiguous effect as it may decrease the relative cost of children which, in turn, may increase the demand for children or, because of a lower income, it may decrease the demand for children; [Becker, 1965, Heckman and Walker, 1990]. Even if temporary, households might respond as though these changes are permanent if people are myopic or uncertain about the nature of the changes ( [Dehejia and Lleras-Muney, 2004]). This was most likely the case in Romania, with most households perceiving the wage cut as permanent, because of numerous inconsistent enforcement of laws.

[12]At that time Romania experienced an increase in the unemployment rates in the private sector, that rose from a relatively stable level of 4 to 5% before 2009 to 8% in March 2010.

[13]Source: Industrial Relations in Europe 2010 Report, European Commission

[14]Source: Statistics Romania.

### 2.2.2 The impact of the austerity measures at household level

To understand the size of the impact of the austerity, we use the Romanian Household Budget Survey (RHBS), the main tool of assessing population expenditures and revenues, covering about 30,000 households/year and containing detailed income and expenditure information. We compare here households with at least one publicly employed member and households with no publicly employed members, just before (January-July 2010) and after (August-December 2010) the austerity measures implementation. The results in Table 2.1 indicate a significant decrease in household wage related income of 16.7% and in total household income of about 7% for households with at least one publicly employed member.[15] Not surprisingly, the wage related income drop is larger for high-educated households (about 21.7% in column 2) because the high-educated publicly employed members were more likely to attract more wage related income (through bonuses, in-kinds wage related transfers) which were also annulled. Overall, the households affected by the shock seem to have no significant changes in food-related (column 4) or alcohol and cigarettes (column 5) expenditures, but they have significantly reduce non-food (column 6) and services expenditures (column 7). Finally, column (8) seem to indicate that households react to the wage shock by decreasing the (formal) savings with about 11.9%.

### 2.2.3 Data and working sample

In our main empirical exercise we use the Vital Statistics Natality (VSN) records for years 2007 through to 2010,[16] as our main dataset. The VSN records cover the universe 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

[15] It is not surprising that the wage drop was not 25% (or higher) as the data provides information at the household level. Also, we show these results only for urban households (see the explanations in the next section).

[16] In 2011, Statistics Romania changed the data registration process for the VSN, and no longer collects information on a wide array of maternal and child characteristics which we use in the current analysis. Also, we cannot use pre-2007 VSN datasets since we do not have access to RHBS datasets prior to 2007, hence we cannot calculate the predicted probability of public employment, as will be discussed shortly.

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.

We restrict our sample to mothers between 16 and 45 years of age that were pregnant on May $7^{th}$ each year and exclude multiple births, which leaves us with a sample of 846,778 births over the period 2007-2010. In the baseline estimations we will focus on children born from mothers living in urban areas, accounting for 465,754 of all births. Given the nature of the policy change, there are reasons to expect that effects would be concentrated among urban rather than rural households. Firstly, among the employed women of fertile age, living in rural areas, only about 8% work in the public sector compared to about 30% of the employed women from urban areas (source: RHBS). Secondly, we suspect that the wage cut policy affected the rural households much less relative to the urban households because in wage income represents less than 20% of the total household income in rural areas, compared to an average of 60% for families living in urban areas ( [Firici and Thomson, 2002]).[17] Even though our empirical analysis discusses urban households, we also show that our main results hold when we look at all households. Summary statistics for our main variables for the urban mothers are found in Table 2.2, column block 1.[18]

A key variable in 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.

Because the policy specifically targeted the public sector employees, we proceed by making use of the RHBS for the 2007-2010 period, which includes the same socio-economic characteristics as the VSN and in addition records the sector of employment. We estimate a conditional probability that an employed woman works in the public (vs. the private) sector and conduct out of sample estimation to assign mothers in the VSN probabilities of public employment (we will come

[17] Agricultural own-production income is estimated as high as 46% for rural households and about 13% for urban households ( [Firici and Thomson, 2002]).

[18] Appendix Table 2.12 in the Appendix A shows the descriptive statistics for the urban and rural sample.

back to this issue in Section 3.2.2. and in Appendix B). Next, to define our treatment group, we make use of information provided by the Romanian Ministry of Labour, Family and Social Protection (MLFSP) regarding the recipients of child care allowance.[19] At the end of 2010, among the employed mothers receiving child care allowance, 20% were working in the public sector and 80% in the private sector.[20] We use this percentile split and classify as publicly employed the employed mothers with the 20% highest predicted probabilities. We will conduct several sensitivity analyses with respect to the choice of the threshold percentile (including estimating a model using the continuous measure of the probability rather than a binary indicator) and the assignment into the treatment group (see Section 3.2.2 and Appendix B).

The main characteristics of the publicly employed mothers as defined by the 20-80 split are shown in Table 2.2, column block 2. Compared to the sample of all employed mothers, shown in column block 3, the publicly employed mothers are, on average, older, more likely to be married and more educated. Reassuringly, this composition matches very well that of the publicly employed women in the RHBS data.[21] Relative to all mothers or to all employed mothers, the publicly employed mothers seem to have healthier children as measured by birth weight and gestation length. In column block 4 we show the main characteristics of the housewives mothers, accounting for about 30% of all mothers in urban area.[22] Housewives mothers are, on average, younger, less likely to be married, lower educated and have children with worse outcomes at birth relative to mothers in column blocks 1 to 3.

At this stage we also check possible anticipatory effects in terms of selection into motherhood of the austerity measures. Overall, from Table 2.2 we observe that

[19]Child care allowance is awarded to either one of the parents who has obtained any form of taxable income in the 12 months preceding the birth of the child. Basically all employed mothers receive this allowance.

[20]MLFSP does not hold centralized information on the number of recipients of child care allowance by the child's month, year and county of birth and mother's sector of employment.

[21]Albeit a small sample, among the 230 mothers (with a child one year old or less between 2007 and 2010) employed in the public sector, 77% have high education, while only 6% have secondary education. Among the employed women in the private sector who have recently become mothers (1,102), only 30% have higher education, 40% have high-school education and 22% have secondary education. This matches very well with the composition we obtain in our treatment group based on the 80-20 split.

[22]For the entire sample including urban and rural women, the occupational structure reveals that 47.8% of women giving birth in 2010 are employed; 42.6%, housewives; 0.15%, business-owners; 1%, self-employed in non-agricultural activities; 0.2%, self-employed in agriculture; 1.8%, unemployed; 0.2%, pensioners; and 6.25%, other situations. This structure is quite stable over the years and the area of residence.

employed mothers who give birth in later years in urban areas seem to be better educated (more likely to have higher education) which may be due to a positive selection into motherhood, but also because a well-recognised trend in education in Romania.[23] The publicly employed mothers, even though are on average more educated compared to the other occupational categories, in 2010 (relative to before) they are less likely to have a higher degree and more likely to only have a post-high school degree, suggesting a negative selection.

To address the issue more formally, in Table 2.3, for each occupational category we run regressions with mothers' observable characteristics as outcomes. Overall, mothers pregnant on May $7^{th}$, 2010, relative to those pregnant before, are more likely to be more educated and slightly older. This is also true for the housewives and particularly for the privately employed mothers. The effects are significant and quite large as a percentage change from the mean. However, publicly employed women pregnant at the time of the announcement seem to be less educated (more have only secondary or high school and fewer have a higher education) and they are less likely to be married. Albeit statistically significant, the changes relative to the mean are not as large as for the privately employed or for the housewives mothers.[24] Overall, our results indicate that, even though the austerity measures were most likely unanticipated, the overall economic context has influenced the fertility timing decision of Romanian women and has altered the composition of mothers becoming pregnant. These findings are in line with other studies (see [Dehejia and Lleras-Muney, 2004] for the US) that show that in turbulent economic times, we may observe an increase fertility of low-skilled women (as measured by education) and a negative selection for the high-skilled ones.[25] It is important to note that using our

[23] See Appendix A, Figure 2.4. The significant increase in the number of higher educated individuals is due to the massive increase in the number of private universities. Figure 2.5 shows that over the 2003-2010 period, while the proportion of employed mothers with primary education is relatively constant across years, there is an increase in the employed mothers with higher education matching the decrease of the employed mothers with secondary education.

[24] An alternative way to analyse the selection into fertility issue is to estimate the baseline difference in difference regressions comparing the characteristics of the publicly employed mothers with those of the housewife mothers, pregnant at the time of the austerity measures announcement relative to the same period in previous years. In accordance with the previous findings, we find that relative to housewife mothers, publicly employed mothers from urban areas are less educated (lower probability to have higher degree and higher probability to have secondary education), younger, less likely to be married and have an unemployed husband. The results are presented in Appendix Table 2.13.

[25] The net effect of an economic shock is theoretically ambiguous and hinges upon the mother's skill depreciation rate and on whether capital markets are perfect ( [Dehejia and Lleras-Muney, 2004]). One may hypothesize that low-skilled women are less likely to have a human capital that depreciates during a temporary absence from a job during pregnancy and after birth (and assuming that capital markets are

difference-in-difference identification strategy, a negative selection in the treatment group and a small positive selection in the control group would bias the results towards zero and thus any significant results would not be driven by this selection.

## 2.3 Identification and main results

### 2.3.1 Identification strategy

To test whether the austerity measures changed the outcomes at birth of the children in utero at the time of the announcement (May $7^{th}$, 2010) relative to children conceived in earlier years, we rely on a difference-in-difference (DD) specification. Our treatment group consists of pregnant women classified as working in the public sector while our control group consists of pregnant housewives. Thus, we compare outcomes at birth between children in utero on May $7^{th}$, 2010, and May $7^{th}$ of the previous years (2007-2009), with mothers working in the public sector and housewives. Housewife mothers is our preferred control group as they are least likely to have been affected by the austerity measures: they are out of the labour force and they do not receive any social assistance benefits (such as unemployment or maternity leave benefits).[26] Moreover, they are the second most numerous group by mothers' occupational status, after employed mothers. We acknowledge that housewives may not be an ideal control group and therefore we will also consider the mothers classified as privately employed as an alternative control group. They are not our preferred control group because they are also defined based on the 20-80 split; moreover, we have also shown in the previous section a substantial (positive) change in the composition of privately employed women who become pregnant in 2010 which, most likely, will bias our results towards zero.

We measure health at birth using the low birth weight indicator, defined as a birth weight less than 2,500 grams.[27] Our baseline specification, estimated using

perfect); if so, then in low-wage periods, we may observe an increase fertility of low-skilled women.

[26]Housewife is defined as a person engaged in domestic work such as preparing food, maintenance and home care, domestic industry activities not intended for sale, care and education of children and who does not receive a formal income.

[27]Using birth weight as a continuous outcome provides fairly similar results. We focus on low birth weight since it is a more accurate measure of neonatal health and a better predictor for infant health, being the leading cause of neonatal and infant mortality ( [Stein et al., 2006]).

ordinary least squares, is the following:

$$Outcome_{imrt} = \alpha + \beta_1 Public_i + \beta_2 Public_i * Utero2010_i + \\ \eta_t + \gamma_1 X_i + \theta_r + \theta_r t + \delta_m + \sigma_{crt} + \epsilon_{imrt} \quad (2.1)$$

where $i$ indexes a child born in month $m$ by a mother living in county $r$ in year $t$; $Public_i$ is an indicator that equals 1 if the mother of child $i$ works in the public sector and 0 if she is a housewife (or works in the private sector in an alternative specification). Our key coefficient is $\beta_2$, on the interaction between Public and an indicator whether the child was in utero in May $7^{th}$ 2010. This measures the change in outcomes after the 2010 announcement relative to earlier years, among women that work in the public sector relative to housewives. $\eta_t$ are year indicators that equals 1 if child $i$ was in utero on May $7^{th}$ in year $t$; $X_i$ is a vector of control variables for maternal and child characteristics: child's gender, mother's age at birth and its square, mother's education, ethnicity, marital status, child's parity, number of children alive, indicator for prenatal control, gestation month of the first prenatal care visit in the current pregnancy and an indicator for home delivery. Our main specifications also include the father's age and its square together with indicators for his employment status (whether employed, entrepreneur, self-employed in agricultural activities, self-employed in non-agricultural activities, unemployed, retiree or other situations) at the time of the child birth.[28] $\theta_r$ are 42 county indicators, while $\theta_r t$ are linear county specific trends; $\delta_m$ are months of birth indicators; with $\sigma_{crt}$, we control for the female unemployment rate in the month of conception for each county and year of conception.[29] We cluster the standard errors at the county level (42 clusters), even though we get very similar standard errors without clustering.

The key identification assumption in a DD framework is that, absent the policy change, we would not observe any difference in our outcomes between publicly employed mothers and housewives in 2010 relative to earlier years (the parallel

[28]Information for the fathers is available regardless of the mother's marital status. However, it is missing for about 23 percent of the unmarried mothers. For this sample, albeit very small, we have imputed the missing information with the relevant locality average. Our results are not sensitive to including or not this sample.

[29]The VSN does not include information on mothers drinking or smoking habits. Including controls for the average expenditures on cigarettes and alcohol, at the county level, for each year and gestational month c from conception to birth does not change our results. Same if we include the average consumption expenditures on food at the county level for each gestational month from conception to birth. Results available.

trend assumption). To examine the plausibility of this assumption we add two interaction terms to the baseline model: the Public indicator interacted with year indicators Utero2008 and Utero2009.[30]

Because the literature suggests that the effects of in utero shocks may vary according to the stages of gestation, we will explore the fact that at the time of the shock children were in different gestational stages. The VSN data contains the gestational age in number of weeks at birth and we are able to infer the gestational age at the date of the austerity announcement.[31] Using this information, we split our sample into the following categories according to their gestational age at May $7^{th}$, the time of the policy announcement: 1) children in the $1^{st}$ trimester (up to 12 weeks), who were exposed the longest to the policy: to the announcement shock in early pregnancy and to diminished income later in gestation;[32] (2) children in the $2^{nd}$ trimester (13-24 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 (more than 25 weeks), exposed only to the announcement shock in late gestation. It is important to clarify that in our experiment the de-facto wage cut occurred in early August 2010, when public employees received the wages for July 2010. Hence in the first three months following the announcement in utero children were not exposed to reduced income but possibly to stress related factors. Due to insufficient variation of policy exposure by gestational age, we are not able to clearly disentangle between the effect of the austerity announcement per se and that of reduced income. We further discuss this issue in the next sections. Finally, because medical research established that effects of in utero conditions may depend on the gender of the fetus, we will also show our results separately for boys and girls.

[30]A graphical illustration of the trends in the outcome of interest is presented in Appendix A, Figure 2.6.

[31]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.

[32]Because we cannot use the 2011 VSN, our $1^{st}$ trimester sample includes only children born in 2010. However, for comparability, we do the same restriction for all years and hence this sample is artificially smaller for all years we use.

### 2.3.2 Results

#### Main estimates

This section presents the baseline results from Equation 2.1 for the low birth weight indicator. Table 2.4 shows the results for the urban households from the DD estimation for all (Panel A) and separately for boys (Panel B) and girls (Panel C).[33] Each three columns of each panel shows the results for children who were in their $1^{st}$ trimester, $2^{nd}$ and $3^{rd}$ trimester of gestation at the moment of the austerity shock. For each trimester, we first show the interaction term Public*Utero2010 controlling only for year and county indicators, and county specific trends;[34] next we add the individual level controls; finally, we show the estimated coefficients from the fully interacted model, conditional on pre-treatment dynamics.

Panel A shows that the austerity measures affected only children in their $1^{st}$ trimester of gestation. The impact of the shock in columns (1)-(2) is negative and significant suggesting an reduction of the low birth weight incidence by 2 pp, hence an improved average health. The magnitude becomes 1.4 pp in column (3), after we control for pre-treatment dynamics (13$\%$ of the mean). This may be surprising as these children were exposed to the shock in utero the longest, starting with the very early developmental stages. The estimates for the 2009 and 2008 year-specific public indicators are positive and not statistically significant suggesting that children born from the publicly employed and housewives mothers do not differ significantly in their evolution of the low birth weight outcome during the pre-treatment years, thus supporting the parallel trend assumption. Moreover, since we employ the same procedure to classify publicly employed mothers in all years, the significant coefficient for the 2010 interaction cannot be a mechanical result of our imputation method. Our results for children in the $2^{nd}$ and $3^{rd}$ trimesters of gestation show a similar pattern, but the magnitude of our main coefficient of interest is smaller and never significant.[35]

The results in Panel B indicate a significant decrease of the probability low birth weight for the sample of boys in utero in the $1^{st}$ trimester on May $7^{th}$, 2010;

[33] Appendix Table 2.14 shows the results we also include rural households. The results are in line with the urban sample, slightly lower in magnitude and significant at a lower level.

[34] Our results are not sensitive to excluding the county specific trends.

[35] The reason why the Public dummy is insignificant is that a very large share of the publicly employed mothers have tertiary education, and so the Public dummy will actually capture the high education dummy. If we exclude the tertiary education among the controls the Public indicator becomes significant and the interaction Public*Utero2010 does not change sign, magnitude or significance.

this effect is stable across specifications, of 3.2 pp in columns (1)-(2) and 2.9 pp in column (3) (29% of the mean). This effect holds even though we have shown in the previous section a negative selection among publicly employed mothers in 2010 (relative to before and also to the other occupational categories), which would render our results as lower bounds effects of the policy. The $2^{nd}$ and $3^{rd}$ trimesters of gestation indicate qualitatively similar results but smaller in magnitude and not significant. Finally, the results for girls in Panel C show no effect on low birth weight.

To gain a better understanding about the effects at different gestational ages at the time of the shock, we use a moving window approach in which we "glide" the treatment over cohorts defined in 12 weeks periods, instead of trimesters, at May $7^{th}$. Figure 2.1 presents the estimated coefficient of interest for each of the 12 weeks intervals, for all, boys and girls, respectively, together with the corresponding standard errors. For the sample of boys, the effects are decreasing in absolute value and remain significant up until the cohort who was 11 to 23 weeks at May $7^{th}$, which indicates that children in early second trimester were also affected. For girls, the only significant impacts, in the same direction as for the boys, are observed for girls who were between 14-26 up to 17-29 weeks. Overall, boys appear significantly more affected, both in intensity and in number of children affected, with the results indicating a significant decrease of the probability of low birth weight.

#### Sensitivity analysis

Before we discuss the possible mechanisms in place, we subject our results to some robustness tests that address three potential issues: corrected standard errors due to the generated regressors; the definition of the treatment group; and the composition of the control group.

**a) Corrected standard errors.** To account for the fact that we define our treatment group based on a generated regressor (i.e. the predicted probability of public employment), we use bootstrapping to estimate the standard errors of the parameters of interest, under the assumption that the OLS estiamtor is consistent (details of how we conducted the bootstrapping procedure are presented in Appendix B4).[36] Table 2.5 presents the coefficient of interest, *Public*Utero2010*, for

[36] [Murphy and Topel, 2002]

the richest specification presented in Table 2.4 column (3), the robust standard errors from our main regression and the bootstrapped standard errors obtained from 500 replications. The bootstrapped standard errors for the coefficient of interest are very close to the robust standard errors that we use in our main specification, leading to the same t statistics and the same significance levels for our estimates, leaving the inference unaffected.

**b) Sensitivity to the definition of the treatment group.** So far, given the limited information provided by the Romanian MLFSP, we have used the 20-80 percentile split of the probabilities of a mother's employment to define our treatment. To check the sensitivity of the effects on the low birth weight indicator with respect to this split, we use different definitions of the treatment group based on varying the threshold percentile from the $80^{th}$ to the $50^{th}$ (i.e., employed mothers with predicted probabilities above the threshold percentile are included in the treatment group). Figure 2.2 confirms that our results, especially for the boys in the $1^{st}$ trimester of gestation at the time of the shock, are not sensitive to different thresholds though and remain negative and significant at 5%, but increasingly biased towards 0 as we increasingly misclassify the treatment group and include more privately employed mothers. We also used the predicted probability from the RHBS as a continuous variable and look at the sample of all employed mothers. Our findings are qualitatively similar and indicate that the mothers with higher predicted probability are less likely to have low birth weight boys, but this effect is not statistically significant. Finally, in Appendix B2 we show some further robustness checks.

**c) The composition of the control group.** As mentioned before, one possible concern is that housewives mothers are not an ideal control group to the employed mothers. We address this issue in several ways.

First, because publicly employed mothers have a high educational level, and that recent evidence seems to indicate that economic shocks on pregnant women may have a different impact according to the mother's SES ( [Bozzoli and Quintana-Domeque, 2013]), we compare only mothers (public and housewives) with high education (high school and above). Our results, presented in Table 2.6, show that the improvement of the low birth weight indicator we uncovered earlier is driven by the boys belonging to highly educated mothers.[37] However, we cannot do the same comparison for low educated mothers because of a low share of low educated mothers in the treated group (see Table 2.2).

Secondly, we use as an alternative control group the privately employed mothers defined as mothers with the predicted probabilities below the $80^{th}$ percentile, while keeping the same definition as in the main specification for the publicly employed mothers. Reassuringly, the results in Table 2.7 have a similar pattern as our main outcomes in Table 2.4, especially for the children in the $1^{st}$ trimester at the time of the shock, but they are smaller magnitude given the (large) positive selection into fertility in the private sector.

## 2.4 Potential mechanisms

In this section we attempt to explain our seemingly counterintuitive results by investigating the potential mechanisms in place. There are three main mechanisms through which an income shock generated by an unexpected cut in a pregnant woman's wage may affect children's outcomes at birth: (1) selection into motherhood, (2) nutrition and prenatal care, and (3) prenatal maternal stress.

### 2.4.1 Selection into fertility and abortions

We try to mitigate some concerns related to changes in the composition of pregnant publicly employed women by using the fact that the Romanian austerity measures were unexpected, and by looking at the sample of already pregnant mothers at the time of the announcement. We have shown in Section 2.2 that some selection into fertility occurred prior to the announcement because of the overall economic situation but, given the nature of the selection, the size and direction of these selections do not invalidate our main results.

Yet, already pregnant women may react to the austerity measures by terminating their pregnancy using abortion. Abortion in Romania is available up to 12 gestational weeks. Although we do not have individual data on abortion procedures,

[37] We have also used a simple matching strategy (nearest neighborhood and 1-to-1 matching, no replacement) based on pre-treatment characteristics. The effect on the low birth weight indicator is quite similar to our baseline estimates, even though less precisely estimated. Additionally, we used a matched double difference specification in which the housewives constituting the control group were weighted with the inverse of their propensity score; again, the effect on the low birth weight indicator had a similar pattern as our main specification. All results available upon request.

we investigate whether the quarterly aggregate number of abortions increased significantly after the wage cut announcement.[38] Reassuringly, we find no significant increase in the total number of abortions, but we must acknowledge that the abortion data is not available by women's employment status.

Because our main findings concern only boys, one may worry that sex selective abortion could potentially alter our results. While we are not aware of any evidence on gender preferences in Romania, one way to formally address this concern is to examine the pattern of sex-ratio for different child parities over time. In cultures with sex preferences, sex-ratios are usually normal at first parity but may change with parity ( [Almond et al., 2009]). Using the VSN data we find no indication of sex-selection across years or across occupational categories. Finally, our results on low birth weight hold for a parity larger than two. Moreover, in Romania the child's gender is not routinely detected before 18 gestational weeks whereas abortion is permitted until the $12^{th}$ week of gestation, which makes gender-based selective abortion, in most cases, impossible.

### 2.4.2 Nutrition and prenatal care

**Prenatal nutrition.** A reduced disposable income after July 2010 may lower the quantity or the quality of food intake of the mother which, in turn, may lead to an insufficient nutritional supply to the fetus. Such nutritional restrictions may adversely affect the 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]).[39] Importantly, insufficient caloric intake seems to result in a lower birth weight only in late pregnancy, during the $3^{rd}$ trimester ( [Stephenson and Symonds, 2002]); boys seem, on average, more vulnerable to food shortages than girls ( [Eriksson et al., 2010]). [Almond et al., 2011] show that, in the US, pregnancies exposed to the Food Stamp Program three months before birth resulted in an increased birth weight. [Bozzoli and Quintana-Domeque, 2013] find worsening health outcomes at birth for children exposed in

[38]We use data from the Ministry of Health and estimate a panel fixed effects model in which our dependent variable is county-by-quarter number of abortions and control for county time trends, seasonality and a dummy indicating post-announcement quarters, quarter 3 and quarter 4 in 2010. The results are available upon request.

[39]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]).

the $3^{rd}$ trimester to negative economic fluctuations in Argentina, and only for children of low educated mothers who were likely credit constrained. Yet, [Almond and Mazumder, 2011] look at relatively mild forms of nutritional disruptions imposed by Ramadan daylight fasting during pregnancy and find a negative impact on birth weights, but only for children exposed during the first two trimesters of pregnancy.

From this evidence, it is safe to conclude that possible nutritional restrictions suffered by the fetus would lead to worsening (or unchanged) weight at birth, whereas we find improvements in birth weight. Additionally, we show in Table 2.1, column (4) that there were no significant change in foodstuff expenditures following the wage cut. Overall, we may safely conclude that the nutrition channel is not driving our main results.

**Health damaging goods.** A decrease in household income may also induce a reduction in the consumption of health-damaging goods, such as cigarettes and alcohol. The medical literature shows that maternal smoking or alcohol consumption during pregnancy correlate with the increased risk of miscarriage and low birth weight ( [Floyd et al., 1993]). [Ruhm and Black, 2002] and [Ruhm, 2003] show that health-related behavioural improvements, in the form of decreased consumption of alcohol and cigarettes, have a counter-cyclical pattern and the average health level improves during recessions. [Dehejia and Lleras-Muney, 2004] find significant improvements in infant health outcomes at birth due to changes in individual behaviour of white mothers who significantly reduced smoking and alcohol consumption during pregnancy. These behavioural improvements were sufficiently strong to offset the simultaneous negative selection into motherhood.

Unfortunately, information on mothers smoking or drinking habits is not included in the VSN. Evidence from RHBS in Table 2.1 shows no change in alcohol and cigarettes expenditures per capita induced by the austerity measures. These expenditures reflect the behaviour of the average individual/households and not pregnant women. Even if behavioural improvements did occur, we observe significant changes for boys only, in their $1^{st}$ trimester of pregnancy and, to our knowledge, it has not been determined that boys would benefit more than girls from behavioural improvements (in early gestation). We argue the behavioural improvements of pregnant mothers is not likely to be the main channel through which the austerity measures influenced health at birth, though we can not dismiss its role.

[Nilsson, 2014] finds that boys exposed early in utero to an increase in the availability of alcohol in Sweden were the most negatively affected at birth as measured by a reduced share of males, which indicates that boys highly exposed to alcohol were more likely to be spontaneously aborted. If pregnant women reacted to the austerity-induced shock by increasing alcohol intake (especially before pregnancy recognition), we may also explain our results through increased spontaneous abortions of the weakest male fetuses. We will investigate this in the next section.

**Prenatal care and labor supply.** A decrease in wage may also lower the opportunity cost of leisure and health-improving activities (bed rest in high-risk pregnancies), and may induce a shift in the labour supply of pregnant women from full- to part-time employment which would positively influence children's outcomes at birth. This is unlikely due to the rigidity of the public sector employment in Romania and the limited opportunities of part time public employment: less than 1% of public sector employees have a part-time contract (source: RHBS). Women employed in the private sector could have reacted to the significant wage cut by: 1) 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; 2) changing occupational status; RHBS reveals a very high degree of persistence in the occupational status, with about 99% women having the same occupational status as in the last 12 months (both for employed and housewife mothers);[40] also, there is no change after the wage cut announcement in the share of housewives that used to be employed in the prior 12 months; 3) changing sector of employment; this channel seems unlikely since the unemployment rates in the public sector were high and rising, and that employment rates in the public sector were stable over the entire period.

A wage cut may potentially restrict the antenatal medical supervision by lowering the number of prenatal medical visits. In Romania, prenatal care is free of charge and is available to all pregnant women irrespective of their employment status, therefore it is unlikely that publicly employed mothers would reduce their use of prenatal care.

[40] We check if women potentially on the margin of leaving the labor force due to a problematic or a first-child pregnancy are more likely to exit the labor force and become housewives after the wage cut. 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: very 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.

### 2.4.3 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 the staff in the public vs. the private Romanian sector ( [Spielberger et al., 2010]). The psychological stress caused by the austerity shock experienced by the pregnant women may influence the fetal development through higher levels of cortisol, a stress hormone that reaches the fetus. The exposure to high cortisol levels induces structural adaptations in order to accelerate the maturation of the fetus and ensure her survival in a predicted stressful environment, but also to modify her ulterior response to stress ( [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 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 to the medical literature, there is a growing interest among economists to quantify the effects of maternal stress on infant birth outcomes by exploiting instances in which stress is generated by exogenous events. The evidence shows 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 landmine 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 occur during the $1^{st}$ trimester of pregnancy and increased probability of low birth weight. [Bozzoli and Quintana-Domeque, 2013] find increased low birth weight incidence due to negative macroeconomic fluctuations for children in the $1^{st}$ trimester which they attribute to maternal stress, occurring both to high and low educated mothers. On the other hand, [Aizer et al., 2009] 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.

**Selection in utero.** The evidence so far suggests that prenatal stress scars survivors, leading to worse health outcomes at birth. However, 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. In particular, the theory of selection in utero hypothesises that weaker fetuses are spontaneously aborted because of significant maternal stress, and that the weak male fetuses are being aborted more often than weak female fetuses. [Trivers and Willard, 1973] hypothesis postulates that the selection mechanism preponderantly selects against weaker male fetuses, as the likelihood of reproductive success of a weak male is relatively lower than that of a weak female. An alternative explanation for the more frequent miscarriage of males relative to females is related to males' more rapid growth rate during early pregnancy, which makes males more predisposed to abnormalities than female fetuses and thus more exposed to risk of spontaneous abortion. Medical evidence indicates that selection in utero affects fetuses in their early developmental stages ( [Hobel et al., 1999], [Owen and Matthews, 2003], [Catalano et al., 2012]).

The selective mortality mechanism is reflected in a decrease of the sex-ratio at birth and in the improvement of the average health level for the male cohort exposed in utero to the stressor. [Catalano et al., 2012] find an inverse relationship between maternal cortisol levels during pregnancy and male cohort size and conclude that elevated maternal stress culls cohorts by "raising the fitness criterion", thus resulting in healthier males. [Catalano et al., 2010] show that mass layoffs predict lower secondary sex ratios as a consequence of significant maternal stress during pregnancy due to adverse economic conditions that preponderantly selects against weak male fetuses. [Sanders and Stoecker, 2011] evidnece that gender ratios at birth can be used to infer fetal death rates of males, which are more vulnerable to maternal stress. Finally, [Valente, 2015] finds evidence of selection in utero due to maternal conflict exposure. Our results so far indicate significant improvements in health at birth of male cohorts exposed to the shock in early gestation.

## 2.5 Further evidence of selection in utero

The evidence from Section 2.4 seems to indicate that selection in utero, caused by in-utero maternal stress and/or increased smoking or alcohol intake, may help explain our positive effect on health at birth for boys exposed to the shock starting very early in the pregnancy. Because we do not have data on miscarriages, a common problem in the literature, we proceed to examine the effects on the secondary sex-ratio and the cohort size.

### 2.5.1 Sex-ratio at birth

Similar to other studies with individual level data, we model the sex-ratio at birth as the probability of a male birth. Panel A of Table 2.8 presents the results of the DD estimation for the probability of a live birth being a male, using a similar framework as before, while Panel B show results for the high educated mothers. The overall effect on the probability of a child being a boy, in Panel A, for the children who were in the $1^{st}$ trimester of gestation at the time of the shock is negative and significant in columns (1) and (2) with a magnitude of about 3.3 pp (6.7 % of the mean), and marginally significant (p-value=0.105) in column (3), when we include the pre-treatment dynamics. This effect seems to be driven by the high-educated mothers (in Panel B), who were 4.5 pp less likely to have a boy if they were in their $1^{st}$ trimester at the time of the shock.

Figure 2.3 presents the sensitivity of the results on the probability of a male birth to the definition of the treatment group, analogue to Figure 2.2. For children who were in the first trimester at the time of the announcement, the results remain significant at the 10% significance level for all definitions of the treatment group (ranging from above $80^{th}$ percentile to above the median of the predicted probability of public employment).[41]

### 2.5.2 Cohort size

Since selection in utero leads to the spontaneous abortion of the least fit male fetuses (more frequent than of female fetuses), it is expected that it would also be reflected in a smaller male cohort size.[42] We calculate cohort size at locality level by gender and gestational stage at the time of the austerity measures announcement, for publicly employed and housewife mothers. In addition, we also record cohort size by the mother's educational level. We estimate a simple DD, with locality and year fixed effects and county-specific time trends. In Table 2.9 we present the effect

[41] Additionally, we have also checked the robustness of these results when using the privately employed mothers as an alternative control group. The results (shown in Appendix A, Table 2.15) are not statistically significant and they are much in magnitude.

[42] Under the assumption that absent the selection in utero process, the cohort size would have remained unchanged. Obviously, with selection out of or into fertility, this assumption would not hold.

on the male cohort size (Panel A), and for males (Panel B) and females (Panel C) belonging to high-educated mothers. The results in Panel B show a significant negative effect on the cohort size for males that were in the $1^{st}$ trimester of pregnancy at the time of the shock, for the children belonging to highly educate mothers. As such, there are on average 1.6 fewer boys per locality (18% of the mean) born to highly educated publicly employed mothers relative to housewives. Importantly, there is no such reduction of the female cohort size.[43]

To summarize, for the children who were in the $1^{st}$ trimester of gestation at the time of the policy announcement who were exposed to the shock the most, we find: (1) improvements in the health at birth outcomes for males but not for females, (2) a reduced probability of a male birth and (3) a reduced cohort size for males but not for females. These results are driven by the effects on the children of highly educated publicly employed mothers. This evidence fits the selection in utero hypothesis, which postulates that significant maternal prenatal stress causes weaker males to be spontaneously aborted in early pregnancy. As such, in the light of the three main potential mechanisms through which the austerity measures could affect health at birth outcomes, we conclude that the effects we observe are mainly consistent with the hypothesis that prenatal maternal stress induced selection in utero.

The fact that we find no significant effect for girls may imply that girls are substantially more robust. Another possibility is that lower-tail boys are weaker than lower-tail females, but the median boy is stronger than the median girl, such that the effects on both the tail and the median of the female birth weight distribution leads to an insignificant effect for girls (some suggestive evidence on this is provided using the birth weight distributions, shown in Appendix Figure 2.7).

[43] We also looked whether we find a smaller cohort size for males in utero in 2010 vs. 2009 and before. We calculate the cohort size at locality level by gender and gestational stage at the time of the austerity measures announcement, for publicly employed and housewife mothers (about 998 clusters). Next, we simply compare the log(boys) for the publicly employed mothers, separately for each trimester and the effect for the $1^{st}$ trimester is negative and significant [-0.188*(0.100)], while for the $2^{nd}$ and $3^{rd}$ is not significant[-0.034(0.096) and 0.084(0.108)]. When we consider the housewives sample the effects are insignificant for each trimester [-0.016(0.056); -0.072*(0.036); 0.019(0.041)]. The effects for girls for the publicly employed are also negative and similar in magnitude as those for boys but not significant.

## 2.6 Further sensitivity checks

Finally, we attempt to address two more concerns that may potentially bias our results, one related to the mothers' unobservable characteristics and one concerning possible indirect effects at the household level through fathers working in the public sector. Overall, the results using these specifications point in the same direction as our main results.

### 2.6.1 Mothers' fixed effects

One concern is that mothers may have different unobserved characteristics correlated with their stress response that may affect their behaviour and could, in turn, lead to an improvement in the health at birth of their child ( [Aizer et al., 2009]). One way to control for these unobservable differences and other time invariant omitted variables is to use mother fixed effects and compare the children in utero on May $7^{th}$, 2010, to their elder siblings.

To construct the sibling sample we first select all employed mothers from the 2010 VSN that report having at least one more child. Next, we make use of the 2003-2009 VSN files in an attempt to construct the siblings' sample.[44] Unfortunately, our data does not include the mothers' personal number and we cannot directly link the data but we do have information on the mother's place of residence, mother's ethnicity and the mother's exact date of birth (day, month and year). To increase the precision of our matching, we further restrict our sample to children belonging to mothers married to the same fathers, by exploiting the fact that the VSN provides information on the exact date of marriage (based on the marital certificate) and the father's birth date (day, month and year). Thus, we obtain a selected sample of 60,931 children belonging to 25,392 mothers.

In Table 2.10 our main variables of interest is the exposed sibling indicator, which equals 1 if the child was in utero on May $7^{th}$, 2010 and 0 if the child was an elder sibling, for the selected sample of mothers who were predicted to be publicly employed in 2010. In columns (1a) — (4a) we look at the low birth weight outcome, while in columns (1b)—(4b) at the probability of the youngest child to

[44] The reason for not using data collected before 2003 is that the structure of the VSN files has been changed in 2003, and some important socio-economic characteristics of the parents are not available in earlier records.

be a boy.[45] Our controls include child-specific characteristics, the age of parents at conception, and a linear time trend to control for other changes that may allow mother's behaviour to adapt to e.g., health or education trends.[46] Columns (1) and (2) are simple sibling estimates, column (3) is the mother FE specification, while column (4) is similar to column (3) but only for the high educated mothers. The results are quite stable and indicate that the siblings who were exposed to the austerity shock in utero seem less likely to have a low birth weight compared to their unexposed siblings (columns a) and also it seems less likely to be a boy (columns b). For the latter effects, while the sign is negative, as expected, we only find a significant effect for the highly educated mothers (in column 4b). We have conducted a similar strategy for the housewives mothers and we see similar effects but much lower in magnitude (-0.013(0.010) the equivalent of column (4a) and -0.008(0.023) the equivalent of column (4b)). Finally, our baseline DD regression on this siblings sample of housewives and publicly employed mothers shows a similar pattern, but the interaction term is only significant for the low birth weight outcome.

### 2.6.2 Income shocks through father's employment status

Finally, the employment sector of the father may also influence the (intensity of the) experienced shock, as households may have been more severely affected by the policy if both parents were employed in the public sector. According to data in RHBS, about 30% of publicly employed women and only 8% of housewife women are married to a publicly employed man. Unfortunately, from the VSN, we do not have information on the sector of employment of the employed fathers, and neither do we have the other covariates which would allow us to proceed in an analogue manner to mothers and obtain their predicted probabilities of public employment. To better capture the household level nature of the shocks, we compare households with publicly employed mothers with employed fathers vs. households with housewives and fathers with an occupational status other than employed, e.g. business owner, self-employed in agriculture.

The estimation results, presented in Table 2.11 for the low birth weight indicator

[45] The results are for the occupational in 2010, but the effects are similar if we restrict the sample to always employed.

[46] To control for possible changes in education over time within the same household, we also include the level of education and the occupational status of the parents at the time of each birth. The results remain robust to this specification. While the results hold the expected sign, we do not find significant results when we restrict to the same-sex siblings sample.

are very similar to our main results, suggest that our main specification is not biased by indirect shocks.

## 2.7 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 shocks affect child health at birth through what appears to be selection in utero, in which maternal exposure to significant fetal stressors - directly through stress per se or indirectly through smoking or alcohol consumption - selects against frail fetuses, with male fetuses significantly more predisposed to spontaneous abortions than females. We infer this "culling" process after detecting significant improvements in health outcomes at birth in the male cohorts exposed to the stressor early in gestation, coupled with evidence of a reduced sex-ratio at birth for the cohort that was in the $1^{st}$ trimester of gestation at the time of the announcement and a reduced size of that particular male cohort.

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 mainly a biological response to severe stressors, then there is less scope for reversing the policy impact, and this needs to be taken into consideration when such drastic measures are implemented.

Our findings suggest that unexpected policy changes, albeit temporary, may act as sufficiently severe stressors on the population to such an extent that selective fetal mortality may have large effects, even in economies where the baseline health level is relatively high. Given the medical evidence on the latent effects of prenatal exposure to stressors, if these apparently healthier children were culled through such a mechanism, they may show adverse outcomes later on during their lifetimes.

# Bibliography


[Abrams et al., 2000] Abrams, B., Altman, S. L., and Pickett, K. E. (2000). Pregnancy weight gain: still controversial. *The American journal of clinical nutrition*, 71(5):1233s–1241s.

[Aizer et al., 2009] Aizer, A., Stroud, L., and Buka, S. (2009). Maternal stress and child well-being: Evidence from siblings. *Unpublished manuscript, Brown University, Providence, RI.*

[Almond, 2006] Almond, D. (2006). Is the 1918 influenza pandemic over? Long-term effects of in utero influenza exposure in the post-1940 US population. *Journal of Political Economy*, 114(4):672–712.

[Almond and Currie, 2011] Almond, D. and Currie, J. (2011). Killing me softly: The fetal origins hypothesis. *The Journal of Economic Perspectives*, pages 153–172.

[Almond et al., 2009] Almond, D., Edlund, L., and Milligan, K. (2009). O sister, where art thou? The role of son preference and sex choice: evidence from immigrants to Canada.

[Almond et al., 2007] Almond, D., Edlund, L., and Palme, M. (2007). Chernobyl's subclinical legacy: prenatal exposure to radioactive fallout and school outcomes in Sweden.

[Almond et al., 2011] Almond, D., Hoynes, H. W., and Schanzenbach, D. W. (2011). Inside the war on poverty: the impact of food stamps on birth outcomes. *The Review of Economics and Statistics*, 93(2):387–403.

[Almond and Mazumder, 2011] Almond, D. and Mazumder, B. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. *American Economic Journal-Applied Economics*, 3(4):56.

[Barker, 1990] Barker, D. J. (1990). The fetal and infant origins of adult disease. *BMJ: British Medical Journal*, 301(6761):1111.

[Becker, 1965] Becker, G. S. (1965). A theory of the allocation of time. *The economic journal*, pages 493–517.

[Beydoun and Saftlas, 2008] Beydoun, H. and Saftlas, A. F. (2008). Physical and mental health outcomes of prenatal maternal stress in human and animal studies: a review of recent evidence. *Paediatric and perinatal epidemiology*, 22(5):438–466.

[Bozzoli and Quintana-Domeque, 2013] Bozzoli, C. and Quintana-Domeque, C. (2013). The weight of the crisis: Evidence from newborns in Argentina. *Review of Economics and Statistics*.

[Burlando, 2010] Burlando, A. (2010). Impact of transitory income on birth weights: Evidence from a blackout in Zanzibar.

[Camacho, 2008] Camacho, A. (2008). Stress and birth weight: evidence from terrorist attacks. *The American Economic Review*, pages 511–515.

[Catalano et al., 2012] Catalano, R., Saxton, K., Bruckner, T., Pearl, M., Anderson, E., Goldman-Mellor, S., Margerison-Zilko, C., Subbaraman, M., Currier, R., and Kharrazi, M. (2012). Hormonal evidence supports the theory of selection in utero. *American Journal of Human Biology*, 24(4):526–532.

[Catalano et al., 2010] Catalano, R., Zilko, C. E. M., Saxton, K. B., and Bruckner, T. (2010). Selection in utero: a biological response to mass layoffs. *American Journal of Human Biology*, 22(3):396–400.

[Catalano, 2003] Catalano, R. A. (2003). Sex ratios in the two Germanies: a test of the economic stress hypothesis. *Human Reproduction*, 18(9):1972–1975.

[Catalano and Bruckner, 2005] Catalano, R. A. and Bruckner, T. (2005). Economic antecedents of the Swedish sex ratio. *Social science & medicine*, 60(3):537–543.

[Catalano et al., 2009] Catalano, R. A., Saxton, K., Bruckner, T., Goldman, S., and Anderson, E. (2009). A sex-specific test of selection in utero. *Journal of theoretical biology*, 257(3):475–479.

[Dehejia and Lleras-Muney, 2004] Dehejia, R. and Lleras-Muney, A. (2004). Booms, busts, and babies' health. *The Quarterly Journal of Economics*, 119(3):1091–1130.

[Eriksson et al., 2010] Eriksson, J. G., Kajantie, E., Osmond, C., Thornburg, K., and Barker, D. J. (2010). Boys live dangerously in the womb. *American Journal of Human Biology*, 22(3):330–335.

[Firici and Thomson, 2002] Firici, M. C. and Thomson, K. J. (2002). Distributional impacts of cap adoption on Romanian households. *Zaragoza (Spain)*, 28:31.

[Floyd et al., 1993] Floyd, R. L., Rimer, B. K., Giovino, G. A., Mullen, P. D., and Sullivan, S. E. (1993). A review of smoking in pregnancy: effects on pregnancy outcomes and cessation efforts. *Annual review of public health*, 14(1):379–411.

[Fowles, 2004] Fowles, E. R. (2004). Prenatal nutrition and birth outcomes. *Journal of Obstetric, Gynecologic, & Neonatal Nursing*, 33(6):809–822.

[Gluckman and Hanson, 2005] Gluckman, P. and Hanson, M. (2005). The fetal matrix: evolution, development and disease.

[Glynn et al., 2001] Glynn, L. M., Wadhwa, P. D., Dunkel-Schetter, C., Chicz-DeMet, A., and Sandman, C. A. (2001). When stress happens matters: effects of earthquake timing on stress responsivity in pregnancy. *American journal of obstetrics and gynecology*, 184(4):637–642.

[Heckman and Walker, 1990] Heckman, J. J. and Walker, J. R. (1990). The relationship between wages and income and the timing and spacing of births: evidence from Swedish longitudinal data. *Econometrica: journal of the Econometric Society*, pages 1411–1441.

[Hobel et al., 1999] Hobel, C. J., Dunkel-Schetter, C., Roesch, S. C., Castro, L. C., and Arora, C. P. (1999). Maternal plasma corticotropin-releasing hormone associated with stress at 20 weeks' gestation in pregnancies ending in preterm delivery. *American journal of obstetrics and gynecology*, 180(1):S257–S263.

[Lindo, 2011] Lindo, J. M. (2011). Parental job loss and infant health. *Journal of Health Economics*, 30(5):869–879.

[Maconochie et al., 2007] Maconochie, N., Doyle, P., Prior, S., and Simmons, R. (2007). Risk factors for first trimester miscarriage-results from a UK-population-based case–control study. *BJOG: An International Journal of Obstetrics & Gynaecology*, 114(2):170–186.

[Mansour and Rees, 2012] Mansour, H. and Rees, D. I. (2012). Armed conflict and birth weight: Evidence from the al-Aqsa Intifada. *Journal of Development Economics*, 99(1):190–199.

[Mulder et al., 2002] Mulder, E., Robles de Medina, P., Huizink, A., Van den Bergh, B., Buitelaar, J., and Visser, G. (2002). Prenatal maternal stress: effects on pregnancy and the (unborn) child. *Early human development*, 70(1):3–14.

[Murphy and Topel, 2002] Murphy, K. M. and Topel, R. H. (2002). Estimation and inference in two-step econometric models. *Journal of Business and Economic Statistics*, 20(1):88–97.

[Nilsson, 2014] Nilsson, J. (2014). Alcohol availability, prenatal conditions, and long-term economic outcomes. *Institute for International Economic Studies, Stockholm University, unpublished manuscript*.

[Owen and Matthews, 2003] Owen, D. and Matthews, S. G. (2003). Glucocorticoids and sex-dependent development of brain glucocorticoid and mineralocorticoid receptors. *Endocrinology*, 144(7):2775–2784.

[Painter et al., 2005] Painter, R. C., Roseboom, T. J., and Bleker, O. P. (2005). Prenatal exposure to the dutch famine and disease in later life: an overview. *Reproductive toxicology*, 20(3):345–352.

[Paxson and Schady, 2005] Paxson, C. and Schady, N. (2005). Child health and economic crisis in Peru. *The World Bank Economic Review*, 19(2):203–223.

[Roseboom et al., 2011] Roseboom, T. J., Painter, R. C., van Abeelen, A. F., Veenendaal, M. V., and de Rooij, S. R. (2011). Hungry in the womb: what are the consequences? Lessons from the Dutch famine. *Maturitas*, 70(2):141–145.

[Ruhm, 2003] Ruhm, C. J. (2003). Good times make you sick. *Journal of health economics*, 22(4):637–658.

[Ruhm and Black, 2002] Ruhm, C. J. and Black, W. E. (2002). Does drinking really decrease in bad times? *Journal of health economics*, 21(4):659–678.

[Sanders and Stoecker, 2011] Sanders, N. J. and Stoecker, C. F. (2011). Where have all the young men gone? Using gender ratios to measure fetal death rates.

[Spielberger et al., 2010] Spielberger, C., Vagg, P., Pitariu, H., Iliescu, D., Livinti, R., and Hangan, M. (2010). Manual tehnic pentru Job Stress Survey. *Cluj-Napoca: Sinapsis*.

[Stein et al., 2006] Stein, R. E., Siegel, M. J., and Bauman, L. J. (2006). Are children of moderately low birth weight at increased risk for poor health? a new look at an old question. *Pediatrics*, 118(1):217–223.

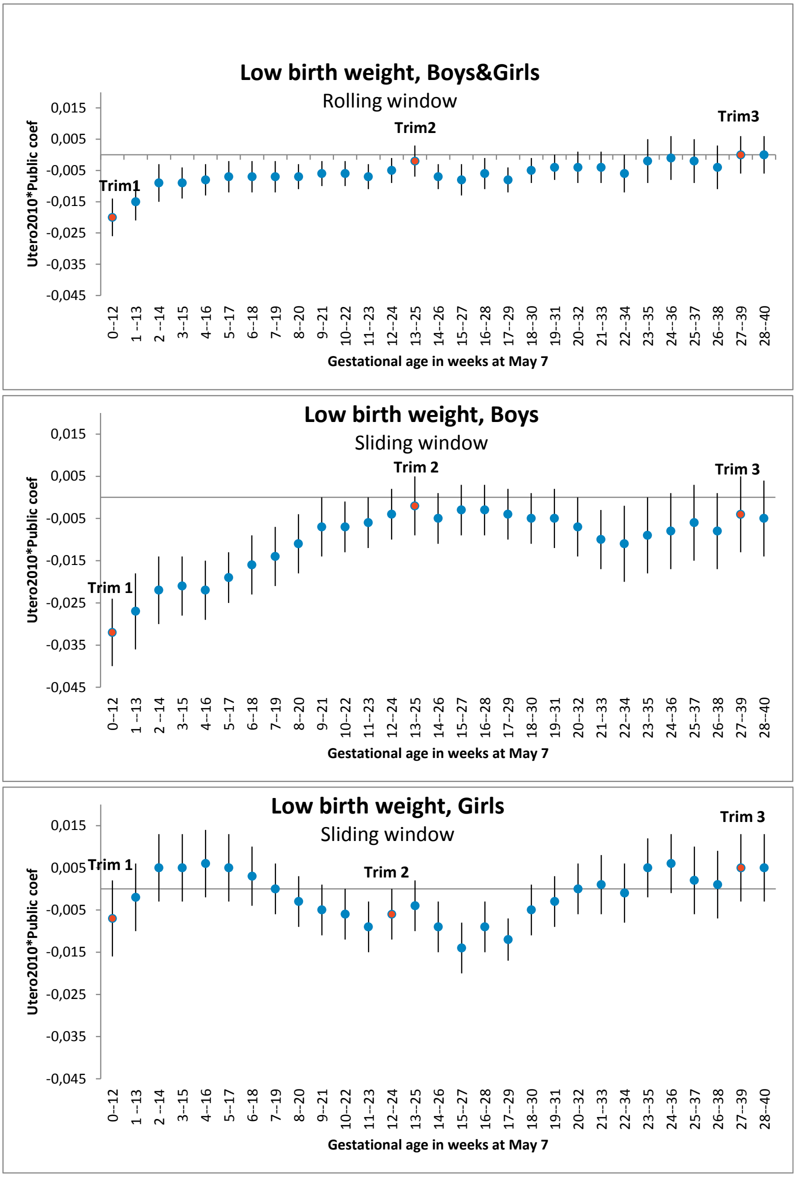
[Stephenson and Symonds, 2002] Stephenson, T. and Symonds, M. (2002). Maternal nutrition as a determinant of birth weight. *Archives of Disease in Childhood Fetal and Neonatal Edition*, 86(1):F4.

[Trivers and Willard, 1973] Trivers, R. L. and Willard, D. E. (1973). Natural selection of parental ability to vary the sex ratio of offspring. *Science*, 179(4068):90–92.

[Valente, 2015] Valente, C. (2015). Civil conflict, gender - specific fetal loss, and selection: A new test of the Trivers-Willard hypothesis. *Journal of Health Economics*, 39(0):31–50.

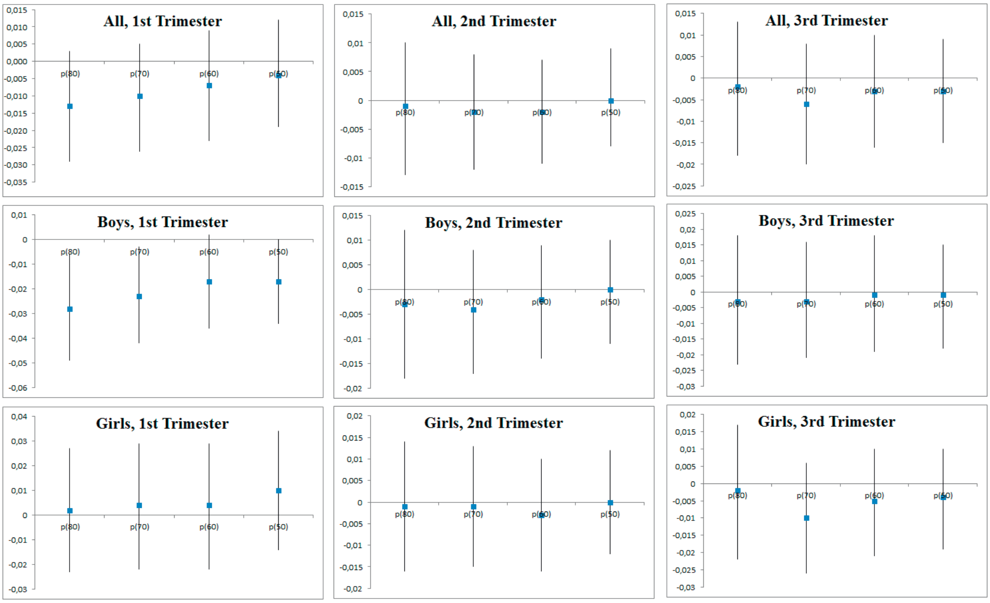
[Vîrgă et al., 2012] Vîrgă, D., Macsinga, I., and Sulea, C. (2012). Occupational health psychology in Romania: Managers' and employees' needs and perspective. *Romanian Journal of Applied Psychology*, 14(1):18–23.

Figure 2.1: Low birth weight, treated cohorts defined using a gliding window of 12 gestational weeks



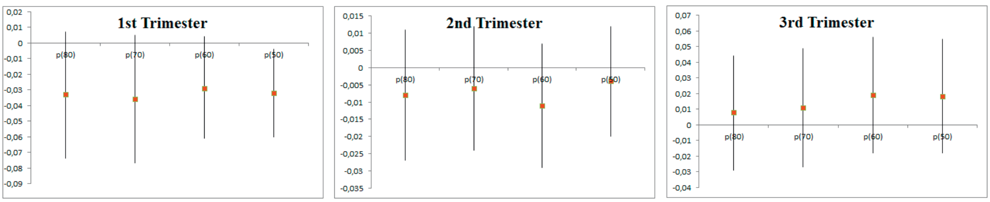

Notes: the figures show the point estimates and the 95% confidence interval for the parameter of interest, the Public*Utero2010 interaction term, for subsamples of 12 gestational weeks.

Figure 2.2: Sensitivity to the definition of the treatment group, Low birth weight




Notes: the figure shows the point estimates and the 95% confidence interval for the parameter of interest, the Public*Utero2010 interaction term as we vary the threshold percentile of the predicted probability.

Figure 2.3: Sensitivity to the definition of the treatment group, Probability of a male birth




Notes: the figure shows the point estimates and the 95% confidence interval for the parameter of interest, the Public*Utero2010 interaction term as we vary the threshold percentile of the predicted probability.

Table 2.1: Household income and expenditures pattern

| | (1) Log HH wage income | (2) Log HH wage income (high educated) | (3) Log HH income | (4) Log Foodstuff expenditures per capita | (5) Log Alcohol & cigarettes exp. per capita | (6) Log Non-foodstuff expenditures per capita | (7) Log Expenditures Services Services | (8) Log (formal) savings |
|---|---|---|---|---|---|---|---|---|
| **Panel A: 2010** | | | | | | | | |
| Public*after | -0.167* | -0.217** | -0.070*** | -0.017 | -0.043 | -0.062** | -0.051* | -0.119** |
| | (0.095) | (0.104) | (0.022) | (0.018) | (0.080) | (0.030) | (0.028) | (0.057) |
| after | 0.077 | 0.159 | 0.013 | 0.219*** | 0.508*** | 0.138*** | -0.106*** | -0.011 |
| | (0.095) | (0.131) | (0.019) | (0.020) | (0.071) | (0.038) | (0.025) | (0.032) |
| public | 2.395*** | 2.133*** | 0.121*** | -0.008 | -0.015 | -0.014 | 0.032 | 0.003 |
| | (0.151) | (0.156) | (0.028) | (0.027) | (0.122) | (0.046) | (0.034) | (0.058) |
| Controls | yes | yes | yes | yes | yes | yes | yes | yes |
| HH no | 14,328 | 7,789 | 14,328 | 14,328 | 14,328 | 14,328 | 14,328 | 14,328 |
| R-squared | 0.688 | 0.699 | 0.587 | 0.385 | 0.209 | 0.186 | 0.399 | 0.048 |
| **Panel B: 2009** | | | | | | | | |
| Public*after | -0.040 | -0.004 | -0.049 | -0.001 | 0.023 | 0.014 | 0.040 | 0.002 |
| | (0.060) | (0.082) | (0.030) | (0.016) | (0.082) | (0.041) | (0.029) | (0.050) |
| after | -0.068 | 0.096 | 0.073*** | 0.248*** | 0.431*** | 0.216*** | -0.107*** | 0.088** |
| | (0.072) | (0.103) | (0.019) | (0.017) | (0.071) | (0.044) | (0.029) | (0.040) |
| public | 2.259*** | 2.121*** | 0.175*** | 0.028 | 0.004 | 0.116** | 0.125*** | -0.014 |
| | (0.127) | (0.131) | (0.036) | (0.024) | (0.125) | (0.052) | (0.042) | (0.086) |
| Controls | yes | yes | yes | yes | yes | yes | yes | yes |
| Observations | 14,598 | 7,869 | 14,598 | 14,598 | 14,598 | 14,598 | 14,598 | 14,598 |
| R-squared | 0.699 | 0.697 | 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 2.2: Descriptive statistics, working sample: urban households, fertile age mothers

| | | *All* | | | | *Publicly employed* (20-80)* | | | | *Employed* | | | | *Housewives* | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | *(1)* | | | | *(2)* | | | | *(3)* | | | | *(4)* | | | |
| | | **2007** | **2008** | **2009** | **2010** | **2007** | **2008** | **2009** | **2010** | **2007** | **2008** | **2009** | **2010** | **2007** | **2008** | **2009** | **2010** |
| ***Mother's characteristics at childbirth*** | | | | | | | | | | | | | | | | | |
| | Age | 27,582 | 27,704 | 27,925 | 28,241 | 32,854 | 33,488 | 34,078 | 33,686 | 28,919 | 29,128 | 29,34 | 29,644 | 25,11 | 24,973 | 25,181 | 25,451 |
| Education | Secondary | 0,287 | 0,284 | 0,262 | 0,242 | 0,003 | 0,015 | 0,006 | 0,02 | 0,105 | 0,09 | 0,082 | 0,07 | 0,596 | 0,592 | 0,584 | 0,573 |
| | High-school | 0,397 | 0,372 | 0,351 | 0,324 | 0,001 | 0 | 0,001 | 0,003 | 0,421 | 0,388 | 0,346 | 0,303 | 0,363 | 0,363 | 0,358 | 0,355 |
| | High education | 0,316 | 0,345 | 0,387 | 0,434 | 0,996 | 0,984 | 0,993 | 0,977 | 0,474 | 0,523 | 0,573 | 0,628 | 0,041 | 0,045 | 0,058 | 0,072 |
| | Married | 0,774 | 0,764 | 0,766 | 0,769 | 0,95 | 0,936 | 0,911 | 0,881 | 0,897 | 0,892 | 0,893 | 0,893 | 0,57 | 0,552 | 0,547 | 0,544 |
| Ethnicity: | Romanian | 0,932 | 0,929 | 0,934 | 0,932 | 0,971 | 0,966 | 0,954 | 0,966 | 0,943 | 0,946 | 0,95 | 0,949 | 0,911 | 0,903 | 0,91 | 0,906 |
| | Hungarian | 0,045 | 0,043 | 0,04 | 0,04 | 0,022 | 0,034 | 0,038 | 0,027 | 0,052 | 0,048 | 0,045 | 0,045 | 0,031 | 0,03 | 0,029 | 0,028 |
| | Other | 0,023 | 0,028 | 0,026 | 0,027 | 0,008 | 0 | 0,008 | 0,008 | 0,005 | 0,005 | 0,004 | 0,006 | 0,058 | 0,067 | 0,061 | 0,066 |
| | Prenatal control | 0,868 | 0,867 | 0,834 | 0,789 | 0,943 | 0,932 | 0,879 | 0,816 | 0,926 | 0,921 | 0,881 | 0,818 | 0,798 | 0,81 | 0,791 | 0,764 |
| | No. of births | 1,599 | 1,586 | 1,594 | 1,608 | 1,605 | 1,657 | 1,704 | 1,809 | 1,438 | 1,429 | 1,431 | 1,44 | 2 | 2,022 | 2,042 | 2,077 |
| | No. of living children | 1,604 | 1,588 | 1,593 | 1,608 | 1,61 | 1,662 | 1,703 | 1,808 | 1,443 | 1,432 | 1,43 | 1,44 | 2,008 | 2,023 | 2,039 | 2,077 |
| | Hospital delivery | 0,99 | 0,987 | 0,983 | 0,985 | 0,999 | 0,999 | 0,999 | 0,999 | 0,999 | 0,998 | 0,998 | 0,998 | 0,988 | 0,988 | 0,989 | 0,991 |
| ***Child's characteristics at birth*** | | | | | | | | | | | | | | | | | |
| | Girl | 0,485 | 0,48 | 0,484 | 0,487 | 0,492 | 0,485 | 0,488 | 0,495 | 0,484 | 0,478 | 0,482 | 0,489 | 0,487 | 0,484 | 0,489 | 0,48 |
| | Birth weight | 3230,04 | 3238,739 | 3233,877 | 3224,18 | 3315,247 | 3318,813 | 3305,611 | 3292,18 | 3284,204 | 3294,707 | 3282,664 | 3273,225 | 3136,077 | 3149,135 | 3149,45 | 3141,317 |
| | Low birth weight | 0,064 | 0,059 | 0,061 | 0,065 | 0,043 | 0,045 | 0,048 | 0,049 | 0,051 | 0,047 | 0,049 | 0,05 | 0,094 | 0,09 | 0,09 | 0,094 |
| | Gestation duration (weeks) | 38,884 | 38,791 | 38,78 | 38,765 | 38,886 | 38,728 | 38,707 | 38,69 | 38,946 | 38,842 | 38,844 | 38,806 | 38,776 | 38,698 | 38,696 | 38,699 |
| | Premature delivery | 0,07 | 0,07 | 0,072 | 0,068 | 0,057 | 0,061 | 0,063 | 0,063 | 0,059 | 0,059 | 0,058 | 0,058 | 0,088 | 0,088 | 0,089 | 0,084 |
| ***No. observations*** | | **76697** | **79517** | **79894** | **76160** | **9827** | **10219** | **10437** | **9801** | **48257** | **49803** | **50789** | **48867** | **22216** | **22228** | **21860** | **20128** |

Notes: Mean values for pregnancies in utero at May $7^{th}$, in each corresponding year, that resulted in live births. Source: Authors' calculations using the VSN files for 2007, 2008, 2009 and 2010. * "Publicly employed (20-80)" refers to the women classified as publicly employed based on their predicted probabilities of working in the public sector, 20-80 split.

Table 2.3: Selection into fertility

| | (1) Secondary education | (2) High school education | (3) Higher education | (4) Age | (5) Married | (6) Unemployed father |
|---|---|---|---|---|---|---|
| ***All*** | | | | | | |
| utero_2010 | -0.007*** | -0.026*** | 0.033*** | 0.166*** | -0.097*** | 0.010*** |
| | (0.002) | (0.002) | (0.001) | (0.008) | (0.002) | (0.001) |
| Observations | 312,268 | 312,268 | 312,268 | 312,268 | 86,432 | 312,268 |
| R-squared | 0.035 | 0.025 | 0.073 | 0.031 | 0.036 | 0.030 |
| ***Publicly employed*** | | | | | | |
| utero_2010 | 0.032*** | 0.0004** | -0.033*** | 0.341*** | -0.019*** | 0.002*** |
| | (0.001) | (0.001) | (0.001) | (0.031) | (0.001) | (0.001) |
| Observations | 40,284 | 40,284 | 40,284 | 40,284 | 40,284 | 40,284 |
| R-squared | 0.022 | 0.007 | 0.025 | 0.060 | 0.016 | 0.013 |
| ***Privately employed*** | | | | | | |
| utero_2010 | -0.045*** | -0.132*** | 0.177*** | 0.861*** | 0.015*** | 0.008*** |
| | (0.001) | (0.003) | (0.003) | (0.012) | (0.001) | (0.001) |
| Observations | 157,167 | 157,167 | 157,167 | 157,167 | 157,167 | 157,167 |
| R-squared | 0.028 | 0.049 | 0.076 | 0.016 | 0.013 | 0.019 |
| ***Housewives*** | | | | | | |
| utero_2010 | -0.034*** | 0.008*** | 0.025*** | 0.013 | 0.037*** | -0.097*** |
| | (0.002) | (0.002) | (0.001) | (0.021) | (0.002) | (0.002) |
| Observations | 86,432 | 86,432 | 86,432 | 86,432 | 86,432 | 86,432 |
| R-squared | 0.026 | 0.020 | 0.014 | 0.011 | 0.017 | 0.036 |

Each cell reports the estimated coefficient on the observable mothers characteristics from an OLS regression. All regressions include child's month of birth, county specific indicators, (linear) time trends and county specific trends. We only consider children born in urban areas that were in utero May-December 2007-2010. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 2.4: Low birth weight. Publicly employed vs. Housewife mothers

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ***Panel A: All*** | | | | | | | | | |
| public_utero2010 | -0.015** | -0.020*** | -0.014* | 0.003 | -0.004 | -0.002 | 0.001 | -0.000 | -0.001 |
| | (0.007) | (0.006) | (0.008) | (0.005) | (0.004) | (0.006) | (0.007) | (0.006) | (0.008) |
| public_utero2009 | | | 0.014 | | | 0.004 | | | 0.000 |
| | | | (0.009) | | | (0.006) | | | (0.005) |
| public_utero2008 | | | 0.004 | | | -0.001 | | | -0.003 |
| | | | (0.008) | | | (0.006) | | | (0.006) |
| public | -0.047*** | -0.005 | -0.011 | -0.046*** | -0.017*** | -0.018*** | -0.040*** | -0.016*** | -0.015*** |
| | (0.004) | (0.009) | (0.010) | (0.003) | (0.005) | (0.006) | (0.004) | (0.004) | (0.005) |
| Observations | 27,401 | 27,401 | 27,401 | 57,318 | 57,318 | 57,318 | 41,997 | 41,997 | 41,997 |
| R-squared | 0.145 | 0.329 | 0.330 | 0.093 | 0.261 | 0.261 | 0.016 | 0.141 | 0.141 |
| ***Panel B: Boys*** | | | | | | | | | |
| public_utero2010 | -0.032*** | -0.032*** | -0.029** | 0.001 | -0.003 | -0.004 | -0.003 | -0.005 | -0.002 |
| | (0.008) | (0.008) | (0.011) | (0.008) | (0.006) | (0.008) | (0.009) | (0.009) | (0.010) |
| public_utero2009 | | | 0.007 | | | -0.001 | | | 0.006 |
| | | | (0.009) | | | (0.009) | | | (0.008) |
| public_utero2008 | | | 0.001 | | | -0.000 | | | 0.002 |
| | | | (0.010) | | | (0.008) | | | (0.007) |
| public | -0.034*** | 0.001 | -0.002 | -0.039*** | -0.016*** | -0.016* | -0.033*** | -0.017** | -0.019** |
| | (0.005) | (0.009) | (0.012) | (0.004) | (0.005) | (0.008) | (0.004) | (0.007) | (0.008) |
| Observations | 13,949 | 13,949 | 13,949 | 29,502 | 29,502 | 29,502 | 21,64 | 21,64 | 21,64 |
| R-squared | 0.162 | 0.357 | 0.357 | 0.104 | 0.280 | 0.280 | 0.015 | 0.147 | 0.147 |
| ***Panel C: Girls*** | | | | | | | | | |
| public_utero2010 | 0.002 | -0.007 | 0.002 | 0.005 | -0.005 | -0.002 | 0.006 | 0.005 | -0.001 |
| | (0.010) | (0.009) | (0.012) | (0.006) | (0.006) | (0.008) | (0.008) | (0.008) | (0.010) |
| public_utero2009 | | | 0.021 | | | 0.008 | | | -0.007 |
| | | | (0.014) | | | (0.008) | | | (0.008) |
| public_utero2008 | | | 0.004 | | | -0.002 | | | -0.009 |
| | | | (0.015) | | | (0.008) | | | (0.008) |
| public | -0.061*** | -0.009 | -0.018 | -0.053*** | -0.018** | -0.021** | -0.048*** | -0.015* | -0.010 |
| | (0.005) | (0.013) | (0.018) | (0.004) | (0.008) | (0.010) | (0.005) | (0.008) | (0.010) |
| Observations | 13,452 | 13,452 | 13,452 | 27,816 | 27,816 | 27,816 | 20,357 | 20,357 | 20,357 |
| R-squared | 0.136 | 0.309 | 0.309 | 0.086 | 0.246 | 0.246 | 0.021 | 0.138 | 0.138 |
| Controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year&County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: child gender, gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynaecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 2.5: Low birth weight. Bootstrapped standard errors

| | *1st Trimester* | *2nd Trimester* | *3rd Trimester* |
|---|---|---|---|
| | (1) | (2) | (3) |
| ***Panel A: All*** | | | |
| public_utero2010 | -0.014* | -0.002 | -0.001 |
| Robust se | (0.008) | (0.006) | (0.008) |
| Bootstrapped se | (0.009) | (0.006) | (0.006) |
| ***Panel B: Boys*** | | | |
| public_utero2010 | -0.029** | -0.004 | -0.002 |
| Robust se | (0.011) | (0.008) | (0.010) |
| Bootstrapped se | (0.012) | (0.009) | (0.007) |
| ***Panel C: Girls*** | | | |
| public_utero2010 | 0.002 | -0.002 | -0.001 |
| Robust se | (0.012) | (0.008) | (0.010) |
| Bootstrapped se | (0.013) | (0.009) | (0.009) |
| Controls | Yes | Yes | Yes |
| Year&County FE | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes |

Notes: Robust standard errors clustered at the county level shown in parentheses (42 clusters). Bootstrapped standard errors obtained from 500 replications. $***p < 0.01, **p < 0.05, *p < 0.10$

Table 2.6: Low birth weight. Publicly employed vs. Housewife mothers, Highly educated women

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ***Panel A: All*** | | | | | | | | | |
| public_utero2010 | -0.020** | -0.024** | -0.011 | 0.006 | -0.001 | 0.007 | -0.004 | -0.007 | -0.006 |
| | (0.009) | (0.009) | (0.011) | (0.006) | (0.006) | (0.007) | (0.008) | (0.009) | (0.010) |
| public_utero2009 | | | 0.012 | | | 0.013* | | | 0.001 |
| | | | (0.011) | | | (0.007) | | | (0.009) |
| public_utero2008 | | | 0.025* | | | 0.009 | | | 0.001 |
| | | | (0.013) | | | (0.008) | | | (0.009) |
| public | -0.010** | -0.012 | -0.025** | -0.017*** | -0.015*** | -0.022*** | -0.012*** | -0.013*** | -0.014* |
| | (0.004) | (0.007) | (0.011) | (0.003) | (0.005) | (0.007) | (0.004) | (0.004) | (0.007) |
| Observations | 14,088 | 14,088 | 14,088 | 29,619 | 29,619 | 29,619 | 21,272 | 21,272 | 21,272 |
| R-squared | 0.139 | 0.319 | 0.319 | 0.083 | 0.247 | 0.247 | 0.010 | 0.122 | 0.122 |
| ***Panel B: Boys*** | | | | | | | | | |
| public_utero2010 | -0.040*** | -0.040*** | -0.025* | 0.008 | 0.003 | 0.011 | -0.008 | -0.010 | -0.012 |
| | (0.012) | (0.011) | (0.014) | (0.008) | (0.007) | (0.008) | (0.011) | (0.011) | (0.014) |
| public_utero2009 | | | 0.015 | | | 0.011 | | | -0.003 |
| | | | (0.013) | | | (0.008) | | | (0.010) |
| public_utero2008 | | | 0.028* | | | 0.012 | | | -0.005 |
| | | | (0.015) | | | (0.010) | | | (0.011) |
| public | -0.001 | -0.005 | -0.019* | -0.016*** | -0.014** | -0.022*** | -0.005 | -0.007 | -0.005 |
| | (0.006) | (0.008) | (0.012) | (0.004) | (0.006) | (0.008) | (0.005) | (0.006) | (0.008) |
| Observations | 7,161 | 7,161 | 7,161 | 15,243 | 15,243 | 15,243 | 10,888 | 10,888 | 10,888 |
| R-squared | 0.173 | 0.356 | 0.356 | 0.102 | 0.280 | 0.280 | 0.013 | 0.139 | 0.139 |
| ***Panel C: Girls*** | | | | | | | | | |
| public_utero2010 | 0.006 | -0.001 | 0.010 | 0.004 | -0.005 | 0.003 | 0.001 | -0.004 | 0.002 |
| | (0.013) | (0.013) | (0.015) | (0.009) | (0.009) | (0.010) | (0.011) | (0.012) | (0.013) |
| public_utero2009 | | | 0.010 | | | 0.016 | | | 0.007 |
| | | | (0.018) | | | (0.010) | | | (0.013) |
| public_utero2008 | | | 0.022 | | | 0.006 | | | 0.009 |
| | | | (0.020) | | | (0.012) | | | (0.013) |
| public | -0.020*** | -0.019** | -0.030* | -0.019*** | -0.016** | -0.023*** | -0.020*** | -0.019*** | -0.024** |
| | (0.006) | (0.009) | (0.016) | (0.004) | (0.006) | (0.008) | (0.005) | (0.007) | (0.011) |
| Observations | 6,927 | 6,927 | 6,927 | 14,376 | 14,376 | 14,376 | 10,384 | 10,384 | 10,384 |
| R-squared | 0.120 | 0.295 | 0.295 | 0.071 | 0.222 | 0.222 | 0.017 | 0.117 | 0.118 |
| Controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year&County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: child gender, gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynaecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 2.7: Low birth weight. Publicly employed vs. Privately employed mothers

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ***Panel A: All*** | | | | | | | | | |
| public_utero2010 | -0.010** | -0.016*** | -0.010 | 0.005 | -0.002 | -0.004 | 0.003 | 0.001 | 0.003 |
| | (0.004) | (0.004) | (0.006) | (0.004) | (0.003) | (0.006) | (0.004) | (0.004) | (0.005) |
| public_utero2009 | | | 0.008 | | | -0.005 | | | 0.005 |
| | | | (0.008) | | | (0.006) | | | (0.004) |
| public_utero2008 | | | 0.009* | | | -0.002 | | | 0.000 |
| | | | (0.005) | | | (0.006) | | | (0.004) |
| Public | -0.004* | -0.007*** | -0.013*** | -0.005** | -0.009*** | -0.007 | -0.007*** | -0.008*** | -0.010*** |
| | (0.002) | (0.003) | (0.004) | (0.002) | (0.002) | (0.005) | (0.002) | (0.003) | (0.003) |
| Observations | 41,37 | 41,37 | 41,37 | 89,591 | 89,591 | 89,591 | 66,49 | 66,49 | 66,49 |
| R-squared | 0.127 | 0.320 | 0.320 | 0.088 | 0.244 | 0.244 | 0.006 | 0.119 | 0.119 |
| ***Panel B: Boys*** | | | | | | | | | |
| public_utero2010 | -0.021*** | -0.023*** | -0.014 | 0.000 | -0.008 | -0.011 | -0.000 | -0.003 | -0.001 |
| | (0.007) | (0.008) | (0.010) | (0.007) | (0.005) | (0.008) | (0.005) | (0.005) | (0.006) |
| public_utero2009 | | | 0.010 | | | -0.007 | | | 0.005 |
| | | | (0.011) | | | (0.007) | | | (0.007) |
| public_utero2008 | | | 0.016** | | | -0.003 | | | 0.001 |
| | | | (0.006) | | | (0.008) | | | (0.006) |
| Public | 0.004 | -0.001 | -0.010 | -0.002 | -0.008** | -0.005 | -0.004 | -0.006* | -0.008 |
| | (0.003) | (0.005) | (0.008) | (0.004) | (0.003) | (0.007) | (0.003) | (0.003) | (0.005) |
| Observations | 21,324 | 21,324 | 21,324 | 46,33 | 46,33 | 46,33 | 34,409 | 34,409 | 34,409 |
| R-squared | 0.155 | 0.340 | 0.340 | 0.100 | 0.259 | 0.259 | 0.008 | 0.126 | 0.126 |
| ***Panel C: Girls*** | | | | | | | | | |
| public_utero2010 | 0.003 | -0.008 | -0.006 | 0.010** | 0.004 | 0.003 | 0.005 | 0.005 | 0.007 |
| | (0.007) | (0.006) | (0.007) | (0.004) | (0.004) | (0.006) | (0.004) | (0.005) | (0.007) |
| public_utero2009 | | | 0.006 | | | -0.003 | | | 0.005 |
| | | | (0.009) | | | (0.006) | | | (0.008) |
| public_utero2008 | | | 0.000 | | | -0.001 | | | -0.000 |
| | | | (0.008) | | | (0.006) | | | (0.007) |
| Public | -0.012*** | -0.013* | -0.015** | -0.009*** | -0.010*** | -0.009* | -0.010*** | -0.010** | -0.012 |
| | (0.003) | (0.007) | (0.006) | (0.002) | (0.003) | (0.005) | (0.003) | (0.005) | (0.007) |
| Observations | 20,046 | 20,046 | 20,046 | 43,261 | 43,261 | 43,261 | 32,081 | 32,081 | 32,081 |
| R-squared | 0.106 | 0.307 | 0.307 | 0.079 | 0.233 | 0.233 | 0.006 | 0.117 | 0.117 |
| Controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year&County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: child gender, gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 2.8: Probability of a live birth being male. Publicly employed vs. Housewife mothers

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ***Panel A: All mothers*** | | | | | | | | | |
| public_utero2010 | -0.030** | -0.033** | -0.034 | -0.009 | -0.009 | -0.007 | -0.008 | -0.006 | 0.003 |
| | (0.014) | (0.014) | (0.020) | (0.009) | (0.008) | (0.009) | (0.011) | (0.011) | (0.017) |
| public_utero2009 | | | 0.011 | | | -0.002 | | | 0.015 |
| | | | (0.025) | | | (0.012) | | | (0.014) |
| public_utero2008 | | | -0.014 | | | 0.008 | | | 0.013 |
| | | | (0.022) | | | (0.010) | | | (0.020) |
| public | 0.007 | 0.016 | 0.017 | -0.005 | -0.004 | -0.006 | -0.007 | -0.006 | -0.016 |
| | (0.007) | (0.013) | (0.020) | (0.005) | (0.011) | (0.013) | (0.007) | (0.010) | (0.015) |
| Observations | 27,401 | 27,401 | 27,401 | 57,318 | 57,318 | 57,318 | 41,997 | 41,997 | 41,997 |
| R-squared | 0.004 | 0.020 | 0.020 | 0.002 | 0.021 | 0.021 | 0.002 | 0.024 | 0.024 |
| ***Panel B: High-educated mothers*** | | | | | | | | | |
| public_utero2010 | -0.042** | -0.045*** | -0.057* | -0.017 | -0.019 | -0.028* | 0.003 | -0.000 | 0.007 |
| | (0.018) | (0.017) | (0.029) | (0.013) | (0.013) | (0.015) | (0.013) | (0.014) | (0.018) |
| public_utero2009 | | | -0.003 | | | -0.020 | | | 0.007 |
| | | | (0.032) | | | (0.015) | | | (0.019) |
| public_utero2008 | | | -0.032 | | | -0.005 | | | 0.013 |
| | | | (0.032) | | | (0.017) | | | (0.024) |
| public | 0.010 | 0.003 | 0.015 | -0.007 | -0.010 | -0.002 | -0.004 | 0.000 | -0.006 |
| | (0.009) | (0.012) | (0.024) | (0.008) | (0.008) | (0.013) | (0.006) | (0.007) | (0.014) |
| Observations | 14,088 | 14,088 | 14,088 | 29,619 | 29,619 | 29,619 | 21,272 | 21,272 | 21,272 |
| R-squared | 0.006 | 0.028 | 0.028 | 0.003 | 0.028 | 0.028 | 0.004 | 0.029 | 0.029 |
| Controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year&County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies and also birth weight; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 2.9: Cohort size at locality level. Publicly employed vs. Housewife mothers

| | *1st Trimester* | | *2nd Trimester* | | *3rd Trimester* | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: Male cohort size, all mothers** | | | | | | |
| public_utero2010 | -0.365 | 0.144 | 1.058 | 2.321* | 0.673 | 0.678 |
| | (0.716) | (0.856) | (1.032) | (1.213) | (0.675) | (1.000) |
| public_utero2009 | | 1.369 | | 2.216 | | 0.093 |
| | | (0.975) | | (1.322) | | (0.678) |
| public_utero2008 | | 0.154 | | 1.498 | | -0.078 |
| | | (1.130) | | (0.959) | | (1.145) |
| public | -7.758*** | -8.259*** | -16.308*** | -17.553*** | -13.148*** | -13.153*** |
| | (1.501) | (1.492) | (2.590) | (2.601) | (1.855) | (2.038) |
| Observations | 1,435 | 1,435 | 1,678 | 1,678 | 1,552 | 1,552 |
| R-squared | 0.846 | 0.847 | 0.867 | 0.867 | 0.856 | 0.856 |
| **Panel B: Male cohort size, highly educated mothers** | | | | | | |
| public_utero2010 | -1.630* | -1.228 | -1.314 | -0.709 | -0.102 | -0.181 |
| | (0.828) | (0.870) | (1.036) | (0.949) | (0.566) | (0.785) |
| public_utero2009 | | 0.720 | | 1.148 | | -0.163 |
| | | (0.797) | | (1.130) | | (0.767) |
| public_utero2008 | | 0.474 | | 0.643 | | -0.074 |
| | | (0.932) | | (0.690) | | (0.820) |
| public | 4.630* | 4.231* | 6.543* | 5.945 | 4.638* | 4.716* |
| | (2.415) | (2.275) | (3.836) | (3.552) | (2.606) | (2.547) |
| Observations | 1,127 | 1,127 | 1,38 | 1,38 | 1,286 | 1,286 |
| R-squared | 0.734 | 0.734 | 0.761 | 0.761 | 0.761 | 0.761 |
| **Panel C: Female cohort size, highly educated mothers** | | | | | | |
| public_utero2010 | -0.756 | -0.280 | -0.415 | 0.270 | -0.061 | -0.455 |
| | (0.771) | (0.997) | (0.551) | (0.608) | (0.528) | (0.751) |
| public_utero2009 | | 0.547 | | 1.589* | | -0.488 |
| | | (0.770) | | (0.865) | | (0.539) |
| public_utero2008 | | 0.869 | | 0.440 | | -0.694 |
| | | (0.834) | | (0.627) | | (0.891) |
| public | 4.260* | 3.786 | 6.470* | 5.795 | 4.591* | 4.983* |
| | (2.244) | (2.494) | (3.669) | (3.598) | (2.545) | (2.837) |
| Observations | 1,127 | 1,127 | 1,38 | 1,38 | 1,286 | 1,286 |
| R-squared | 0.747 | 0.747 | 0.760 | 0.760 | 0.758 | 0.758 |

Notes: The dependent variable is aggregated at locality, gender of the child, trimester of gestation and maternal occupation level. Controls include: birth year fixed effects, locality fixed effects and county specific time trends. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

Table 2.10: Mothers' Fixed Effects

| | Low Birth Weight | | | | Probability of a male birth | | | |
|---|---|---|---|---|---|---|---|---|
| | (1a) | (2a) | (3a) | (4a) | (1b) | (2b) | (3b) | (4b) |
| Exposed sibling | -0.063** | -0.062** | -0.076** | -0.056 | -0.037 | -0.050 | -0.097 | -0.249* |
| | (0.021) | (0.021) | (0.092) | (0.054) | (0.069) | (0.069) | (0.086) | (0.149) |
| Controls | no | yes | yes | yes | no | yes | yes | yes |
| Family FE | no | no | yes | yes | no | no | yes | yes |
| Month of birth FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Time Trend | yes | yes | yes | yes | yes | yes | yes | yes |
| Observations | 3,819 | 3,819 | 3,819 | 2,603 | 3,819 | 3,819 | 3,819 | 2,603 |
| No of groups | | | 1,819 | 1,788 | | | 1,819 | 1,788 |
| R-squared | 0.004 | 0.009 | 0.001 | 0.001 | 0.001 | 0.005 | 0.001 | 0.001 |
| Mean dep. var. | 0.041 | 0.041 | 0.041 | | 0.532 | 0.532 | 0.532 | |

Notes: All regressions are based on the urban sample of publicly employed mothers (based on their 2010 employment status); in columns (1a)-(4a) we consider look at the low birth weight and our controls include a gender dummy, pregnancy order, assistance at birth, gestation month of the first gynaecological visit; parents characteristics: the age of mother and father at conception, calendar month of birth dummies and a time trend. In columns (1b)-(4b) we show the probability that a birth is a male. Controls include the above (except gender) and gestation (in weeks) at birth. These specifications are based on the mother's status at the time of birth in 2010. Columns (4a) and (4b) are on the subsample of highly educates women. Source: Authors' calculation using the 2003-2010 Vital Statistics. Robust standard errors shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

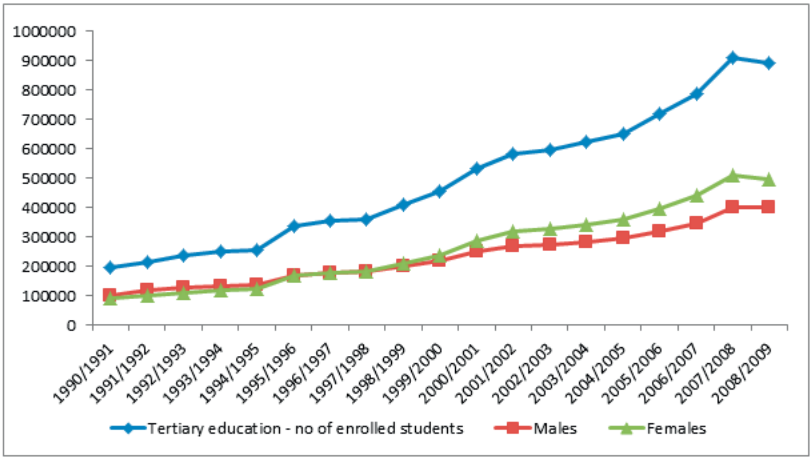
Table 2.11: Low Birth weight. Publicly employed with employed husbands vs. Housewives with husbands with occupational status different from employed

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ***Panel A: All*** | | | | | | | | | |
| public_utero2010 | -0.011 | -0.017** | -0.005 | 0.002 | -0.008 | -0.010 | 0.002 | -0.001 | 0.002 |
| | (0.009) | (0.008) | (0.010) | (0.007) | (0.005) | (0.008) | (0.009) | (0.008) | (0.010) |
| public_utero2009 | | | 0.022* | | | 0.001 | | | 0.006 |
| | | | (0.012) | | | (0.008) | | | (0.007) |
| public_utero2008 | | | 0.008 | | | -0.008 | | | 0.001 |
| | | | (0.009) | | | (0.006) | | | (0.006) |
| public | -0.071*** | -0.033* | -0.044** | -0.062*** | -0.041*** | -0.039*** | -0.056*** | -0.044*** | -0.046*** |
| | (0.007) | (0.020) | (0.021) | (0.005) | (0.011) | (0.012) | (0.006) | (0.013) | (0.015) |
| Observations | 17,537 | 17,537 | 17,537 | 36,121 | 36,121 | 36,121 | 26,044 | 26,044 | 26,044 |
| R-squared | 0.159 | 0.354 | 0.355 | 0.109 | 0.283 | 0.283 | 0.024 | 0.155 | 0.155 |
| ***Panel B: Boys*** | | | | | | | | | |
| public_utero2010 | -0.029** | -0.029** | -0.019 | -0.001 | -0.008 | -0.013 | 0.002 | -0.002 | 0.009 |
| | (0.012) | (0.012) | (0.012) | (0.010) | (0.007) | (0.011) | (0.012) | (0.011) | (0.013) |
| public_utero2009 | | | 0.015 | | | -0.005 | | | 0.017** |
| | | | (0.012) | | | (0.010) | | | (0.008) |
| public_utero2008 | | | 0.010 | | | -0.011 | | | 0.011 |
| | | | (0.012) | | | (0.012) | | | (0.008) |
| public | -0.053*** | -0.010 | -0.019 | -0.057*** | -0.030** | -0.024* | -0.051*** | -0.057*** | -0.067*** |
| | (0.007) | (0.022) | (0.024) | (0.006) | (0.012) | (0.013) | (0.005) | (0.017) | (0.018) |
| Observations | | | | | | | | | |
| R-squared | 8,919 | 8,919 | 8,919 | 18,413 | 18,413 | 18,413 | 13,324 | 13,324 | 13,324 |
| ***Panel C: Girls*** | | | | | | | | | |
| public_utero2010 | 0.008 | -0.002 | 0.010 | 0.005 | -0.009 | -0.008 | 0.001 | 0.001 | -0.004 |
| | (0.016) | (0.013) | (0.019) | (0.008) | (0.008) | (0.010) | (0.009) | (0.009) | (0.012) |
| public_utero2009 | | | 0.027 | | | 0.007 | | | -0.005 |
| | | | (0.021) | | | (0.011) | | | (0.010) |
| public_utero2008 | | | 0.002 | | | -0.006 | | | -0.008 |
| | | | (0.016) | | | (0.009) | | | (0.009) |
| public | -0.090*** | -0.052** | -0.063** | -0.067*** | -0.053*** | -0.053*** | -0.061*** | -0.031 | -0.026 |
| | (0.009) | (0.024) | (0.029) | (0.006) | (0.016) | (0.017) | (0.009) | (0.020) | (0.021) |
| Observations | 8,618 | 8,618 | 8,618 | 17,708 | 17,708 | 17,708 | 12,72 | 12,72 | 12,72 |
| R-squared | 0.152 | 0.337 | 0.337 | 0.102 | 0.264 | 0.265 | 0.030 | 0.156 | 0.156 |
| Controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year&County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: child gender, gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynaecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses (42 clusters). $***p < 0.01, **p < 0.05, *p < 0.10$.

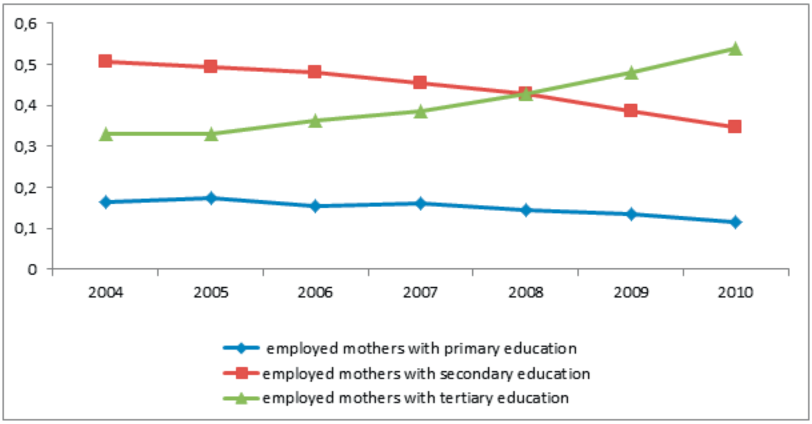
# Appendix A

Figure 2.4: Higher education enrollment in Romania



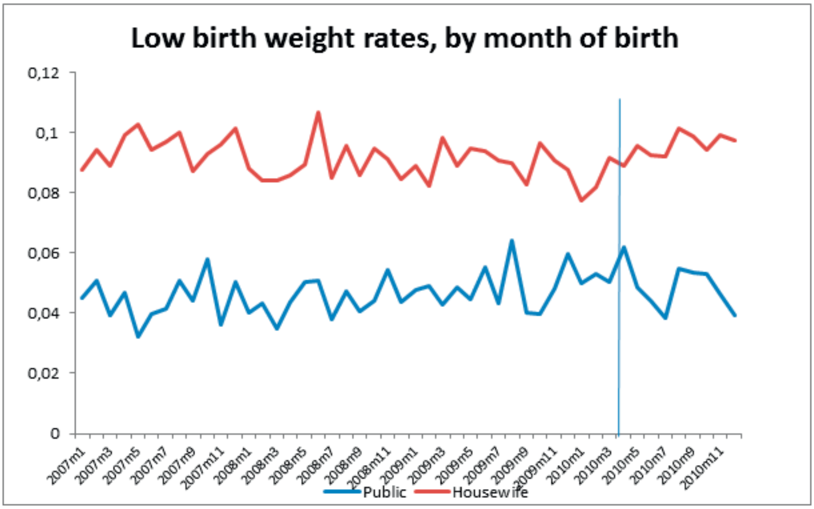

Notes: Source Statistics Romania.

Figure 2.5: Educational level of employed mothers



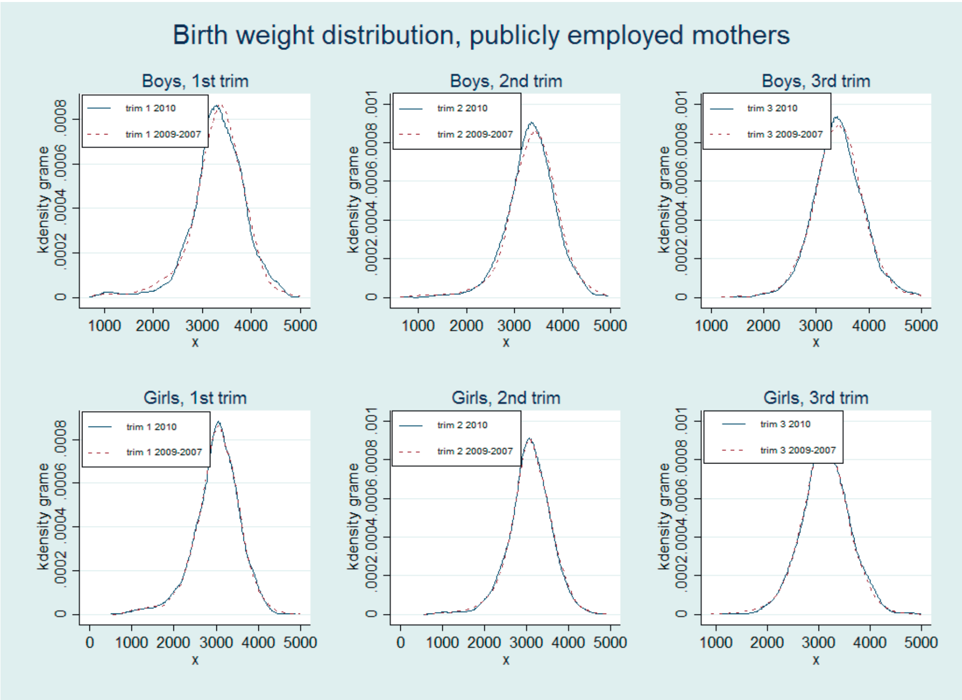

Notes: Source Statistics Romania.

Figure 2.6: Low birth weight incidence, by month of birth




Notes: Authors' calculations using the 2007-2010 VSN.

Figure 2.7: Birth weight distributions for children of publicly employed mothers




Notes: Authors' calculations using the 2007-2010 VSN.

Table 2.12: Descriptive statistics,urban and rural sample

| | | *All* | | | | *Publicly employed* (20-80)* | | | | *Housewives* | | | | *Employed* | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 2007 | 2008 | 2009 | 2010 | 2007 | 2008 | 2009 | 2010 | 2007 | 2008 | 2009 | 2010 | 2007 | 2008 | 2009 | 2010 |
| ***Mother's characteristics at childbirth*** | | | | | | | | | | | | | | | | | |
| Age | | 26,631 | 26,74 | 26,916 | 27,203 | 31,881 | 32,292 | 32,789 | 31,87 | 24,965 | 24,901 | 25,056 | 25,25 | 28,424 | 28,628 | 28,833 | 29,162 |
| Education | Secondary | 0,44 | 0,432 | 0,409 | 0,389 | 0,005 | 0,012 | 0,009 | 0,02 | 0,7 | 0,695 | 0,682 | 0,674 | 0,157 | 0,142 | 0,13 | 0,114 |
| | High-school | 0,361 | 0,348 | 0,34 | 0,322 | 0,001 | 0,001 | 0,001 | 0,006 | 0,279 | 0,283 | 0,289 | 0,289 | 0,448 | 0,42 | 0,382 | 0,34 |
| | Higher ed. | 0,199 | 0,22 | 0,251 | 0,289 | 0,995 | 0,987 | 0,99 | 0,974 | 0,021 | 0,022 | 0,03 | 0,037 | 0,394 | 0,438 | 0,488 | 0,546 |
| Urban | | 0,547 | 0,551 | 0,552 | 0,559 | 0,786 | 0,734 | 0,724 | 0,594 | 0,354 | 0,354 | 0,347 | 0,35 | 0,734 | 0,731 | 0,734 | 0,742 |
| Married | | 0,745 | 0,733 | 0,729 | 0,728 | 0,949 | 0,937 | 0,92 | 0,904 | 0,627 | 0,606 | 0,595 | 0,587 | 0,891 | 0,885 | 0,886 | 0,886 |
| Ethnicity: | Romanian | 0,917 | 0,914 | 0,92 | 0,918 | 0,956 | 0,948 | 0,942 | 0,941 | 0,903 | 0,901 | 0,909 | 0,905 | 0,93 | 0,931 | 0,937 | 0,936 |
| | Hungarian | 0,049 | 0,049 | 0,046 | 0,046 | 0,036 | 0,05 | 0,049 | 0,052 | 0,035 | 0,033 | 0,032 | 0,031 | 0,064 | 0,063 | 0,058 | 0,058 |
| | Other | 0,034 | 0,037 | 0,034 | 0,037 | 0,008 | 0,001 | 0,009 | 0,007 | 0,062 | 0,066 | 0,059 | 0,064 | 0,006 | 0,006 | 0,006 | 0,006 |
| | Prenatal control | 0,848 | 0,85 | 0,821 | 0,79 | 0,934 | 0,928 | 0,877 | 0,826 | 0,805 | 0,813 | 0,791 | 0,775 | 0,914 | 0,911 | 0,877 | 0,824 |
| | No. of births | 1,822 | 1,808 | 1,818 | 1,833 | 1,58 | 1,609 | 1,67 | 1,726 | 2,178 | 2,196 | 2,21 | 2,246 | 1,505 | 1,496 | 1,502 | 1,508 |
| | No. of living children | 1,826 | 1,809 | 1,816 | 1,833 | 1,585 | 1,613 | 1,669 | 1,726 | 2,181 | 2,195 | 2,206 | 2,245 | 1,512 | 1,5 | 1,5 | 1,508 |
| | Hospital delivery | 0,985 | 0,983 | 0,982 | 0,983 | 0,999 | 0,999 | 0,998 | 0,998 | 0,983 | 0,982 | 0,984 | 0,984 | 0,998 | 0,997 | 0,997 | 0,997 |
| ***Child's characteristics at birth*** | | | | | | | | | | | | | | | | | |
| Girl | | 0,486 | 0,483 | 0,485 | 0,487 | 0,491 | 0,482 | 0,488 | 0,493 | 0,487 | 0,486 | 0,487 | 0,487 | 0,484 | 0,479 | 0,483 | 0,488 |
| | Birth weight | 3206,466 | 3215,966 | 3217,471 | 3207,87 | 3315,183 | 3323,001 | 3308,198 | 3299,508 | 3144,569 | 3155,117 | 3161,942 | 3151,45 | 3278,428 | 3289,368 | 3281,505 | 3271,363 |
| | Low birth weight | 0,071 | 0,068 | 0,067 | 0,071 | 0,044 | 0,043 | 0,047 | 0,047 | 0,091 | 0,089 | 0,086 | 0,09 | 0,053 | 0,049 | 0,05 | 0,052 |
| | Gestation duration (weeks) | 38,859 | 38,763 | 38,772 | 38,764 | 38,908 | 38,772 | 38,753 | 38,761 | 38,787 | 38,693 | 38,717 | 38,723 | 38,949 | 38,839 | 38,85 | 38,819 |
| | Premature delivery | 0,077 | 0,077 | 0,077 | 0,074 | 0,056 | 0,057 | 0,061 | 0,059 | 0,088 | 0,09 | 0,088 | 0,085 | 0,061 | 0,061 | 0,059 | 0,06 |
| | ***No. observations*** | 140250 | 144256 | 144782 | 136331 | 13403 | 13982 | 14201 | 13284 | 62818 | 62709 | 62909 | 57588 | 65743 | 68117 | 69168 | 65881 |

Notes: Mean values for pregnancies in utero at May 7th, in each corresponding year, that resulted in live births. Source: Authors' calculations using the VSN files for 2007, 2008, 2009 and 2010. * "Publicly employed (20-80)" refers to the women classified as publicly employed based on their predicted probabilities of working in the public sector, 20-80 split (see Section 4 for a detailed description).

Table 2.13: Selection into fertility, DD specification

| VARIABLES | (1)<br>Secondary<br>education<br>or less | (2)<br>High school<br>education | (3)<br>Higher<br>gher education | (4)<br>Age | (5)<br>Married | (6)<br>Unemployed<br>father |
|---|---|---|---|---|---|---|
| pub80_utero10 | 0.045*** | 0.009 | -0.055*** | -1.196*** | -0.028** | -0.008** |
| | (0.011) | (0.009) | (0.005) | (0.281) | (0.012) | (0.004) |
| pub80_utero09 | 0.020** | 0.003 | -0.023*** | -0.055 | -0.011 | -0.005 |
| | (0.009) | (0.009) | (0.003) | (0.274) | (0.010) | (0.004) |
| pub80_utero08 | 0.018** | -0.001 | -0.017*** | 0.304 | 0.006 | -0.004 |
| | (0.009) | (0.008) | (0.004) | (0.218) | (0.008) | (0.005) |
| Public | -0.587*** | -0.361*** | 0.948*** | 12.865*** | 0.382*** | -0.011*** |
| | (0.010) | (0.009) | (0.003) | (0.440) | (0.016) | (0.003) |
| Observations | 126,716 | 126,716 | 126,716 | 126,716 | 126,716 | 126,716 |
| R-squared | 0.312 | 0.163 | 0.833 | 0.250 | 0.157 | 0.054 |

Notes: Controls include: child gender, gestational age at birth in weeks; 42 county dummies, 9 month of birth dummies, birth year fixed effects and county specific time trends. Robust standard errors clustered at the county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10

Table 2.14: Low birth weight: Publicly employed vs. housewives mothers, full sample

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| ***Panel A: All*** | | | | | | | | | |
| public_utero2010 | -0.009* | -0.015*** | -0.009 | 0.004 | -0.002 | -0.004 | -0.001 | -0.002 | -0.002 |
| | (0.005) | (0.005) | (0.006) | (0.003) | (0.003) | (0.003) | (0.005) | (0.005) | (0.006) |
| public_utero2009 | | | 0.011 | | | -0.002 | | | 0.003 |
| | | | (0.007) | | | (0.004) | | | (0.004) |
| public_utero2008 | | | 0.004 | | | -0.004 | | | -0.004 |
| | | | (0.007) | | | (0.004) | | | (0.004) |
| Public | -0.047*** | -0.012* | -0.017** | -0.045*** | -0.012*** | -0.010*** | -0.040*** | -0.016*** | -0.016*** |
| | (0.003) | (0.007) | (0.007) | (0.003) | (0.004) | (0.003) | (0.002) | (0.003) | (0.005) |
| Observations | 64,593 | 64,593 | 64,593 | 135,298 | 135,298 | 135,298 | 100,987 | 100,987 | 100,987 |
| R-squared | 0.134 | 0.318 | 0.318 | 0.088 | 0.251 | 0.251 | 0.014 | 0.140 | 0.140 |
| ***Panel B: Boys*** | | | | | | | | | |
| public_utero2010 | -0.020*** | -0.020*** | -0.015* | -0.001 | -0.006 | -0.005 | -0.000 | -0.002 | -0.004 |
| | (0.007) | (0.007) | (0.007) | (0.004) | (0.004) | (0.005) | (0.007) | (0.006) | (0.008) |
| public_utero2009 | | | 0.010 | | | 0.002 | | | 0.001 |
| | | | (0.007) | | | (0.006) | | | (0.006) |
| public_utero2008 | | | 0.006 | | | 0.000 | | | -0.008* |
| | | | (0.007) | | | (0.005) | | | (0.005) |
| Public | -0.036*** | -0.007 | -0.012 | -0.038*** | -0.012*** | -0.013** | -0.036*** | -0.019*** | -0.017** |
| | (0.005) | (0.009) | (0.011) | (0.003) | (0.004) | (0.006) | (0.003) | (0.005) | (0.007) |
| Observations | 33,095 | 33,095 | 33,095 | 69,371 | 69,371 | 69,371 | 51,865 | 51,865 | 51,865 |
| R-squared | 0.143 | 0.332 | 0.332 | 0.100 | 0.267 | 0.267 | 0.013 | 0.145 | 0.145 |
| ***Panel C: Girls*** | | | | | | | | | |
| public_utero2010 | 0.002 | -0.009 | -0.004 | 0.010** | 0.002 | -0.004 | -0.001 | -0.002 | 0.000 |
| | (0.008) | (0.007) | (0.009) | (0.005) | (0.004) | (0.006) | (0.005) | (0.005) | (0.007) |
| public_utero2009 | | | 0.011 | | | -0.007 | | | 0.005 |
| | | | (0.011) | | | (0.006) | | | (0.006) |
| public_utero2008 | | | 0.003 | | | -0.009 | | | 0.000 |
| | | | (0.013) | | | (0.006) | | | (0.006) |
| Public | -0.058*** | -0.016* | -0.021* | -0.052*** | -0.011* | -0.006 | -0.044*** | -0.012** | -0.014* |
| | (0.005) | (0.008) | (0.012) | (0.004) | (0.006) | (0.007) | (0.004) | (0.006) | (0.007) |
| Observations | 31,498 | 31,498 | 31,498 | 65,927 | 65,927 | 65,927 | 49,122 | 49,122 | 49,122 |
| R-squared | 0.130 | 0.306 | 0.306 | 0.079 | 0.238 | 0.238 | 0.016 | 0.136 | 0.136 |

Notes: Controls include: child gender, gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10

Table 2.15: Probability of a birth being male: Publicly employed vs. Privately employed mothers

| | *1st Trimester* | | | *2nd Trimester* | | | *3rd Trimester* | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| public_utero2010 | -0.000 | -0.005 | -0.000 | 0.008 | 0.007 | 0.007 | -0.010 | -0.012 | -0.015 |
| | (0.010) | (0.011) | (0.012) | (0.010) | (0.009) | (0.011) | (0.009) | (0.008) | (0.012) |
| public_utero2009 | | | 0.007 | | | -0.001 | | | -0.008 |
| | | | (0.024) | | | (0.011) | | | (0.011) |
| public_utero2008 | | | 0.007 | | | 0.000 | | | -0.001 |
| | | | (0.021) | | | (0.009) | | | (0.015) |
| Public | -0.008 | -0.008 | -0.013 | -0.009* | -0.015** | -0.015* | -0.006 | 0.000 | 0.003 |
| | (0.006) | (0.008) | (0.014) | (0.005) | (0.006) | (0.008) | (0.006) | (0.008) | (0.012) |
| Observations | 41,37 | 41,37 | 41,37 | 89,591 | 89,591 | 89,591 | 66,49 | 66,49 | 66,49 |
| R-squared | 0.002 | 0.026 | 0.026 | 0.001 | 0.025 | 0.025 | 0.001 | 0.030 | 0.030 |
| Controls | No | Yes | Yes | No | Yes | Yes | No | Yes | Yes |
| Year&County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| County trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: child gender, gestational age at birth in weeks; mother's age at birth and its square, mother's education dummies, ethnicity dummies, marital status dummy, child's parity, number of children alive, number of antenatal visits, gestation month of the first gynecological visit, an indicator for home delivery, father's age and its square, father's employment status dummies; 42 county dummies, 9 month of birth dummies; female unemployment rate in the month of conception for each county and year of birth. Robust standard errors clustered at the county level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

# Appendix B

## The likelihood of maternal employment in the public sector

Employment in the public sector is the key variable in our identification strategy. However the 2007-2010 Vital Statistics Natality (VSN) files contain information only on the employment status of the mother without specification of the sector, whether private or public. We address this problem by using the 2007-2010 Romanian Household Budget Survey (RHBS), a nationwide representative survey and the main tool of Statistics Romania of assessing representative population expenditures and revenues, which provides detailed socio-economic information on every member of the household, to construct a characteristics-based likelihood of employment in the public sector for each mother. The RHBS has the same employment categories as VSN and also disentangles between public and private sector employment.

## B1. Main specification

The RHBS records the occupational status for each household member with the same categories as the VSN except that, for the employed members, we know the sector of employment, whether public or private. We start by estimating the simple conditional probability that an employed woman works in the public (vs. the private) sector using the RHBS data. In particular, we estimate a reduced form Probit model separately for each year on the restricted sample of employed women of fertile age (16 to 45), and include as explanatory variables all the socio-economic characteristics of mothers that are available in the VSN: age (and age square), marital status (married, divorced, widow, single), education (no schooling, primary, secondary, high school, technical college, post high-school, higher and above), ethnicity (Romanian, Hungarian, other), number of living children, father's age and father's occupational status (employed, entrepreneur, self-employed, unemployed, pensioner, other situation), region of residence and an indicator for urban area. To control for the important differences between urban and rural areas we also include interaction terms between mother's education and the urban indicator. We cluster the standard errors at the region level. For each RHBS data set we use in these regressions the corresponding frequency weights as suggested by Statistics Romania, thus, ending up with a sample a bit over 2 million observations. The coefficients with and without using the frequency weights are very similar and not statistical different (available upon request). These results are shown in Table 2.16 below.

The goodness of fit of this model suggest that 75% of our observations are correctly classified as public or private using the Cramer maximum probability rule. In the next section (Appendix B2) we address several concerns related to this estimation.

Next, we proceed to doing out-of-sample predictions for the VSN dataset and obtain a predicted probability of public employment for each employed mother. In the last step we use the predicted probabilities to split the VSN sample of employed mothers into most and least likely employed in the public sector (following the rule discussed in the paper).

## B2. Robustness checks

We proceed next to some robustness checks of our results in the previous section. First, we estimate a similar model to the one shown above where we combine all years of the RHBS data from 2007-2010, include year fixed effects and county time trends and obtain a new set of predicted probabilities. Using these predictions and our results (available upon request) are very similar to our main results.

Secondly, a potential concern is that the predicted probabilities for the publicly employed women of fertile age but who are not necessarily mothers are not representative for the publicly employed mothers in VSN. One way to deal with this is to estimate the probability of being employed in the public sector on the restricted sample of employed mothers of fertile age in the 2007-2010 RHBS to obtain the predicted probability for the VSN mothers. Using these new predicted probabilities more than 96% of the VSN mothers are identically classified as public when we employ the 80-20 split.

Finally, we estimate the same as above using an extended Probit specification where we also include all relevant variables available in the RHBS, but not available in the VSN data: type of employment contract (permanent or temporary), the in-kind benefits received at the workplace (such as telephone or company car), analogous to an exclusion restriction. Since we continue to assign probabilities to the mothers in the VSN only on their observable characteristics included in the VSN and the additional covariates are kept at the RHBS sample mean. Using these predicted probabilities, our main results are very similar to the ones in our main analysis (results available upon request).

## B3. Validity of RHBS for out of sample predictions

We showed that in-sample predictions are reliable for the household survey sample using the classification table; however, it is important to understand whether they would be as reliable out of sample, when applied to the mothers in the Natality files. To verify this assumption, we proceed to predict the housewife status versus the employed status using the RHBS and doing out of sample predictions using the VSN files. Since we know the true occupational status, whether housewife or employed, for the women in the VSN, we can verify whether the procedure of out of sample prediction is sensible or not. We use the same specification for the first stage Probit in the RHBS sample, where the dependent variable is 1 if the woman is housewife and 0 if she is employed (irrespective of the sector of employment). We then make the out of sample predictions for women in the VSN sample. We use the predicted probability of being a housewife and use the median probability as a threshold, since aggregate official statistics show that each year there is approximately equal number of employed and housewife women giving birth. We then verify how many true housewives were classifies as housewives based on their predicted probability, and how many were classified as employed. We do the same for true employed women. Our procedure classifies correctly around 89% of the housewives, and around 67% of the employed mothers, which suggests that the RHBS can be reliably used to make out of sample predictions for the VSN sample.

## B4. Bootstrapped standard errors

We define our treatment group based on a generated regressor -i.e we use the prediction of the model using RHBS data as a regressor in the model using VSN data. Although this approach leads to a consistent estimate of our parameter of interest (on the interaction term Public*Utero2010), the estimated covariance matrix for the second model needs to be adjusted to take into account the variability of the generated regressor, as shown by [Murphy and Topel, 2002]. We use bootstrapping for this correction and estimate the standard errors of the parameters of interest using 500 replications.

To account for the variability of the generated regressor, we employ a two stage bootstrapping procedure to obtain the bootstrapped standard errors of the estimate of our parameter of interest $\beta_2$ from Equation 1. In the first step, we draw with replacement a sample from the RHBS, and estimate the probit model of public employment conditional on employment; using the estimated coefficients, we employ the out of sample estimations using the VSN dataset, and each employed mothers is assigned a predicted probability of public employment. Using the sample's 80th percentile of the predicted public employment, we define the treatment group as a the employed women with predicted probabilities above this threshold (hence we make use of the generated regressor). In the second step, we then draw with replacement a sample from the VSN, and estimate the regression of interest where the independent variable is the low birth weight indicator and the main explanatory variable is the Public sector of employment dummy interacted with the indicator for being in utero in 2010 at the time of the policy change announcement. We save the estimated coefficient of interest, $\hat{\beta_2}^*$ from Equation 2.1. We repeat the first and second step 500 times, which gives us 500 replications of our statistics of interest, $\hat{\beta_2}^*$: $\hat{\beta}_{2,1}^*$ ... $\hat{\beta}_{2,500}^*$. We then apply the standard formula for estimating the variance:

$$s^2_{\hat{\beta}_2,boot} = \frac{1}{499}\sum_{b=1}^{500}(\hat{\beta}_{2,b}^* - \bar{\hat{\beta}}_2^*) \tag{2.2}$$

where

$$\bar{\hat{\beta}}_2^* = \frac{1}{500}\sum_{b=1}^{500}\hat{\beta}_{2,b}^* \tag{2.3}$$

$s_{\hat{\beta}_2,boot}$ is the bootstrapped standard error for the estimate of the parameter of interest $\beta_2$, and is presented in Table 2.5 in the main text alongside the robust standard error used in the main analysis.

Table 2.16: Reduced form Probit estimation

| VARIABLES | 2007 | 2008 | 2009 | 2010 |
|---|---|---|---|---|
| age | 0.135*** | 0.073* | 0.136*** | 0.055 |
| | (0.033) | (0.039) | (0.035) | (0.041) |
| age squared | -0.002*** | -0.001 | -0.002*** | -0.000 |
| | (0.001) | (0.001) | (0.001) | (0.001) |
| married | 0.101 | 0.098 | 0.051 | -0.010 |
| | (0.077) | (0.085) | (0.087) | (0.071) |
| divorced | -0.113 | 0.071 | -0.005 | 0.084 |
| | (0.077) | (0.102) | (0.107) | (0.100) |
| widowed | 0.164 | 0.098 | -0.193 | 0.071 |
| | (0.213) | (0.176) | (0.232) | (0.260) |
| secondary school | -0.376 | -0.256 | -0.148 | -0.074 |
| | (0.406) | (0.320) | (0.325) | (0.546) |
| professional school | -0.692* | -0.365 | -0.589* | -0.452 |
| | (0.411) | (0.323) | (0.319) | (0.532) |
| high school | -0.227 | 0.033 | -0.181 | -0.032 |
| | (0.404) | (0.320) | (0.333) | (0.524) |
| post high school | 0.710 | 0.920*** | 0.413 | 1.181** |
| | (0.447) | (0.351) | (0.359) | (0.591) |
| higher education | 1.332*** | 1.331*** | 0.947*** | 1.339** |
| | (0.418) | (0.304) | (0.328) | (0.534) |
| other situations | -0.645 | -0.453 | -0.423 | |
| | (0.692) | (0.842) | (0.609) | |
| Hungarian | -0.174** | -0.041 | -0.016 | -0.137 |
| | (0.084) | (0.072) | (0.112) | (0.103) |
| other ethnicity | 0.281 | -0.423* | 0.254 | 0.103 |
| | (0.177) | (0.219) | (0.163) | (0.188) |
| number of children | 0.057** | 0.057** | 0.060*** | 0.113*** |
| | (0.026) | (0.025) | (0.023) | (0.027) |
| urban | 0.020 | 0.646 | 0.174 | 0.562 |
| | (0.421) | (0.557) | (0.387) | (0.732) |
| secondary x urban | 0.232 | -0.709 | -0.192 | -0.793 |
| | (0.454) | (0.555) | (0.430) | (0.729) |
| professional x urban | -0.069 | -0.762 | -0.098 | -0.562 |
| | (0.440) | (0.561) | (0.381) | (0.735) |
| high-school x urban | -0.126 | -0.793 | -0.353 | -0.675 |
| | (0.421) | (0.557) | (0.387) | (0.759) |
| post-high-school x urban | -0.122 | -0.809 | -0.089 | -0.639 |
| | (0.483) | (0.568) | (0.435) | (0.790) |
| higher education x urban | -0.904** | -1.338** | -0.689* | -1.296* |
| | (0.460) | (0.526) | (0.400) | (0.716) |
| husbandâĂŹs age | 0.007 | 0.003 | 0.007 | 0.008 |
| | (0.005) | (0.004) | (0.004) | (0.005) |
| Husband occupation: | | | | |
| employed | -0.132 | -0.048 | -0.151 | 0.092 |
| | (0.085) | (0.084) | (0.097) | (0.084) |
| entrepreneur | -0.503** | -0.590** | -0.319 | 0.115 |
| | (0.235) | (0.246) | (0.274) | (0.218) |
| self-employed in non-agriculture | -0.106 | -0.112 | -0.117 | 0.039 |
| | (0.136) | (0.119) | (0.143) | (0.121) |
| self-employed in agriculture | -0.097 | 0.100 | 0.059 | 0.316** |
| | (0.166) | (0.138) | (0.165) | (0.134) |
| unemployed | -0.113 | -0.013 | -0.206 | 0.101 |
| | (0.115) | (0.128) | (0.153) | (0.103) |
| pensioner | -0.005 | -0.033 | -0.132 | 0.091 |
| | (0.134) | (0.134) | (0.111) | (0.219) |
| other situations | -0.255 | 0.113 | -0.044 | 0.517** |
| | (0.289) | (0.388) | (0.235) | (0.206) |
| region 2 | -0.203** | -0.163 | -0.208** | -0.085 |
| | (0.081) | (0.110) | (0.091) | (0.108) |
| region 3 | -0.191* | -0.296** | -0.223** | -0.279** |
| | (0.114) | (0.140) | (0.099) | (0.114) |
| region 4 | -0.001 | -0.172 | -0.090 | 0.059 |
| | (0.130) | (0.148) | (0.096) | (0.135) |
| region 5 | -0.197** | -0.290 | -0.342*** | -0.261** |
| | (0.093) | (0.188) | (0.118) | (0.127) |
| region 6 | -0.234** | -0.399*** | -0.362*** | -0.123 |
| | (0.109) | (0.112) | (0.123) | (0.112) |
| region 7 | -0.210** | -0.143 | -0.188** | -0.095 |
| | (0.098) | (0.119) | (0.094) | (0.143) |
| region 8 | -0.517*** | -0.381*** | -0.412*** | -0.220** |
| | (0.078) | (0.106) | (0.062) | (0.094) |
| Pseudo R2 | 0.1214 | 0.1066 | 0.1022 | 0.1305 |
| Observations | 2,156,214 | 2,205,766 | 2,156,058 | 2,041,875 |

Notes: Dependent variable Public is 1 if the woman is employed in the public sector and 0 if she is employed in the private sector. Authors' calculations using 2007-2010 Romanian Household Budget Surveys. Clustered standard errors at the region level shown in parentheses. $***p < 0.01, **p < 0.05, *p < 0.10$. 

# Paper II

# Chapter 3

## Bridging the Gap for Roma Women. The Effects of a Health Mediation Program on Roma Prenatal Care and Child Health at Birth

Simona Bejenariu* Andreea Mitrut†

**Abstract**

Roma, Europe's largest minority, face poverty, social exclusion and life-long inequalities. We analyze a large-scale public program that aimed to improve the health of pregnant Roma women and children, with the help of trained Roma health mediators. Using rich data from Romania we exploit the spatial and temporal variation in the implementation of the program and find large increases of the take-up of prenatal care services among Roma women, but no change in the probability of low birth weight or premature delivery. Our results show a decrease in the number of stillbirths and infant deaths. We investigate the potential mechanisms.

**JEL classification codes:** J13; J15; I14

**Keywords:** Roma ethnics; program take-up; health at birth; program evaluation

*Department of Economics, University of Gothenburg. simona.bejenariu@economics.gu.se

†Department of Economics, University of Gothenburg. andreea.mitrut@economics.gu.se;
We thank the Roma Center for Health Policies for providing the data on the program implementation. We also thank Hans Grönqvist, Randi Hjalmarsson, Mikael Lindahl, Björn Lindgren, Mikael Svensson, Måns Söderbom and seminar participants at University of Gothenburg and SWEGPEC for very helpful comments on earlier versions of this paper. Andreea Mitrut gratefully acknowledges support from Jan Wallanders and Tom Hedelius Fond.

## 3.1 Introduction

Roma ethnics, Europe's largest minority with over 10 million individuals, face poverty, social exclusion and life-long inequalities: about 90 percent live below national poverty lines, enrollment in primary education does not exceed 50 percent, more than half experienced discrimination in the past because of their ethnicity and, relative to non-Roma in Europe, they are four times more likely to be employed in unskilled jobs, three times less likely to afford prescription drugs, Roma children under the age of 14 are about five times less likely to be vaccinated and their average life expectancy is ten years less [European Union et al., 2013]. These staggering inequalities are already present at birth: in Romania, a country with one of the most numerous Roma communities,[1] in 2000 the low birth weight rate among Roma children was twice as large as for non-Roma children, more than 14% of Roma infants were born prematurely compared to 8% of non-Roma,[2] while the infant mortality rate of Roma ethnics was 50.6 per 1000 live births, compared to 26.9 per 1000 births for non-Roma. These disparities, which are large even compared to other disadvantaged ethnic groups, such as African-Americans in the US, are particularly worrisome since the economic literature has established that health at birth is a predictor of adult outcomes and of the outcomes of future generations [Black et al., 2007, Currie and Moretti, 2007, Royer, 2009] . Even more so, the health-induced inequalities start even before birth, in the prenatal period, and widen as the individual ages [Almond and Currie, 2011, Currie and Almond, 2011] . One of the potential causes for poor health at birth among Roma children is the low utilization of medical services during the prenatal period: in 2000, less than 40% of pregnant Roma women in Romania sought prenatal care. Moreover, recent surveys across EU member states showed that the average situation of Roma women in all areas of social life, such as education, employment, health and access to medical care, is more precarious than that of Roma men, mainly because of the hierarchical family structure and strong social norms [European Union and Agency for Fundamental Rights, 2014, Corsi et al., 2008, Voicu and Popescu, 2009].

This paper investigates a unique public health program designed to improve the health outcomes of Roma women and children: the Roma Health Mediation

[1]The Roma minority is the third largest ethnic group in Romania, with 619,000 (3.2% of the total population) self-identifying as Roma in the 2011 census, while unofficial estimates put the number of Roma in Romania at 2 million (`http://goo.gl/MwmwpW`).

[2]Source: Our calculations using year 2000 Romanian Vital Statistics Natality Files.

Program (RHM). The program was implemented gradually starting with 2002, in Romania, in localities with large Roma communities. The main goal of the RHM was to improve the health status of pregnant and postpartum Roma women, infants and children by providing basic health education and facilitating the communication between the Roma ethnics and healthcare practitioners, with the help of Roma health mediators. The mediators were women from the local communities, trained and employed by the Romanian Ministry of Health to: 1) explain to Roma pregnant women the necessity of prenatal care and inform them about the right to free preventive care during pregnancy, facilitate the verbal communication with family physicians, usually by accompanying them during the medical visit, and 2) increase awareness about family planning (particularly modern contraceptives use and decrease abortion) among Roma women and emphasize the importance of best practices regarding child rearing, such as breastfeeding and vaccination. In our empirical exercise we investigate the effects of this program on Roma prenatal care take-up rates and on child health at birth using rich Romanian Natality Files containing all registered live births with ethnicity information, over the period 2000-2008. We also investigate whether the program affected fertility, as measured by the cohort size, and other measures of health status in the cohort, captured by number of stillbirths and infant deaths, using information from Romanian Mortality Files.

Although the program underwent a substantial expansion in Romania and was also implemented in several other countries with large Roma minorities,[3] this is the first empirical investigation of the effects of the RHM program. Understanding the impact of this program is especially important as increased access to medical care during pregnancy and improved child health could reduce the health inequalities Roma children face even before birth, which, in turn, could weaken the intergenerational transmission of poor health and other associated outcomes. Moreover, although institutional efforts aimed at improving Roma socio-economic status have intensified, with up to 26.5 billion euros of EU funding available as of 2014 for ethnically targeted programs, this is, to our knowledge, the first quantitative evaluation of a health program that targeted Roma ethnics.

[3]The Roma Health Mediation program was subsequently implemented in Bulgaria (2003), Slovakia (2005), Moldova (2006), Serbia (2008), Macedonia (2009) and Ukraine (2010), following the Romanian model.

Our analysis also contributes to the literature regarding the causes of the low-take up of social programs despite high need and eligibility. In particular, because the RHM aimed at increasing the use of medical services that were available free of charge even before the implementation of the program, out study brings evidence on whether information provision can be an effective means of increasing take-up. A growing literature argues that factors such as the lack of knowledge about the program, hassle costs and procrastination are important barriers which explain the low take-up of welfare programs (see [Bertrand et al., 2006] for a review). Outreach programs were found to increase the information about already available services, reducing the non-price barriers to care and leading to increases in the take-up of welfare programs among marginalized individuals. [Currie and Grogger, 2002] find that administrative measures to encourage the use of prenatal care among Medicaid-eligible women have increased the take-up rates and reduced fetal deaths, especially for Black ethnics. [Aizer, 2007] also shows that outreach programs are successful in increasing the take-up rate of Medicaid among already eligible individuals, especially for minorities that face language barriers, and improve health status by increased use of preventive care and lower hospitalization rates for preventable diseases. However, to our knowledge there is no study which has addressed the issue of program take-up in the context of ethnically targeted programs.

Finally, as the RHM had a public health education component, the present study also contributes to the literature that investigates whether information and health education, especially for disadvantaged groups, can be effective in increasing health status and reducing disease burden. However, while this line of research indicates complementarities between health education and subsidies for health-care, with the effect of the information being stronger in the presence of price effects [Ashraf et al., 2013, Duflo et al., 2006], the RHM did not provide any price subsidies or (un)conditional transfers, which could reduce the effects on the health outcomes studied.

Because the RHM program was not randomly implemented, we exploit the spatial and temporal variation in the implementation of the program in a difference in difference strategy in which we consider all Roma children from the natality registers over the period 2000-2008, Our findings indicate that the program successfully improved the prenatal health-seeking behavior of Roma women, particularly so in rural, more traditional localities. Relative to children born before the program implementation in their locality of residence, we find a 7 and a 30 percentage point increase (13% and 56% of the mean) in the prenatal care rate for children born up to and more than two years after the implementation, respectively. Similarly, the estimated effect on the number of months under prenatal supervision is a half month increase (16% of the mean) for the children born up to two years after the program initiation, but a roughly two month increase (52% of the mean) for the children born more than two years afterward. These very large improvements in prenatal care related outcomes are not reflected in an improvement in the probability of low birth weight and premature delivery. However, our findings indicate a decrease in the numbers of stillborn children and infant mortality at the locality level. In urban areas and when we consider the full sample of localities the effects are largely in the same direction, but smaller in magnitude and not always significant statistically. Finally, using additional survey data we look at other outcomes, not collected in our registered data, but which are directly related to the program implementation. These findings suggest that after the program implementation, Roma women feel less discriminated in general and when seeking medical care in particular, they are less likely to use abortion and more likely to exclusively breastfeed their babies.

The potential mechanisms that are consistent with the pattern of the observed results entail that the RHM program effectively influenced the reproductive behavior of Roma women and also increased prenatal care rates of mothers who do conceive, which may have improved the survival rate of the marginal children of the presumably worse-off mothers who conceived. This would leave the average heath at birth in the cohort unchanged, but would lead to a smaller stillbirth rate for Roma infants, which is consistent with the decrease in the number of stillbirths and infant deaths we observe.

The rest of the paper is structured as follows. Section 3.2 presents the Roma Health Mediation program, with focus on the implementation process at the locality level. Section 3.3 describes the data and our identification strategy, the difference in difference specification in which we exploit the timing and geographical variation in program implementation. Section 3.4 presents our main results on outcomes relating to prenatal medical take-up and child health at birth, and at aggregate level on number of live births, stillbirths and infant mortality. Section 3.5 provides a series of robustness tests, including a falsification test in which we analyze the sample of Romanian ethnic children in the localities in which the RHM program was implemented, and a simulation exercise in which we allocate the initiation date of the program randomly among the localities. Section 3.6 brings additional evidence using survey data and Section 3.7 discusses the potential channels which could explain the results. Section 3.8 concludes our paper.

## 3.2 The Roma Health Mediators Program

### 3.2.1 The program's aims

The RHM national program was initiated by Romani CRISS, one of the most influential Roma NGO's in Romania, together with the Romanian Ministry of Health in 2002 and rolled out gradually in localities with large Roma communities, and it is still ongoing.[4]

The general aim of the program was to improve the health of Roma ethnics, and especially of pregnant and postpartum Roma women, infants and children, by facilitating access to health care services and offering basic health education. The program was carried out in the field by specially trained Roma women, called mediators, from the local communities. The gender and the ethnic component of the mediators were both essential for the program: health mediators were expected to approach sensitive issues, such as pre- and post-natal care and family planning, whereas in many Roma castes strong social norms forbid these discussions with/in the presence of men. A Roma woman from the local community would also be more easily accepted and effective due to a higher level of trust and an in-depth knowledge about specific local social norms, culture and circumstances.

To increase pre- and post-natal care among Roma women the mediators conducted home visits to Roma families to explain the necessity of medical check-ups and, if necessary, also accompanied Roma women to the healthcare practitioners, facilitating their communication with the doctors. This was an important objective of the program because previous evidence showed that social norms, lack of financial resources and perceived discrimination were the main reasons for not attending prenatal health appointments among Roma women. Language is often a barrier in seeking and receiving medical assistance, as a considerable share of Roma ethnics

[4]Most of the expansion occurred between 2002 and 2008, the period of our study. Before becoming a national program, the RHM was rolled as a pilot program in a small number of communities during 1999-2001. We cannot conduct a separate analysis for this period since we do not have access for the Natality Files for that period.

speaks Romani chib, the traditional language, unrelated to the official Romanian language spoken by family physicians. Additionally, the mediators were trained to inform pregnant Roma women about the right to free public medical insurance for children younger than 18 years of age and pregnant women with no without the payment of an insurance contribution. This is particularly important because most Roma women, especially in rural areas, are housewives with no formal employment and no other source of income. For pregnant and postpartum women, the mediators also promoted the importance of breastfeeding, healthy nutrition and basic information about reproductive health. Family planning and reproductive health became a more salient part of the program after 2006, when mediators in certain counties started to promote contraceptive use in order to reduce the intensive use of abortions as a fertility control method.[5] It is important to note that the mediators were not authorized to perform any medical act.

The mediators were mostly engaged in fieldwork, providing home outreach services by conducting house visits to the Roma ethnics, which reduced the potential self-selection of the individuals who received RHM counseling that would have occurred had the program not been designed as a home outreach program. Finally, the legislative requirements stipulated that one Roma woman health mediator should serve a population of 500 to 750 Roma individuals, counted as children up to 16 years of age and fertile age women. For more information about the RHM please see Appendix B and the World Health Organization report [World Health Organization and Regional Office for Europe, 2013].

### 3.2.2 Program implementation

In 2002 the nationwide implementation of the RHM program started in 42 localities and reached 281 localities in 2008, served by 419 employed RHM.[6] Figure 3.1 shows the evolution of the number of localities in which the program was implemented, while Figure 3.2 presents the timing and the geographical distribution of these localities. Although the program continued after 2008, in our empirical anal-

[5]The mediators did not have the authorization to perform any medical procedure or to distribute contraceptives. In our regressions, we will control for the counties in which the mediators had additional training for family planning.

[6]These numbers are likely to be slightly higher because the original database we used to identify the localities in which the program was implemented did not record the year of initiation of the program for 43 localities and the date of employment for 96 trained health mediators (either they were not employed, or their date of employment is missing from the records).

ysis we will not consider the period after 2008 since in 2009 the program was transferred from the authority of the Ministry of Health to the Local Councils of the localities and, as a consequence, the initiation rate decreased significantly, and there is evidence that the lack of funding related to the economic crisis affected the program in the localities which had previously entered the program.[7]

According to official information from the Ministry of Health, the selection of localities where the program was implemented was done by the Commission of Roma Minority within the Ministry of Health and considered the Ministry's budget constraint, the requests from the District Public Health Authorities and, most importantly, the collaboration capacity of Roma civil society in the proposed localities. Because the selection of the localities into the program was not random, we will include locality fixed effects and birth year fixed effects in our analysis. However, to understand the selection of localities into the program, we use a discrete-time hazard model of the probability of a locality being included in the RHM and the timing of implementation, as a function of a broad set of covariates presented in detail in Appendix B. Our results (Appendix B, Table 3.15) show that, indeed, variables reflecting Roma civic involvement and the Roma population are among the most important factors in determining the introduction of the program at locality level. When we only look at the localities that implemented the program up until 2008, the Roma civic involvement and variables such as share of employed women or share Roma with a low birth weight remain significant determinants of the timing of implementation (see Appendix B, Table 3.17).[8] In the next section we describe our identification and how we attempt to deal with the non-random aspect of the program implementation.

## 3.3 Data and methodology

### 3.3.1 Identification strategy

Because the RHM program was not randomly implemented across localities, we exploit the timing and the geographical variation in the program implementation in a difference-in-difference strategy. By comparing outcomes between localities

[7]The negative effects of the economic crisis were beginning to show and, due to the tightened budgets of the local authorities, a large number of health mediators were not re-employed by the local councils [World Health Organization and Regional Office for Europe, 2013].

[8]A similar pattern is also found when using a panel fixed effect approach (Appendix B, Table 3.18).

within same year, we control for unobserved cohort characteristics, whereas by comparing outcomes within the same locality between years we circumvent issues created by unobserved heterogeneity at the locality level which is constant over time. Because the program was designed to help Roma ethnics exclusively, in our main analysis we only consider the children whose ethnicity is registered as Roma. Ethnicity is declared by the parents when the birth is registered at the local authority. One concern is that we may not capture the entire Roma population.[9] We will come back to this issue shortly. Moreover, in our main specification we focus on the sample of localities that implemented the program until 2008 and exclude the localities that never implemented the program, but our results are not sensitive to this exclusion.[10] We also control for locality-specific time trends to allow for a differential development of the outcomes of interest at locality level and to control for unobserved locality characteristics that may evolve differently over time between localities.

We start by estimating the following specification:

$$Y_{ilt} = \alpha + \beta_1 Treated_{ilt} + \gamma' X_{ilt} + \theta_l + \theta_t + \theta_m + \theta_l t + \epsilon_{ilt} \tag{3.1}$$

where $i$ indexes a Roma mother/child, in locality $l$, in year $t$. $Treated_{ilt}$ is the treatment indicator which is 1 if the program had been implemented for at least 1 year in locality $l$ and year $t$ at the time of the birth of child $i$, and 0 if the program had not been initiated yet (but would be in the future) or had been for less than a year until the birth.[11] Because qualitative evidence suggests that after the initiation of the program the mediators needed time to promote the program and gain the trust of the community, in our preferred specification we account for non-linear effects of the length of exposure by using three indicators: $Exposure02_{ilt}$, $Exposure24_{ilt}$ and $Exposure47_{ilt}$, which capture whether the program was implemented up to two years, between two and four years, or between four and seven years before birth, respectively.

[9] The 2001 Romanian Barometer of Inter-ethnic Relations revealed that around 33% of the Roma ethnics declare themselves Romanians.

[10] In our sensitivity checks we show the results where we include Roma ethnics in both implementing and non-implementing localities.

[11] We consider at least 1 year prior to the birth of child $i$ to account for the period of pregnancy and for the fact that for some of the localities in the sample we only have the year of the implementation, and not the exact date. We come back to this later.

Our preferred specification becomes:

$$Y_{ilt} = \alpha + \beta_1 Exposure02_{ilt} + \beta_2 Exposure24_{ilt} + \beta_3 Exposure47_{ilt} + \gamma' X_{ilt} + \theta_l + \theta_t + \theta_m + \theta_l t + \epsilon_{ilt} \quad (3.2)$$

In both specifications, $Y_{ilt}$ is our outcome of interest at the individual level reflecting: a) prenatal medical care: (i) prenatal medical supervision take-up (an indicator equal to 1 if the mother had any prenatal care), and (ii) the number of months under prenatal supervision (continuous variable); b) child health at birth measured by: (iii) low birth weight (an indicator equal to 1 if birth weight is below 2,500 grams), and (iv) premature delivery (an indicator equal to 1 for births occurring before week 37 of gestation).[12] Finally, because of data constraints that will be discussed later, we can only investigate some outcomes at the locality level: cohort size (number of live births), number of stillbirths (deaths at birth) and number of infant deaths (deaths occurring before the age of one year).

$X_{ilt}$ is a vector of background characteristics: child's gender, mother's age at birth and its square, whether the mother has any education, marital status, whether the mother is a housewife (as opposed to employed outside the home), child's parity, number of children alive, an indicator for hospital delivery, an indicator if father's information is registered (proxy for the father's legal recognition of the child), and the father's age and its square together with indicators for his employment status. For the low birth weight indicator, we also control for the gestational length. We also include an indicator for conception after January 2007 in counties where the Roma health mediators received extra training on reproductive health (but our results are not sensitive to excluding this indicator). $\theta_l$ and $\theta_t$ are locality and year of birth fixed effects, while $\theta_l t$ represents linear locality specific time trends. $\theta_m$ represent month of birth fixed effects. We cluster standard errors at the locality level. In Section 3.5 we show the sensitivity of our results to various fixed effects, time trends and clustering levels.

[12] In Appendix A, Table 3.13 we show the results for several additional outcomes available in the data. In particular we consider indicators for whether the birth occurred in a hospital and a doctor was present at the birth, for continuous measures of birth weight and gestation length (in number of weeks) and also a composite indicator comprised of both birth weight and gestation length, the indicator for small for gestational age. Even though the results point in the same direction as our main results, we do not include these as main outcomes since they are either the default option (most births occur in a hospital with a doctor present at birth), or that we prefer dichotomized measures of health at birth, as established in the previous literature.

### 3.3.2 Data and working sample

In our empirical exercise we use information from several population registers: the 2000-2008 Vital Statistics Natality (VSN) files for live births, the Vital Statistics Mortality (VSM) files that register stillbirths and the Mortality files to identify infant deaths.

The VSN records cover the universe of births from individual birth certificates, with detailed information about the newborns and the socio-economic characteristics of the parents, including ethnicity and the locality of residence at the time of birth. In particular, we know: (a) child characteristics: date of birth, gender, ethnicity, whether single or multiple birth, birth weight and duration of gestation in number of weeks; (b) information about the mother: date of birth, occupational status, education, marital status and date of marriage, county and locality of residence, together with detailed information about her fertility history, such as number of births (children born alive and fetal deaths), the number of prenatal visits and an indicator for home delivery; (c) some information about the father: date of birth and his occupational status.

The VSM files register all pregnancies ending in still births, and have a similar informational structure to the VSN files, except that it does not collect the ethnicity. The Mortality files record all deceased individuals together with some of their characteristics, including the locality of residence, but do not record any information about the parents, nor their ethnicity.

To identify the localities in which the RHM program was implemented we use registries provided by the Roma NGO SASTIPEN,[13] which contain information on all Roma health mediators ever employed in the program starting in 2002, with their exact date when their employment started and the area in which they operated.[14] In Romania there are about 3,000 localities, which are either defined as communes (with each commune encompassing several villages) if they are rural settlements,

[13]The public authorities do not have a centralized official list of the health mediators, even though the health mediators were public employees employed by the District Public Health Authorities before 2008.

[14]Two issues arise: unknown employment dates (for 17% of the listed mediators) and incomplete employment dates, in which we only know year of employment, but not the exact month the mediators got employed (23% of the listed mediators). For the localities for which we cannot retrieve the initiation date due to unknown employment dates of the mediators, we verify whether they differ in a wide array of socio-economic characteristics from the localities for which we do know the initiation date; they do not, and hence we exclude them from the analysis. For the localities in which we only know the year of initiation, we either adjust the initiation date at the locality level by using additional data sources or impute an initiation date of July 1. Our results are not sensitive to these adjustments.

or as cites if they are urban settlements. To define the treated localities we proceed as follows: for urban areas, the database only registers the town or city where the program was implemented, and, even though the mediators only served some neighborhoods within these localities, we consider the respective urban locality as treated. For rural areas, the database registers the villages, which are geographical sub-units of a rural locality.[15] On average, rural localities in Romania comprise 6.3 villages, with an average population of 800 persons in each village. For each village we identify the locality to which it belongs administratively, and consider that rural locality as treated. The treated rural localities, as defined, comprise of, on average, 5.5 villages. After we define treatment at locality level in this way we can match the RHM localities with the register files. Relevant average characteristics of the localities included in the RHM program such as population, share of Roma ethnics, average socio-economic characteristics are presented in Appendix Table 3.15. We define the initiation date of the program at the locality level as the earliest date a mediator is employed in the locality. Because the database does not contain the date when the employment of a mediator ended, we cannot asses with certainty the number of mediators acting in a community at a certain point, but, during the period studied, there were an average of 1.5 mediators ever employed in each locality.[16] While the legislative requirements stipulated that one Roma woman health mediator should serve a population of 500 to 750 Roma children up to 16 years of age and fertile age women, we cannot have more information about the actual size of Roma population that each mediator served so we cannot evaluate the effects of the program with respect to treatment intensity.[17]

Because the VSN files do not record whether the mother received counseling from a Roma health mediators, and we only know whether a mediator was present in her locality of residence before she gave birth, our estimates need to be interpreted as Intended to Treat (ITT). Moreover, defining the treatment the way de-

[15]The village is defined as a rural human settlement that is not a legal local administrative unit, but is subordinated administratively to a commune. It is identified uniquely through its SIRUTA inferior code, which can then be linked uniquely to the administrative unit it belongs to, the commune, which has a SIRUTA superior code. The SIRUTA superior code is what is registered as the locality of residence in the VSN, VSM and Mortality files.

[16]This potential turnover of mediators also raises the question of whether the program was discontinued in the locality at some point prior to 2008, which we would not observe. However, selective discontinuation of the program is not mentioned in any of the qualitative surveys available on the program. Moreover, such discontinuations would attenuate our treatment effects towards 0.

[17]According to some anecdotal evidence it was often the case that a mediator would serve larger communities than stipulated.

scribed above means that we consider some untreated Roma women as treated, leading to a downward bias of our results. This is one reason for which we show our results separately for rural and urban localities, as be believe the results for the rural localities are less downwards biased and hence closer to the true treatment effects, while the results for large urban localities are expected to be more downward biased.

For our empirical exercise we restrict our sample to Roma children born between 2000 and 2008 in the localities in which the RHM program is initiated until 2008, leading to a sample of 13,685 observations (6,888 in rural areas) for most of the analysis except when we look at the prenatal control and number of months of prenatal check-ups, which were not recorded in VSN for 2 years (2000 and 2002), resulting in a sample of 10,885 observations (5,449 in rural areas) for these outcomes.

One common difficulty when analyzing programs targeting individuals based on their ethnicity in general and Roma population in particular is the self-identification issue [Kligman, 2001, Ladányi and Szelényi, 2001, Csepeli and Simon, 2004]. Some studies indicate that self-identification among Roma is more problematic in large cities, especially among the more educated people, but is of less concern in smaller, more traditional communities, where people speak the Romani language and have more traditional social norms. This is another reason why we conduct the analysis separately for the rural and the urban sub-sample.[18] Moreover, some previous qualitative reports analyzing the RHM program implementation suggested that the mediators were more effective in smaller, traditional communities, which further justifies a separate analysis for rural and urban localities [(OSI), 2011].

Table 3.1 presents descriptive statistics of child outcomes and maternal characteristics, and the average difference between pre-treatment and post-treatment at the locality level, for the localities that were included in the RHM program until 2008. From these simple differences the program appears to have improved prenatal care rates and increased the number of months under prenatal supervision, especially for rural localities, but the improvements in child health do not appear large. Figure 3.3 presents the pre-implementation trends in the outcomes analyzed in rural and urban localities, depending on the year of program implementation at locality level. The trends in pre-implementation outcomes are generally well behaved, with the exception of the rural localities that implemented the program in

[18] We present the results for the pooled sample in Appendix A. Table 3.9

2005, probably due to the very small number of births that occurred here. However, excluding these localities from the estimation sample does not change our results.

## 3.4 Results

### 3.4.1 Individual level outcomes

In Table 3.2, we present the results from estimating Equation 3.1, when the treatment is defined as a single binary exposure indictor that equals 1 if the child is born in a locality where the RHM program was implemented at least 1 year before and 0 otherwise. For each outcome, we first present the baseline estimates, with only locality and birth year fixed effects and locality time trends, without controlling for individual level characteristics and month of birth indicators, whereas in the second column we include the full set of controls. In rural localities (Panel A), there is a 7 percentage points increase in the probability of prenatal controls and a half of month increase in the number of months under medical supervision. Child health at birth appears to also be affected, with a 4 percentage point increase in the probability of low birth weight, which appears counterintuitive. We will discuss this in Section 3.7. There are no effects on the probability of premature delivery. In urban localities (Panel B), there are no significant effects, with the estimated treatment effects much smaller in magnitude but in the same direction as those in the rural area.

Table 3.3 presents the results for our preferred specification, for the rural localities in Panel A and for urban localities in Panel B, in a similar format as the previous table. For the rural subsample, there are large and significant increases in the two outcomes related to prenatal supervision take-up: a 7 percentage points increase (13% of the mean) in the prenatal care rate for children born up to two years after the implementation of the program relative to children born before the program implementation in their locality of residence, and a 30 percentage points increase (56% of the mean) in prenatal care rates for children born more than 2 years after the program initiation in their locality. The same pattern is observed when considering the number of months under prenatal supervision: a one-half month increase (16% of the mean) for the children born up to two years after the program initiation and a roughly two month increase (52% of the mean) for the children born more than two years after the program started. These are very large effects, both in absolute terms and relative to the mean, suggesting a very large impact of the program on prenatal care take-up, which increases over time. Given that prenatal maternal supervision was free of charge both before and after the RHM program implementation, an increase in the take-up rate is likely mediated through the increased awareness and the information provided by the RHM. Despite the significant improvements in prenatal care take-up in the rural areas, there are no significant changes in the health at the birth of children. Although not significant, the low birth weight indicator is positive in line with the results from the baseline specification, while the preterm delivery indicator seems to suggest an improvement at birth.

For the urban subsample, even if the pattern is generally similar to that observed for the rural subsample, the effects are smaller in magnitude and less significant. A small improvement in prenatal care take-up seems to have also occurred in urban areas, but only for children born between two and four years after program implementation, which is not significantly different from the corresponding effect in the rural sample. The number of months under prenatal supervision seems to have been affected more significantly, but only at half the magnitude relative to the rural areas (albeit due to the large standard errors in both samples, the effects in the urban and the rural sample are not statistically different at the 5%, but significant at the 10% level for the first two exposure dummies). This is not surprising because, as we explained before, the RHM program in urban areas targeted only certain neighborhoods, and so the treated population was only a small share of the total Roma population residing in the city, which would bias these results toward zero. This is supported by the fact that we find significant effects, very close in magnitude to those uncovered in the rural sample, when we restrict the sample to births occurring in localities with fewer than 50,000 inhabitants (which account for about 80% of our sample). As in the rural subsample, child health at birth does not seem affected by the program.

Appendix Table 3.9 presents the estimation results on the pooled sample: in Panel A the specification with one single exposure variable, and in Panel B the preferred specification with three exposure variables to capture nonlinear effects in time. As expected, they are smaller in magnitude than the effects in the rural sample, but they are still significant and have the same qualitative interpretation.

### 3.4.2 Further outcomes of the program

In addition to increasing maternal and child health by promoting prenatal care, the health mediators were also trained, starting with 2006, in offering basic information about contraceptive use and reproductive health, which could lead to a change in the composition of the women who become mothers and/or to changes in fertility and cohort size. Understanding whether and how fertility changes occurred could provide an explanation for the unclear effects on child health at birth despite the significant improvements in prenatal care take-up rates. Finally, the RHM program could have affected child health via the channels of fetal mortality (stillbirths) and infant mortality.

**Characteristics of mothers giving birth**

We first investigate whether there are significant changes in the observable maternal characteristics of the women giving birth after the program was initiated in their locality of residence by estimating a model analogous to our preferred specification, in which the outcome variables are observable maternal characteristics. We analyze age, age of first time mothers, early motherhood (age of mother below 19), schooling (whether the mother has any education), marital status at birth, housewife versus employed status, and whether the father legally recognized the child. These results are presented in Table 3.4.

In rural areas (Panel A), Roma women seem to be giving birth at older ages the longer the RHM is implemented. This effect seems to hold particularly for first-time mothers. Additionally, although not significant, the effects indicate that Roma women are less likely to give birth before 19 years of age, more likely to have some schooling, and less likely to be unmarried, which would suggest a positive, albeit insignificant, selection of women giving birth, but they are also more likely to be housewives (i.e., not engaged in any income generating activity). In urban areas (Panel B), Roma women giving birth are significantly less likely to be teens the longer RHM is implemented, but are more likely to be unmarried and in the first two years after program implementation, the father is less likely to legally recognize their child. The other characteristics are not significant and do not show a clear pattern. Overall, these results suggest that the RHM program did not have a large effect, especially so in rural areas, on the composition of Roma women giving birth, and that the improvement of the outcomes related to prenatal care are most likely not driven entirely by a positive selection of Roma women who give birth.

### Locality level outcomes: cohort size, stillbirths and infant mortality

Next, we consider outcomes which we can only measure at the locality level: live births, stillbirths and infant mortality. Because the Mortality files do not include information about ethnicity we consider the number of stillbirths and infant deaths at locality level for all ethnicities.

We estimate the following equations:

$$Y_{lt} = \alpha + \beta_1 Treatment_{lt} + \theta_l + \theta_t + \theta_l t + X_{ct} + \epsilon_{lt} \tag{3.3}$$

and similarly with Equation 3.2, our preferred estimation strategy is:

$$Y_{lt} = \alpha + \beta_1 Exposure02_{lt} + \beta_2 Exposure24_{lt} + \beta_3 Exposure47_{lt} + \theta_l + \theta_t + \theta_l t + X_{ct} + \epsilon_{lt} \tag{3.4}$$

where $Y_{lt}$ for locality $l$ year $t$ is: (i) the cohort size measured as the number of births, (ii) the number of stillbirths or (iii) the number of infant deaths. Similar to our individual level specifications, in Equation (3), $Treatment_{lt}$ is 1 if the program had been initiated for at least 1 year in year $t$ and locality $l$, while in Equation (4) $Exposed02_{lt}$, $Exposed24_{lt}$ and $Exposed47_{lt}$ are indicators of whether the RHM program had been implemented up to two, between two and four and respectively between four and seven years in locality $l$ at time $t$. $\theta_l$ and $\theta_t$ have the same interpretation as in our main specification, whereas $X_{ct}$ is an indicator variable for extra training on reproductive health having been conducted in the year and the county to which locality $l$ belonged to. We cluster standard errors at the locality level.

Results are shown in Table 3.5. First two columns present the results for the live births, the next two columns the number of stillbirths, and the last columns the number of infant deaths at locality level. For each outcome, the first column shows the estimation from Equation 3.3 and finally our preferred specification from Equation 3.4.

The results for live births, in columns (1) and (2) show no significant change after the RHM program implementation, neither in rural nor in urban localities. This result on the live births cohort size also allows us to exclude a potential mechanism, namely that the RHM program led to an increase in the number of registered births (but not the total number of Roma conceptions or births births). Roma ethnics have historically lived in migrant communities and non-negligible shares of the adult population did not have identification documents, which may raise the issue that not all Roma births are registered, and so the RHM program may led to the increase in the number of registered births, which would then mechanically increase the live cohort size. We argue that this is most likely not the case. In Romania, over the last decades, Roma ethnics settled in existing communities and can no longer be regarded as migrants, which has also led to the decrease of the number of Roma without identification documents. Anecdotal evidence indicates that the number of unregistered Roma children is very small, especially because the registration of birth entitles the parents to receive the universal child allowance, awarded for all children until the age of 18 irrespective of the occupational status of the parents. This is often an important source of income for Roma ethnics, who lack formal employment, and may also entitle them to other social benefits. Therefore, we argue that increased registration of births is not a potential effect of the program. In the same time, the lack of effects on the live cohort size also excludes another potential scenario in which Roma parents are more willing to declare the ethnicity of their children after the implementation of the program, as they perceive less discrimination. We will come back to this issue in Section 3.7.

In columns (3)-(6) we observe a significant decrease in both the numbers of stillborn children and infant deaths at the locality level. The effects are the largest for the longest exposure to the program: on average, 1.18 fewer stillbirths and 1.5 fewer infant deaths per year for localities in which the program was implemented for more than four years. These results indicate an improvement in child health induced by the RHM program, which, as the pattern of the earlier results has shown, increases with time since program initiation. Interestingly, almost the same pattern of results for the three outcomes also holds for urban localities: significant decreases in the number of stillbirths; infant deaths have the same magnitude but are not significant. Although these outcomes are not disentangled by ethnicity, we argue that any changes in these outcomes are more likely to come from Roma ethnics, as the rates are much higher for this ethnic minority than the national average: for children born between July 1989 and June 1999, Roma ethnics had a neonatal mortality (stillbirth) rate of 25.6 per 1000 births compared to 17.3 per 1000 births for Romanian ethnics, and an infant mortality rate of 50.6 per 1000 births, relative to 26.9 per 1000 births for Romanian ethnics [Şerbănescu et al., 2001].

## 3.5 Robustness tests

We conduct several robustness checks to test whether the effects we have uncovered are indeed attributable to the RHM program. Potential threats are time varying unobserved characteristics of the localities (e.g., improvements in the quality of the medical control act, infrastructure upgrades) or other national public health programs targeting the general population, which would benefit all residents of the locality and not Roma ethnics exclusively. To this end, we test: (1) whether we observe the same effects for Romanian mothers and children residing in the treated localities, and (2) whether such effects were likely to emerge under a random date of initiation of the program. All these robustness tests support our main findings. We also show that our results are robust to defining the treatment variable as a continuous variable, capturing the number of years between the birth of the child and the initiation date of the program in the locality of residence of the child. Finally, we also perform some further tests to check if our results are sensitive to different specifications.

### 3.5.1 Romanians in treated localities

We estimate our preferred specification for the sample of Romanians in the treated localities, and present the results in Table 3.6. In rural areas, when we do not control for any individual covariates the effects are weakly significant but much smaller relative to the Roma sample in the baseline specifications and they become insignificant after controlling for individual characteristics, suggesting that the previously found take-up effects for Roma mothers are indeed attributable to the RHM program, and do not reflect an increased provision of medical services for all residents of the locality. The composition of the Romanian sample of mothers is much more heterogeneous compared to the Roma mothers, so it is not surprising that individual controls have a larger impact for this sample and the effect disappears after we include these controls. The same holds for the Romanian urban subsample, but the magnitudes are even lower. Regarding child health outcomes at birth, we find that in rural localities, there is an increase in the probability of low birth weight (column 6), especially for children born more than two years after the implementation of the RHM program. This could possibly indicate an over-crowding of the health provision system in rural areas, due to the increased use of prenatal care services by Roma women.

As an alternative approach, we also estimate a triple difference specification in which Romanians are considered the non-participant group; this would also absorb the potential serial correlation at locality level. The results, presented in Appendix Table 3.10, are very much in line with those obtained in the preferred specification for the rural sample, whereas we find no significant effects in the urban sample.[19] Relative to Romanian women, after the program implementation Roma women in rural areas had significantly increased probabilities of having prenatal controls, and earlier controls but no significant change in the outcomes capturing health at birth of their children.

### 3.5.2 Random allocation of initiation dates for treated localities

Next, we randomly allocate the actual initiation dates among the treated localities and estimate our main specification defining the treatment according to a placebo initiation date. We repeat the procedure 500 times and plot histograms of the coefficients on the placebo-treatment indicator for each of our outcomes of interest obtained from the Roma sample, overlaid with the estimated coefficient of treatment from our main specification in which we used the true initiation date. Figure 3.4 presents the simulation results for the sample of births occurring in rural localities.

For the outcomes for which we previously found significant improvements after the initiation on the RHM program, namely the prenatal care indicator and number of months under prenatal medical supervision, the histograms of the coefficients obtained using the placebo initiation dates indicate that it is very unlikely that the estimated coefficient on the (true) treatment indicator could have been drawn from these distributions; moreover, the empirical distributions obtained are centered on 0, as expected, which validates our test.

### 3.5.3 Alternative definitions of the treatment variable

We redefine our main variable of interest as a continuous variable, capturing the number of years between the birth of the child and the initiation date of the program

[19]The lack of significance in the urban subsample stems from the fact that such a triple difference with controls would constrain the marginal effects of the observable characteristics of mothers to be the same for Roma and Romanians, and we have seen that Romanians in urban areas are far more heterogeneous than Roma in terms of observables, and that it is more important to control for observables in the Romanian sample than in the Roma sample.

in the locality of residence of the child.[20] The results are shown in Appendix A, Table 3.11, and indicate that the program induced higher rates of prenatal care but at a decreasing rate. The average effects which would be obtained using these estimates are in fact close to those we obtain in Table 3.3. For the effect on child health outcomes, there is a significant and positive effect on the probability of low birth weight, in line with the effect that we uncovered in Table 3.2. This suggests that average child health appears to worsen after the RHM implementation, despite the increased take-up of medical services in the prenatal period. We will provide a potential explanation for this counter-intuitive result in the mechanisms section.[21]

Finally, we implement a fully flexible specification in which we include leads and lags for each exposure or pre-exposure one-year period, without grouping them in larger intervals as in our preferred specification. This also provides a test of the identifying assumption of parallel trends. Results are presented in Appendix A, Table 3.12. We observe that there are no significant effects for any of the pre-treatment years, whereas for the outcomes that we have observed significant effects in our preferred specification, the estimated coefficients in the post-implementation period are almost always close in magnitude and significance level, which validates our identification strategy.

### 3.5.4 Sensitivity to specification

To further test the robustness of the main results, we estimate variations of our main specification. Table 3.7, Panel A presents the results for the rural sample, and Panel B for the urban sample. The baseline results are presented in column (1) for comparison. The following columns differ from the main specification by: (2) excluding the localities that initiated the program in 2005, given the rather diverging pattern for average outcomes in the pre-implementation period observed in Figure 3.3; (3) excluding cohorts born in 2000 and 2002, such that for all outcomes estimation is done on the same sample; (4) not including any time trend; (5) including linear time trends grouped by year of implementation instead of locality-

[20]For children born prior to program initiation, the variable takes the value 0. To account for non-linear effects, we also include a squared term for our main variable of interest.

[21]We also test two additional treatment indicators: the treatment equals 1 if the health mediator was in the locality at the conception date of the child and 0 if the program started after the birth of the child; and another indicator equals 1 if there was a health mediator in the locality of residence at the conception date of the child and 0 if the mediation program in the locality started after the birth of the child. The results, available upon request, are in line with our findings using our main single treatment indicator.

specific time trends; (6) including month-year fixed effects replacing the year fixed effects; (7) not clustering standard errors at locality level; (8) including individuals from localities that never implemented the program; (9) including locality-by-year fixed effects; (10) estimating the specification using weighted least squares, with weights inverse to the population size of the locality, in an attempt to mitigate the bias towards 0 induced by the fact that in larger communities, there was a lower probability to be visited by a mediator. The results are very robust to all specifications, except for when we exclude any type of time trends in column (4), when the results loose magnitude and for some exposure indicators even significance. This is not surprising because other important changes at the locality level might have happened during the eight years of analysis. When we are excluding the time trend from the specification where treatment is defined as a continuous variable, the magnitude also gets slightly smaller but the effect is still significant. Very importantly, the estimated coefficients from columns (1) which includes locality-specific time trends and column (5) which includes time trends grouped by year of implementation, are very similar, which suggests that unobserved locality-specific changes coincidental with the program implementation are unlikely to bias our results; this can be regarded as an indirect test of the parallel trend hypothesis.

## 3.6 Further evidence using survey data

In this section we attempt to probe further the effect of the RHM on a few outcomes that are not available in the registered files, but that were targeted by the program; we test whether RHM led to: an increase in the use of modern contraceptives, a decrease in abortion rates,[22] an increase in breastfeeding and an increase in the take-up of health supervision which we can proxy though an indicator of perceived discrimination of Roma in the medical system. We make use of the 2006 Roma Inclusion Barometer which includes a sample of 1,417 Roma individuals.[23] In this sample, 641 (45.24%) reside in localities in which the RHM program was implemented at some point in time, and, of these individuals, 308 (48.05%) live in

[22] Survey evidence from 2004 indicates that a very low share of Roma women use contraceptives and that their main family planning method is abortion [Şerbănescu et al., 2001].

[23] The data was collected by the Soros Foundation Romania in November 2006; the Roma sample is representative for the Roma population in Romania. Subsequent waves were, unfortunately, not conducted. The questionnaire addresses social inclusion, perceived discrimination, living and economic conditions, family composition and fertility decisions, and human and social capital.

localities in which the RHM program had already been initiated prior to November 2006, and so were already treated under the program. Given that there is only one wave of the survey, we are reserved in claiming any causality between program implementation and the outcomes we observe, but the results may offer further indications about the effects of the program which we cannot observe using the registered data.

We compare Roma women in localities in which the program was already implemented by 2006 with Roma women in localities in which the program was going to be implemented after 2006, a specification in the same spirit as our preferred regression. The functional form we are estimating is:

$$Y_{il} = \alpha + \beta_1 RHM_{active02_l} + \beta_2 RHM_{active25_l} + \gamma' X_{il} + \theta_l + \epsilon_{il} \qquad (3.5)$$

where $Y_{il}$, for a Roma woman $i$ from locality $l$ measures whether: 1) she felt discriminated , 2) she felt discriminated in a hospital or medical clinic, 3) she was registered with a family physician, 4) she uses modern contraception, 5) she has had an abortion and 6) the number of months of exclusive breastfeeding of the youngest child. $RHM_{active02_l}$ takes the value 1 if the program was active in 2006 (the year of the survey) in locality l for at most two years, or 0 otherwise, and $RHM_{active25_l}$ is taking the value 1 if the program was active in 2006 in locality l for more than two years, or 0 otherwise. We control for individual characteristics (age, educational level dummies, income level dummies, occupational status dummies, number of children under 7) and locality fixed effects, and cluster standard errors at the locality level. The results are presented in Table 3.8.

Relative to the Roma in late implementing localities, Roma in localities in which the program was active for more than two years feel significantly less discriminated in general, and also less discriminated in hospitals and medical facilities; there was no significant effect on the perception of discrimination for women in localities in which the program had been implemented for less than 2 years. This could reflect the duration of the process of improving discrimination perceptions and/or practices, with effects visible over time rather than immediately, but does suggest that health mediators increased the social inclusion of Roma ethnics. Despite the positive and significant effects on discrimination, Roma women do not appear to be more likely to be registered with a family physician: the coefficients are small and not significant. Interestingly, Roma women in the localities in which the program was already active, irrespective of the length of time since initiation, are significantly less likely to have an abortion, but no significant change with respect to the use of modern contraceptives (pill, injections, and condoms). This may be explained by the fact the Roma health mediators were specially trained in family planning and reproductive health only after 2006, while decreasing abortion rate among Roma women was one of their targets already when the program was launched. Importantly, also in line with the RHM goals, we also find that mothers exclusively breastfeed their children an average of two months longer in localities where the program had been active the longest, relative to localities in which the program had not yet been implemented or localities which had just started the program at most two years prior to the survey. Overall, despite data constraints, this evidence tends to suggest that the RHM program impacted Roma women and children beyond the pre-natal care and child health at birth.

## 3.7 Potential Mechanisms and Discussion

Our findings indicate that the RHM program successfully increased prenatal care rates for Roma ethnics, especially in rural areas, but that this was not accompanied by significant improvements in preterm delivery rates, while the results for the low birth weight indicator, albeit marginally insignificant, suggest an apparent worsening of children's health at birth as proxied by this measure. At aggregate level, we find no effects on the cohort size, but a significant decrease in the number of stillborn children and children that die until the age of one. Also, our results do not show a clear compositional change in the observable characteristics of women giving birth. The additional evidence obtained from the survey data suggests that the program was correlated with less perceived discrimination, decreased abortion rates and higher investments in child health after birth. In this section, we investigate the potential mechanisms that could lead to these observable effects.

The RHM program was designed to influence the reproductive, prenatal and postnatal behaviors of Roma women, with the goal of improving maternal and child health. In terms of reproductive behavior, the mediators promoted, especially after 2006, the use of modern (oral) contraceptives and the decrease of abortion usage, and qualitative evaluations of the RHM program suggest that mediators did increase contraception use and decreased abortion rates among Roma women [Centrul pentru Politici si Servicii de Sanatate, 2006]. If Roma women increased their contraceptive use, this would give rise to two distinct selections among mothers giving birth and the health outcomes of their children: on one hand, there would be a negative selection among mothers giving birth, as, according to anecdotal evidence, we expect the more conservative and/or less educated women less likely to respond to information about contraceptive use; this, in turn, may lead to lower prenatal care rates, worse health outcomes at birth, and potentially higher stillbirth and infant mortality rates. On the other hand, some of the mothers giving birth would be a positively selected sample, who, even if they would otherwise respond to the information about contraceptive use, they would actively decide to have children; implicitly, they would have higher prenatal care rates and children with better health outcomes at birth, lower stillbirth and infant mortality rates. However, modern contraceptives use has remained low among Roma women, with abortion being considered one of the most important contraceptive methods. Regarding abortion usage, [Mitrut and Wolff, 2011] show that women with low education are the main users of abortion in Romania, which also explains the very high prevalence of the practice among Roma women, who have on average a very low educational level. Hence, if the mediators would indeed be able to promote a reduction in the abortion usage among Roma women, relatively more worse off Roma women would give birth (instead of using abortion); this would lead to a negative selection on observables among women who give birth. If both increase in contraception and decrease in abortion usage occur simultaneously as a consequence of the program, the effects on selection of mothers, child health at birth, cohort size, stillbirth and infant mortality rate would be consistent with the pattern that we observe in our results.

The second goal of the program was to improve the prenatal behavior of Roma women during pregnancy. Our results indicate that in the rural areas the RHM program had a large positive effect on the take-up rates of prenatal medical care, whereas it also had a positive, albeit smaller, effect in urban areas. If prenatal medical check-ups would be effective in improving the health outcomes at birth for the fetuses that would be anyway carried to term, there should be no change in the observable characteristics of women who give birth, health outcomes at birth should improve, cohort size and stillbirth rates should be unaffected and infant mortality should potentially decrease. The same effects should be observed if prenatal medical check-ups would not have any direct medical effects but due to the information received during the medical visits, the mothers would improve their behaviors during pregnancy, by, for example, decreasing smoking and alcohol consumption or by improving their nutrition. However, these effects are not consistent with the pattern that we observe in our results. Prenatal medical visits could, however, have a direct positive effect on the marginal pregnancies that would otherwise not be carried to term. In this case, the increased prenatal care take-up would lead to these marginal children being born, which would then be reflected in a negative selection on observables of mothers giving birth, a slight decrease in the average health at birth in the cohort (as these marginal children would have on average worse health outcomes), an increase in the cohort size, and a decrease in the stillbirth and infant mortality rates. These effects are to a large extent consistent with the pattern of results we observe in our empirical exercise.

The third aim of the RHM program was to improve infant health. Although we cannot provide causal evidence due to lack of administrative data on these outcomes, our results from the analysis of survey data suggests that these improvements is likely to have occurred, as indicated by the increased probability of exclusive breastfeeding.

To conclude, it is difficult to understand the exact mechanism that lead to the changes we observe in our analysis; most probably, the effects that we observe are consistent with any, or a combination of, these channels: changes in reproductive behavior, in particular a reduction in abortion usage among the worse off Roma mothers, and the medical effectiveness of prenatal controls that improved the survival probability of the marginal children that are now carried to term instead of miscarried.[24] There is also suggestive evidence that the program improved infant health by increasing health-promoting behaviors such as exclusive breastfeeding.

A remaining concern that requires further discussion is the self-reporting of ethnicity. A potential scenario would thus entail that the RHM program did not have any effect on the take-up rates, nor did it influence the reproductive behavior of Roma women, but changed the propensity of Roma women of declaring the ethnicity of their child as Roma on the birth certificate. Because we measure the outcomes using official data, such a problem would occur if the mediators also increased the ethnic consciousness of the Roma women and/or decreased discrimi-

[24]These could be the children of the presumably worse-off mothers who conceived because they were not responsive to the information about contraceptive use, or those who, as a consequence of the mediator intervention, do not use abortion. In the absence of prenatal care, these marginal children would have not survived the prenatal period or would have died at birth.

nation perceptions so that they are now more likely to self-identify as Roma. If the better-off Roma women become more likely to declare their newborn children as Roma ethnics but have no behavioral changes during pregnancy due to the RHM program, then the observed increase in the prenatal care take-up would be a mechanical result due to observing more women in the Roma sample, who would have had more and earlier prenatal care regardless of the program. Yet, if that was the case, it should be accompanied by an increase in the cohort size, and at least an improvement in the observable characteristics of the registered mothers and an improvement in child health outcomes at birth. Moreover, given the evidence on ethnic self-identification in rural versus urban areas discussed earlier, we would expect that this issue of increased propensity to self-declare as Roma would be more prevalent in urban areas, whereas our largest results are for the rural areas. If the worse-off Roma women would become more likely to declare their newborn children as Roma ethnics, then, given their lower prenatal care take-up rates (which remain uninfluenced by the RHM program, as per our assumption), the prenatal care take-up rate would be mechanically lowered in the post-initiation period. This is also contradicted by our findings, so we conclude that a change in the propensity of self-declaring ethnicity is not likely to be the mechanism that explains the effects we observe.

## 3.8 Conclusions

Despite the increasing awareness about the social challenges that extend beyond the lack of material resources, governments have achieved little in alleviating the plight of Roma, Europe's most marginalized ethnic minority. While large investments are made in programs that aim to improve the situation of Roma ethnics, especially concerning health and education, there is surprisingly little empirical evidence on the effectiveness of these programs. In this paper, we investigate the effects a major public health initiative that was implemented in Romania starting in 2002 and was subsequently introduced in several other countries with large Roma minorities. The RHM program aimed to improve the health status of Roma women and children mainly by increasing the access to prenatal care. Specially trained mediators provided information about access to medical care and facilitated visits with the family physician, and promoted family planning and best practices in infant rearing, without offering any direct medical assistance.

We find that the program significantly increased prenatal maternal care rates and the number of months under prenatal medical supervision, narrowing the gap in prenatal care take-up between Roma and non-Roma ethnics, especially in rural areas. This finding indicates that provision of information in an appropriate manner can significantly increase the take-up rates of medical services among a highly disadvantaged population. Yet, the positive effects we observe for the prenatal care were not directly reflected in improvements in indicators of health at birth, such as low birth weight and preterm delivery, which, in fact appear to worsen, but insignificantly. We also show that the program led to a significant decrease in the number of stillbirths and infant mortality at locality level, particularly in the rural communities targeted by the program. We argue that these observed effects are likely to be a result of a combination between two underlying mechanisms: changes in reproductive behavior, in particular a reduction in abortion usage among the worse off mothers, and the effectiveness of prenatal controls that improved the survival probability of the marginal children. This makes the absence of significant effects on child health a "blessing in disguise" and, as additional survey evidence suggests, may have been accompanied by improvements in outcomes which we do not observe in our register data, such as breastfeeding and vaccination rates for the newborn children.

Long term inequality between Roma and non-Roma ethnics starts at birth and widens with age. Therefore, improving the health outcomes at birth of Roma children is a highly desirable policy objective and its achievement should be monitored closely. To this end, further research is needed to assess the causal impacts of the numerous strategies and programs that target Roma ethnics, such that the most efficient solutions could be disseminated to address the community's challenges.

# Bibliography


[Aizer, 2007] Aizer, A. (2007). Public health insurance, program take-up, and child health. *The Review of Economics and Statistics*, 89(3):400–415.

[Almond and Currie, 2011] Almond, D. and Currie, J. (2011). Killing me softly: The fetal origins hypothesis. *The journal of economic perspectives: a journal of the American Economic Association*, 25(3):153.

[Ashraf et al., 2013] Ashraf, N., Jack, B. K., and Kamenica, E. (2013). Information and subsidies: Complements or substitutes? *Journal of Economic Behavior & Organization*, 88:133–139.

[Bertrand et al., 2006] Bertrand, M., Mullainathan, S., and Shafir, E. (2006). Behavioral economics and marketing in aid of decision making among the poor. *Journal of Public Policy & Marketing*, 25(1):8–23.

[Black et al., 2007] Black, S. E., Devereux, P. J., and Salvanes, K. G. (2007). From the cradle to the labor market? The effect of birth weight on adult outcomes. *The Quarterly Journal of Economics*, 122(1):409–439.

[Centrul pentru Politici si Servicii de Sanatate, 2006] Centrul pentru Politici si Servicii de Sanatate, I. d. C. s. F. J. R. (2006). Cercetare-evaluare a programului mediatorilor sanitari in comunitatile de romi in Romania. Technical report, Mimeo.

[Corsi et al., 2008] Corsi, M., Crepaldi, C., Lodovici, M. S., Boccagni, P., Vasilescu, C., et al. (2008). Ethnic minority and roma women in Europe: a case for gender equality?

[Csepeli and Simon, 2004] Csepeli, G. and Simon, D. (2004). Construction of roma identity in Eastern and Central Europe: perception and self-identification. *Journal of Ethnic and Migration Studies*, 30(1):129–150.

[Currie and Almond, 2011] Currie, J. and Almond, D. (2011). Human capital development before age five. *Handbook of labor economics*, 4:1315–1486.

[Currie and Grogger, 2002] Currie, J. and Grogger, J. (2002). Medicaid expansions and welfare contractions: offsetting effects on prenatal care and infant health? *Journal of Health Economics*, 21(2):313–335.

[Currie and Moretti, 2007] Currie, J. and Moretti, E. (2007). Biology as destiny? Short and long run determinants of intergenerational transmission of birth weight. *Journal of Labor Economics*, 25(2):pp. 231–264.

[Duflo et al., 2006] Duflo, E., Dupas, P., Kremer, M., and Sinei, S. (2006). Education and HIV/AIDS prevention: evidence from a randomized evaluation in Western Kenya. *World Bank Policy Research Working Paper*, (4024).

[European Union and Agency for Fundamental Rights, 2014] European Union and Agency for Fundamental Rights (2014). *Discrimination against and living conditions of Roma women in 11 EU Member States: Roma survey : data in focus.* Publications Office, Luxembourg.

[European Union et al., 2013] European Union, Agency for Fundamental Rights, and United Nations Programme for development (UNDP) (2013). *The situation of Roma in 11 EU Member States: survey results at a glance*. Publications Office, Luxembourg.

[Kligman, 2001] Kligman, G. (2001). On the social construction of" otherness" identifying" the roma" in post-socialist communities. *Review of Sociology*, 7(2):61–78.

[Ladányi and Szelényi, 2001] Ladányi, J. and Szelényi, I. (2001). The social construction of roma ethnicity in Bulgaria, Romania and Hungary during market transition. *Review of Sociology*, 7(2):79–89.

[Mitrut and Wolff, 2011] Mitrut, A. and Wolff, F.-C. (2011). The impact of legalized abortion on child health outcomes and abandonment. Evidence from Romania. *Journal of health economics*, 30(6):1219–1231.

[(OSI), 2011] (OSI), O. S. I. (2011). Roma health mediators: Successes and challenges. Technical report, Mimeo.

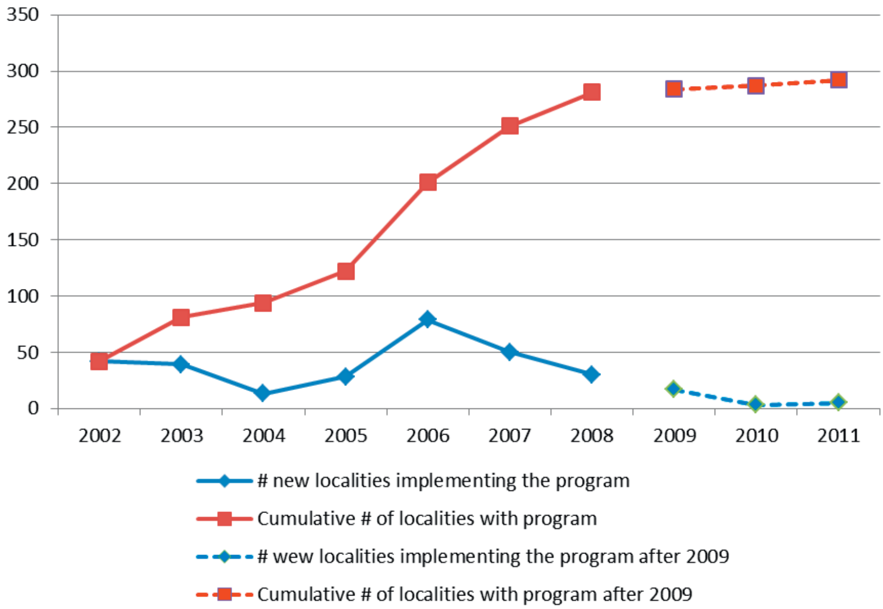
[Royer, 2009] Royer, H. (2009). Separated at girth: US twin estimates of the effects of birth weight. *American Economic Journal: Applied Economics*, 1(1):49–85.

[Şerbănescu et al., 2001] Şerbănescu, F., Morris, L., and Marin, M. (2001). Reproductive Health Survey Romania, 1999.

[Voicu and Popescu, 2009] Voicu, M. and Popescu, R. (2009). Roma women - known and unknown. family life and the position of the woman in the roma communities. Technical report, Mimeo.

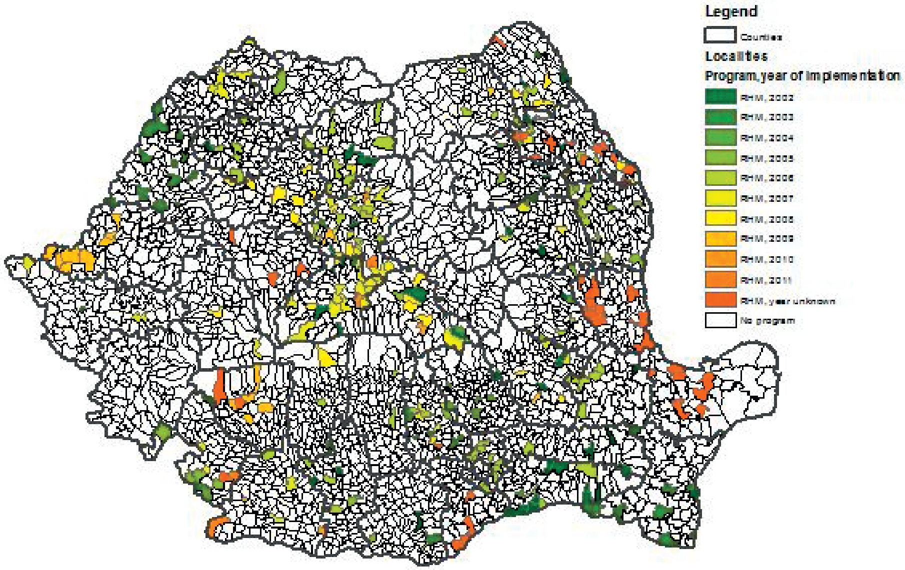
[World Health Organization and Regional Office for Europe, 2013] World Health Organization and Regional Office for Europe (2013). *Roma health mediation in Romania*. World Health Organization, Regional Office for Europe, Copenhagen.

Figure 3.1: Program implementation: number of localities by year of implementation



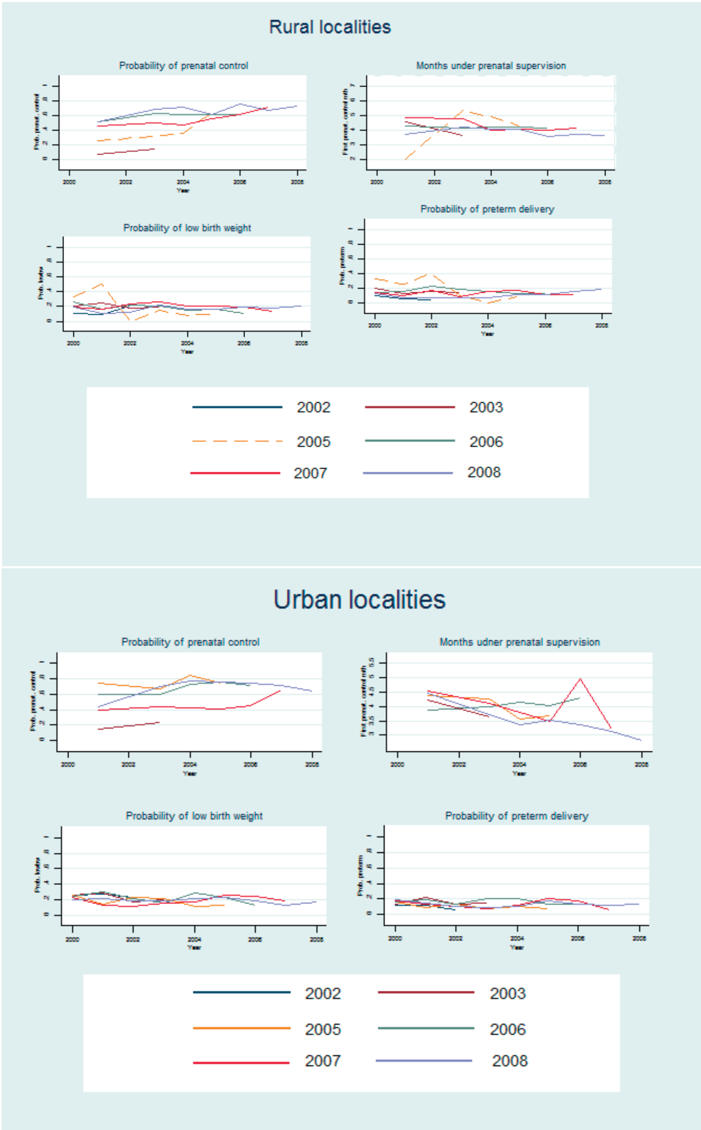

Source: Authors' calculations using data from the Roma Health Mediators registry provided by SASTIPEN.

Figure 3.2: Geographic distribution of localities in which the program was implemented, by year of implementation



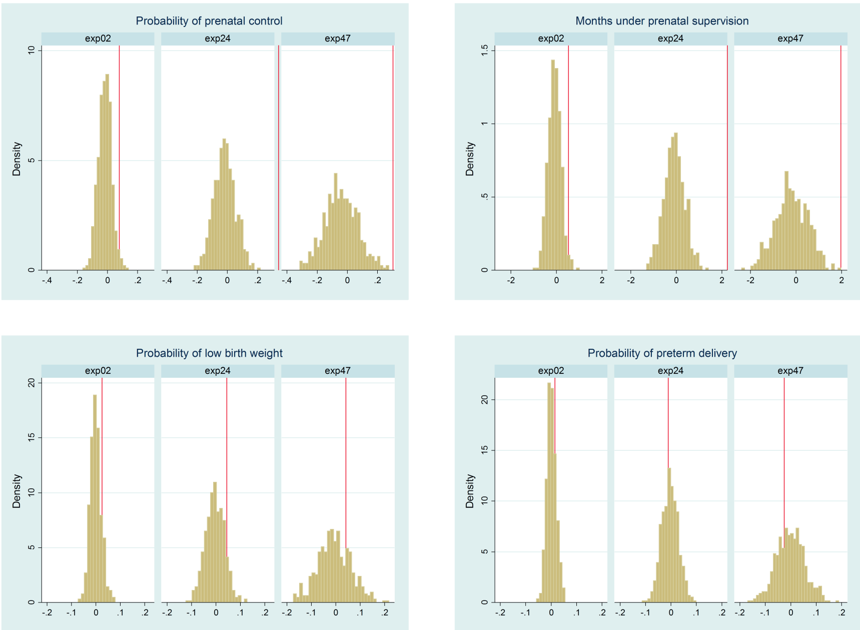

Source: Authors' calculations using data from the Roma Health Mediators registry provided by SASTIPEN.

Figure 3.3: Pre-implementation average outcomes in localities that implemented the program




Notes: Each line represents the average outcomes in localities that entered the RHM program in a specific year. Localities that entered the program in 2004 are excluded due to the very small number of annual observations in the VSN data, but are not excluded from the main estimations; their exclusion would not change the results. Source: Authors' calculations using data from the Roma Health Mediators registry provided by SASTIPEN and Vital Statistics Natality Files.

Figure 3.4: Simulation: Placebo program initiation date, rural sample




The distribution presents the coefficient estimates obtained from the main regressions when a placebo initiation date is used. Each distribution contains 500 estimated coefficients. The red vertical lines reflect the coefficient estimates in Table 3.3, Panel A. Source: Authors' calculations using data from the Roma Health Mediators registry provided by SASTIPEN and Vital Statistics Natality Files.

Table 3.1: Average difference between post and pre-treatment characteristics at the locality level, for the sample of localities that implemented the program

| Variables | All | | Rural | | Urban | |
|---|---|---|---|---|---|---|
| | Sample mean | Average pre-post difference | Sample mean | Average pre-post difference | Sample mean | Average pre-post difference |
| Share with Prenatal care | .533 | 0.128* | .506 | 0.140* | .559 | 0.101* |
| | (.498) | (0.313) | (.500) | (0.323) | (.496) | (0.293) |
| Number of months of prenatal supervision | 3.237 | 0.911* | 3.033 | 0.961* | 3.441 | 0.805* |
| | (3.248) | (2.060) | (3.198) | (2.079) | (3.285) | (2.039) |
| Low birth weight share | .185 | -0.016 | .181 | -0.041* | .190 | 0.029* |
| | (.389) | (0.261) | (.385) | (0.271) | (.392) | (0.239) |
| Preterm delivery share | .158 | 0.017 | .148 | -0.009 | .168 | 0.062* |
| | (.365) | (0.283) | (.356) | (0.258) | (.374) | (0.320) |
| Share of girls | .486 | -0.010 | .477 | 0.017 | .496 | -0.058* |
| | (.499) | (0.361) | (.499) | (0.375) | (.500) | (0.333) |
| Maternal age | 22.954 | 0.211 | 23.082 | -0.053 | 22.824 | 0.677* |
| | (5.623) | (3.404) | (5.697) | (3.450) | (5.543) | (3.299) |
| Share mothers with any schooling | .582 | 0.048* | .576 | 0.079* | .588 | -0.006 |
| | (.493) | (0.350) | (.494) | (0.297) | (.492) | (0.424) |
| Share housewife mothers | .849 | -0.019 | .860 | 0.001 | .837 | -0.055* |
| | (.357) | (0.278) | (.346) | (0.279) | (.368) | (0.273) |
| Share unmarried mothers | .205 | -0.057* | .220 | -0.076* | .189 | -0.025* |
| | (.403) | (0.216) | (.414) | (0.215) | (.391) | (0.215) |
| Share legitimate children | .188 | 0.081* | .202 | 0.097* | .174 | 0.052* |
| | (.391) | (0.213) | (.401) | (0.217) | (.379) | (0.202) |
| Share of children with father's information | .622 | 0.132* | .649 | 0.194* | .593 | 0.022 |
| | (.484) | (0.347) | (.477) | (0.304) | (.491) | (0.391) |

Notes: Sample mean refers to the unweighted average of the outcome in the entire sample used in the estimation. Average pre-post difference is the differences between average post-implementation outcomes and the average pre-implementation outcomes at locality level, averaged over all localities that implemented the program until 2008. Standard deviations in parantheses. * indicates that the average pre-post difference in the sample of 335 implementing localities is significantly different from 0 at a 5% significance level. Source: Authors' calculations using data from Vital Statistics Natality Files.

Table 3.2: Main outcomes: Treatment defined as a single binary exposure indicator

| VARIABLES | (1) Prenatal control | (2) Prenatal control | (3) Months of prenatal supervision | (4) Months of prenatal supervision | (5) Low birth weight | (6) Low birth weight | (7) Preterm delivery | (8) Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | |
| Treated | 0.088*** | 0.069** | 0.581*** | 0.461** | 0.043** | 0.042** | -0.026 | -0.019 |
| | (0.031) | (0.032) | (0.197) | (0.204) | (0.017) | (0.017) | (0.017) | (0.018) |
| Mean dep. var. | 0.506 | 0.506 | 3.033 | 3.033 | 0.181 | 0.181 | 0.148 | 0.148 |
| Observations | 5,449 | 5,449 | 5,449 | 5,449 | 6,888 | 6,888 | 6,888 | 6,888 |
| R-squared | 0.331 | 0.351 | 0.306 | 0.332 | 0.059 | 0.307 | 0.113 | 0.132 |
| **Panel B: Urban** | | | | | | | | |
| Treated | 0.018 | 0.014 | 0.260 | 0.243 | -0.011 | -0.006 | -0.006 | -0.004 |
| | (0.035) | (0.035) | (0.212) | (0.216) | (0.021) | (0.017) | (0.020) | (0.021) |
| Mean dep. var. | 0.559 | 0.559 | 3.441 | 3.441 | 0.190 | 0.190 | 0.168 | 0.168 |
| Observations | 5,436 | 5,436 | 5,436 | 5,436 | 6,794 | 6,794 | 6,794 | 6,794 |
| R-squared | 0.224 | 0.272 | 0.212 | 0.268 | 0.036 | 0.296 | 0.225 | 0.234 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Individual controls include: child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father*s information; 9 month of birth dummies. When "Low birth weight" is the dependent variable, we also include the gestation length at birth, in weeks. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.3: Main outcomes: treatment defines as a non-linear exposure

| VARIABLES | (1) Prenatal control | (2) Prenatal control | (3) Months of prenatal supervision | (4) Months of prenatal supervision | (5) Low birth weight | (6) Low birth weight | (7) Preterm delivery | (8) Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panle A: Rural** | | | | | | | | |
| exp02 | 0.079** | 0.072* | 0.532*** | 0.478** | 0.026 | 0.025 | 0.007 | 0.015 |
| | (0.033) | (0.038) | (0.189) | (0.230) | (0.022) | (0.021) | (0.018) | (0.019) |
| exp24 | 0.340*** | 0.328*** | 2.238*** | 2.152*** | 0.024 | 0.045 | -0.026 | -0.011 |
| | (0.083) | (0.099) | (0.528) | (0.643) | (0.035) | (0.033) | (0.027) | (0.030) |
| exp47 | 0.302*** | 0.307** | 1.964*** | 1.926** | 0.046 | 0.041 | -0.021 | -0.026 |
| | (0.103) | (0.132) | (0.656) | (0.843) | (0.064) | (0.061) | (0.049) | (0.050) |
| Mean dep. var. | 0.506 | 0.506 | 3.033 | 3.033 | 0.181 | 0.181 | 0.148 | 0.148 |
| Observations | 5,449 | 5,449 | 5,449 | 5,449 | 6,888 | 6,888 | 6,888 | 6,888 |
| R-squared | 0.340 | 0.360 | 0.316 | 0.342 | 0.058 | 0.307 | 0.113 | 0.132 |
| **Panel B: Urban** | | | | | | | | |
| exp02 | -0.035 | -0.036 | -0.152 | -0.126 | 0.031 | 0.014 | 0.009 | 0.008 |
| | (0.041) | (0.037) | (0.278) | (0.255) | (0.019) | (0.017) | (0.020) | (0.021) |
| exp24 | 0.075 | 0.073* | 0.744* | 0.788** | -0.002 | -0.027 | -0.010 | -0.008 |
| | (0.050) | (0.043) | (0.373) | (0.340) | (0.032) | (0.027) | (0.029) | (0.030) |
| exp47 | 0.191*** | 0.110 | 1.510*** | 1.112** | 0.003 | -0.036 | -0.033 | -0.011 |
| | (0.070) | (0.070) | (0.463) | (0.474) | (0.057) | (0.047) | (0.037) | (0.038) |
| Mean dep. var. | 0.559 | 0.559 | 3.441 | 3.441 | 0.190 | 0.190 | 0.168 | 0.168 |
| Observations | 5,436 | 5,436 | 5,436 | 5,436 | 6,794 | 6,794 | 6,794 | 6,794 |
| R-squared | 0.228 | 0.277 | 0.216 | 0.273 | 0.037 | 0.297 | 0.225 | 0.235 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Individual Controls include child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information; binary indicator variable if the RHM mediators in the county received reproductive health training after 2006; 9 month of birth dummies. When "Low birth weight" is the dependent variable, we also include the gestation length at birth, in weeks. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.4: Characteristics of Roma mothers giving birth

| VARIABLES | (1) Age | (2) Age if first birth | (3) Mother's age<19 | (4) Mother any school | (5) Unmarried | (6) Mother is housewife | (7) Father information on certificate |
|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | |
| exp02 | 0.446 | 0.467 | -0.027 | 0.016 | -0.011 | 0.033 | -0.019 |
| | (0.352) | (0.376) | (0.032) | (0.020) | (0.024) | (0.023) | (0.033) |
| exp24 | 0.493 | 1.006 | -0.034 | 0.022 | 0.012 | 0.027 | 0.014 |
| | (0.563) | (0.611) | (0.055) | (0.056) | (0.050) | (0.044) | (0.045) |
| exp47 | 2.172** | 1.961* | -0.124 | 0.026 | -0.106 | 0.057 | 0.057 |
| | (1.073) | (1.111) | (0.089) | (0.082) | (0.090) | (0.050) | (0.060) |
| Mean dep. var. | 23.08 | 19.07 | 0.231 | 0.576 | 0.775 | 0.860 | 0.649 |
| Observations | 6,888 | 2,187 | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 |
| R-squared | 0.059 | 0.245 | 0.086 | 0.201 | 0.104 | 0.221 | 0.289 |
| **Panel B: Urban** | | | | | | | |
| exp02 | 0.004 | 0.090 | -0.043** | 0.022 | 0.048** | 0.030 | -0.054* |
| | (0.280) | (0.409) | (0.019) | (0.022) | (0.019) | (0.021) | (0.028) |
| exp24 | -0.042 | -0.166 | -0.081** | 0.019 | 0.076*** | 0.024 | -0.056 |
| | (0.485) | (0.492) | (0.032) | (0.054) | (0.027) | (0.031) | (0.044) |
| exp47 | 0.742 | 0.692 | -0.179*** | -0.021 | 0.066 | -0.045 | -0.063 |
| | (0.756) | (0.703) | (0.037) | (0.059) | (0.056) | (0.053) | (0.052) |
| Mean dep. var. | 22.82 | 19.30 | 0.240 | 0.588 | 0.805 | 0.837 | 0.593 |
| Observations | 6,794 | 2,395 | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 |
| R-squared | 0.053 | 0.200 | 0.081 | 0.124 | 0.043 | 0.144 | 0.219 |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: All regressions include binary indicator variable if the RHM mediators in the county received reproductive health training after 2006. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Locality level outcomes: live births, stillbirths, infant deaths

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | **Live births** | | **Stillbirths** | | **Infant deaths** | |
| **Panel A: Rural** | | | | | | |
| Treated | -.363 | | -.146* | | -.198 | |
| | (.413) | | (.085) | | (.178) | |
| exp02 | | -0.231 | | -0.439*** | | -0.213 |
| | | (0.436) | | (0.090) | | (0.168) |
| exp24 | | -0.948 | | -0.708*** | | -0.495* |
| | | (0.812) | | (0.164) | | (0.296) |
| exp47 | | -0.473 | | -1.180*** | | -1.539*** |
| | | (1.461) | | (0.296) | | (0.500) |
| Mean dep. var. | 8.239 | 8.239 | 0.445 | 0.445 | 1.05 | 1.05 |
| Observations | 1,286 | 1,286 | 1,17 | 1,17 | 883 | 883 |
| **Panel B: Urban** | | | | | | |
| Treated | .078 | | -.258 | | -1.23* | |
| | (.879) | | (.288) | | (.690) | |
| exp02 | | 0.270 | | -0.504 | | -0.215 |
| | | (0.920) | | (0.307) | | (0.865) |
| exp24 | | 0.216 | | -1.250** | | -1.251 |
| | | (1.647) | | (0.541) | | (1.430) |
| exp47 | | 0.323 | | -1.756* | | -1.549 |
| | | (2.754) | | (0.932) | | (2.316) |
| Mean dep. var. | 15.233 | 15.233 | 2.883 | 2.883 | 6.296 | 6.296 |
| Observations | 671 | 671 | 684 | 684 | 499 | 499 |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: All regressions control for locality fixed effects, birth year fixed effects and a dummy for the counties and years in which the mediators undertook specialized reproductive health courses. Live births refer to the number of live births declared of Roma ethnicity at locality-year level, whereas Stillbirths and Infant deaths refer to the total number of stillbirths and infant deaths at locality-year level, of all ethnicities, since ethnicity is not recorded in the respective administrative records. The Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Robustness checks: Romanian sample

| VARIABLES | (1) Prenatal control | (2) Prenatal control | (3) Months of prenatal supervision | (4) Months of prenatal supervision | (5) Low birth weight | (6) Low birth weight | (7) Preterm delivery | (8) Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | |
| exp02 | 0.015 | 0.007 | 0.139 | 0.051 | 0.001 | 0.007 | 0.001 | 0.003 |
| | (0.015) | (0.015) | (0.095) | (0.096) | (0.005) | (0.004) | (0.006) | (0.006) |
| exp24 | 0.053** | 0.032 | 0.451*** | 0.222 | 0.000 | 0.016** | -0.009 | -0.004 |
| | (0.024) | (0.025) | (0.158) | (0.166) | (0.008) | (0.008) | (0.010) | (0.011) |
| exp47 | 0.026 | -0.012 | 0.383* | -0.024 | 0.014 | 0.032** | -0.002 | 0.000 |
| | (0.032) | (0.034) | (0.205) | (0.213) | (0.015) | (0.014) | (0.016) | (0.017) |
| Mean dep. var. | 0.732 | 0.732 | 4.789 | 4.789 | 0.115 | 0.115 | 0.105 | 0.105 |
| Observations | 66,136 | 66,136 | 66,136 | 66,136 | 87,352 | 87,352 | 87,352 | 87,352 |
| R-squared | 0.243 | 0.289 | 0.240 | 0.301 | 0.012 | 0.244 | 0.141 | 0.167 |
| **Panel B: Urban** | | | | | | | | |
| exp02 | -0.010 | -0.017 | -0.015 | -0.083 | 0.003 | 0.004 | -0.002 | 0.001 |
| | (0.015) | (0.016) | (0.122) | (0.127) | (0.002) | (0.002) | (0.003) | (0.004) |
| exp24 | 0.046 | 0.032 | 0.425* | 0.271 | 0.003 | 0.004 | -0.001 | 0.003 |
| | (0.032) | (0.031) | (0.224) | (0.220) | (0.003) | (0.004) | (0.006) | (0.006) |
| exp47 | 0.044 | 0.026 | 0.348 | 0.140 | 0.007* | 0.005 | 0.009 | 0.013 |
| | (0.033) | (0.033) | (0.228) | (0.230) | (0.004) | (0.006) | (0.008) | (0.009) |
| Mean dep. var. | 0.787 | 0.787 | 5.677 | 5.677 | 0.084 | 0.084 | 0.079 | 0.079 |
| Observations | 299,66 | 299,66 | 299,66 | 299,66 | 381,52 | 381,52 | 381,52 | 381,52 |
| R-squared | 0.312 | 0.382 | 0.315 | 0.406 | 0.006 | 0.228 | 0.144 | 0.154 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Individual controls include: child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information; binary indicator variable if the RHM mediators in the county received reproductive health training after 2006; 9 month of birth dummies. When "Low birth weight" is the dependent variable, we also include the gestation length at birth, in weeks. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Specification sensitivity checks

| | | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | | | | |
| Prenatal control | exp02 | 0.072* | 0.072* | 0.072* | 0.018 | 0.069* | 0.081** | 0.072*** | 0.077** | 0.015 | 0.074*** |
| | | (0.038) | (0.040) | (0.038) | (0.028) | (0.037) | (0.037) | (0.025) | (0.038) | (0.043) | (0.024) |
| | exp24 | 0.328*** | 0.333*** | 0.328*** | 0.177*** | 0.294*** | 0.283*** | 0.328*** | 0.340*** | 0.319*** | 0.336*** |
| | | (0.099) | (0.101) | (0.099) | (0.055) | (0.098) | (0.090) | (0.046) | (0.100) | (0.096) | (0.046) |
| | exp47 | 0.307** | 0.316** | 0.307** | 0.104 | 0.264** | 0.248** | 0.307*** | 0.323** | 0.419*** | 0.318*** |
| | | (0.132) | (0.134) | (0.132) | (0.078) | (0.132) | (0.116) | (0.075) | (0.126) | (0.128) | (0.072) |
| Months prenatal supervision | exp02 | 0.478** | 0.452* | 0.478** | 0.121 | 0.464** | 0.531** | 0.478*** | 0.503** | 0.060 | 0.478*** |
| | | (0.230) | (0.243) | (0.230) | (0.165) | (0.229) | (0.225) | (0.162) | (0.227) | (0.258) | (0.159) |
| | exp24 | 2.152*** | 2.128*** | 2.152*** | 1.183*** | 1.956*** | 1.896*** | 2.152*** | 2.202*** | 1.915*** | 2.152*** |
| | | (0.643) | (0.660) | (0.643) | (0.336) | (0.635) | (0.576) | (0.296) | (0.646) | (0.656) | (0.302) |
| | exp47 | 1.926** | 1.924** | 1.926** | 0.564 | 1.630* | 1.577** | 1.926*** | 1.982** | 2.507*** | 1.926*** |
| | | (0.843) | (0.865) | (0.843) | (0.502) | (0.857) | (0.750) | (0.488) | (0.807) | (0.831) | (0.479) |
| Low birth weight | exp02 | 0.025 | 0.031 | 0.027 | 0.006 | 0.023 | 0.022 | 0.025 | 0.029 | 0.033 | 0.028 |
| | | (0.021) | (0.021) | (0.020) | (0.020) | (0.020) | (0.019) | (0.019) | (0.023) | (0.032) | (0.018) |
| | exp24 | 0.045 | 0.051 | 0.043 | 0.005 | 0.047 | 0.052 | 0.045 | 0.028 | 0.024 | 0.052* |
| | | (0.033) | (0.034) | (0.035) | (0.022) | (0.032) | (0.036) | (0.034) | (0.035) | (0.047) | (0.032) |
| | exp47 | 0.041 | 0.051 | 0.040 | -0.022 | 0.039 | 0.037 | 0.041 | 0.021 | 0.069 | 0.051 |
| | | (0.061) | (0.062) | (0.064) | (0.035) | (0.058) | (0.062) | (0.056) | (0.067) | (0.073) | (0.053) |
| Preterm delivery | exp02 | 0.015 | 0.009 | 0.023 | 0.019 | 0.014 | 0.013 | 0.015 | 0.005 | -0.001 | 0.018 |
| | | (0.019) | (0.018) | (0.019) | (0.015) | (0.019) | (0.016) | (0.019) | (0.019) | (0.027) | (0.020) |
| | exp24 | -0.011 | -0.020 | -0.014 | 0.021 | -0.011 | 0.025 | -0.011 | -0.031 | -0.034 | -0.004 |
| | | (0.030) | (0.030) | (0.030) | (0.020) | (0.031) | (0.026) | (0.035) | (0.031) | (0.037) | (0.035) |
| | exp47 | -0.026 | -0.038 | -0.042 | 0.046 | -0.034 | 0.016 | -0.026 | -0.076 | -0.076* | -0.019 |
| | | (0.050) | (0.049) | (0.047) | (0.029) | (0.049) | (0.045) | (0.057) | (0.052) | (0.045) | (0.058) |
| **Panel B: Urban** | | | | | | | | | | | |
| Prenatal control | exp02 | -0.036 | -0.047 | -0.036 | -0.046 | -0.035 | -0.031 | -0.036 | -0.020 | -0.067* | -0.034 |
| | | (0.037) | (0.039) | (0.037) | (0.033) | (0.035) | (0.034) | (0.026) | (0.042) | (0.034) | (0.026) |
| | exp24 | 0.073* | 0.068 | 0.073* | 0.061 | 0.058 | 0.063 | 0.073* | 0.062 | 0.003 | 0.078* |
| | | (0.043) | (0.054) | (0.043) | (0.058) | (0.044) | (0.046) | (0.041) | (0.049) | (0.079) | (0.042) |
| | exp47 | 0.110 | 0.093 | 0.110 | 0.077 | 0.084 | 0.095 | 0.110* | 0.180** | 0.060 | 0.120* |
| | | (0.070) | (0.073) | (0.070) | (0.056) | (0.068) | (0.070) | (0.064) | (0.085) | (0.105) | (0.064) |
| Months prenatal supervision | exp02 | -0.126 | -0.171 | -0.126 | -0.310 | -0.115 | -0.127 | -0.126 | -0.037 | -0.418* | -0.127 |
| | | (0.255) | (0.272) | (0.255) | (0.238) | (0.249) | (0.252) | (0.173) | (0.303) | (0.229) | (0.180) |
| | exp24 | 0.788** | 0.731* | 0.788** | 0.439 | 0.686* | 0.702* | 0.788*** | 0.659* | 0.041 | 0.804*** |
| | | (0.340) | (0.405) | (0.340) | (0.400) | (0.351) | (0.379) | (0.269) | (0.399) | (0.518) | (0.276) |
| | exp47 | 1.112** | 0.965* | 1.112** | 0.442 | 1.004** | 1.061** | 1.112*** | 1.500*** | 0.461 | 1.616*** |
| | | (0.474) | (0.499) | (0.474) | (0.411) | (0.468) | (0.496) | (0.423) | (0.536) | (0.676) | (0.410) |
| Low birth weight | exp02 | 0.014 | 0.022 | 0.007 | 0.007 | 0.014 | 0.010 | 0.014 | 0.025 | 0.051** | 0.014 |
| | | (0.017) | (0.017) | (0.020) | (0.015) | (0.017) | (0.018) | (0.018) | (0.023) | (0.022) | (0.018) |
| | exp24 | -0.027 | -0.020 | -0.051* | -0.041** | -0.028 | -0.030 | -0.027 | 0.012 | -0.037 | -0.027 |
| | | (0.027) | (0.030) | (0.029) | (0.018) | (0.027) | (0.029) | (0.030) | (0.036) | (0.041) | (0.029) |
| | exp47 | -0.036 | -0.033 | -0.072 | -0.056** | -0.037 | -0.041 | -0.036 | 0.021 | -0.094* | -0.037 |
| | | (0.047) | (0.050) | (0.046) | (0.023) | (0.045) | (0.043) | (0.047) | (0.051) | (0.054) | (0.044) |
| Preterm delivery | exp02 | 0.008 | 0.010 | 0.006 | 0.015 | 0.009 | -0.008 | 0.008 | 0.005 | 0.039 | 0.008 |
| | | (0.021) | (0.024) | (0.023) | (0.019) | (0.020) | (0.019) | (0.018) | (0.023) | (0.028) | (0.019) |
| | exp24 | -0.008 | -0.014 | 0.002 | 0.018 | 0.001 | -0.017 | -0.008 | -0.001 | 0.024 | -0.008 |
| | | (0.030) | (0.032) | (0.036) | (0.026) | (0.030) | (0.026) | (0.029) | (0.029) | (0.034) | (0.030) |
| | exp47 | -0.011 | -0.015 | 0.010 | 0.033 | -0.007 | -0.016 | -0.011 | -0.012 | 0.016 | -0.011 |
| | | (0.038) | (0.040) | (0.047) | (0.036) | (0.039) | (0.029) | (0.047) | (0.033) | (0.051) | (0.047) |

Notes: Each numbered column represents a specification where the dependent variable is found in the first column. All regressions include individual controls. (1) Baseline specification. The following specifications differ from the baseline in: (2) Excludes localities that initiated the program in 2005. (3) Excludes cohorts born in 2000 and 2002, such that for all outcomes, estimation is done on the same sample. (4) No time trends. (5) Includes linear time trends grouped by year of implementation. (6)Includes month-year fixed effects replacing the year fixed effects. (7) No clustering at locality level. (8) Including individuals from localities in which the RHM program was never implemented or was implemented after 2009. (9) includes locality-by-year fixed effects. (10) Estimated using weighted least squares, with weights inverse to the population size. *** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Further outcomes of the program using additional survey data from the Roma Inclusion Barometer

| VARIABLES | (1) Feels discriminated | (2) Feels discriminated in hospitals | (3) Registered at family physician | (4) Modern contraceptive use | (5) Any abortion | (6) Exclusive breastfeeding youngest child |
|---|---|---|---|---|---|---|
| Active RHM 0-2 years | 0.020 | -0.086 | -0.085 | -0.028 | -0.446** | 0.501 |
| | (0.084) | (0.106) | (0.090) | (0.068) | (0.169) | (0.703) |
| Active RHM 2-5 years | -0.259*** | -0.373*** | -0.012 | 0.034 | -0.579*** | 2.332*** |
| | (0.061) | (0.051) | (0.037) | (0.045) | (0.167) | (0.602) |
| Observations | 476 | 476 | 476 | 313 | 313 | 233 |
| R-squared | 0.289 | 0.367 | 0.393 | 0.358 | 0.507 | 0.624 |

Notes: Controls include respondent's age, educational level dummies, income level dummies, occupational status dummies, number of children under 7 and locality FE. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

# Appendix A

Table 3.9: Main outcomes: all localities

| VARIABLES | (1) Prenatal control | (2) Prenatal control | (3) Months of prenatal supervision | (4) Months of prenatal supervision | (5) Low birth weight | (6) Low birth weight | (7) Preterm delivery | (8) Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Single exposure variable** | | | | | | | | |
| Treated | 0.051* | 0.041* | 0.396** | 0.330** | 0.013 | 0.017 | -0.013 | -0.012 |
| | (0.028) | (0.024) | (0.174) | (0.156) | (0.016) | (0.014) | (0.012) | (0.013) |
| Mean dep. var. | 0.533 | 0.533 | 3.237 | 3.237 | 0.185 | 0.185 | 0.158 | 0.158 |
| Observations | 10,885 | 10,885 | 10,885 | 10,885 | 13,682 | 13,682 | 13,682 | 13,682 |
| R-squared | 0.278 | 0.310 | 0.259 | 0.297 | 0.046 | 0.300 | 0.171 | 0.183 |
| **Panel B: Multiple exposure variables** | | | | | | | | |
| exp02 | 0.021 | 0.016 | 0.175 | 0.151 | 0.023 | 0.014 | 0.006 | 0.007 |
| | (0.026) | (0.026) | (0.167) | (0.168) | (0.015) | (0.014) | (0.014) | (0.016) |
| exp24 | 0.186*** | 0.178*** | 1.349*** | 1.317*** | 0.004 | -0.002 | -0.018 | -0.015 |
| | (0.050) | (0.053) | (0.331) | (0.354) | (0.026) | (0.023) | (0.021) | (0.024) |
| exp47 | 0.229*** | 0.188*** | 1.612*** | 1.390*** | 0.016 | -0.007 | -0.030 | -0.019 |
| | (0.059) | (0.071) | (0.355) | (0.434) | (0.043) | (0.036) | (0.030) | (0.034) |
| Mean dep. var. | 0.533 | 0.533 | 3.237 | 3.237 | 0.185 | 0.185 | 0.158 | 0.158 |
| Observations | 10,885 | 10,885 | 10,885 | 10,885 | 13,682 | 13,682 | 13,682 | 13,682 |
| R-squared | 0.282 | 0.314 | 0.263 | 0.302 | 0.046 | 0.300 | 0.171 | 0.183 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Individual controls include child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information; binary indicator variable if the RHM mediators in the county received reproductive health training after 2006; 9 month of birth dummies. When "Low birth weight" is the dependent variable, we also include the gestation length at birth, in weeks. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Main outcomes: Triple difference specification

| VARIABLES | (1)<br>Prenatal control | (2)<br>Prenatal control | (3)<br>Months of prenatal supervision | (4)<br>Months of prenatal supervision | (5)<br>Low birth weight | (6)<br>Low birth weight | (7)<br>Preterm delivery | (8)<br>Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | |
| Exp02 | 0.018 | 0.010 | 0.151 | 0.068 | 0.001 | 0.007* | 0.001 | 0.003 |
| | (0.015) | (0.015) | (0.096) | (0.098) | (0.005) | (0.004) | (0.006) | (0.006) |
| Exp24 | 0.060** | 0.040 | 0.489*** | 0.270 | 0.001 | 0.017** | -0.010 | -0.006 |
| | (0.025) | (0.027) | (0.169) | (0.181) | (0.008) | (0.008) | (0.010) | (0.011) |
| Exp47 | 0.039 | 0.003 | 0.449** | 0.062 | 0.014 | 0.033** | -0.006 | -0.005 |
| | (0.034) | (0.037) | (0.215) | (0.230) | (0.015) | (0.015) | (0.016) | (0.017) |
| Roma*Exp02 | 0.036 | 0.029 | 0.241 | 0.190 | 0.015 | 0.009 | 0.011 | 0.015 |
| | (0.029) | (0.030) | (0.172) | (0.173) | (0.023) | (0.019) | (0.016) | (0.015) |
| Roma*Exp24 | 0.172*** | 0.165*** | 1.144*** | 1.103*** | 0.009 | 0.009 | 0.003 | 0.011 |
| | (0.048) | (0.048) | (0.300) | (0.295) | (0.029) | (0.021) | (0.022) | (0.021) |
| Roma*Exp47 | 0.102* | 0.090* | 0.622* | 0.532 | 0.014 | -0.002 | 0.021 | 0.025 |
| | (0.053) | (0.053) | (0.329) | (0.328) | (0.044) | (0.032) | (0.039) | (0.037) |
| Mean dep. var. Roma | 0.506 | 0.506 | 3.033 | 3.033 | 0.181 | 0.181 | 0.148 | 0.148 |
| Observations | 71,585 | 71,585 | 71,585 | 71,585 | 94,24 | 94,24 | 94,24 | 94,24 |
| R-squared | 0.263 | 0.306 | 0.260 | 0.317 | 0.018 | 0.251 | 0.138 | 0.163 |
| **Panel B: Urban** | | | | | | | | |
| Exp02 | -0.010 | -0.016 | -0.011 | -0.079 | 0.003* | 0.004 | -0.002 | 0.000 |
| | (0.015) | (0.016) | (0.122) | (0.127) | (0.002) | (0.002) | (0.003) | (0.004) |
| Exp24 | 0.047 | 0.032 | 0.435* | 0.281 | 0.003 | 0.004 | -0.002 | 0.003 |
| | (0.032) | (0.031) | (0.225) | (0.221) | (0.003) | (0.004) | (0.006) | (0.006) |
| Exp47 | 0.046 | 0.027 | 0.364 | 0.156 | 0.008* | 0.005 | 0.008 | 0.012 |
| | (0.034) | (0.034) | (0.229) | (0.232) | (0.004) | (0.006) | (0.008) | (0.009) |
| Roma*Exp02 | -0.040 | -0.032 | -0.322 | -0.258 | 0.019 | 0.013 | 0.006 | 0.003 |
| | (0.040) | (0.038) | (0.273) | (0.263) | (0.017) | (0.016) | (0.018) | (0.018) |
| Roma*Exp24 | -0.031 | -0.008 | -0.257 | -0.069 | -0.025 | -0.029 | -0.000 | -0.005 |
| | (0.057) | (0.056) | (0.429) | (0.417) | (0.024) | (0.019) | (0.027) | (0.027) |
| Roma*Exp47 | 0.047 | 0.062 | 0.208 | 0.324 | -0.038 | -0.034 | -0.017 | -0.021 |
| | (0.063) | (0.059) | (0.449) | (0.432) | (0.030) | (0.027) | (0.039) | (0.039) |
| Mean dep. var. Roma | 0.559 | 0.559 | 3.441 | 3.441 | 0.190 | 0.190 | 0.168 | 0.168 |
| Observations | 305,096 | 305,096 | 305,096 | 305,096 | 388,314 | 388,314 | 388,314 | 388,314 |
| R-squared | 0.313 | 0.382 | 0.319 | 0.408 | 0.010 | 0.231 | 0.147 | 0.158 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Roma dummy equals 1 if the birth is declared of Roma ethnicity and 0 if the birth is declared of Romanian ethnicity. Individual controls include child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information; binary indicator variable if the RHM mediators in the county received reproductive health training after 2006; 9 month of birth dummies. When "Low birth weight" is the dependent variable, we also include the gestation length at birth, in weeks. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.11: Main outcomes: Treatment defined as years of exposure

| VARIABLES | (1) Prenatal control | (2) Prenatal control | (3) Months of prenatal supervision | (4) Months of prenatal supervision | (5) Low birth weight | (6) Low birth weight | (7) Preterm delivery | (8) Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | |
| Years of exposure | 0.123*** | 0.095** | 0.844*** | 0.695*** | 0.028* | 0.035** | -0.009 | -0.002 |
| | (0.035) | (0.041) | (0.212) | (0.250) | (0.015) | (0.016) | (0.016) | (0.018) |
| Years of exposure squared | -0.016*** | -0.013** | -0.106*** | -0.085** | -0.003 | -0.007** | 0.001 | -0.001 |
| | (0.006) | (0.006) | (0.035) | (0.035) | (0.003) | (0.003) | (0.003) | (0.003) |
| Mean dep. var. | 0.506 | 0.506 | 3.033 | 3.033 | 0.181 | 0.181 | 0.148 | 0.148 |
| Observations | 5,449 | 5,449 | 5,449 | 5,449 | 6,888 | 6,888 | 6,888 | 6,888 |
| R-squared | 0.334 | 0.354 | 0.309 | 0.335 | 0.058 | 0.307 | 0.112 | 0.132 |
| **Panel B: Urban** | | | | | | | | |
| Years of exposure | 0.073* | 0.078** | 0.641 | 0.714*** | 0.001 | -0.000 | -0.015 | -0.016 |
| | (0.037) | (0.033) | (0.000) | (0.224) | (0.000) | (0.015) | (0.018) | (0.019) |
| | 0.006 | 0.002 | 0.021 | -0.003 | 0.001 | 0.001 | -0.000 | 0.000 |
| Years of exposure squared | (0.006) | (0.005) | (0.000) | (0.033) | (0.000) | (0.001) | (0.003) | (0.003) |
| Mean dep. var. | 0.559 | 0.559 | 3.441 | 3.441 | 0.190 | 0.190 | 0.168 | 0.168 |
| Observations | 5,436 | 5,436 | 5,436 | 5,436 | 6,794 | 6,794 | 6,794 | 6,794 |
| R-squared | 0.228 | 0.277 | 0.216 | 0.272 | 0.036 | 0.296 | 0.225 | 0.235 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Individual controls include: child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information; binary indicator variable if the RHM mediators in the county received reproductive health training after 2006; 9 month of birth dummies. When "Low birth weight" is the dependent variable, we also include the gestation length at birth, in weeks. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.12: Main outcomes: Fully flexible specification

| VARIABLES | (1)<br>Prenatal control | (2)<br>Prenatal control | (3)<br>Months of prenatal supervision | (4)<br>Months of prenatal supervision | (5)<br>Low birth weight | (6)<br>Low birth weight | (7)<br>Preterm delivery | (8)<br>Preterm delivery |
|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | |
| Birth 6 to 7 years before | -0.118 | -0.103 | -1.241 | -1.174 | 0.016 | 0.015 | 0.006 | 0.007 |
| | (0.162) | (0.158) | (1.040) | (1.009) | (0.100) | (0.103) | (0.070) | (0.071) |
| Birth 5 to 6 years before | -0.158 | -0.137 | -1.245 | -1.108 | -0.015 | -0.021 | 0.003 | 0.000 |
| | (0.127) | (0.123) | (0.811) | (0.784) | (0.073) | (0.074) | (0.057) | (0.059) |
| Birth 4 to 5 years before | -0.032 | -0.022 | -0.485 | -0.428 | 0.019 | 0.016 | -0.006 | -0.007 |
| | (0.098) | (0.094) | (0.610) | (0.583) | (0.055) | (0.057) | (0.044) | (0.046) |
| Birth 3 to 4 years before | -0.085 | -0.071 | -0.641 | -0.547 | 0.005 | 0.004 | 0.023 | 0.024 |
| | (0.071) | (0.069) | (0.449) | (0.430) | (0.041) | (0.042) | (0.032) | (0.034) |
| Birth 2 to 3 years before | -0.007 | 0.002 | -0.015 | 0.046 | -0.028 | -0.033 | 0.024 | 0.019 |
| | (0.051) | (0.050) | (0.318) | (0.306) | (0.028) | (0.029) | (0.025) | (0.025) |
| Birth 1 to 2 years before | -0.008 | 0.005 | -0.005 | 0.087 | 0.022 | 0.016 | 0.006 | 0.000 |
| | (0.033) | (0.034) | (0.219) | (0.223) | (0.024) | (0.024) | (0.020) | (0.020) |
| Birth 0 to 1 years after | 0.058* | 0.060* | 0.429** | 0.446** | 0.021 | 0.019 | 0.021 | 0.018 |
| | (0.030) | (0.031) | (0.173) | (0.173) | (0.025) | (0.025) | (0.019) | (0.019) |
| Birth 1 to 2 years after | 0.066* | 0.058 | 0.473** | 0.412* | 0.069** | 0.070** | -0.004 | -0.003 |
| | (0.039) | (0.040) | (0.228) | (0.237) | (0.031) | (0.031) | (0.022) | (0.021) |
| Birth 2 to 3 years after | 0.370*** | 0.363*** | 2.453*** | 2.390*** | 0.063 | 0.067 | -0.043 | -0.043 |
| | (0.071) | (0.078) | (0.455) | (0.513) | (0.044) | (0.046) | (0.029) | (0.030) |
| Birth 3 to 4 years after | 0.083 | 0.075 | 0.443 | 0.377 | 0.059 | 0.063 | -0.020 | -0.021 |
| | (0.069) | (0.071) | (0.420) | (0.442) | (0.070) | (0.073) | (0.053) | (0.052) |
| Birth 4 to 5 years after | 0.076 | 0.059 | 0.317 | 0.155 | 0.106 | 0.115 | -0.041 | -0.040 |
| | (0.085) | (0.095) | (0.538) | (0.619) | (0.088) | (0.092) | (0.064) | (0.063) |
| Birth 5 to 5 years after | 0.174** | 0.170* | 0.927 | 0.854 | 0.112 | 0.122 | -0.041 | -0.044 |
| | (0.086) | (0.091) | (0.573) | (0.625) | (0.115) | (0.118) | (0.076) | (0.075) |
| Birth 6 to 7 years after | 0.300*** | 0.252** | 1.514* | 1.148 | 0.191 | 0.207 | 0.021 | 0.030 |
| | (0.111) | (0.127) | (0.850) | (0.973) | (0.244) | (0.249) | (0.241) | (0.245) |
| Mean dep. var. | 0.506 | 0.506 | 3.033 | 3.033 | 0.181 | 0.181 | 0.148 | 0.148 |
| Observations | 5,449 | 5,449 | 5,449 | 5,449 | 6,888 | 6,888 | 6,888 | 6,888 |
| R-squared | 0.349 | 0.368 | 0.326 | 0.350 | 0.060 | 0.075 | 0.113 | 0.130 |
| **Panel B: Urban** | | | | | | | | |
| Birth 6 to 7 years before | -0.331 | -0.300 | -1.837 | -1.565 | -0.031 | -0.052 | 0.015 | -0.001 |
| | (0.228) | (0.211) | (1.437) | (1.337) | (0.055) | (0.058) | (0.084) | (0.088) |
| Birth 5 to 6 years before | -0.098 | -0.078 | -0.400 | -0.230 | -0.008 | -0.024 | 0.002 | -0.009 |
| | (0.159) | (0.146) | (1.034) | (0.940) | (0.044) | (0.044) | (0.073) | (0.077) |
| Birth 4 to 5 years before | 0.016 | 0.031 | 0.352 | 0.489 | -0.031 | -0.048 | -0.010 | -0.023 |
| | (0.103) | (0.097) | (0.674) | (0.632) | (0.033) | (0.035) | (0.052) | (0.053) |
| Birth 3 to 4 years before | -0.003 | -0.003 | 0.196 | 0.215 | 0.018 | 0.012 | 0.032 | 0.027 |
| | (0.082) | (0.077) | (0.544) | (0.515) | (0.031) | (0.032) | (0.040) | (0.041) |
| Birth 2 to 3 years before | 0.015 | 0.024 | -0.027 | 0.054 | 0.015 | 0.005 | 0.020 | 0.012 |
| | (0.058) | (0.054) | (0.409) | (0.378) | (0.022) | (0.022) | (0.027) | (0.028) |
| Birth 1 to 2 years before | 0.021 | 0.025 | 0.196 | 0.242 | -0.003 | -0.012 | 0.023 | 0.017 |
| | (0.039) | (0.038) | (0.265) | (0.253) | (0.026) | (0.027) | (0.021) | (0.021) |
| Birth 0 to 1 years after | -0.018 | -0.015 | -0.106 | -0.083 | 0.047* | 0.043 | 0.015 | 0.011 |
| | (0.043) | (0.040) | (0.275) | (0.266) | (0.026) | (0.026) | (0.022) | (0.022) |
| Birth 1 to 2 years after | 0.019 | 0.009 | 0.242 | 0.168 | 0.034 | 0.035 | -0.008 | -0.009 |
| | (0.065) | (0.064) | (0.422) | (0.421) | (0.027) | (0.027) | (0.031) | (0.032) |
| Birth 2 to 3 years after | 0.116* | 0.111 | 1.002** | 0.958** | -0.000 | 0.004 | -0.027 | -0.026 |
| | (0.068) | (0.068) | (0.452) | (0.463) | (0.035) | (0.036) | (0.035) | (0.036) |
| Birth 3 to 4 years after | 0.043 | 0.045 | 0.614 | 0.575 | 0.029 | 0.040 | -0.048 | -0.047 |
| | (0.103) | (0.092) | (0.682) | (0.636) | (0.047) | (0.047) | (0.060) | (0.060) |
| Birth 4 to 5 years after | 0.122 | 0.098 | 1.160 | 0.952 | 0.027 | 0.038 | -0.073 | -0.070 |
| | (0.108) | (0.099) | (0.775) | (0.721) | (0.066) | (0.067) | (0.072) | (0.071) |
| Birth 5 to 5 years after | 0.189 | 0.147 | 1.635* | 1.292 | 0.010 | 0.028 | -0.134 | -0.126 |
| | (0.127) | (0.120) | (0.918) | (0.887) | (0.074) | (0.074) | (0.089) | (0.088) |
| Birth 6 to 7 years after | 0.113 | 0.062 | 1.063 | 0.628 | 0.064 | 0.079 | -0.102 | -0.100 |
| | (0.171) | (0.163) | (1.255) | (1.223) | (0.098) | (0.099) | (0.107) | (0.106) |
| Mean dep. var. | 0.559 | 0.559 | 3.441 | 3.441 | 0.190 | 0.190 | 0.168 | 0.168 |
| Observations | 5,436 | 5,436 | 5,436 | 5,436 | 6,794 | 6,794 | 6,794 | 6,794 |
| R-squared | 0.234 | 0.278 | 0.222 | 0.275 | 0.038 | 0.050 | 0.226 | 0.235 |



Notes: Individual controls include: child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.13: Additional outcomes: treatment defines as a non-linear exposure

| VARIABLES | (1) Hospital delivery | (2) Hospital delivery | (3) Doctor at birth | (4) Doctor at birth | (5) Birth weight | (6) Birth weight | (7) Pregnancy duration | (8) Pregnancy duration | (9) Small for gestational age | (10) Small for gestational age |
|---|---|---|---|---|---|---|---|---|---|---|
| **Panel A: Rural** | | | | | | | | | | |
| exp02 | 0.003 | -0.004 | -0.014 | -0.017 | 8.910 | 4.296 | -0.014 | -0.036 | -0.006 | -0.008 |
| | (0.008) | (0.008) | (0.011) | (0.011) | (31.044) | (25.078) | (0.102) | (0.109) | (0.020) | (0.020) |
| exp24 | -0.005 | -0.017 | -0.024 | -0.029 | -12.552 | -57.308 | 0.226 | 0.192 | 0.005 | 0.004 |
| | (0.016) | (0.017) | (0.020) | (0.022) | (47.923) | (46.521) | (0.179) | (0.195) | (0.038) | (0.042) |
| exp47 | -0.033 | -0.057** | -0.050 | -0.078 | 0.164 | -42.537 | 0.185 | 0.315 | -0.015 | -0.011 |
| | (0.023) | (0.027) | (0.038) | (0.047) | (75.925) | (74.536) | (0.282) | (0.269) | (0.055) | (0.062) |
| Mean dep. var. | 0.951 | 0.951 | 0.934 | 0.934 | 2958 | 2958 | 38.35 | 38.35 | 0.165 | .0165 |
| Observations | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 | 6,888 |
| R-squared | 0.385 | 0.398 | 0.398 | 0.406 | 0.072 | 0.404 | 0.097 | 0.116 | 0.068 | 0.077 |
| **Panel B: Urban** | | | | | | | | | | |
| exp02 | 0.004 | 0.003 | 0.021 | 0.019 | -0.667 | 20.210 | -0.132 | -0.134 | 0.000 | -0.004 |
| | (0.012) | (0.012) | (0.015) | (0.015) | (32.737) | (20.306) | (0 .133) | (0.133) | (0. 017) | (0.020) |
| exp24 | -0.011 | -0.012 | -0.000 | -0.002 | 28.762 | 53.902 | -0.184 | -0.210 | -0.031 | -0.037 |
| | (0.018) | (0.018) | (0.022) | (0.022) | (54.720) | (41.137) | (0.191) | (0.193) | (0. 044) | (0.049) |
| exp47 | 0.035 | 0.033 | 0.049 | 0.038 | 81.615 | 105.545 | -0.165 | -0.258 | -0.066 | -0.071 |
| | (0.029) | (0.028) | (0.032) | (0.033) | (97.758) | (72.706) | (0. 264) | (0.251) | (0. 080) | (0.088) |
| Mean dep. var. | 0.974 | 0.974 | 0.946 | 0.946 | 2934 | 3934 | 38.38 | 38.38 | 0.190 | .0190 |
| Observations | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 | 6,794 |
| R-squared | 0.157 | 0.198 | 0.350 | 0.370 | 0.052 | 0.400 | 0.103 | 0.117 | 0.040 | 0.048 |
| Individual controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| Locality FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Locality time trends | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Controls include: child gender, mother's age at birth and its square, binary indicator of whether the mother has any education, binary indicator variable if the mother is housewife; binary indicator variable if the mother is married, child's parity, number of children alive, an indicator for hospital delivery, a dummy for existing father's information; binary indicator variable if the RHM mediators in the county received reproductive health training after 2006; 42 county dummies, 9 month of birth dummies. Robust standard errors clustered at the locality level shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1

# Appendix B

## Additional information in the RHM program

The Roma mediators were Roma women from the local communities trained and subsequently employed to act as a liaison between health care practitioners and the Roma community. Both the gender and ethnic component of the mediators were essential for the program: health mediators were expected to approach sensitive issues (such as prenatal care), whereas in many Roma castes strong social norms forbid these discussions with/in the presence of men. Additionally, having a Roma woman from within the community would increase her acceptability and effectiveness through a higher level of trust toward the mediator and an in-depth knowledge of the mediator about specific local social norms, culture and circumstances.[25] An additional requirement was that the mediators have completed at least secondary education (eight grades), which is more than the average educational attainment of Roma women in Romania (only about 20% have more than secondary education).[26]

The initial training to become a health mediator included theoretical courses and practical preparation alongside family physicians. The theoretical courses were run by the large Roma NGO SASTIPEN, the Roma Center for Health Policies, which also provided technical assistance to the local authorities for implementation of the program. The training courses covered health mediation, focused on communication skills, knowledge about the functioning of the medical system in Romania and the general right of access to preventive and curative services, information regarding the process of enrolling in the health insurance system, and first aid concepts. The practical training required that the mediator spend three months alongside the family physician from the locality she would serve. At the end of the training period, the person received a health mediator certificate and started her job in the Roma community, supervised by the family physician working in the community for which they were employed.[27]

[25] There are very strong and different social norms among different Romani castes. E.g., in some Romani castes a woman is considered impure during pregnancy and up to two month after birth and is forbidden to undertake a wide range of activities, including leaving the house because of the shame produced by her condition (source: Introduction to Roma Culture).

[26] In the unusual case of more than one candidate for a locality, the employee was chosen on a competitive basis.

[27] In 2002, the Roma health mediators became a legally recognized profession in Romania. They were employed on a fixed term contract (one year, renewable) by the Ministry of Health through the District Public Health Authorities. In addition to their regular duties, the monthly priority activities of the health mediators are established by the District Public Health Authorities according to the current

For a detailed description of the program and its implementation, see also WHO (2013): "Roma health mediation in Romania: case study."

## Implementation at local level: hazard model

To understand the selection process, we use a discrete-time hazard model of the probability of a locality being included in the RHM program and the timing of implementation, as a function of a broad set of covariates presented in detail in Appendix Table 3.14.

We model the hazard rate as a Cox proportional hazard, where the observational unit is the locality and "failure" in a given year is the start (implementation) of the RHM program, i.e. when the locality is selected into the program.[28] The sample includes all localities in Romania, therefore there are observations that do not fail in the set time frame (2000-2008). Appendix Table 3.15 presents the average values of the observable characteristics of all localities in Romania, separately for implementers and non-implementers.

Appendix Table 3.16 presents the maximum likelihood estimation results for the Cox proportional hazard model, where the coefficients are hazard ratios. Columns (1)-(2) present the estimation results for the entire sample, columns (3)-(4) the results for the rural sample, and columns (5)-(6) for the urban sample. We start by including locality characteristics, in odd columns: yearly population, number of Roma ethnics[29] in the locality, yearly share of employed population in the locality, share of females in the locality, yearly average water consumption per capita from the water distribution network, a time-invariant locality development index and a rural indicator. We also include proxies for the political representation of Roma ethnics at locality level from local council election data (2000 and 2004 elections): whether there was any candidate representing a Roma party and whether this candidate was elected in the local council. We also include the yearly number of publicly

public health campaigns; the health mediator presents weekly activity reports to the medical practitioner to whom she is assigned and monthly reports to the District Public Health Authorities representative.

[28] Cox proportional hazard, as a semi-parametric model, imposes no restrictions on the functional form of the baseline hazard, and makes no assumptions about the shape of the hazard over time. The only assumption is that, regardless of the shape of the hazard, it is the same for all subjects, which in our case are the localities; given the nature of the RHM program and the implementation criteria, this assumption appears reasonable.

[29] Number of Roma ethnics is calculated using the share of Roma ethnics at locality level at the 2000 Census, multiplied by the population of the locality in year t. There is no official yearly estimation of the ethnical structure at locality level.

employed doctors per 1000 inhabitants and the yearly number of family physicians per 1000 inhabitants to proxy for the healthcare supply at locality level. In even columns we then include covariates relating to the fertility behavior of women and infant health at locality level: number of live birth (all and Roma, number of still births, the average maternal characteristics (schooling and occupation), the share of children born with low birth weight and the share of mothers that went to prenatal controls. Standard errors are clustered at the locality level.

The selection process appears different between rural and urban communities. In rural localities, the variables reflecting Roma civic involvement and the Roma population are the most important factors in determining the introduction and timing of the Roma Health Mediation program, with localities having candidates in the local council elections having a significantly higher probability of initiating the RHM program, and also implementing it earlier; in addition a positive, but smaller, effect is generated by the actual election of a Roma representative in the local council. This is consistent with qualitative evidence that the program was first implemented in localities with socially involved Roma communities, a more important criterion than the officially reported size of the local Roma population, usually underreported which also shows up as a significant factor determining selection into the program, along with the total population of the locality, but with a magnitude smaller than the variables proxying for the Roma civic involvement. Other factors influencing the selection into the program, for rural communities are the number of family physicians per 1000 inhabitants at locality level (positive), the number of live births (positive but very small), the share of women giving birth who have at least primary schooling (negative) and the share of low birth weight Roma children (positive). The share of housewife (out of the labor force) women has a surprisingly high positive effect, but it is in accordance with the findings that the program was implemented earlier in more disadvantaged localities, with higher Roma communities, since Roma women have very low employment rates. For urban localities, the most important fact affecting the implementation decision and its timing was population size (positive), and Roma civic involvement -although for these localities, only the election into the local council, and not just the candidacy of a Roma representative, had a significant effect. The total number of publicly employed doctors per 1000 inhabitants had a positive effect, whereas the number of live births has a slightly negative effect. Given these very different patterns of selection into the program, we will conduct our analysis separately for urban and rural communities.

Appendix Table 3.17 presents the estimation results for the Cox proportional hazard model on the sample of localities that implemented the RHM program until 2008. Columns (1)-(2) present the estimation results for the entire sample, columns (3)-(4) the results for the rural sample, and columns (5)-(6) for the urban sample. Odd columns include time varying characteristics of the locality reflecting the socio-economic climate, such as yearly population, yearly share of employed population in the locality and Roma political representation in the local council, and even columns additionally include time-varying characteristics relating to the fertility behavior of women and infant health at locality level. For rural localities, conditional on implementing the RHM program until 2008, the local representation of Roma, proxied by Roma party members being elected in the local council, remains a significant determinant of the timing, with localities which have Roma representative elected in the earlier mandate (2000-2004) implementing the program earlier. Localities with a higher share of employed mothers implement the program relatively later, whereas localities with a higher share of Roma children born with low birth weight implement the program relatively earlier. For urban localities, conditional on implementing the program until 2008, the only determinant of the timing of implementation that remains significant is the share of employed individuals, with localities with a larger share of employed individuals implementing the program later.

## Implementation at local level: panel fixed effects model

An alternative way to model the selection process at locality level is a panel fixed effects model. We estimate the following specification:

$$Y_{it} = \alpha + \theta_l + \theta_t + \gamma' X_{lt} + \theta_l t + \epsilon_{lt} \tag{3.6}$$

Where $Y_{lt}$ is an indicator variable equal to 1 if in locality l and year t the RHM program was in place. We regress this on year and locality fixed effects and on the same time-varying covariates at locality level that we use in the hazard model. In addition, we also include locality-specific time trends. Standard errors are the clustered at locality level. This specification resembles closely our main specifications. Estimation results for the localities that implemented the program until 2008 are presented in Appendix Table 3.18. Once we control for locality specific time trends, both in rural and in urban localities the implementation year of the program is correlated with very few locality level outcomes.

Table 3.14: Description of the variables used in the hazard model

| Variable | Description |
|---|---|
| Log number of Roma | Share of Roma at locality level is determined using the 10% random sample of the 2000 Population census. Number of Roma is calculated as the total yearly population of the locality multiplied by the share of Roma from the 2000 Census. |
| Log population | Yearly population at locality level. Data source: Statistics Romania. |
| Share employed individuals | Share of employed individuals at locality level on a yearly basis is obtained using the average yearly number of employed individuals at locality level and the yearly population at locality level. Data source: Statistics Romania. |
| Avg. water consumption | Total yearly water consumption from the public water distribution network, divided by the yearly population of the locality. Source: Statistics Romania. |
| Rural | Indicator variable, time invariant, Statistics Romania classification. |
| Roma candidate Local Council | Electoral results for the 2000 and 2004 Local Council elections at locality level. Variable takes the value of 1 for years 2000-2003 if in the 2000 elections there was at least one candidate running for the Local Council representing one of Roma political parties, and 0 otherwise. Variable takes the value of 1 for years 2004-2008 if in the 2004 elections there was at least one candidate running for the Local Council representing one of Roma political parties, and 0 otherwise. Data source: "Romanian Electoral Data" platform, Babes-Bolyai University, Romania, Political Science Department |
| Roma elected in Local Council | Electoral results for the 2000 and 2004 Local Council elections at locality level. Variable takes the value of 1 for years 2000-2003 if in the 2000 elections there was at least one candidate elected into the Local Council representing one of Roma political parties, and 0 otherwise. Variable takes the value of 1 for years 2004-2008 if in the 2004 elections there was at least one candidate elected into the Local Council representing one of Roma political parties, and 0 otherwise. Data source: "Romanian Electoral Data" platform, Babes-Bolyai University, Romania, Political Science Department |
| Family physicians/1000inhab. | Average yearly number of family physicians at locality level. Data source: Statistics Romania. Scaled to obtain number of family physicians per 1000 inhabitants. |
| Physicians/1000inhab. | Average yearly number of publicly employed physicians at locality level. Data source: Statistics Romania. Scaled to obtain number of publicly employed physicians per 1000 inhabitants. |

| Variable | Description |
|---|---|
| Number live births | Number of live births per year at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania. |
| Number live births Roma ethn. | Number of live births per year at locality level, declared of Roma ethnicity. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Number stillbirths | Number of still births per year at locality level. Data source: Vital Statistics Mortality Files, from Statistics Romania |
| Share mothers with any school. | Share of women giving birth with at least primary education, yearly average at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Share housewives mothers | Share of women self-reporting to be housewives, yearly average at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Share pregn. wh. prenat. contr. | Share of women who undertook prenatal controls during the pregnancy, yearly average at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Share low birth weight | Share of children who are born with a birth weight smaller than 2500 grams, yearly average at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Share prenatal controls Roma | Share of Roma women who undertook prenatal controls during the pregnancy, yearly average at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Share low birth weight Roma | Share of Roma children who are born with a birth weight smaller than 2500 grams, yearly average at locality level. Data source: Vital Statistics Natality Files, from Statistics Romania |
| Local development index 2008 | Composite index at locality level calculated for 2008 which reflects the human, physical and social capital in each locality; it is comprised of (1) the educational stock at locality level in 2002; (2) average age of inhabitants over 14 years old in 2008; (3) life expectancy at birth between 2006-2008; (4) (log) vehicles per 1000 inhabitants in 2007; (5) average surface of dwelling units in 2008; (6) natural gas consumption per inhabitant; (7) category of locality residence size Data source: Dumitru Sandu -Social Disparities in the Regional Development and Policies of Romania. |

Notes: Sources of each variable is mentioned in each description.

Table 3.15: Descriptive statistics at locality level: all localities, RHM implementers and non-implementers

| | All localities | | | Implementers | | | Non-implementers | | |
|---|---|---|---|---|---|---|---|---|---|
| | Mean | Std. Dev. | N | Mean | Std. Dev. | N | Mean | Std. Dev. | N |
| Avg. number of Roma | 157,847 | 392,214 | 2841 | 581,934 | 884,129 | 302 | 107,405 | 235,552 | 2539 |
| Avg. population | 6642,73 | 20075,94 | 2950 | 22425,25 | 51097,17 | 335 | 4620,877 | 9223,745 | 2615 |
| Avg. share employed | 0,086 | 0,095 | 2949 | 0,132 | 0,122 | 335 | 0,081 | 0,09 | 2614 |
| Share localities with Roma candidate running 2000-2003 | 0,243 | 0,429 | 3180 | 0,643 | 0,48 | 353 | 0,193 | 0,395 | 2827 |
| Share localities with Roma candidate elected LC 2000-2003, conditional on participation | 0,285 | 0,451 | 3180 | 0,677 | 0,468 | 353 | 0,236 | 0,425 | 2827 |
| Share localities with Roma candidate running 2004-2008 | 0,263 | 0,703 | 773 | 0,405 | 0,849 | 227 | 0,203 | 0,624 | 546 |
| Share localities with Roma candidate elected LC 2004-2008, conditional on participation | 0,209 | 0,508 | 906 | 0,343 | 0,654 | 239 | 0,16 | 0,435 | 667 |
| Avg. number family physicians per 100 inhabitants | 0,943 | 0,064 | 2941 | 0,906 | 0,078 | 335 | 0,948 | 0,06 | 2606 |
| Avg. number of physicians per 1000 inhabitants | 6,675 | 6,163 | 2420 | 10,444 | 10,752 | 309 | 6,124 | 4,928 | 2111 |
| Avg. number births | 68,022 | 178,296 | 2941 | 222,003 | 447,742 | 335 | 48,228 | 82,055 | 2606 |
| Avg. number of Roma births | 1,939 | 5,774 | 2941 | 5,798 | 12,095 | 335 | 1,443 | 4,087 | 2606 |
| Avg. number stillbirths | 0,387 | 0,861 | 2941 | 1,129 | 2,077 | 335 | 0,291 | 0,452 | 2606 |
| Avg. share mothers with any schooling | 0,943 | 0,064 | 2941 | 0,906 | 0,078 | 335 | 0,948 | 0,06 | 2606 |
| Avg. share housewife mothers | 0,656 | 0,172 | 2941 | 0,615 | 0,203 | 335 | 0,661 | 0,167 | 2606 |
| Avg. share low birth weight children | 0,115 | 0,034 | 2941 | 0,123 | 0,032 | 335 | 0,113 | 0,034 | 2606 |
| Avg. share low birth weight Roma children | 0,188 | 0,079 | 427 | 0,196 | 0,071 | 101 | 0,186 | 0,081 | 326 |
| Avg. share mothers with prenatal controls | 0,609 | 0,147 | 2941 | 0,58 | 0,146 | 335 | 0,613 | 0,147 | 2606 |
| Avg. share Roma mothers with prenatal controls | 0,437 | 0,18 | 427 | 0,436 | 0,178 | 101 | 0,438 | 0,181 | 326 |

Notes: Authors' calculations using the RHM registry and data from Statistics Romania

Table 3.16: Duration analysis: probability of program initiation and time to initiation, 2000-2008, for all localities

| VARIABLES | (1) All | (2) All | (3) Rural | (4) Rural | (5) Urban | (6) Urban |
|---|---|---|---|---|---|---|
| Log number of Roma | 1.168*** | 1.131*** | 1.223*** | 1.181*** | 1.062 | 1.045 |
| | (0.036) | (0.034) | (0.055) | (0.054) | (0.039) | (0.042) |
| Log population | 1.822*** | 2.071*** | 1.720** | 1.143 | 2.480*** | 3.307*** |
| | (0.289) | (0.398) | (0.408) | (0.366) | (0.502) | (1.008) |
| Share employed individuals | 0.977** | 0.981* | 0.978 | 0.983 | 0.976* | 0.979 |
| | (0.010) | (0.010) | (0.019) | (0.018) | (0.013) | (0.013) |
| Avg. water consumption | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Roma candidate Local Council | 2.756*** | 2.629*** | 3.177*** | 3.083*** | 1.304 | 1.156 |
| | (0.476) | (0.451) | (0.649) | (0.635) | (0.384) | (0.339) |
| Roma elected in Local Council | 1.669*** | 1.596*** | 1.813*** | 1.752*** | 1.863*** | 1.720** |
| | (0.166) | (0.180) | (0.208) | (0.237) | (0.392) | (0.399) |
| Local development index 2004 | 0.998 | 1.004 | 1.001 | 1.011 | 0.985 | 0.987 |
| | (0.009) | (0.010) | (0.011) | (0.011) | (0.019) | (0.020) |
| Family physicians/1000inhab. | 1.453 | 1.469 | 2.009 | 2.018* | 0.668 | 0.654 |
| | (0.487) | (0.474) | (0.860) | (0.829) | (0.407) | (0.386) |
| Physicians/1000inhab. | 1.102 | 1.171** | 1.043 | 1.051 | 1.154* | 1.220** |
| | (0.082) | (0.088) | (0.176) | (0.178) | (0.094) | (0.107) |
| Rural | 0.571** | 0.623* | | | | |
| | (0.141) | (0.155) | | | | |
| Number live births | | 0.999 | | 1.010** | | 0.999* |
| | | (0.000) | | (0.005) | | (0.001) |
| Number live births Roma ethnics. | | 0.986* | | 0.981 | | 0.989 |
| | | (0.008) | | (0.013) | | (0.012) |
| Number stillbirths | | 1.078* | | 0.961 | | 1.042 |
| | | (0.049) | | (0.122) | | (0.047) |
| Share mothers with any school. | | 0.057*** | | 0.154* | | 0.003*** |
| | | (0.038) | | (0.157) | | (0.005) |
| Share housewife mothers | | 3.474*** | | 3.429** | | 1.334 |
| | | (1.613) | | (1.929) | | (1.425) |
| Share pregn. wh. prenat. contr. | | 1.143 | | 1.299 | | 1.113 |
| | | (0.503) | | (0.729) | | (0.788) |
| Share low birth weight | | 1.281 | | 0.724 | | 0.126 |
| | | (1.304) | | (0.813) | | (0.413) |
| Share prenatal controls Roma | | 0.901 | | 0.942 | | 0.806 |
| | | (0.192) | | (0.243) | | (0.275) |
| Share low birth weight Roma | | 1.166 | | 1.898* | | 0.647 |
| | | (0.415) | | (0.722) | | (0.427) |
| County indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 26,526 | 26,526 | 24,077 | 24,077 | 2,449 | 2,449 |

Notes: Estimates present the maximum likelihood estimation results for the Cox proportional hazard model, where the coefficients presented are hazard ratios. The estimation includes all localities in Romania, and spans over the 2000-2008 period. Standard errors are clustered al locality level. Source: AuthorsâĂŹ calculations using data from Statistics Romania.

Table 3.17: Duration analysis: time to initiation only among localities that implemented the program

| VARIABLES | (1)<br>All | (2)<br>All | (3)<br>Rural | (4)<br>Rural | (5)<br>Urban | (6)<br>Urban |
|---|---|---|---|---|---|---|
| Log number of Roma | 1.005 | 1.011 | 1.032 | 1.031 | 0.991 | 0.999 |
| | (0.020) | (0.020) | (0.029) | (0.031) | (0.041) | (0.048) |
| Log population | 0.991 | 1.054 | 1.104 | 0.895 | 1.078 | 1.107 |
| | (0.103) | (0.144) | (0.152) | (0.243) | (0.357) | (0.676) |
| Share employed individuals | 0.983** | 0.984* | 1.006 | 1.006 | 0.955*** | 0.959*** |
| | (0.008) | (0.008) | (0.007) | (0.007) | (0.014) | (0.014) |
| Avg. water consumption | 1.000 | 1.000 | 0.999 | 1.000 | 1.000 | 1.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Roma candidate Local Council | 1.081 | 1.077 | 1.098 | 1.103 | 0.816 | 0.850 |
| | (0.118) | (0.115) | (0.157) | (0.153) | (0.207) | (0.242) |
| Roma elected in Local Council | 1.212** | 1.220** | 1.249** | 1.272** | 0.968 | 0.927 |
| | (0.096) | (0.101) | (0.132) | (0.148) | (0.168) | (0.154) |
| Local development index 2004 | 1.010* | 1.011** | 1.007 | 1.007 | 1.023 | 1.029 |
| | (0.005) | (0.006) | (0.006) | (0.007) | (0.029) | (0.042) |
| Family physicians/1000inhab. | 1.072 | 1.243 | 1.277 | 1.615 | 0.585 | 0.543 |
| | (0.254) | (0.288) | (0.404) | (0.517) | (0.358) | (0.387) |
| Physicians/1000inhab. | 1.061 | 1.038 | 0.860 | 0.833 | 1.135 | 1.108 |
| | (0.063) | (0.065) | (0.108) | (0.114) | (0.099) | (0.118) |
| Rural | 0.828 | 0.807 | | | | |
| | (0.146) | (0.154) | | | | |
| Number live births | | 1.000 | | 1.004 | | 1.000 |
| | | (0.000) | | (0.004) | | (0.001) |
| Number live births Roma ethnics. | | 0.986* | | 0.991 | | 0.995 |
| | | (0.008) | | (0.010) | | (0.014) |
| Number stillbirths | | 0.991 | | 0.999 | | 0.977 |
| | | (0.037) | | (0.097) | | (0.054) |
| Share mothers with any school. | | 0.212*** | | 0.208** | | 0.127 |
| | | (0.110) | | (0.139) | | (0.178) |
| Share housewife mothers | | 1.647 | | 1.217 | | 1.828 |
| | | (0.571) | | (0.502) | | (2.144) |
| Share pregn. wh. prenat. contr. | | 1.876 | | 2.290* | | 3.165 |
| | | (0.739) | | (1.133) | | (2.356) |
| Share low birth weight | | 0.138** | | 0.096** | | 0.103 |
| | | (0.127) | | (0.089) | | (0.377) |
| Share prenatal controls Roma | | 0.763 | | 0.818 | | 0.559 |
| | | (0.145) | | (0.200) | | (0.226) |
| Share low birth weight Roma | | 1.448 | | 2.155** | | 0.936 |
| | | (0.414) | | (0.691) | | (0.593) |
| County indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,619 | 1,619 | 1,096 | 1,096 | 523 | 523 |

Notes: Estimates present the maximum likelihood estimation results for the Cox proportional hazard model, where the coefficients presented are hazard ratios. The estimation includes localities that implemented the RHM program until 2008, and the data spans over the 2000-2008 period. Standard errors are clustered al locality level. Source: Authors' calculations using data from Statistics Romania.

Table 3.18: Program initiation: panel fixed effects

| VARIABLES | (1) Rural | (2) | (3) Urban | (4) |
|---|---|---|---|---|
| 2001 | 0.140** | 0.019 | 0.205** | 0.100 |
| | (0.067) | (0.072) | (0.086) | (0.146) |
| 2002 | 0.152** | -0.088 | 0.246*** | -0.120 |
| | (0.061) | (0.080) | (0.076) | (0.116) |
| 2003 | 0.303* | -0.130 | 0.618*** | 0.004 |
| | (0.154) | (0.166) | (0.123) | (0.162) |
| 2004 | 0.325** | -0.217 | 0.642*** | -0.131 |
| | (0.146) | (0.146) | (0.119) | (0.138) |
| 2005 | 0.357** | -0.331** | 0.707*** | -0.216* |
| | (0.153) | (0.131) | (0.125) | (0.128) |
| 2006 | 0.601*** | -0.203 | 0.826*** | -0.203* |
| | (0.165) | (0.131) | (0.128) | (0.117) |
| 2007 | 0.903*** | -0.036 | 1.033*** | -0.061 |
| | (0.119) | (0.093) | (0.110) | (0.084) |
| Log number of Roma | 0.423 | 1.806 | -0.481 | 2.072 |
| | (1.891) | (2.615) | (1.261) | (4.228) |
| Log population | -1.225 | -2.963 | -0.606 | -8.168** |
| | (1.830) | (2.659) | (1.120) | (3.758) |
| Share employed individuals | 0.001 | 0.004 | 0.003 | 0.007 |
| | (0.007) | (0.009) | (0.007) | (0.018) |
| Avg. water consumption | 0.001** | 0.001* | -0.001 | 0.001 |
| | (0.0001) | (0.0001) | (0.001) | (0.001) |
| Roma candidate Local Council | 0.004 | -0.088 | 0.059 | -0.010 |
| | (0.053) | (0.104) | (0.071) | (0.180) |
| Family physicians/1000inhab. | 0.403** | 0.557** | 0.009 | 0.125 |
| | (0.172) | (0.213) | (0.116) | (0.183) |
| Number live births | 0.004* | 0.006** | -0.000 | 0.001* |
| | (0.002) | (0.003) | (0.000) | (0.001) |
| Number live births Roma ethnics. | 0.003 | -0.002 | 0.001 | 0.002 |
| | (0.005) | (0.008) | (0.002) | (0.003) |
| Number stillbirths | 0.013 | 0.000 | -0.003 | -0.003 |
| | (0.031) | (0.034) | (0.010) | (0.016) |
| Share mothers with any school. | -0.358 | -0.131 | -0.037 | -0.687 |
| | (0.374) | (0.537) | (0.424) | (0.570) |
| Share housewife mothers | -0.087 | -0.020 | 0.880*** | 0.361 |
| | (0.192) | (0.239) | (0.298) | (0.565) |
| Share pregn. wh. prenat. contr. | -0.208 | -0.110 | -0.289** | -0.258 |
| | (0.157) | (0.214) | (0.138) | (0.254) |
| Share low birth weight | -0.303 | -0.302 | 0.044 | -0.232 |
| | (0.399) | (0.572) | (0.808) | (0.812) |
| Share prenatal controls Roma | -0.055 | -0.089 | 0.060 | 0.125 |
| | (0.080) | (0.117) | (0.073) | (0.092) |
| Share low birth weight Roma | -0.099 | -0.073 | 0.152** | 0.172** |
| | (0.070) | (0.101) | (0.074) | (0.084) |
| Locality FE | Yes | Yes | Yes | Yes |
| Locality specific time trnds | No | Yes | No | Yes |

Notes: The estimation includes localities that implemented the RHM program until 2008, and the data spans over the 2000-2008 period. Standard errors are clustered al locality level. *** p<0.01, ** p<0.05, * p<0.1

# Paper III

# Chapter 4

## The Effects of Financial Incentives on Fertility and Early Investments in Child Health

Simona Bejenariu*

**Abstract**

This paper investigates the effects of maternity leave benefits on fertility and early investments in child health by exploiting an unanticipated policy change occurring Romania in 2004 that involved the switch from proportional to fixed and very high benefits. Using Reproductive Health Survey data in a Double Difference design, I find that the change in financial incentives led to marginally insignificant increases in conception rates and decreases in the probability of abortion for women who benefited from the change; these women appear to have worse prenatal behaviors, but have children with better health outcomes at birth. Women who were negatively affected by the policy change compensate by investing more in early infant health.

**JEL classification codes:** J13; J18

**Keywords:** maternity leave benefits; fertility; child health; Romania

*Department of Economics, University of Gothenburg. simona.bejenariu@economics.gu.se
I would like to thank Randi Hjalmarsson and Andreea Mitrut for numerous comments and support. I also thank James Fenske, Hans Grönqvist, Dan-Olof Rooth, Måns Söderbom, Anja Tolonen and participants at various workshops and seminars.

## 4.1 Introduction

Maternity leave benefits (MLB), in the form of entitlements to paid leave after the birth of children, are financial support schemes aiming to subsidize childbearing and encourage fertility, and are widely implemented in developed countries. All OECD countries with the exception of the US have in place governmentally funded policies granting paid maternity leave for employed parents and the median duration of the leave increased from 14 weeks in 1980 to 42 in 2011.[1] The design of these benefits, however, differs significantly across countries, with varying replacement rates of the labor income and lengths of time for receiving benefits after the birth of the child. The varying incentive structure reflects different policy objectives and can influence fertility decisions. For example, a fixed entitlement favors low income families, whereas proportional transfers conditional on the pre-birth labor income encourage female labor supply and a more rapid return to the labor force after the birth of the child. Despite their wide implementation, there is still relatively scarce evidence of the effects of maternity leave benefits policies on fertility, mostly due to difficulties in finding exogenous changes in these financial incentives and the opportunity cost of having children.

In this paper, I exploit an unexpected turn in the legislative process, announced in 2003 and implemented in 2004 in Romania, which changed the way in which maternal leave benefits were awarded. This change significantly altered the opportunity cost of childbearing for employed mothers, but did not affect out of the labor force women. Prior to the policy change, MLB were awarded proportional to the mother's pre-birth income. The monthly benefit amounted to 85% of the taxable earnings averaged over the 10 months preceding childbirth, and was awarded for a maximum of two years. However, the policy changed such that all benefits requested after January 1 2004 would be awarded as a fixed sum equal to 85% of the national average salary of *both* men and women. Benefits requested prior to that date were unaffected and would continue to be calculated proportional to each mother's pre-birth earnings. Because of a very large gender gap in wages, calculating the fixed MLB in reference to the national average salary meant that more than 80% of employed women would potentially gain from the reform, and could receive maternity benefits larger than their salary. As the reform drastically and unexpectedly changed the financial incentives of fertility, this policy change provides

[1] Source: OECD Family Database.

an excellent natural experiment to investigate several aspects related to fertility behavior and child health investments. As such, this paper contributes to the literature on the effects of maternity leave benefits on fertility by providing a comprehensive analysis of the behavioral responses of fertile age women to financial incentives. The analysis includes both conception behavior and abortions, which are usually not observed in available data, as well as prenatal and postnatal investments.

This paper addresses the following three research questions. First, is there a change in the conception behavior induced by the prospect of increased financial incentives for most employed women? Second, conditional on a conception having occurred, does the policy change announcement alter the probability that the pregnancy ends in abortion as opposed to live birth? This is especially relevant in the Romanian context since abortion rates have been, historically, among the highest in Europe after the (re)liberalization of abortions in 1990. The abortion rate in Romania in 2002 was of 1174 abortions per 1000 live births, relative to an European average of 274 abortions per 1000 live births (source: World Health Organization), as abortions were regarded as a contraception method. In this setting, it is very important to understand if financial incentives can influence this aspect of reproductive behavior. Finally, conditional on a pregnancy being carried to term, is there any effect on the health outcomes at the birth of the child and on the early investments in child health?

The Becker model of fertility ( [Becker, 1991]) predicts that the relative increase (decrease) in maternity leave benefits will lead to higher (lower) fertility levels, but also to a decreased (increased) quality of children. Quality of children can be influenced through prenatal investments, child health at birth and early investments in infant health. However, in addition to the price effect, the change in MLB also induces an income effect, which may affect investments in the quality of children in the opposite direction, leading to an ambiguous net effect on the quality of children. This paper aims to bring empirical evidence of the short term effects of maternity leave benefits on the quantity and quality of children, in a reduced form.

The analysis uses the 2004 wave of the Romanian Reproductive Health Survey (RHS-Ro), containing a representative sample of 4441 fertile aged women. The survey includes detailed information about all pregnancies, irrespective of how they ended, detailed information about health outcomes and investments in the youngest child, as well as socio-economic characteristics of the woman and the household in which she lives.

The primary strategy to identify the effects of the policy change announcement is a Difference in Difference design that exploits the fact that only employed women were affected by the policy change, and uses out of the labor force women as a control group, as they were, theoretically, unaffected by the policy. Although marginally insignificant, the findings indicate that in the post policy change announcement period, employed women had increased conception rates and decreased in the probability of abortion relative to housewife women. The effect is driven by women from poorer households, who benefited more from the policy change, with women from richer households having, if anything, an opposite behavior. All employed women appear to have worse prenatal behaviors, but women who benefited from the change have children with better health outcomes at birth. Women who were negatively affected by the policy change compensate by investing more in early infant health. Unfortunately, most treatment effects are statistically indistinguishable from 0 at conventional significance levels. The main robustness check I conduct uses an alternative control group consisting of employed women from Republic of Moldova, a neighboring country with majority Romanian population, where MLB did not change in the period of interest. I also use a mother fixed effects design. The results obtained in the robustness tests point in the same direction as my main specification.

The paper brings several contributions to the literature on the relationship between financial incentives and fertility. The main contribution of this paper is the comprehensive analysis of reproductive behavior and early investments in child health as a response to changes in financial incentives. Previous studies analyzed one or few outcomes: using time series of births from vital statistics, they were constrained to analyzing only the effect on pregnancies carried to term [Baughman and Dickert-Conlin, 2003, Milligan, 2005, González, 2013], and in very few cases aggregate data on number of abortions [González, 2013]. Due to the nature of the Romanian Reproductive Health Survey, I am able to explore the entire spectrum of individual level decisions related to fertility: decision to conceive, decision to carry the pregnancy to term, and several important outcomes conditional on live birth, such as maternal behavior during pregnancy, child health at birth and early investments in child health. This is, to my knowledge, the first study to use reproductive health surveys to evaluate the effects of financial incentives on fertility outcomes in the context of quasi-natural experiments. Secondly, the Romanian policy reform modified only the level of the financial benefits attached to childbirth for employed mothers, and not the length of time for which they were awarded; some policies previously explored entailed a change in both the financial benefits and the length of time of protected employment [Carneiro et al., 2011], making it more difficult to disentangle the effects attributable to monetary incentives. This paper investigates the exclusive role of pecuniary incentives in determining fertility and early investments in child health. Finally, the policy change exploited in this paper affects a very large share of the population, namely employed women, by a large margin and applies to births of any rank. Some previous work exploits changes which only affected a particular subgroup of the population, for example women at the third or higher birth [Cohen et al., 2013] or women working in large firms [Rossin, 2011], thus limiting the external validity and the generalization of the findings.

The remainder of the paper proceeds as follows. Section 4.2 offers a review of the literature on financial incentives and fertility, presents the details of the Romanian policy reform affecting the maternity leave benefits and discusses the potential effects of the policy change, within the Becker framework of fertility demand. Section 4.3 describes the Romanian Reproductive Health Survey and the analysis sample. Section 4.4 presents the identification strategy, a Difference in Difference design that uses out of the labor force mothers as a control group, and discusses threats to identification. Section 4.5 presents the main results of the paper, for the three categories of outcomes of interest, and shows the heterogeneous effects with respect to household asset index, a proxy for treatment intensity. Section 4.6 discusses robustness checks and alternative specifications, including using an alternative Reproductive Health Survey dataset to draw the control group and a mother fixed effects design. Section 4.7 investigates other potential heterogeneities in the treatment effects. Section 4.8 concludes.

## 4.2 Background

### 4.2.1 Literature review

The relationship between fertility and income has long elicited academic interest; the seminal work of [Becker, 1960] and [Becker and Lewis, 1974] established that the demand for children responds to changes in the price of a marginal child, but there is a limited effect of income changes on fertility. With the expansion of welfare polices, financial benefits related to childbirth (e.g. tax rebates/stimulus, transfers or maternity leave benefits), have become a salient part of family policies. However, whether fertility responds to financial incentives, and to what extent, is still a very active topic of research, with studies showing mixed results. At the same time, there is also a growing interest in evaluating the effects of these financial incentives on both short term and long term outcomes, of both children and mothers.

Methodologically, evaluating the effects of financial incentives on fertility and other related outcomes has moved away from cross country comparisons towards quasi-natural experimental settings entailing changes in family or tax policies.[2] There is now a rather large body of evidence on the effects of tax incentive schemes on fertility and maternal labor supply, which typically finds small but significant effects of tax systems on fertility. [Baughman and Dickert-Conlin, 2003] use the expansion of the Earned Income Tax Credit, one of the most important safety nets for families with children in the US, as a large exogenous variation in the price of childbearing and identify its effect using birth certificate data at state level between 1990 and 1999. They find that increases in the income support provided by the EITC encouraged first births for non-white married women, albeit by a small amout, with an elasticity of 0.06. In a continuation study [Baughman and Dickert-Conlin, 2009], they find that expanding the credit produced only very small reductions in higher order fertility among white women. [Brewer et al., 2012] exploit a welfare reform in the UK, implemented in 1999, that targeted low-income households and entailed an up to 50% increase in government spending on child-contingent cash transfers. Using a Difference in Difference framework, they find no increase in births among single women, but an approximately 15% increase in births among married women. Other papers analysing the U.S. tax provisions and the policies targeted at families with children find no or small effects [Kearney, 2004, Rosenzweig, 1999, Robert, 1998].

Another strand of the literature investigates the effects of direct transfers related to birth (child benefits). [González, 2013] studies the impact of a universal child benefit on fertility and maternal labor supply, exploiting the unanticipated introduction of a sizable, one-off, benefit in Spain in 2007. Using a regression dis-

[2] Cross-country evidence finds mixed, weak, or insignificant effects of child subsidies on fertility. [Demeny, 1986] reviews the mixed evidence on pro-fertility policies in France, Romania, Germany, and Hungary. [Gauthier, 2007] includes a review of studies that provide mixed conclusions as to the effect of policies on fertility -either a small positive effect of policies on fertility is found in numerous studies, or no statistically significant effect; however, there is some evidence that the effect of policies tends to be on the timing of births rather than on completed fertility.

continuity design, she finds that the benefit significantly increased the number of live births, in part through a reduction in abortions. [Cohen et al., 2013] use the variation in Israel's child subsidy awarded for the third child to identify the impact of changes in the price of a marginal child on fertility. They identify the effect from changes over time in the extent of the monthly benefit, within families with the same number and age-structure of children. The results indicate a positive and significant price effect on overall fertility, with a 1% increase in the price of raising a marginal (third) child reducing the probability of pregnancy by 0.496%, and a benefit (i.e. child subsidy) elasticity of 1.76%. However, the study has a limited external validity given that the policy only changed the financial incentives for the third child. [Milligan, 2005] exploits the introduction of a pro-natalist transfer policy that paid a lump sum to families having a child in the Canadian province of Quebec, with the size of the benefit being larger for higher order births; he finds a strong effect of the policy on fertility, with the average benefit elasticity of 0.107.

The empirical evidence of the effects of MLB policies as a specific type of financial incentive conditional in childbirth has been, on the other hand, rather scarce and most of the quasi-natural experiments used to identify the effects of these benefits regard the changes in the non-monetary aspect pf MLB, such as the duration of protected leave. [Rossin, 2011] evaluates the impacts of unpaid maternity leave provisions instituted in 1993 in US on children's birth and infant health outcomes in the United States. Exploring the fact that only women in large companies were entitled to unpaid leave in a triple difference strategy, she find that maternity leave led to small increases in birth weight, decreases in the likelihood of a premature birth, and substantial decreases in infant mortality for children of college-educated and married mothers. However, the policy exploited in this paper does not have a financial benefits component, so it is not directly comparable to the Romanian context. [Lalive and Zweimüller, 2009] also explore changes in the parental leave provisions concerning only the duration of protected leave after childbirth (and not the financial component). Exploiting a major Austrian reform that increased the duration of parental leave from one year to two years for any child born on or after July 1, 1990, they find strong effects on both short run fertility and excess long run fertility; higher order fertility increases by about 5% for mothers that benefit from the longer maternity leave. Partially reversing the 1990 extension, a second 1996 reform reduces the spacing between births. [Carneiro et al., 2011] study the impact on the long run labor market outcomes of children of increasing paid and unpaid maternity leave benefits in Norway in 1977. The significant increases in the maternity benefits led to a 2.7 percentage points decline in high school dropout and a 5% increase in wages at age 30. However, the authors argue that the effects are driven by the increased amount of time spent by the mother with the child, and not an increase in the disposable income after the birth of the child, as is the case in the Romanian context. [Dahl et al., 2013] investigate the subsequent Norwegian series of policy reforms which expanded paid leave further, from 18 to 35 weeks, without changing the length of job protection, and claim that these extensions were costly, had poor redistribution properties and had no measurable effect on a wide variety of outcomes, such as children's educational achievements, parental earnings and labor market participation, completed fertility, marriage or divorce. However, the authors argue that these extensions did not result in any change in the total family income, so their estimates capture the effects of parental time on child and family outcomes, and not income effects. In addition, a number of papers study the effect of MLB on other outcomes, including labor market outcomes of the mothers, spacing of births and long run outcomes of the children.[3]

The evidence on the effects of the financial incentives component of the maternity leave benefits is much more scarce. [Raute, 2014] uses the 2007 change in Germany, which entailed the move from a means-tested maternity leave benefits scheme in which only a small subset of mothers received the benefit at all, to a benefit proportional to the pre-birth income for all employed women. She finds that this change in maternity leave benefits led to an increase in fertility, especially for women in the middle and upper-end of the education and income distribution. In a cross-country comparison framework, [Björklund, 2006] exploits the expansion of benefits awarded by family policies. Specifically, he examines the evolution of completed fertility patterns for Swedish women born in 1925-1958 and makes comparisons to women in neighboring countries where the policies were not extended as much as in Sweden. The results suggest that parental leave benefits closely tied to the mother's previous labor market engagement raised the level of fertility, shortened the spacing of births, and induced fluctuations in the period fertility rates, but it did not change the negative relationship between women's educational level and completed fertility.

[3] E.g. [Thevenon and Solaz, 2013], [Carneiro et al., 2011], [Schönberg and Ludsteck, 2014], [Lalive and Zweimüller, 2009], [Ruhm, 1998], [Lalive et al., 2014].

### 4.2.2 Institutional details of the reform

Between 1966 and 1989, the communist regime in place in Romania instituted family policies aimed at rapid population growth by imposing a strict, centrally planned, fertility control. This included an abortion and contraceptives ban, mandatory fertility controls for fertile age women, and supplementary taxes for childless families. After the fall of the regime, in December 1989, Romania liberalized fertility choices, removed the ban on abortions and transitioned to a system of family policies centered around proportional maternity leave benefits. Although paid at various rates, MLB were constantly awarded on a contribution-based manner proportional to pre-birth earnings (ranging from 65 to 85% of the pre-birth taxable income of the mother), and were paid for a period between one and two years. Starting with April 2000, to receive the MLB, a mother needed to (i) be a tax contributor for at least 6 of the 12 months preceding the birth of the child, and (ii) needed to apply for the MLB after the birth of the child. The quantum of the benefit was calculated as 85% of the mother's earnings (taxable labor income) averaged over the 10 months preceding the birth of the child. There was no minimum or maximum cap set for the MLB, and the benefit was awarded for a period of two years, until the child reached the age of two years, or three years for children with disabilities. Although mothers could formally return to work before the child's second birthday without loosing the benefit, the very low availability of formal child care for children below the age of three entailed that mothers would most often stay at home for the entire duration of the protected leave - more on this issue will be discussed later in this section.

In March 2003, the Romanian Government concluded that the MLB entailed disproportional costs relative to other social security benefits, and issued an Emergency Ordinance[4] to modify the MLB so as to reduce public expenditures with these benefits. The Emergency Ordinance[5] set a maximum cap for all benefits,

[4] An Emergency Ordinance is issued by the Government, which holds the executive power, and is an exceptional prerogative that intervenes in the legislative process, which is normally the responsibility of the Parliament. It comes into effect after it is published in the Official Monitor and after the Parliament has been notified for debate, de facto changing the laws it refers to. The two Chambers of the Parliament must convene to debate the Emergency Ordinance and emits a law of approval or rejection of the Emergency ordinance. However, until this acceptance or rejection law is passed by both the Chambers of the Parliament, the Emergency Ordinance produces legal effects. In Romania, Emergency Ordinances are a common procedure, and in the overwhelming majority they are approved by the Parliament, as the Government which proposes the Ordinance is necessarily formed from the party or alliance which has the majority in the Parliament.

[5] EO 9/2003 issued on March 19, 2003.

starting with January 1, 2004. The maximum sum to be paid as MLB was calculated as 85% of the official national average salary, leaving unchanged the MLB smaller than the cap. The Emergency Ordinance also extended the mandatory contribution period to 10 out the 12 months preceding the birth of the child.

The legislative changes were met with opposition from the civil society and the opposition parties in the Parliament; in a sudden change of strategy, the Government issued another Emergency Ordinance[6] not even one month later, on April 14, 2003, which significantly altered the way the MLB were awarded, citing equality of opportunity arguments. Mothers who would apply for MLB after January 1, 2004 would receive a fixed benefit, irrespective of their pre-birth earnings; this benefit was set at 85% of the official national average salary. The required contribution period for mothers applying after January 1, 2004 was set at 10 out the 12 months preceding the birth of the child. Mothers who were already receiving MLB and those who would apply for MLB until December 31, 2003 would not be affected: they would continue to receive the proportional benefits, calculated as 85% of their average pre-birth income, with no maximum cap, even after January 2004. They were *not* entitled to re-apply for the benefits and it was impossible for them to receive the fixed MLB. For these mothers, the required tax contribution period remained 6 out of the 12 months preceding the birth of the child. Since the new law was based on the application date to the MLB and not on the date of birth of the child, a mother could delay the application to the benefits until January 2004 *only* if she had not yet applied; this issue will be discussed in more detail shortly. The benefit was awarded for a maximum period of two years, until the child reached the age of two for normally abled children or three dor children with disabilities, irrespective of when the mother applied for the MLB.[7] The bill was passed into law in October 2003, and gained rapid popular support.

The official national average salary[8] to be used in the calculation of the MLB was 7,682,000 lei (235 USD); the fixed MLB level would then be 6,529,700 lei (200 USD).[9] In November 2003 approximately 80% of employed women had an after-

[6]EO 23/2003

[7]The duration of awarding the MLB was modified in 2005 to be applied starting with 2006, when it would be discontinued when the mother returned to work; she would then receive an additional and fixed benefit entitled "reinsetion stimulent" to compensate for the loss of the MLB.

[8]The official national average salary was set yearly in the Law of Social Insurance Budget, and was the same for men and women. For 2004, the value of the official national average salary was published in December 2003.

[9]However, when the policy was announced in April 2003, the official national average salary was 6,962,000 (213 USD), set in December 2002, and entailed a benefit of 5,917,000 lei (181 USD). This

tax income smaller than the fixed MLB set for 2004 (Source: Statistics Romania). This suggests that the vast majority of employed women would receive a higher MLB under the fixed scheme than under the proportional scheme if they applied for the benefit after January 2004.[10] It is important to bear in mind that mothers who would have applied for the MLB prior to January 2004 would continue to receive MLB proportional to their pre-birth earnings. The potential MLB which would have been paid starting with January 2004 under the alternative regimes are presented in Figure 4.1.

A key fact for the identification strategy is that the policy change was unexpected. I argue this is most likely the case with the second and final policy change, at least in terms of direction and magnitude. First, the Government initially attempted to limit the public expenditures on MLB by placing a cap on all paid benefits, but shortly after reconsidered the measure and awarded a fixed benefit for all new MLB requests. Second, this was the first time after the fall of communism that MLB would be paid as a fixed amount, and not proportional to pre-birth earnings of the mother. In fact, the quantum of the fixed benefit was so large relative to the average MLB previously paid, that in 2004 the total amount paid as MLB from the social security budget was 2.2 times larger than that paid in 2003 (source: National Bank of Romania Annual Report 2004). Third, in the central media the topic of changing MLB was only discussed in March-April 2003, when the Emergency Ordinances were passed, and again in October 2003 when the law was also changed.[11]

The changes came in effect for all benefits requested after January 1, 2004. Unfortunately, there is not a sharp discontinuity based on the date of the birth of the child, since the MLB can be requested at any time until the child turns two years. Although mothers giving birth after January 2004 would receive the fixed MBL

was, most likely, the salary upon which individuals formed their expectations.

[10]The fact that most working age mothers could potentially benefit from the policy change was acknowledged in the Parliamentary debates, with a Government representative declaring that an estimated 92% of the potential mothers would benefit from the reform.

[11]"Adevarul", a nationwide daily newspaper with one of the highest circulations in written press, published related articles only between March 26, 2003 and April 14, 2003 with an average of two articles daily (Source: author's content analysis on the 2002 and 2003 "Adevarul" archive). A potential concern is related to differential access to information between high and low earning women, or different perceptions about the probability that the announced change would actually be implemented. However, this is not likely to pose threats to the identification since low earning women, who may be considered as having lower access to information, are actually not exclusively low educated women -in 2003, a very large share of low earning women were employed in the educational and health sector, which had amongst the lowest wages in the economy, and the highest educational level.

with certainty (since one cannot apply for MLB before the birth of the child), there could also be mothers giving birth in 2003 that receive the fixed MLB and not the proportional ones. Mothers giving birth in 2003 after the final policy change announcement could strategically delay the application process until 2004, foregoing several months of proportional MLB to receive the fixed MLB for less than two years. This issue could be attenuated by the fact that some mothers giving birth in 2003 planned the birth of their child considering the 6 months tax contribution requirement, so delaying application until January would be impossible as they would no longer qualify for MLB.[12]

Women who had not earned taxable income in the previous 10 months, so out of the labor force women (most of whom are housewives) - are not eligible for the maternity leave benefits, but do qualify for a fixed child support allowance which is given for all children irrespective of the occupational status of the mother, which was substantially lower than the maternity leave benefit. Between 2002 and 2004, the child state allowance was 225,000 ROL (6,88 USD), and between 2004 and 2005 is was 240,000 ROL (between 7,40 USD and 8,30 USD) - therefore approximately 4 percent of the quantum of the fixed maternity benefit to which employed mothers were entitled to.

The monthly number of births in Romania for the period 2000-2010, together with a de-seasonalized series, which controls for month of the year dummies, and fitted values are presented in Figure 4.2. In the de-seasonalized data, there is a very steep downward trend in the pre-2004 period, whereas after 2004 there is almost no trend in the residuals. Formally testing for a discontinuous jump in the number of births after January 2004 (incidentally 9 months after the policy change announcement), after including a third order polynomial in month-year of births to account for smooth fertility trends and for seasonality through calendar month dummies, gives an estimate of 749 (s.e 191) births extra per month, which is approximately 4% of the pre-policy average monthly number of births. This is entirely driven by the increase in the number of monthly births by employed women (an increase of 612, s.e. 112, which is approximately 10% of the monthly number of births of employed women), whereas there is no such discontinuous change in

[12]To verify whether this this strategic behavior occurred, I requested the number of new MLB decisions by month to investigate if there are significantly fewer requests for the months preceding January 2004 and a significant jump after.Unfortunately, the documents for 2003-2004 are not at the institution currently administrating the MLB application, so I requested the information from the Ministry of Labor; I am currently waiting for a reply.

the monthly number of births by housewife women. This is indicative evidence that the MLB policy may have reversed the downward trend in natality.

**Early childcare and time at home after birth** An important issue for the identification strategy is whether the policy change also altered the duration of the temporary exit from the labor market of employed women after giving birth. I have previously claimed the the majority of women take the full extent of the protected leave. The first evidence to support this comes from aggregate data. According to Statistics Romania, in 2003 and 2004 only 2.1% of children between 0 and 3 years were enrolled in childcare facilities, and it slightly increased in 2005 and 2006 to 2.4%. The low rate of the formal childcare is due to capacity constraints (in 2004, only 289 childcare facilities were operating on the entire territory of Romania) and is significantly below the European average of 30%. This is in contrast with the 70% enrollment rate in 2004 in kindergartens for children between 3 and 6 year old.

Additional evidence on the time at home after the birth of children at individual level comes from the Generations and Gender Survey, Wave 1 Romania, which was conducted in December 2005.[13] Although it is not possible to conduct an individual level analysis of the time spent at home/out of the labor force after the birth of a child, the data reveals that of the women active of the labor market that have a child younger than one year, 84% are on maternility leave, and rest of 16% are working. Of the women active on the labor market that have a child between one and two years old, 80.5% are on maternity leave and 19.5% are working. This suggests that the vast majority of women active on the labor market enjoy the full extent of the protected leave, but there exists a non-negliable share that do work while they would be entitled to maternity leave benefits. This would be problematic to the analysis in this study if earlier return to work would be a response to the policy, whereby the high earning mothers would return to the labor market because the fixed MLB would be smaller than a proportional MLB. Unfortunately, due to the timing of the survey (December 2005), women who would, theoretically, still be entitled to maternity leave benefits would have to have given birth the earliest in January 2004, which precisely corresponds with the implementation of the

[13]The Generations and Gender Survey is a longitudinal survey of 18-79 year olds conducted in 19 countries includin Romania, and it is designed to understand family and relationship dynamics. It covers a wide array of topics including fertility, partnership, economic activity, care duties for children and within the household.

policy change studied in this paper -therefore I cannot observe the length of maternity leave for women who have given birth before December 2003. This makes it impossible to investigate whether the duration of the maternity leave changed for women who gave birth after January 2004 relative to those who gave birth before. However, indicative evidence comes from Figure 4.3, which plots the distribution of wages of all employed women in the sample and the distribution of wages of women who have children under the age of 2 (hence would have been entitled to be on MLB) and work. If the duration of the leave changed because of the smaller benefits for high earning mothers, it would be expected that most of the mothers who return to work before their child's second birtday would be the high earning women, with after tax wages above the fixed level of the MLB (6.5 million lei). However, the distributions in Figure 4.3 show that the women who retun to work before their child's second birthday are not concentrated among the highest earning employed women, but in fact are relatively more concentrated among low earners, those who gained from the policy change. As expected, there are also relatively more high earning mothers that return to work than in the overall distribution, but coupleted with the previous finding it could suggest that return to work prior to the child's second birthday is not related to the policy change, but to some unobserved preference for participation in the labor market for some new mothers, that could have existed before 2004 as well.

Along the same line, a survey conducted in 2012 [Paunescu and Apostu, 2012] shows that 96.2% of children up to one year old were in the exclusive care of their parents; of children aged one to two years, 87.9% were in the care of their parents, 7.2% were in the care of their grandparents and the rest in formal childcare facilities. This also supports the claim that most mothers take the full extent of the protected leave after childbirth and do not use informal care to a great extent in the first two years of the child's life. I conclude that the change in MLB likely did not change the lengths of time that mothers spent at home after the birth of children.

### 4.2.3 Theoretical framework and expected effects

The Becker model of fertility [Becker, 1991] assumes that a family maximizes a utility function which depends on the quantity of children, $n$, the quality of children, $q$, and an aggregate commodity that includes all other goods it consumes, $Z$, subject to a budget constraint dependent on the family income $I$. The central point in Becker's model is the interaction between quantity and quality of children, through the total amount spent on children: $p_c qn$, where $p_c$ is the cost of a unit of quality; this makes the budget constraint non-linear in the commodities which enter the utility function. This interaction between quantity and quality, Becker argues, is the reason why the demand for children is highly responsive to price effects, and to a smaller extent to income effects, even if children have no close substitutes.

In addition, the model introduces a fixed cost per child, $p_n$, which is independent of the quality of children, and expenditures on quality of children, $p_q$, which are independent of the number of children. Therefore, the family is faced with the following optimization problem:

$$max\, U(n, q, Z)\; s.t.\; p_c qn + p_n n + p_q q + \pi_z Z = I \tag{4.1}$$

Comparative statics indicate that a decrease in $p_n$, the fixed cost of *n*, would induce a substitution towards *n* and away from *q* and *Z*, as the shadow price of *n* would decrease relative to both *q* and *Z*. The interaction between *n* and *q* entails that the decrease in *q* further lowers the shadow price of *n*, while the increase in *n* increases the shadow price of *q*, which leads to even more substitution away from *q* and towards *n*.

In the Becker model, one of the main components of $p_n$ is the negative cost of governmental child allowance, where an increase in the governmental child allowance would lower $p_n$. Given this interpretation of $p_n$, the Becker model can be used to make predictions about the consequences of the change in the maternity leave benefits induced by the policy reform in Romania analysed in this paper. An increase (decrease) in maternity leave benefits lowers (raises) the fixed cost, and hence the price, of a child, so according to the model it should lead to an increase (decrease) in the number of optimal children per family, *n*. Therefore, for women who would receive a relatively higher maternity benefit, there should be an increase in the conception rate and/or a decrease in the abortion rate, both leading to an increased fertility, with the opposite effect for women who would receive relatively lower benefits as a consequence of the policy.

At the same time, the Becker model predicts that the increased (decreased) maternity leave benefits would decrease (increase) the optimal quality of children. Quality of children may refer to any of the components that form the child's human capital (e.g. health, education, skills). However, numerous studies have shown that the accumulation of human capital is determined, or at least influenced, already from the prenatal period by fetal shocks [Almond and Currie, 2011, Almond et al., 2007]) and/or maternal investments during pregnancy [Nilsson, 2014, Almond and Mazumder, 2011]. Prenatal investments usually refer to nutrition, medical care, and (abstinence from) the consumption of health damaging goods that affect fetal development, such as alcohol and tobacco. Hence, the decrease (increase) in the quality of children predicted by the model may be reflected in lower (higher) prenatal investments by the mothers, with the same reasoning applying to early investments in child health, which can also be included in the generic concept of quality of children. On the other hand, the change in maternity leave benefits may also generate income effects. The increase (decrease) of the benefits may therefore lead to an increase (decrease) in the consumption of other goods, which may enhance child quality (e.g. better nutrition, more prenatal medical care) or decrease child quality (increased consumption of health damaging goods such as alcohol and tobacco).

To summarize, the fertility demand framework makes clear predictions about the effect of the change in maternity leave benefits on fertility, but there is no clear prediction on the direction of the net effect on early investments in child health (i.e. on prenatal investment, child health at birth and investments in infant heaths) due to the opposing price and income effects. This paper aims to evaluate the net effects, with the reserve that with the data in hand I cannot analyse completed fertility, and so I may capture short term changes in fertility or timing effects, due to the relatively short time span between the policy and the time at which the data is recorded.

In addition to the expected effects on the outcomes of interest, one must also discuss the expected selection effects of the policy change announcement, due the fact that women could act strategically, as discussed in the previous section. This is particularly important since "treatment" (i.e. the receipt of fixed maternity benefits) is not based on an un-manipulable observable characteristic, such as birth date, but on the application date to MLB, which is decided by the mother after the birth of the child. This intuitively explains the need of an additional category of "potentially treated", which will be addressed in the section presenting the identification strategy.

The potential selections into conception and into live birth are summarized in Figure 4.4. The pregnancies conceived before the announcement that were carried to term or terminated before April 2003 should be unaffected by the policy change announcement. For the pregnancies conceived before the announcement that were above the legal abortion limit (the first trimester of pregnancy) at the time of the announcement, in April 2003, there should be no selection into conception among women, irrespective of their earnings, and there should be no selection into live birth induced by the announcement. Although these children would be born in 2003, prenatal and child investments may be affected as these mothers have the option of acting strategically regarding the application date to the MLB. For the pregnancies conceived before the announcement that were in the first trimester of pregnancy in April 2003, there should be no selection into conception among women, irrespective of their earnings. However, given that abortion on request was still an available option given the gestational stage of the pregnancy (for the pregnancies not already terminated until April 2003), the pregnancies that are carried to term could be a selected sample due to the policy change announcement. Both high earning and low earning mothers would have increased incentives to carry the pregnancy to term.[14] The pregnancies conceived after the policy change announcement in April 2003, that would be carried to term in 2004, are in fact those covered by the Becker model discusses above, and may be affected by both selection into conception and selection into carrying the pregnancy to term.

## 4.3 Data

### 4.3.1 Romanian Reproductive Health Survey

The main dataset used in this analysis is the 2004 wave of the Romanian Reproductive Health Survey (RHS-Ro). The survey was ordered by the Romanian Ministry of Health and the World Bank, and was conducted by several reputed international organizations.[15] The structure of the survey and the questions are fairly similar to

[14]High earning mothers would have decreased incentives to abort given that the opportunity cost of the already conceived child who would be born in 2003 (and would be entitled to the proportional MLB) is lower than that of a future-conceived child (who would be, presumably, entitled to the fix MLB), making it less beneficial to postpone childrearing. Low earning mothers, who are the potential gainers mothers of the reform, would have decreased incentives to abort given the opportunity to act strategically and delay the application to MLB until January 2004, to receive the fixed, higher benefit.

[15]RHS-Ro 2004 was designed to document the priority interventions required as part of the second phase of the Romanian health sector reform, financed by the Word Bank. The survey was conducted by the partnership between United Nations Population Fund , UNICEF, United States Agency for International Development, Center for Disease Control, World Health Organisation and the Romanian Institute

those in Demographic and Health Survey, albeit not as extensive. The data were collected between October and December 2004.

RHS-Ro includes a representative sample of 4441 women, aged 15-44, for whom it collects detailed records of all pregnancies, prenatal care indicators and early investments in child health for the most recent live birth of the woman, and detailed reproductive health information. In addition, it provides detailed socio-economic characteristics of the woman and the household in which she lives. The most important observable characteristics of the women in the sample are the date of birth, education, occupational status, marital status and household assets level. Descriptive statistics for the observable characteristics of all women included in the survey are presented in Table 4.1, column (1). Maternal education is coded using 10 educational categories, which I group in three levels: low (no schooling; primary education; secondary education), medium (upper secondary; professional education; high school education; post-high school education) and high (short term university degree; long term university degree; post-graduate degree). Most women in the sample have medium education (63%). Occupational status is recorded as *"Employed"* (49%) or "Not Employed", the later containing 10 subcategories, the most numerous being *"Housewife"* (27%).[16]. For employed women, despite the rich set of socio-economic characteristics available, RHS-Ro does not directly record the woman's wage income. Marital status is recorded using six categories, which are then grouped into two broad groups: married (legally married or cohabiting) and unmarried (never married, divorced, widowed, separated). The household assets level is captured by a composite measure of the household's cumulative living standard, calculated using data on the ownership of selected assets (such as TV sets; sanitation facilities etc.). It is given as a continuous index measure[17] based on which households are divided into 3 assets holding levels: "Low" (37%), "Medium" (51%) and "High" (12%).[18]

These socio-economic characteristics are recorded only at the date of the survey, with no retrospective questions. A potential problem that arises is that the new MLB policy may have changed the labor supply of women by making it profitable to work (even for a low wage) for a limited period of time and benefit from the

for Mother and Child. This insures the high quality of the data collected.

[16]Studying; Job Seeking; Unemployed; Not requiring work; Sick Leave; Prenatal Leave; Maternity leave; Housewife; Unable to work; Other.

[17]The points are on a scale from 0 to 15.

[18]5 levels categorization is also available, with the categories "Very Low", "Low", "Medium", "High" and "Very High".

high fixed MLB. Since I observe the occupational status at the time of the survey, the employed category may include, alongside women who were employed at the time of the policy change announcement, women who were housewives in March 2003, but entered the labor force to be able to claim the fixed MLB after 10 months of tax contributions. To investigate whether this occurred, Figure 4.5 presents graphical evidence on the evolution of the occupational status of women at aggregate level between 2002 and 2005, both as quarterly stock and quarterly rates.[19] Neither the stock or the rate graphics indicate that there would be a significant increase in the number/rate of employed women that would coincide with the policy change announcement, nor with the date of its implementation. This would suggest a rigid adjustment of the female labor force participation, with out of the labor force women going through an even lengthier process of finding employment. Moreover, even before the policy change, being employed even for a low wage was incentive compatible from the MLB perspective, since it would entitle the women to receive MLB in addition to the child benefits awarded to all mothers, which, as discussed previously, were much smaller than the MLB. In addition, the survey period does not coincide with any abnormal peak in the stock or the rate of employed women, and the fact that we consider pregnancies occurring up until Q2 2004, as will be discussed in the next section, when women had had to be employed for at least 10 months to qualify for the fixed MLB, attenuates the concern that we include in the treatment group women who are employed at the time of the survey but were housewives at the time of the policy change announcement. There is also no significant change, apart from the seasonal fluctuations, in the evolution of the number of the housewives. This attenuates to a certain extent the concern that over the relatively short time span between the policy change announcement and the survey date, there were large changes in the labor force participation of housewife women, but the concern remains valid. I will address this issue further in the robustness checks. Figure 4.5 also excludes another potential effect of the MLB policy: that the very high financial incentives attached to childbirth conditional on taxed labor would determine some of the women employed in the informal sector (without paying tax contributions) to switch to the formal sector. This would have resulted in an immediate increase in the stock of employed women as captured in

[19]The number of housewives is calculated as the difference between the stock of inactive females (defined as housewives, females in school and retirees) aged 20-44 and females 20 and above engaged in education. The stock of inactive females is recorded quarterly, whereas the stock of females engaged in any form of full time education is recorded with a yearly frequency.

Figure 4.5, which, as discussed, is not observed.

Each woman in the sample is asked retrospective questions about *all* her pregnancies: how it ended (live birth, still birth, abortion, spontaneous miscarriage), the date when it ended (month and year) and stage at which it ended (gestational months or weeks). For live births, it also records gender of the child, any disabilities he/she has, and whether it is still alive. There are 9997 recorded pregnancies. Unfortunately, there is no information about the father of these children apart from the marital status of the woman. However, the socio-economic status of the woman and the rearing conditions of the child can, arguably, be well captured by the covariates that are recorded, namely the woman's education, occupation and especially the household assets index.

A common problem with reproductive data derived from retrospective questions is recall bias relating to the accuracy or completeness of retrieved data. Indeed, for 1655 pregnancies I cannot infer the date of conception because the termination month is unknown, but for almost all of these pregnancies, the year of conception, the termination mode and the stage of the pregnancy is known. Women not reporting termination month are significantly older at the time of the survey, and their age at pregnancy is significantly smaller than the age at pregnancy for the pregnancies which do have termination month reported, but there are no significant differences in their other observable characteristics (educational level, marital status, place of residence); this is consistent with recall bias. Thus, I impute the conception month, which would both preserve sample size and correctly account for the prevalence of abortion.[20]

For the last pregnancy that ended in live birth, RHS-Ro collects detailed information about prenatal investments, child health at birth and early investments in child health. The following data is available: status of pregnancy (intended/ unintended/ unwanted), smoking during pregnancy, alcohol consumption during pregnancy, prenatal supervision, prenatal vitamin supplements, birth weight of the child, postnatal visits, number of days in hospital after birth, information related to breastfeeding, infant vitamin supplements.[21] This rich information is usually not

[20] I use a multinomial logistic regression for a nominal variable (the month of conception). The independent variables used are age at pregnancy, the number of children at that specific pregnancy, the number of previous abortions, educational dummies, marital status and urban dummy. In all regressions I include a dummy for pregnancies with imputed conception date. Excluding these pregnancies does not significantly change the estimated effects (results available on request).

[21] RHS-Ro also records detailed information about the last abortion (including questions on motive for abortion, place where it was performed, complication post-abortion, etc.), family planning practices,

available in vital natality files or in other register data, which makes the RHS-Ro a very interesting resource to exploit when studying financial incentives and fertility outcomes.

### 4.3.2 Sample and descriptive statistics

In the main analysis, I consider the pregnancy as the observational unit, and consider pregnancies occurring until (and including) July 2004. As most of the 2004 RHS-Ro interviews were conducted in October and November 2004 and there is a possibility that recently pregnant women would not be aware of the pregnancy, and therefore pregnancies occurring after July 2004 would be under-represented in the survey (which is confirmed by the total number of pregnancies by month of conception); moreover, these pregnancies would be in the first trimester at the time of the survey, hence abortion would still be available, and there would be no possibility to infer if the pregnancy would be carried to term or terminated. I further restrict the sample in the main analysis to pregnancies (or births) occurring at most 15 months before the policy change announcement to obtain a symmetric 15 months window on each side of April 2003. I classify conceptions (births) occurring between January 2002 and March 2003 as occurring in the pre-announcement period, $t_0$, and conceptions (births) occurring between April 2003 and July 2004 as occurring in the post-announcement period, $t_1$. The narrow window reduces the probability that the effects are confounded by time trends in fertility and reproductive behavior, but includes sufficient repeated observations per month to allow controlling of seasonal effects, which are known to influence fertility patterns.

Columns (2)-(4) of Table 4.1 present the descriptive statistics for the observable characteristics of women who conceive, use abortion and respectively give birth in the 31 months window, the analysis period, where the observational unit is the woman. Compared to all women in the sample, who are not necessarily mothers, women who conceive are younger and more likely to be married. Also, housewives are over-represented in the sample of women who conceive, in line with fertility models which link the number of children to the opportunity cost of tine of the woman. Although the distribution in terms of educational attainment is similar when comparing all women in the sample with those who conceive/abort/give birth in the analysis sample, those who carry the pregnancy to term are more likely to

sexual behavior, reproductive health and healthcare utilization, STD knowledge and domestic violence.

live in low or high assets level households.

Table 4.2 presents the descriptive statistics for the observable characteristics of women who conceive/abort/give birth, where the observational unit is the pregnancy, hence a woman who has had multiple pregnancies in the analysis period will be included repeatedly in the sample. Table 4.2 Panel A includes the sample of all conceptions; Panel B includes conceptions that are terminated using abortion and Panel C includes conceptions which are carried to term. In addition, the table presents the raw averages in the pre-announcement period and in the post-announcement period, belonging to all, employed and housewife mothers.

Employed women who conceive after the policy change announcement are negatively selected relative to those conceiving before the announcement: a lower educational achievement level, less likely to be married, lower average household assets index. There is a lower probability that a pregnancy will be terminated using abortion, but not significantly so. This negative selection appears to be equally driven by the women who carry the pregnancy to term and those who use abortion. This pattern is consistent with the theoretical prediction that the increased financial incentives attached to childbirth increase the conception behaviour of the employed women who would benefit the most, i.e women with the lowest wages (which are expected to have the poorest observable characteristics), who then carry the pregnancy to term. For women who use abortion, in the post announcement there is a negative selection, with significantly more low education, low household assets level women. Although insignificant, there also appears to be a larger share of better off women (high education, high household assets level), which may indicate a polarization process, in which women who resort to abortion under the scheme entailing large financial incentives attached to childbirth are either the very worse off for whom the financial benefits would be insufficient, or those better off, who either do not use abortion due to financial constraints, or precisely because they would be those negatively affected by the policy change and they respond to the relative worsening of their financial situation. However, these before-after comparisons are insufficient to appraise the selection effects of the policy change; they do not account for any potential time trends or seasonal effects that may have changed the composition of mothers even in the absence of the policy reform. For housewife women who conceive, the selection on observables is insignificant, although it appears to be mildly negative. This applies for both housewives who carry the pregnancy to term and those who terminate the pregnancy.

## 4.4 Identification strategy

### 4.4.1 Specification

In order to retrieve the causal effects of the policy change on the outcomes of interest I employ a *Double Difference* estimation strategy, adjusted to account for the fact that mothers could act strategically with respect to the date of application to the benefits. The underlying identification assumption is that that the changes over time for a specific group of non-participants provide a proper counterfactual for the participants, i.e. the parallel trend assumption.[22] Assuming that a suitable control group is available, which will be discussed shortly, the richest specification of the Double Difference design is:

$$\begin{aligned} Y_{im} = \alpha + \beta_1 T_{im}^{Cert} + \beta_2 T_{im}^{Pot} + \beta_3 Treated_i + \beta_4 T_{im}^{Cert} * Treated_i + \\ \beta_5 T_{im}^{Pot} * Treated_i + \gamma_1 m + \theta_q + \delta_1 X_{im} + \delta_2 X_i + \epsilon_{im} \end{aligned} \quad (4.2)$$

where $i$ indexes a pregnancy conceived/born in month-year $m$.

The first outcome studied is the occurrence of pregnancy; in this case, the outcome is the monthly aggregated number of conceptions per 1000 women; occurrence of pregnancy at individual level will also be studied in a woman fixed effects framework, whereas in a DD framework one cannot consider the outcome of conception at individual level. The second outcome is the probability of abortion; $Y_{im}$ is 1 if the pregnancy ends in abortion and 0 otherwise. The third set of outcomes are conditional on live birth, capturing: i) prenatal maternal investments: alcohol and smoking during pregnancy indicator (1 if the mother ever smoked or consumed alcohol during pregnancy), month of first prenatal control (continuous variable) and prenatal vitamin supplements during indicator (1 if the mother reports having taken vitamin supplements during pregnancy); ii) child health at birth: low birth weight indicator (1 if birth weight of the child is less than 2500 grams), number of days of hospitalization at birth (continuous variable) and a postnatal control indicator (1 if the mother and child undertook a postnatal medical visit in the first month after birth); iii) early investments in child health: breastfeeding indicator (1 if the child was breastfed), number of months of breastfeeding (continuous vari-

[22]This identification assumption is milder than that entailed by a simple Before-After specification which would require that in the absence of the policy change, the outcomes of the affected group of individuals would not have changed. This is not credible in the context of fertility related outcomes, even after controlling for time trends and seasonality and conditional on the observable characteristics.

able) and infant vitamin supplements indicator (1 if the mother reports giving the infant recommended vitamin supplements).

$T_{im}^{Cert}$ and $T_{im}^{Pot}$ are mutually exclusive indicator variables capturing the time period of conception or birth. As such, for outcomes conditional on conception (occurrence of pregnancy and probability of abortion), $T_{im}^{Cert}$ is 1 for the pregnancies conceived after April 2003 and $T_{im}^{Pot}$ is 1 for the pregnancies conceived between January and March 2003 which were still in utero in the first trimester at the time of the announcement.[23] For outcomes conditional on live birth (prenatal investments, child health at birth and early investments in child health), $T_{im}^{Cert}$ is 1 for the pregnancies delivered after January 2004 which would be certainly affected by the policy change, and $T_{im}^{Pot}$ is 1 for the pregnancies delivered between April and December 2003, as the mothers could potentially delay the application to the MLB and receive the fixed sum after January 2004.

$Treated_i$ is 1 for pregnancies (births) of women in the treatment group, who were affected by the policy change, and 0 for pregnancies (births) women in the control group, who were not affected by the policy change. Treatment and control groups will be discussed shortly. $m$ is a linear time trend standardized to be 0 in April 2003. $\theta_q$ are conception/birth quarter fixed effects. $X_{im}$ is an individual and time specific vector of characteristics to control for the fertility history of the woman prior to the current pregnancy, specifically the number of previous abortions and the number of live children at the time of the pregnancy i, and age at pregnancy $i$. $X_i$ is a vector of individual characteristics of the mother, considered fixed, as they are only measured at the time of the survey: her educational level, marital status, household size, and a rural dummy.[24] $\epsilon_{im}$ is the individual error term. I estimate the regression using ordinary least squares and present robust standard errors to account for potential heteroskedasticity.[25]

[23]This important correction accounts for the fact that some of these pregnancies might have already been terminated prior to the announcement, and therefore not all pregnancies conceived in January-March 2003 are potentially treated.

[24]Since these are only correlated with the actual characteristics at the time of the pregnancy, the fact that they are subsequent to the outcome itself may induce measurement error in our estimations. However, the results are not sensitive to their exclusion.

[25]These specifications use conceptions as the unit of observation, which would suggest clustering at the mother level. However, only 6% of the mothers who conceive in the time-window of the analysis have multiple pregnancies. However, if I do cluster at mother level, the estimated standard errors are very close to the robust standard errors.

### 4.4.2 Treatment and control groups

**Treatment group** Due to the design of the MLB policy in Romania, which conditions the receipt of the benefits on wage tax contributions prior to childbirth, only employed women are entitled to apply to MLB, whereas out of the labor force women are precluded from doing so. Employed women were affected by the policy change depending on their wage level (i.e. benefited from the change if their monthly wage income was below 7.6 mil. ROL (213 USD) and were disadvantaged if they had a monthly wage income above the threshold), as discussed in Section 4.2.3. However, despite the rich set of socio-economic characteristics available, RHS-Ro does not directly record the woman's wage income. As such, given that approximately 80% of women were potential gainers of the reform (as discussed in Section 4.2), I consider a baseline estimation in which the treatment group consists of all employed women. This makes the assumption of a uniform impact, and the coefficients would reflect the average effect on employed mothers.

However, since this average effect is likely composed of two opposing effects that may cancel each other out, I investigate the effects on subsamples determined by the household assets level; although technically employed women were favored or disadvantaged by the new provision of the MLB law depending on their pre-birth earnings, the "bite" of the policy reform may be better reflected by the household wealth. To this end, I use the household asset index to split the sample such that it matches the 80-20 division between gainers and loosers of the reform; this leads to a split into a group with non-high household asset index (women with low and medium household asset index, accounting for 80% of the sample) and a group with high household asset index (accounting for 20% of the sample of women). I estimate Equation 4.2 on these two subsamples.

**Control group** Due to the wage tax contribution requirement, out of the labor force mothers were, in theory, unaffected by this policy change and so they are a natural candidate category for the counterfactual group. The preferred subgroup of the out of the labor force women which I use as the main control group in Equation 4.2 are the housewives (HW). Housewife women constitute an intuitive counterfactual for the employed women as they are non-participants in the treatment by law, and are more comparable in terms of observable characteristics to the employed women than the other out of the labor force categories (students and pensioners) in terms of the fertility cycle. Moreover, they are the second most numerous group by mothers occupational status after employed women. The validity of the parallel trend assumption in the outcomes of housewives and employed women in the absence of the policy is explored in Section 4.4.3 below. A potential problem with the selected control group is that women in this group may change their labor market status, and such an endogenous change of the individual labor force participation as a response to the reform would violate the identifying assumtions of the DiD by making the employed women incomparable over time. In addition to the claims made in Section 4.3.1 regarding the rigid adjustments of the labor market which seem to attenuate this problem, I try to address this issue in several ways, but also resort to a robustness check where I use another distinct control group, employed women in the neighboring country Republic of Moldova, with a more detailed discussion in the Robustness section.

### 4.4.3 Parallel trends assumption

Figure 4.6 plots the average maternal characteristics by quarter of conception between 2000 Q1 to 2004 Q3, for Employed and Housewife mothers.[26] The figures show a fairly similar evolution in the composition of the observable characteristics of employed women and housewife women who conceive in each trimester before the policy change announcement, although there are, as expected, level differences in the anticipated direction. The fact that there is generally no clear diverging trend in the composition of observable characteristics between the treatment and the control group[27] provide evidence supporting the parallel trend assumption, which underlies the double difference identification strategy. Figure 4.7 plots the number of conceptions and share of conceptions ending in abortions for Employed and housewife women, by quarter of conception between 2000 Q1 to 2004 Q2. Figure 4.8 plots the average outcomes conditional on pregnancy which I analyze, by quarter of birth and occupational status of the mothers. Although more noisy than the maternal characteristics, they appear not to contradict the parallel trend assumption required by the double difference strategy. In addition, in the main regressions I control for a linear time trend and for quarter of conception fixed effects.

[26]Except for age at pregnancy, all other maternal characteristics are measured at the time of the RHS survey (Oct/Nov 2004), and not the time of the conception (the observation unit).

[27]With the exception of "married" status, which appears to be slightly diverging in the pre-announcement period and then re-converging after the policy change announcement in the post-announcement period.

## 4.5 Main results

### 4.5.1 Probability of conception

The first stage of the analysis is to examine whether there is a significant change in the number of pregnancies occurring to employed women, irrespective of how they end. Using the conception date of all pregnancies in the sample, I analyze whether there was a significant increase in the number of conceptions per 1000 women in the months following April 2003. Since the dependent variable is at aggregate level rather than individual level, no individual covariates can be used.

Table 4.3 presents the estimation results for the number of conceptions per 1000 women occurring each month. Columns (1)-(2) present the double difference estimation results, first with only a monthly time trend, then with quarter of conception fixed effects. Column (3) estimates the same Difference in Difference on the sample of women with non-high household assets index levels, whereas column (4) uses the restricted sample of women with high household assets index level.

Results indicate that there is an increase in the number of conceptions per 1000 women in the period after the announcement of the policy change of 0.46, but statistically insignificant (tval=1); this is mainly driven by the increase in the number of conceptions per 1000 women with non-high household assets level, of 0.65, but this is still insignificant. In contrast, the effect for high household assets level is an imprecise zero. The difference between the conception rate of women from non-high and high household assets is positive and rather large, but marginally insignificant due to the large standard errors.

I also investigate the compositional changes in the observable characteristics of women who conceive after the policy change announcement. I estimate Equation 4.2 where $Y_{im}$ is, in turn, an observable maternal characteristic of interest: age at pregnancy, educational level (captured by three dummy variables for each broad level of education), marital status, non-high household assets level and place of residence (rural vs. urban). Table 4.4 Panel A presents the estimation results for the observable maternal characteristics of all mothers who conceive, irrespective of how the pregnancy ends. After controlling for time trends and seasonality in the Double Difference framework, the observable characteristics of employed women relative to housewife women do not change significantly. However, they appear to be older, less likely to be married and more likely to be from urban localities. The point estimate on the dummies related to household assets level, which proxy household wealth, are small and have very large standard errors, which precludes even a tentative interpretation of the selection in term of these observables.

Unfortunately, because of the lack of individual data on wages, I cannot calculate the benefit elasticity of the conception rate per 1000 women. However, using aggregate data on female wages to impute the average potential MLB under the proporional benefit regime, I can approximate that the average benefit elasticity of the conception rate for the group of women with non-high households assets would be roughly 0.06. This elasticity is, however, uncomparable to those found in previous studies, since they only observe live births, and not conceptions at individual level. In order to be able to compare the effects found in this study with those in the previous literature, I estimate the effect of the policy change announcement on the number of live births per 1000 women, as does, for instance, [Raute, 2014]. Estimating Equation 4.2 on the number live births per 1000 women, and using the estimated treatment effect for the sample of women with non-high households assets level gives an average benefit elasticity of 0.083. [Raute, 2014] calculates a benefit elasticity of live births per 1000 women of 0.11, [Milligan, 2005] puts the average benefit elasticity at 0.107, whereas [Baughman and Dickert-Conlin, 2003] estimates an elasticity of 0.06. Although these elasticities capture the effects of variuous types of financial incentives attached to childbirth, and not fixed MLB specifically (with the exception of [Raute, 2014]), and use very large administrative datasets, the fact that the approximated elasticity I calculate is well in their range indicates that the results I obtain in the context of the Romanian policy change are reasonable, despite not being significant.

### 4.5.2 Outcomes conditional on conception

Next, I estimate the impact of the policy change announcement on outcomes conditional on conception having occurred, namely how the pregnancy is terminated (abortion versus live birth). I model abortion prevalence at individual level: in Equation 4.2 the outcome variable is 1 if the pregnancy ends in abortion and 0 if it ends in live birth. Table 4.5 presents the estimation results, where the first column shows the simplest double difference specification and the following columns build up to the richest specification, and then presents the estimation results for the sub-samples defined by the household asset index.

The results reveal a rather large but insignificant reduction in the probability that a pregnancy is terminated using abortion, of 4.7 percentage points (approximately 10% of the mean). Controlling for individual characteristics and quarter of conception fixed effects do not seem to affect the estimated treatment effect, but the standard errors remain large, which does not exclude a zero effect. However, when splitting the sample according to the household asset index, the treatment effect for the group with low and medium household asset index, i.e those who would have likely benefited most from the policy change, almost doubles, indicating a reduction of 8.8 percentage points (roughly 20% of the mean), and the relative size of standard errors significantly decreases, reaching a t-statistic of 1.30. In the sample restricted to high household assets levels, the treatment effect is of opposite sign (and almost equal magnitude), but with much larger standard errors, probably due to the much smaller sample size. These two effects go in the expected direction: the non-rich households responded to the increased financial incentives attached to childbirth by reducing the probability of abortion, once a pregnancy occurred, suggesting that financial constraints were an important determinant of the decision to abort. For richer households, the policy change had the opposite effect, increasing the probability of abortion (albeit one cannot exclude a zero effect here due to the very large standard errors), and again indicating that this type of reproductive behavior responds very quickly to financial incentives. The difference between the estimated treatment effects in the two sub-samples based on household assets level is very high, with the non-high household asset level women having a 16 percentage points lower probability to terminate the pregnancy after the policy change announcement; this is, however, indistinguishable from 0 due to the large standard error on this difference.

To understand the mechanism behind the reduction in the probability of abortion, I use the information collected on the most recent recent abortion of each woman in the sample (which then constitutes a sub-sample of all abortions registered in the dataset) regarding the main reason for termination. There are three broad categories: heath reasons (maternal or fetal health status), socio-economics reasons, and the desire to limit fertility. The first thing to note is that for the pre-announcement window, detailed information is recorded for 165 abortions, whereas for the period post announcement, information for 183 abortions is recorded, despite the fact that the total number of abortions decreased in the post announcement period relative to the pre announcement period. Since the module focuses only on the last performed abortion, this suggests that in the pre-announcement period there were more abortions per woman.[28] I use a multinomial logit model to estimate Equation 4.2, where the dependent variable is the categorical variable recording the main reason for abortion. For the whole sample, the marginal effects of the main interaction term, $Treat{*}T^{cert}$, show that the probability of stating health reasons is higher by 8 percentage points (z=0.90), the probability of stating socio-economic reasons reasons is higher by 8 percentage points (z=0.68), and the probability of stating fertility limitation reasons lower by 16 percentage points (z=-1.37), very close to significance. For the sub sample of women with non-high household assets levels, the marginal effects at the mean indicate that the treated group in the post announcement period (i.e. the marginal effect of the $Treat * T^{cert}$ term) have a significantly lower probability of stating limiting fertility as the main reason, by 22 percentage points (z=-1.74), and a higher, but insignificant probability of stating socio-economic reasons (17 percentage points, z=1.33) and health reasons (5 percentage points, z=0.58).[29],[30] This would suggest that the policy (announcement) changed the desired level of fertility.

None of the previous studies that investigated the effects of financial incentives on fertility have access to individual level abortion data, hence my results on the probability of abortion are not directly comparable to any of the previously obtained results. However, [Raute, 2014] uses aggregate quarterly data on abortion and finds that the increase in the potential MLB lead to a discontinuous decrease of roughly 3% of the mean in abortion rates of married women right after the announcement of the policy, and [González, 2013] finds that the one-off child benefit reduced abortions by 6 to 7%. The effects I find, namely a reduction of 8.8 percentage points, which represents approximately 20% of the mean, are therefore much larger. This may be due to the specific context of Romania, where abortions were extensively used as fertility-limiting methods, whereas the rates were much lower in Germany or Spain. Therefore, the reduced use of abortions would be a more effective and rapid means of increasing fertility in Romania, but not so much in the other two, more developed countries. Also, the individual level analysis may capture more accurately the policy affects than an aggregate level analysis.

[28] In fact there were 293/165=1.77 abortions per woman in the pre-announcement period, and 282/183=1,54 abortions per woman in the post announcement period, both conditional on the women having at least one abortion.

[29] For the sub-sample of women with high level of household assets index, the ML does not converge and so Equation 1 cannot be estimated with *mlogit*.

[30] The estimation results are available upon request.

In terms of selection on observables, Table 4.4 Panel B reveals that women who terminate the pregnancy are less likely to be married, less likely to live in households with non-high levels of assets (suggesting that women from poorer households are less likely to abort after the policy change) and less likely to be from a rural area, albeit insignificantly so. The treatment effects on the other observable characteristics is very small in comparison to their estimated standard errors, so they are not interpretable. Panel C shows that employed women who carry the pregnancy to term after the policy change are, relative to housewives, (insignificantly) older, more likely to be from non-high assets level households and more likely to be married, who were likely financially constraint before the policy change. Interestingly, these are appear to be opposing effects, suggesting we have identified the "switchers", the marginal mothers whose behavior is influenced by the policy.

### 4.5.3 Outcomes conditional on live birth

As the final stage of the analysis I study the effects of the policy change announcement on the outcomes of the pregnancies carried to term: 1) prenatal investments: smoking and/or alcohol consumption during pregnancy, month of the first prenatal control, vitamin supplements during pregnancy; 2) child health at birth: low birth weight, number of days in hospital after birth, probability of a postnatal medical control; and 3) early life investments in child health: breastfeeding, months of breastfeeding and infant vitamin supplements. These outcomes were chosen either because they are established in the health economic literature (e.g. the low birth weight indicator), or because medical research has shown they play an important role in determining child health.

It is important to remember that the RHS-Ro registers detailed information about the health at birth and multiple measures of investments in child health at birth for the last born child of each interviewed woman. Thus, the sample of births with information on child health and investments is a subset of the pregnancies recorded as being terminated with live births in the retrospective survey.

Table 4.6 Panel A presents the estimation results for the entire sample for the three sets of outcomes conditional on live birth: pre-birth investments in columns (1)-(3), health at birth of the child in columns (4)-(6) and for early investments in child health in column (7)-(9). All present the estimation results for the richest double difference specification. Panel B presents the results for the sub-sample of women with non-high household assets level and Panel C the results for the women with high household assets level. The last row of the table presents the p-value on the difference between the estimated coefficients on the interaction term of interest between non-high indicator from a fully interacted model.

Employed mothers who give birth after the introduction of the new MLB appear to have a 13 percentage points larger, but insignificant (s.e. = 0.084), probability to smoke or consume alcohol during pregnancy relative to housewife mothers, which may have detrimental effects on the health of the child. Although even more insignificant, the results show negative coefficients on the month of first prenatal control and for probability of the mother taking prenatal vitamin supplements.

Despite the apparent worsening behaviors during pregnancy, the negative treatment effect on the outcome "low birth weight" indicates a marginally insignificant (t = 1.55) improvement in the health at birth of children born after January 2004 (hence conceived after the announcement of the policy change) to employed mothers relative to housewife mothers, reflected in the reduction of the probability that the child is born with low birth.[31] This is in line with the findings in [Rossin, 2011], that finds that the introduction of unpaid maternity leave decreases the likelihood of low birth weight, potentially due to the decreased stress that the mother is subject to in the prenatal period. Despite this, they have a slightly higher probability of having a medical visit in the first month after birth and appear to stay longer in the hospital after birth -with the reserve that the effects are insignificant. Regarding the early investments in child health, results in Panel A indicate point estimates close to zero on the probability of breastfeeding probability of giving the infant vitamin supplements but a somewhat large (but insignificant) increase in the length of breastfeeding for the children that are breastfed.

In Panel B, results indicate that most of the effects on alcohol and tobacco consumption during pregnancy, probability of low birth weight, postnatal consult and days in hospital observed in Panel A are driven by the effects on women with non-high household assets level. In particular, there is a 13 percentage point (s.e. = 0.09) increase in the probability of alcohol and tobacco consumption during pregnancy, but also a 13 percentage points (s.e. = 0.088) decrease in the probability of low birth weight which suggests an (insignificant) improvement in the health outcomes at birth. Again, despite being large in magnitude, these treatment effects are statis-

[31] Including dummy variables to capture the household assets level reveals a significant negative treatment effect on the probability of low birth weight.

tically indistinguishable from 0 due to the large standard errors. Months of breast-feeding, on the other hand, has a virtually 0 estimated coefficient, as opposed to the large point estimate in Panel A. An interesting effect is that on the number of hospitalization days after birth, which increases by 2.3 days for children of women with non-high household assets levels. Although they may appear to suggest worsening health of the children, it may actually capture the increased financial resources of the mothers. In fact, the RHS-Ro records whether the mothers made any informal payments to doctors and nurses in relation to the birth of the child, and if yes, the sum that was paid. Women with non-high household assets are 5 percentage points (s.e. = 0.12) *more likely* to give such informal payments after January 2004, but they give on average 1,430 thousand ROL (s.e. = 1330) (2.3 USD, but 38% relative to the average informal payment) *more* than they did before January 2004, relative to housewives. At the same time, women with high household assets levels are 25 percentage points (s.e = 0.29) less likely to make informal payments, and when they do, these sums are 2387 thousand ROL (s.e. = 2667) (almost 7% of the mean) *smaller* after January 2004 than before, relative to housewives. The large point difference between these estimates suggests that an increase in the anticipated disposable income is positively related to the amount of informal payments, which in this setting equates to the quantity or quality of the medical care the infant receives at birth.

For the high household assets level women (in Panel C), as opposed to the non-high household assets level women, the treatment effect for the binary variable capturing health damaging behaviors (alcohol consumption and smoking) seems to be slightly larger in magnitude, suggesting a 19 percentage point increase, but still insignificant (s.e. = 0.128), and also have a large and negative estimate of the effect on prenatal vitamins durinh pregnancy, of 27% (s.e. = 0.178). Regarding the variables that reflect child health at birth, the estimated coefficients on low birth weight and on number of days in hospital after birth are small in magnitude and very insignificant; probability of postnatal consult is, however, rather large but still insignificant. Despite this, they have a 13 percentage point higher probability of breastfeeding the child, and a significantly longer period of breastfeeding, which may suggest compensatory investments on behalf of these women who might have been negatively affected by the policy change. The last row of the table, containing the p values of the differences between the treatment effects in the two sub-samples reveal that there is a statistically significant difference in the probability of low birth weight, with the non-high household assets level sub-sample having a significantly more negative treatment effect, which suggests that the children of poorer women have a significantly better health at birth as reflected by this indicator. In the same time, richer women breastfeed they children significantly more compared to non-high household assets level women, along the line of compensatory investments outlined earlier.

## 4.6 Robustness checks

### 4.6.1 Restricted sample: validity of the control group

The main challenge to the Double Difference identification strategy is the validity of the control group, mainly the parallel trend assumption. The problem that arises in the particular case of the policy change I am analyzing is the possibility that women in the control group took up the treatment by changing their occupational status as a response to the policy change. In particular, it is possible that some women reporting to be employed in October/November 2004 were out of the labor force in April 2003 but (re)entered the labor market (irrespective of the wage received), so that they could benefit from the fixed MLB after the mandatory contribution period; given that I use a cross-sectional dataset with no retrospective questions on the occupational status, I would include these women in the treatment group which would induce a selection bias in the double difference estimates.[32]

In the absence of retrospective questions about the occupational status of the woman, in order to limit the potential bias arising from the issue of changing occupational status as a response to the policy change announcement I restrict the sample of pregnancies to those conceived within three months around the announcement date. By doing this, I increase the probability that women whose occupational status is "employed" in October 2004 *and* who conceived just after the policy change announcement were also employed at the time of the conception; this is due to the fact that even if housewife women would be able to enter the labor force that rapidly, they would not fulfill the 10 months mandatory contribution criterion

[32] If among the women conceiving after the policy change announcement, employed at the time of the survey, there are relatively more former housewives than before the policy change announcement, and by the logic presented above they would be less likely to abort (since they entered the labor force precisely to gain access to the MLB after giving birth), then there could be an upward bias in a Difference-in-Difference specification.

until the birth of the child if they would conceive in the first three months after the announcement date, and so would not be able to benefit from the fixed MLB. At the same time, women registered as housewives in October 2004 and who conceived around the announcement date were likely to have been housewives at the time of the conception. Hence, the certainly treated group are the employed women conceiving in April, May and June 2003. I still have to acknowledge the fact that employed women conceiving in January-March 2003 were in the first trimester of pregnancy at the time of the policy change announcement, hence both abortion was still available and there existed the possibility of strategic delay of the application process to MLB. This renders this group unsuitable as a valid baseline level for the treatment group in the pre-treatment period. To circumvent this problem and the issue of seasonality in fertility outcomes, I use the conceptions occurring in April-June in the previous years (2000, 2001, 2002) as the baseline pre-treatment levels, with employed women as the control group and housewives as a control group. The results, presented in Appendix, generally have the same direction and are close in magnitude to the main results.

### 4.6.2 Cut-off date variations: $T^{Cert}$ coverage

The first attempt to change the law regarding maternity leave benefits was made public in March 2003, with the first emergency ordinance. However, the final change came in April 2003, with the second emergency ordinance, but the law was modified in October 2003. Although ex post there were no more changes between April and October, ex ante the public expectations might have been different, and there is the possibility that the April 2003 changes were not perceived as final, given the previous radical change of the attitude of the government towards the MLB. However, as the ordinance was voted into law in late October, this may be a more precise signal for the population, and perceived as final. Therefore, I re-estimate the main specifications using October as the policy change announcement date instead of April.

Table 4.7 presents the estimation results for the number of conceptions per 1000 women, which are in line with the main results: an increased, albeit insignificant, conception rate per 1000 women. The difference between conception rates of women from households with non-high and high assets level accentuates, with women from poorer households having a relatively large increase, of 0.73 conceptions per 1000 women, and women from richer households a decrease in the conception rate of 0.39 conceptions per 1000 women. This is in line with our prior, as by October women had the possibility of changing their fertility behavior according to the incentives, given that conception may not be immediate.

The results in Table 4.8, which estimate the probability of abortion using the October threshold also reveal the same pattern of results as in the main estimation, with a reduction in the probability of abortion in the entire sample. Employed women from non-high assets level households have a decreased probability of abortion, whereas women from high assets level households have an increased probability of abortion after change was voted into law, with the point difference between these two sub-samples being even larger than in the main specification.

Table 4.9 estimates the policy effect for the outcomes conditional on live birth using the October threshold. However, in this case, I only modify the potentially exposed group, since there is no uncertainly regarding births that would certainly receive the fixed benefits. As with the previous outcomes, the estimation results are similar to those in the main specification, and some may even become larger and more significant. As such, women from households with high assets levels are significantly more likely to consume alcohol or smoke during pregnancy, and less likely to take prenatal vitamins, whereas these effects are much more diminished for women from poorer households. In the same time, the health at birth of children belonging to women from poorer households significantly improves, whereas it insignificantly worsens for better-off women. This appears to be compensated for by higher postnatal investments in child health at birth, with better off mothers being more likely to breastfeed and conditional on this, they breastfeed their children longer, an effect not observable for women from households with non-high assets levels. For the other outcomes studied, the pattern when using the October threshold is the same as in the main regression, but they remain insignificant.

### 4.6.3 Alternative control group: Moldova DHS

Despite the attenuating circumstances, housewife women mat not be the ideal control group for employed women, as they are likely to have unobserved characteristics that selected them into this occupational category (as opposed to the employed category) which may be correlated to their fertility outcomes and reproductive behaviors. The ideal control group would be employed women in the fertile age who were not affected by the policy change.[33] In an attempt to find such a group, I use the 2005 wave of the Moldova Demographic and Health Survey (Md-DHS) conducted in Republic of Moldova, a neighboring country, in which the majority ethnic group are the Romanians.[34] Post-communist fertility patterns in the two countries are similar, and Moldova has been used as a control group for Romania in earlier fertility studies ( [Pop-Eleches, 2010]). During 2002-2004, the MLB in Moldova were constant, and hence it can provide an alternative counterfactual for the change in the MLB policy that occurred in Romania in 2004; moreover, the Moldova DHS contains almost the same type of information as my primary dataset, and were conducted by the same organization, which increases their comparability.[35] I therefore use employed women in the Republic of Moldova as an alternative control group, and I estimate Equation 4.2 on the same 31 months window. However, these results should be interpreted with caution, as despite the similarities in the historical backgrounds of the two countries and the structure of the two reproductive health surveys, the data may not be completely comparable. Moreover, the Md-DHS does not calculate the household assets index, which prevents me from employing the same type of sample split by household assets level.[36]

Table 4.10 presents the results from this exercise. In Panel A, the effects on the number of conceptions per 1000 women and in Panel B for for the probability of abortion. For the number of conceptions per 1000 women, the results do not appear robust to this alternative specification, with opposite signs from the main results; however, the associated standard errors are more than 10 times larger than the estimated effects, which attenuates the concern on the sign of the estimated treatment effect, since this is, in fact, undistinguishable from 0 in the specification where em-

[33] Employed women with completed fertility would not be a good control group since Romania underwent large changes in fertility pattens after the fall of the communist regime, when contraception methods and abortions were illegal. Therefore older women would not have had a similar fertility pattern as younger women even in the absence of the policy change, due to different historical circumstances in their fertile years.

[34] Republic of Moldova was, until 1943, part of Romania, and was then integrated in the Soviet Union. From 1943 until 1991, Moldova had a communist regime, similar to the one in Romania (but did not have a ban on contraception), and transitioned to a market economy in 1991, soon after Romania's transition of 1990.

[35] The 2005 wave of the Moldova DHS does not include information about the month of pregnancy when the pregnancy terminated, so to obtain the month of conception I use 9 months for pregnancies that end in live birth and 2 months for pregnancies that end in abortion.

[36] In Tables 4.10 and 4.11 I split only the treatment group in Non-High and High assets level, with both sub-samples containing the same control group, i.e. all employed women in Republic of Moldova. This is different from the sample split in the main results in Table 4.6, where both the treatment and the control group are split by their household assets level index.

ployed women from Moldova are the control group. For the probability of abortion outcome, however, the results of this robustness exercise are very close to the main results, showing a decreased probability of abortion after the announcement of the policy change, larger for women from households with non-high assets levels. In this case, the standard errors are much smaller relative to the estimated treatment effect (and in fact close to those in the main specification), but are still too large to detect a significant effect.

Table 4.11 presents estimation results for the outcomes conditional on live birth that are available in both datasets; three outcomes are only recorded in RHS - Ro: alcohol and smoking during pregnancy, days in hospital and vitamin supplements to infant, and hence cannot be analysed in this exercise. The treatment effects on month of first control, low birth weight indicator, postnatal consult and breastfeeding face the same direction and comparable standard errors with the main results, whereas for prenatal vitamin supplements and months of breastfeeding the treatment effects in this exercise have opposite signs relative to the main results. If the result on prenatal vitamin supplements is uninformative due to the large standard errors, the effect on breastfeeding is significant and negative, indicating a relative decrease in the number of months of breastfeeding for Romanian employed women relative to Moldavian employed women, after the policy change announcement.

### 4.6.4 Woman fixed effects

In the main analysis I evaluated the impact of the policy change announcement on the probability of conception by using the aggregate number of conceptions occurring before and after April 2003. An alternative way to analyse the fertility effects of the change in financial incentives attached to MLB is to use individual level observations of occurrence of pregnancy in a pre-post difference design coupled with individual fixed effects, on the entire sample of women in the RHS-Ro. Similarly, one can analyze the within-mother use of abortion, and whether the announcement of the policy change alters the probability that a pregnancy is carried to term relative to the prior pregnancy. Such a fixed effect specification would also be justified if there are reasons to suspect that the OLS estimates in the main analysus suffer from omitted variable bias, and in particular unobserved heterogeneity bias.

I estimate the analogue of Equation 4.2 in the panel data, individual fixed effects design:

$$Y_{it} = \alpha + \beta_1 T_t^{Cert} + \beta_2 Treatment_i * T_t^{Cert} + X_{it} + \gamma_1 m + \theta_q + \theta_i + \epsilon_{it} \quad (4.3)$$

Where $Y_{it}$ is an individual level outcome for mother $i$ time period $t$. To investigate the probability of conception, $Y_{it}$ 1 if woman $i$ becomes pregnant in 15 month period, both before and after the policy change announcement, and 0 otherwise. To study within mother probability of abortion, I use the sample of women with at least two pregnancies and $Y_{it}$ is 1 if the pregnancy is terminated using abortion and 0 otherwise. $T_t^{Cert}$ is 1 for the April 2003-July 2004 period and 0 for the January 2002-March 2003 period. $Treatment_i$ is 1 if the woman is employed and 0 if she is housewife. $X_{it}$ are individual characteristics at the beginning of period t, specifically age, number of previous pregnancies, and number of previous abortions, which can be inferred from the fertility history of each woman in the sample. $m\ and\ \theta_q$ would be a linear time trend and a conception quarter fixed effects, but they cannot be used when analyzing the probability of conception. $\theta_i$ are the woman fixed effects which capture all time invariant unobservable individual characteristics of the woman, but also all characteristics which are only observed at the time of the survey for which I cannot infer the level at the beginning of the periods of interest (education, marital status, household assets level, region of residence). $\beta_2$ is the treatment effect.

Table 4.12 Panel A presents the estimation results for the probability of conception. Although insignificant, the results indicate that employed women are 13 percentage more likely to conceive after the policy change announcement relative to housewives. The results on the sub-samples based on the household assets level index have even higher standard errors, twice as large as the estimated treatment effect, making the interpretation difficult; however, it would appear that women from households with high assets level have a higher probability of conception than women from households with non-high assets levels, which is different from the results using the aggregate number of conceptions per 1000 women.[37]

Table 4.12 Panel B presents the estimation results which analyze whether the announcement of the policy change alters the probability that a pregnancy is car-

[37]Excluding the controls for the number of previous abortions and the number of previous live births at the time of the pregnancy analyzed (as they may be regarded as lags of the dependent variable) does not change the point estimates nor the standard errors for the outcome "probability of conception".

ried to term. They show that pregnancies conceived by employed mothers after the policy change announcement relative to the previous pregnancy have a 20 percentage points (t = 1.44) lower probability of being terminated on request using abortion, with the effect mostly driven by women from households with non-high assets levels, which have a 27 percentage points (t = 1.48) lower probability of terminating a pregnancy conceived after the announcement relative to her previous pregnancy. Although these results are on a selected sample of women, with at least two pregnancies, they are in line with those obtained in the main specification.[38]

The results of these individual fixed effects specifications must, however, be interpreted with caution. Fixed effects estimators are particularly susceptible to measurement error bias, which is more likely to be exacerbated, due to the relatively small number of switches from which the fixed effects estimators are identified of; if there is measurement error, the proportion of misclassified observations will be larger. Secondly, the identifying assumption of the individual fixed effects strategy is that the unobserved heterogeneity that is correlated with the outcome of interest is constant over time, such that it will be differenced away when including the individual fixed effects. However, if there is time-variant unobserved heterogeneity or unobserved characteristics correlated with the outcomes which are non-constant over time (e.g. marital status or occupational status), the fixed effects estimators will be more biased than the OLS estimators.

### 4.6.5 Specification tests

I also conduct several robustness checks to test the sensitivity of the results to various specifications. First, I exclude March 2003 from the analysis, since the first Emergency Ordinance was issued in March 2003 and may have induced uncertainty regarding the level of MLB. Second, I also exclude April 2003. Third, I use conception month fixed effects instead of quarter of conception fixed effects, a more demanding specification. Forth, I include a quadratic trend. And fifth, I include a split trend after the policy change announcement. Results are presented in Table 4.13, where I show only the treatment effect, i.e. the interaction between $Treat * T^{Cert}$. The results are robust to these alternative specifications.

[38]Excluding the controls for the number of previous abortions and the number of previous live births at the time of the pregnancy analyzed (as they may be regarded as lags of the dependent variable) does not change the magnitude of the estimated effects, although it does affect the standard errors from 0.18 to 0.22 for the non-high household assets group and from 0.62 to 0.45 for high household assets group.

### 4.6.6 Efficiency of the estimator

So far, the main results and the robustness checks have all been estimated using OLS with robust estimates for the standard errors, but econometric theory shows that in the presence of heteroskedasticity, the OLS estimator is less efficient than, for example, the feasible generalized least squares (FGLS) estimator. However, the gain in efficiency of the FGLS over the OLS comes at the cost of stronger distributional assumptions about the variance of the error term. This issue can then be addressed by using a weighted least squares estimator (WLS), whereby standard errors are robust to misspecification of the error variance. [Cameron and Trivedi, 2005]. Given that the main results, based on the OLS estimators, are marginally insignificant, for example on the probability of abortion, a potential avenue to increase the precision of the estimates is to use an FGLS or an WLS estimator. Tables 4.14, 4.15 and 4.16 present the main estimated treatment effects using WLS estimators. As expected, efficiency gains exist, but are relatively small for most outcomes. For the probability of abortion, however, the treatment effect becomes significant for the non-high households assets index group: the employed women who most likely benefited from the policy change have a 10 percentage ponts lower probability of terminating a pregnancy using abortion on request, indicating an increase in fertility. Similarly, the probability of breastfeeding becomes significant and is positive in the group of women with high household asset index, whereas the length of breastfeeding remains positive and significant for the same group, suggesting increased investments in infant health for the women who most likely were disadvantaged by the policy change.

## 4.7 Heterogeneity

I investigate the potential heterogeneous treatment effects with respect to several observable characteristics: age at pregnancy (younger versus older women), marital status (married versus unmarried) and place of residence (rural versus urban). In addition, in an attempt to better identify the women who most likely benefited from the reform, i.e. those with wages below the threshold discussed earlier, I employ a synthetic matching (synthetic linkage) procedure in which I use a donor dataset to impute the wage income into the primary dataset, and use these imputed wages to define a sub-sample of employed women who are most likely to have been positively affected by the change.[39] The estimation results for the subsamples of interest are presented in Table 4.17. The effects are generally larger and in the expected direction for the groups most likely to benefit more from the reform, i.e. unmarried, younger and from rural areas, although they remain insignificant. The results based on the sub-sample with imputed wages below the threshold are similar to those obtained in the low households assets index sub-sample.

## 4.8 Discussion and conclusions

This paper investigates the effects of financial incentives on fertility behavior and early investments in child health in a quasi-experimental setting: it uses a largely unanticipated and substantial change in the maternity leave benefits to which employed mothers were entitled, with most of them benefiting from the reform. The policy reform, which entailed the switch from proportional (equal to 85% of the mother's pre-birth earnings) to fixed benefits, was unexpected; moreover, the level of the fixed benefits was larger than the wage income of most employed women, hence potentially benefiting a very large share of the population. Using data from the Romanian Reproductive Health Survey collected one and a half years after the policy change announcement, I am able to explore the entire spectrum of individual level decisions related to fertility: decision to conceive, decision to carry the pregnancy to term, and several important outcomes conditional on live birth (maternal behavior during pregnancy, child health at birth and early investments in child health). I employ a double difference identification design in which employed women are the treatment group and out of the labor force women are the control group. Although insignificant, the main findings are suggestive of the fact that the substantial increase in the financial incentives led to an increase in conception rates and a decrease in the probability of abortion, especially for women from poorer households, who benefited more from the policy change. Despite not observing any significant changes in the observable characteristics of women who conceive following the announcement, all mothers appear to have worse prenatal behaviors. However, poorer mothers have children with better health outcomes at birth, and richer mothers and make more investments in early child health. For most outcomes I cannot exclude a zero effect of the policy, due to imprecisely estimated effects.

[39]The details of the imputation procedure are found in the Appendix.

The majority of the main results, as well as the robustness tests and heterogeneity investigations, are relatively large in magnitude and in the expected direction, but have large standard errors which render them statistically indistinguishable from 0. This can stem from two mutually exclusive causes: there are significant and large effects but due to particularities of the dataset I am using, the analysis lacks power to precisely estimate the effects; or there are in fact no (or very small) effects of financial incentives on fertility and early investments in child health. I argue that most likely the first scenario plays an important role, although I cannot completely rule out the second.

Among the characteristics of the dataset that may lead to imprecisely estimated effects is the number of observations, i.e. individuals, in the dataset. An insufficient number of observations in the dataset would lead to a small sample size problem and a lack of power to detect effects of a certain size. Taking the example of the probability of abortion: assuming that the estimated coefficient on the treatment effect is unbiased, in the non-high household assets level, at an estimated effect of -0,088, for a significant effect to be detected the standard error would need to be 0,045, as opposed to 0,068. Since standard errors are proportional to $1/\sqrt{n}$, this would require the sample size to be increased by a factor of $(0,068/0,045)^2 = 2,28$, so at least 1830 observations, compared to the 801 observations available in my sample. This makes the sample size scenario a plausible explanation for the absence of significant results. Another data-related problem that may render the results imprecise is the short time interval between the actual policy change and the survey date: individuals may strongly react to the financial incentives by changing their fertility behavior, but conception may not occur immediately.[40] Hence the 7 months window that I can observe in the RHS data after the actual policy change, and 15 months from the announcement date, is insufficient to be able to estimate precise effects on fertility. In the same time, the noisy estimates may be caused by outliers in the treatment group. Since I do not observe wage income, I cannot delimit gainers and losers of the reform perfectly and effects in the two samples may cancel each other out; using the household assets index classification only approximates these two groups, and miss-classifications based on this criterion has the same downward bias due to the opposing effects mentioned when analyzing the entire sample; moreover, there may be outliers or influential observations that

[40]Medical evidence shows that each month that she tries, a healthy, fertile 30-year-old woman has only a 20% chance of getting pregnant.

render the results insignificant. And finally, the lack of precision may be caused by an imperfect control group and an inefficient estimator. I argue that these data issues are likely the cause of the imprecisely estimated effects. First, as was shown when discussing the main results, the point estimates I uncover are well within the range of the previously estimated effects for the outcomes that were studied before. Secondly, I show that using a more efficient (but rather uncommon) estimator, such as FGLS, does reduce the estimated standard errors -with some outcomes becoming significant at conventional levels (such as the probability of abortion for the group that most likely benefited from the reform). Thirdly, the effects are in line with the predictions of the Becker model. And lastly, as presented in the discussion of the institutional setting, the analysis of a longer time series of births at national level, shows that the 2004 reform of the MLB had, at least temporarily, reversed the downward trend in natality by discontinuously and significantly increasing the number of births for employed women.

On the other hand, the hypothesis of a true 0 effect of financial incentives on fertility and early investments in child health is could stem from a Ricardian equivalence, where individuals are forward looking and recognize that the significant increase in MLB will affect the Government's budget constrain, which could lead to future permanent increases in taxes; since the MLB would be paid for a determinate, short period of time, whereas investments in children would be long term, they do not change their fertility behavior or their investments in early child health. Although I have found no anecdotal evidence in favor of this hypothesis, I cannot completely rule it out.

In addition to the contribution to the academic debate, understanding how financial incentives affect reproductive behavior and abortion usage is important from a policy perspective in the context of a generalized downward trend in fertility in the developed countries and the large financial commitments on behalf of governments required to support this component of family policies. Understanding the effects of such benefits on the prenatal maternal behavior and on early investments in child health is particularly important given the mounting evidence that early-life conditions can have consequences on individual outcomes throughout the life cycle. This paper provides some preliminary evidence on the role of the monetary incentives that are part of the maternity leave benefits, but more research is needed confirm to the magnitude of the effects and understand the underlying mechanisms.

# Bibliography


[Almond and Currie, 2011] Almond, D. and Currie, J. (2011). Killing me softly: The fetal origins hypothesis. *The Journal of Economic Perspectives*, pages 153–172.

[Almond et al., 2007] Almond, D., Edlund, L., and Palme, M. (2007). Chernobyl's subclinical legacy: prenatal exposure to radioactive fallout and school outcomes in Sweden.

[Almond and Mazumder, 2011] Almond, D. and Mazumder, B. (2011). Health capital and the prenatal environment: the effect of Ramadan observance during pregnancy. *American Economic Journal-Applied Economics*, 3(4):56.

[Baughman and Dickert-Conlin, 2003] Baughman, R. and Dickert-Conlin, S. (2003). Did expanding the EITC promote motherhood? *American Economic Review*, pages 247–251.

[Baughman and Dickert-Conlin, 2009] Baughman, R. and Dickert-Conlin, S. (2009). The Earned Income Tax Credit and fertility. *Journal of Population Economics*, 22(3):537–563.

[Becker, 1960] Becker, G. S. (1960). An economic analysis of fertility. In *Demographic and economic change in developed countries*, pages 209–240. Columbia University Press.

[Becker, 1991] Becker, G. S. (1991). *A treatise on the family*. Harvard University Press, Cambridge, Mass.

[Becker and Lewis, 1974] Becker, G. S. and Lewis, H. G. (1974). Interaction between quantity and quality of children. In *Economics of the family: Marriage, children, and human capital*, pages 81–90. UMI.

[Björklund, 2006] Björklund, A. (2006). Does family policy affect fertility? *Journal of Population Economics*, 19(1):3–24.

[Brewer et al., 2012] Brewer, M., Ratcliffe, A., et al. (2012). Does welfare reform affect fertility? Evidence from the UK. *Journal of Population Economics*, 25(1):245–266.

[Cameron and Trivedi, 2005] Cameron, A. C. and Trivedi, P. K. (2005). *Microeconometrics: methods and applications*. Cambridge university press.

[Carneiro et al., 2011] Carneiro, P., Loken, K. V., and Salvanes, K. G. (2011). A flying start? Maternity leave benefits and long run outcomes of children. Technical report, Discussion paper series//Forschungsinstitut zur Zukunft der Arbeit.

[Cohen et al., 2013] Cohen, A., Dehejia, R., and Romanov, D. (2013). Financial incentives and fertility. *Review of Economics and Statistics*, 95(1):1–20.

[Dahl et al., 2013] Dahl, G. B., Løken, K. V., Mogstad, M., and Salvanes, K. V. (2013). What is the case for paid maternity leave? Technical report, National Bureau of Economic Research.

[Demeny, 1986] Demeny, P. (1986). Pronatalist policies in low-fertility countries: Patterns, performance, and prospects. *Population and Development Review*, pages 335–358.

[Gauthier, 2007] Gauthier, A. H. (2007). The impact of family policies on fertility in industrialized countries: a review of the literature. *Population Research and Policy Review*, 26(3):323–346.

[González, 2013] González, L. (2013). The effect of a universal child benefit on conceptions, abortions, and early maternal labor supply. *American Economic Journal: Economic Policy*, 5(3):160–188.

[Kearney, 2004] Kearney, M. S. (2004). Is there an effect of incremental welfare benefits on fertility behavior? A look at the family cap. *Journal of Human Resources*, 39(2):295–325.

[Lalive et al., 2014] Lalive, R., Schlosser, A., Steinhauer, A., and Zweimüller, J. (2014). Parental leave and mothers' careers: The relative importance of job protection and cash benefits. *The Review of Economic Studies*, 81(1):219–265.

[Lalive and Zweimüller, 2009] Lalive, R. and Zweimüller, J. (2009). How does parental leave affect fertility and return to work? Evidence from two natural experiments. *The Quarterly Journal of Economics*, 124(3):1363–1402.

[Milligan, 2005] Milligan, K. (2005). Subsidizing the stork: New evidence on tax incentives and fertility. *Review of Economics and Statistics*, 87(3):539–555.

[Nilsson, 2014] Nilsson, J. (2014). Alcohol availability, prenatal conditions, and long-term economic outcomes. *Institute for International Economic Studies, Stockholm University, unpublished manuscript*.

[Paunescu and Apostu, 2012] Paunescu, B. and Apostu, O. (2012). Facilitati de ingrijire a copiilor, factor determinant pentru reintoarcerea pe piata muncii a femeilor. Technical report, Programul Operational Sectorial Dezvoltarea Resurselor Umane 2007-2013, ID: POSDRU/97/6.3/S/60002.

[Pop-Eleches, 2010] Pop-Eleches, C. (2010). The supply of birth control methods, education, and fertility evidence from Romania. *Journal of Human Resources*, 45(4):971–997.

[Raute, 2014] Raute, A. (2014). Do financial incentives affect fertility- evidence from a reform in maternity leave benefits.

[Robert, 1998] Robert, A. (1998). The effect of welfare on marriage and fertility. *Welfare, the Family, and Reproductive Behavior: Research Perspectives*, page 50.

[Rosenzweig, 1999] Rosenzweig, M. R. (1999). Welfare, marital prospects, and nonmarital childbearing. *Journal of Political Economy*, 107(S6):S3–S32.

[Rossin, 2011] Rossin, M. (2011). The effects of maternity leave on children's birth and infant health outcomes in the United States. *Journal of Health Economics*, 30(2):221–239.

[Ruhm, 1998] Ruhm, C. J. (1998). The economic consequences of parental leave mandates: Lessons from Europe. *The Quarterly Journal of Economics*, 113(1):285–317.

[Schönberg and Ludsteck, 2014] Schönberg, U. and Ludsteck, J. (2014). Expansions in maternity leave coverage and mothers' labor market outcomes after childbirth. *Journal of Labor Economics*, 32(3):pp. 469–505.

[Thevenon and Solaz, 2013] Thevenon, O. and Solaz, A. (2013). Labour market effects of parental leave policies in OECD countries. OECD Social, Employment and Migration Working Papers 141.

Table 4.1: Descriptive statistics -observable characteristics of women in the RHS-Ro survey

| Variable | (1) Survey | (2) Analysis sample | (3) | (4) |
|---|---|---|---|---|
| Age | 30,278<br>(8,018) | 27,813<br>(5,640) | 28,563<br>(5,820) | 26,840<br>(5,315) |
| Low education | 0,237<br>(0,425) | 0,282<br>(0,450) | 0,303<br>(0,460) | 0,287<br>(0,453) |
| Medium education | 0,639<br>(0,480) | 0,611<br>(0,488) | 0,618<br>(0,487) | 0,583<br>(0,494) |
| High education | 0,123<br>(0,328) | 0,107<br>(0,310) | 0,079<br>(0,270) | 0,130<br>(0,337) |
| Employed | 0,491<br>(0,499) | 0,483<br>(0,500) | 0,437<br>(0,497) | 0,520<br>(0,500) |
| Housewife | 0,271<br>(0,444) | 0,428<br>(0,495) | 0,431<br>(0,496) | 0,429<br>(0,495) |
| Other | 0,237<br>(0,425) | 0,089<br>(0,285) | 0,132<br>(0,339) | 0,051<br>(0,221) |
| Married | 0,664<br>(0,472) | 0,818<br>(0,386) | 0,804<br>(0,397) | 0,826<br>(0,379) |
| Low hh assets | 0,368<br>(0,482) | 0,439<br>(0,497) | 0,477<br>(0,500) | 0,425<br>(0,495) |
| Medium hh assets | 0,508<br>(0,499) | 0,337<br>(0,473) | 0,319<br>(0,466) | 0,342<br>(0,475) |
| High hh assets | 0,123<br>(0,328) | 0,224<br>(0,417) | 0,204<br>(0,404) | 0,233<br>(0,423) |
| Rural | 0,440<br>(0,496) | 0,554<br>(0,497) | 0,565<br>(0,496) | 0,549<br>(0,498) |
| Observations | 4441 | 884 | 455 | 506 |

Notes: Descriptive statistics (standard error in parentheses) for selected observable characteristics: (1) all women included in the RHS survey; (2) women included in the analysis sample, who conceive between January 2002-July 2004; (3) women included in the analysis sample, who terminate a pregnancy using abortion between January 2002-July 2004; (4) women included in the analysis sample, who give birth between January 2002-July 2004.

Table 4.2: Descriptive statistics -observable characteristics of women who conceive

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | | All | | | Employed | | | Housewives | |
| VARIABLES | t0 | t1 | t1-t0 | t0 | t1 | t1-t0 | t0 | t1 | t1-t0 |
| **Panel A: All conceptions** | | | | | | | | | |
| Age at pregn | 27.329 | 27.679 | 0.351 | 28.215 | 28.972 | 0.756 | 26.625 | 26.744 | 0.119 |
| Sh. Low educ | 0.287 | 0.366 | 0.079*** | 0.098 | 0.178 | 0.081*** | 0.468 | 0.545 | 0.076* |
| Sh. Med. educ | 0.627 | 0.530 | -0.097*** | 0.728 | 0.607 | -0.120*** | 0.520 | 0.451 | -0.069 |
| Sh. High educ | 0.086 | 0.104 | 0.018 | 0.175 | 0.215 | 0.040 | 0.011 | 0.004 | -0.007 |
| Married | 0.838 | 0.797 | -0.041* | 0.931 | 0.874 | -0.056** | 0.788 | 0.785 | -0.004 |
| Sh. Low SES | 0.422 | 0.492 | 0.070** | 0.175 | 0.251 | 0.076** | 0.662 | 0.720 | 0.058 |
| Sh. Med. SES | 0.492 | 0.404 | -0.088*** | 0.650 | 0.534 | -0.116*** | 0.327 | 0.276 | -0.051 |
| Sh. High SES | 0.086 | 0.104 | 0.018 | 0.175 | 0.215 | 0.040 | 0.011 | 0.004 | -0.007 |
| Share abortions | 0.526 | 0.514 | -0.012 | 0.467 | 0.441 | -0.026 | 0.543 | 0.545 | 0.002 |
| Observations | 557 | 549 | | 246 | 247 | | 269 | 246 | |
| **Panel B: Abortions** | | | | | | | | | |
| Age at pregn | 27.942 | 28.461 | 0.519 | 29.096 | 29.991 | 0.895 | 27.253 | 27.821 | 0.567 |
| Sh. Low educ | 0.307 | 0.397 | 0.090** | 0.113 | 0.202 | 0.089* | 0.473 | 0.545 | 0.072 |
| Sh. Med educ | 0.638 | 0.528 | -0.110*** | 0.757 | 0.615 | -0.142** | 0.527 | 0.455 | -0.072 |
| Sh. High educ | 0.055 | 0.074 | 0.020 | 0.130 | 0.183 | 0.053 | 0.000 | 0.000 | 0.000 |
| Married | 0.829 | 0.801 | -0.028 | 0.904 | 0.835 | -0.069 | 0.829 | 0.851 | 0.022 |
| Sh. Low SES | 0.447 | 0.539 | 0.092** | 0.217 | 0.275 | 0.058 | 0.658 | 0.731 | 0.074 |
| Sh. Med SES | 0.498 | 0.387 | -0.112*** | 0.652 | 0.541 | -0.111* | 0.342 | 0.269 | -0.051 |
| Sh. High SES | 0.055 | 0.074 | 0.020 | 0.130 | 0.183 | 0.053 | 0.000 | 0.000 | 0.000 |
| Observations | 293 | 282 | | 115 | 109 | | 146 | 134 | |
| **Panel C: Live births** | | | | | | | | | |
| Age at pregn | 26.648 | 26.854 | 0.206 | 27.443 | 28.167 | 0.724 | 25.878 | 25.455 | -0.423 |
| Sh. Low educ | 0.265 | 0.333 | 0.068* | 0.084 | 0.159 | 0.075* | 0.463 | 0.545 | 0.081 |
| Sh. Med. educ | 0.614 | 0.532 | -0.082* | 0.702 | 0.601 | -0.101* | 0.512 | 0.446 | -0.066 |
| Sh. High educ | 0.121 | 0.135 | 0.014 | 0.214 | 0.239 | 0.025 | 0.024 | 0.009 | -0.015 |
| Married | 0.848 | 0.794 | -0.054 | 0.954 | 0.906 | -0.048 | 0.740 | 0.705 | -0.034 |
| Sh. Low SES | 0.394 | 0.442 | 0.048 | 0.137 | 0.232 | 0.094** | 0.667 | 0.705 | 0.039 |
| Sh. Med. SES | 0.485 | 0.423 | -0.062 | 0.649 | 0.529 | -0.120** | 0.309 | 0.286 | -0.051 |
| Sh. High SES | 0.121 | 0.135 | 0.014 | 0.214 | 0.239 | 0.025 | 0.024 | 0.009 | -0.015 |
| Observations | 264 | 267 | | 131 | 138 | | 123 | 112 | |

Notes: Descriptive statistics for selected observable characteristics of all women who conceive (Panel A), women who conceive and terminate the pregnancy using abortion (Panel B), and women who conceive and carry the pregnancy to term (Panel C) in the selected time window: t0=Janyary 2000-March 2003, t1=April 2003-July 2004.

Table 4.3: Probability of conception: conceptions per 1000 women

| VARIABLES | (1) DD | (2) DD | (3) DD Non-high | (4) DD High |
|---|---|---|---|---|
| $Treat * T^{Cert}$ | 0.464 | 0.464 | 0.656 | 0.045 |
| | (0.485) | (0.497) | (0.650) | (0.719) |
| $Treat$ | -0.450 | -0.450 | -2.214*** | 3.412*** |
| | (0.367) | (0.376) | (0.518) | (0.457) |
| $T^{Cert}$ | 0.013 | 0.048 | -0.225 | 0.647 |
| | (0.598) | (0.635) | (0.727) | (0.924) |
| Observations | 62 | 62 | 62 | 62 |
| R-squared | 0.160 | 0.167 | 0.441 | 0.666 |
| Time trend | Yes | Yes | Yes | Yes |
| Quarter FE | No | Yes | Yes | Yes |

Notes: Dependent variable: monthly number of conceptions per 1000 females. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$."Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.4: Selection on observable characteristics

| VARIABLES | (1) Age at pregnancy | (2) Low educ. | (3) Medium educ. | (4) High educ, | (5) Married | (6) Nonhigh hh assets | (7) Rural |
|---|---|---|---|---|---|---|---|
| **Panel A: All conceptions** | | | | | | | |
| $Treat * T^{Cert}$ | 0.538 | -0.029 | 0.002 | 0.024 | -0.035 | -0.019 | -0.083 |
| | (0.721) | (0.056) | (0.064) | (0.038) | (0.047) | (0.050) | (0.059) |
| $Treat$ | 1.663*** | -0.338*** | 0.156*** | 0.173*** | 0.125*** | -0.278*** | -0.338*** |
| | (0.513) | (0.039) | (0.046) | (0.028) | (0.032) | (0.036) | (0.045) |
| $T^{Cert}$ | -1.030 | 0.049 | -0.106 | 0.063 | 0.012 | -0.025 | 0.070 |
| | (0.949) | (0.079) | (0.085) | (0.043) | (0.064) | (0.057) | (0.073) |
| Observations | 1,008 | 1,008 | 1,008 | 1,008 | 1,008 | 1,008 | 1,008 |
| R-squared | 0.044 | 0.171 | 0.062 | 0.127 | 0.038 | 0.162 | 0.191 |
| **Panel B: Abortions** | | | | | | | |
| $Treat * T^{Cert}$ | -0.238 | 0.012 | -0.043 | 0.031 | -0.082 | -0.088 | -0.126 |
| | (1.030) | (0.080) | (0.090) | (0.053) | (0.066) | (0.070) | (0.085) |
| $Treat$ | 2.445*** | -0.349*** | 0.193*** | 0.156*** | 0.065 | -0.296*** | -0.348*** |
| | (0.724) | (0.055) | (0.064) | (0.037) | (0.045) | (0.050) | (0.064) |
| $T^{Cert}$ | -0.277 | -0.053 | 0.024 | 0.029 | 0.045 | 0.058 | 0.109 |
| | (1.275) | (0.112) | (0.118) | (0.053) | (0.080) | (0.073) | (0.095) |
| Observations | 504 | 504 | 504 | 504 | 504 | 504 | 504 |
| R-squared | 0.053 | 0.154 | 0.069 | 0.118 | 0.018 | 0.199 | 0.188 |
| **Panel C: Live births** | | | | | | | |
| $Treat * T^{Cert}$ | 1.429 | -0.063 | 0.047 | 0.016 | 0.010 | 0.050 | -0.053 |
| | (1.039) | (0.081) | (0.093) | (0.058) | (0.069) | (0.073) | (0.086) |
| $Treat$ | 1.173 | -0.324*** | 0.119* | 0.205*** | 0.191*** | -0.291*** | -0.358*** |
| | (0.739) | (0.057) | (0.067) | (0.043) | (0.048) | (0.054) | (0.065) |
| $T^{Cert}$ | -0.686 | 0.164 | -0.246* | 0.082 | -0.002 | -0.119 | 0.093 |
| | (1.421) | (0.113) | (0.126) | (0.075) | (0.101) | (0.089) | (0.112) |
| Observations | 504 | 504 | 504 | 504 | 504 | 504 | 504 |
| R-squared | 0.069 | 0.193 | 0.065 | 0.122 | 0.088 | 0.128 | 0.167 |

Notes: Dependent variable: observable maternal characteristic. Controls: linear time trend, quarter of conception fixed effects. $Treat$ is 1 for employed women and 0 for housewives. In Panel A and B, $T^{Cert}$ is 1 for pregnancies conceived between April 2003 and July 2004. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003. In Panel C, $T^{Cert}$ is 1 for pregnancies delivered between January 2004 and July 2004. $T^{Pot}$ is 1 for pregnancies delivered between April-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.5: Conditional on conception: Probability of abortion

| VARIABLES | (1) DD | (2) DD | (3) DD | (4) DD Non-high | (5) DD High |
|---|---|---|---|---|---|
| $Treat * T^{Cert}$ | -0.031 | -0.046 | -0.047 | -0.088 | 0.081 |
| | (0.065) | (0.059) | (0.059) | (0.068) | (0.178) |
| $Treat$ | -0.069 | 0.013 | 0.016 | 0.017 | 0.045 |
| | (0.048) | (0.046) | (0.046) | (0.052) | (0.121) |
| $T^{Cert}$ | -0.194** | -0.171** | -0.194*** | -0.158* | -0.411** |
| | (0.080) | (0.071) | (0.075) | (0.081) | (0.202) |
| Observations | 1,008 | 1,008 | 1,008 | 801 | 207 |
| R-squared | 0.023 | 0.214 | 0.217 | 0.228 | 0.265 |
| Time trend | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | No | No | Yes | Yes | Yes |
| Ind. cov. | No | Yes | Yes | Yes | Yes |

Notes: Dependent variable: 1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat*T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.6: Outcomes conditional on live birth

| VARIABLES | (1) Alcohol& smoking | (2) Prenatal vitamins | (3) Mth 1st control | (4) Low bweight | (5) Postnatal consult | (6) Days in hospital | (7) Infant vitamins | (8) Breast feeding | (9) Months breastf. |
|---|---|---|---|---|---|---|---|---|---|
| **Panel A: All women** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.136 | -0.060 | -0.012 | -0.112 | 0.107 | 1.497 | 0.002 | -0.015 | 1.219 |
| | (0.084) | (0.083) | (0.315) | (0.072) | (0.099) | (1.255) | (0.076) | (0.065) | (1.104) |
| $Treat$ | -0.065 | 0.065 | -0.407* | 0.038 | 0.063 | 0.182 | 0.023 | -0.022 | -0.920 |
| | (0.050) | (0.061) | (0.225) | (0.045) | (0.063) | (0.715) | (0.049) | (0.044) | (0.947) |
| $T^{Cert}$ | -0.025 | 0.191 | 0.921 | 0.029 | 0.171 | -2.050 | -0.043 | -0.149 | -6.660*** |
| | (0.144) | (0.146) | (0.600) | (0.123) | (0.146) | (1.579) | (0.112) | (0.099) | (2.114) |
| Observations | 520 | 486 | 486 | 520 | 520 | 517 | 520 | 520 | 344 |
| R-squared | 0.122 | 0.108 | 0.186 | 0.026 | 0.100 | 0.020 | 0.061 | 0.024 | 0.206 |
| **Panel B: Non-high hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.131 | 0.014 | -0.141 | -0.130 | 0.136 | 2.336 | -0.003 | -0.046 | -0.001 |
| | (0.094) | (0.096) | (0.373) | (0.088) | (0.113) | (1.680) | (0.091) | (0.083) | (1.501) |
| $Treat$ | -0.125** | 0.024 | -0.412 | 0.034 | 0.085 | -0.031 | 0.031 | -0.017 | -0.182 |
| | (0.053) | (0.070) | (0.259) | (0.053) | (0.067) | (0.666) | (0.055) | (0.053) | (1.135) |
| $T^{Cert}$ | -0.014 | 0.192 | 0.822 | 0.178 | 0.188 | -0.966 | -0.089 | -0.156 | -6.020** |
| | (0.157) | (0.171) | (0.723) | (0.135) | (0.150) | (1.271) | (0.132) | (0.118) | (2.521) |
| Observations | 399 | 366 | 366 | 399 | 399 | 396 | 399 | 399 | 249 |
| R-squared | 0.132 | 0.117 | 0.139 | 0.032 | 0.063 | 0.028 | 0.059 | 0.043 | 0.206 |
| **Panel C: High hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.197 | -0.279 | 0.097 | 0.069 | 0.387 | 0.482 | -0.033 | 0.131 | 6.096*** |
| | (0.128) | (0.178) | (0.807) | (0.058) | (0.308) | (2.115) | (0.061) | (0.085) | (2.079) |
| $Treat$ | 0.197** | 0.179 | -0.019 | 0.008 | -0.349 | 0.154 | -0.021 | -0.110 | -4.664** |
| | (0.091) | (0.168) | (0.453) | (0.038) | (0.232) | (1.770) | (0.053) | (0.068) | (1.945) |
| $T^{Cert}$ | -0.006 | 0.153 | 1.534 | -0.704** | -0.407 | -8.429 | 0.126 | -0.245 | -13.514*** |
| | (0.331) | (0.279) | (1.081) | (0.299) | (0.505) | (7.316) | (0.162) | (0.152) | (3.898) |
| Observations | 121 | 120 | 120 | 121 | 121 | 121 | 121 | 121 | 95 |
| R-squared | 0.211 | 0.089 | 0.206 | 0.283 | 0.129 | 0.196 | 0.059 | 0.159 | 0.446 |
| pval diff | 0.672 | 0.137 | 0.782 | 0.060 | 0.430 | 0.484 | 0.781 | 0.132 | 0.015 |
| Time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Ind cov | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with <2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for births occurring between April-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.7: Probability of conception: conceptions per 1000 women, October threshold

| VARIABLES | (1) DD | (2) DD | (3) DD Non-high | (4) DD High |
|---|---|---|---|---|
| $Treat * T^{Cert}$ | 0.383 | 0.383 | 0.738 | -0.395 |
| | (0.552) | (0.558) | (0.766) | (0.686) |
| $Treat$ | -0.450 | -0.450 | -2.214*** | 3.412*** |
| | (0.372) | (0.376) | (0.511) | (0.449) |
| $T^{Cert}$ | -0.244 | 0.652 | 0.693 | 0.560 |
| | (0.857) | (1.094) | (1.497) | (1.253) |
| Observations | 62 | 62 | 62 | 62 |
| R-squared | 0.135 | 0.165 | 0.446 | 0.666 |
| Time trend | Yes | Yes | Yes | Yes |
| Quarter FE | No | Yes | Yes | Yes |

Notes: Dependent variable: monthly number of conceptions per 1000 females. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after October 2003. Controls: linear time trend, quarter of conception fixed effects. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-September 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.8: Conditional on conception: Probability of abortion, October threshold

| VARIABLES | (1)<br>DD | (2)<br>DD | (3)<br>DD | (4)<br>DD Non-high | (5)<br>DD High |
|---|---|---|---|---|---|
| $Treat * T^{Cert}$ | -0.016 | -0.031 | -0.027 | -0.075 | 0.161 |
| | (0.073) | (0.067) | (0.067) | (0.076) | (0.242) |
| $Treat$ | -0.064 | 0.002 | 0.015 | 0.015 | 0.033 |
| | (0.044) | (0.043) | (0.043) | (0.048) | (0.106) |
| $T^{Cert}$ | 0.260*** | 0.188** | 0.337*** | 0.312*** | 0.313 |
| | (0.088) | (0.079) | (0.091) | (0.098) | (0.306) |
| Observations | 1,008 | 1,008 | 1,008 | 801 | 207 |
| R-squared | 0.019 | 0.207 | 0.217 | 0.228 | 0.267 |
| Time trend | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | No | No | Yes | Yes | Yes |
| Ind. cov. | No | Yes | Yes | Yes | Yes |

Notes: Dependent variable: 1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after October 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-September 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.9: Outcomes conditional on live birth- October threshold

| VARIABLES | (1) Alcohol& smoking | (2) Prenatal vitamins | (3) Mth 1st control | (4) Low bweight | (5) Postnatal consult | (6) Days in hospital | (7) Infant vitamins | (8) Breast feeding | (9) Months breastf. |
|---|---|---|---|---|---|---|---|---|---|
| **Panel A: All women** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.109 | -0.049 | -0.017 | -0.115* | 0.118 | 1.777 | 0.004 | -0.019 | 1.186 |
| | (0.081) | (0.077) | (0.297) | (0.067) | (0.094) | (1.193) | (0.071) | (0.063) | (0.980) |
| $Treat$ | -0.038 | 0.052 | -0.416** | 0.040 | 0.055 | -0.084 | 0.022 | -0.017 | -0.955 |
| | (0.044) | (0.052) | (0.198) | (0.039) | (0.055) | (0.678) | (0.041) | (0.038) | (0.783) |
| $T^{Cert}$ | -0.065 | 0.131 | -0.006 | 0.122* | 0.034 | -0.704 | -0.049 | -0.003 | -4.803*** |
| | (0.089) | (0.092) | (0.361) | (0.071) | (0.101) | (1.183) | (0.076) | (0.068) | (1.202) |
| Observations | 520 | 486 | 486 | 520 | 520 | 517 | 520 | 520 | 344 |
| R-squared | 0.123 | 0.108 | 0.178 | 0.027 | 0.104 | 0.024 | 0.064 | 0.021 | 0.213 |
| **Panel B: Non-high hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.087 | 0.024 | -0.106 | -0.139* | 0.162 | 2.559 | -0.006 | -0.089 | 0.053 |
| | (0.092) | (0.089) | (0.351) | (0.082) | (0.108) | (1.644) | (0.086) | (0.079) | (1.314) |
| $Treat$ | -0.082* | 0.014 | -0.453** | 0.043 | 0.062 | -0.241 | 0.035 | 0.025 | -0.388 |
| | (0.048) | (0.060) | (0.227) | (0.046) | (0.060) | (0.760) | (0.047) | (0.043) | (0.926) |
| $T^{Cert}$ | -0.087 | 0.130 | -0.070 | 0.186** | 0.034 | -0.336 | -0.076 | -0.051 | -3.854*** |
| | (0.099) | (0.107) | (0.417) | (0.082) | (0.108) | (1.439) | (0.088) | (0.076) | (1.459) |
| Observations | 399 | 366 | 366 | 399 | 399 | 396 | 399 | 399 | 249 |
| R-squared | 0.130 | 0.116 | 0.130 | 0.037 | 0.059 | 0.029 | 0.062 | 0.036 | 0.216 |
| **Panel C: High hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.230* | -0.267* | 0.101 | 0.012 | 0.119 | -0.341 | -0.020 | 0.195** | 4.159** |
| | (0.121) | (0.138) | (0.744) | (0.055) | (0.279) | (1.838) | (0.058) | (0.088) | (1.618) |
| $Treat$ | 0.167** | 0.171 | 0.009 | 0.059 | -0.103 | 0.857 | -0.033 | -0.197** | -2.896** |
| | (0.070) | (0.120) | (0.460) | (0.042) | (0.182) | (1.347) | (0.046) | (0.076) | (1.448) |
| $T^{Cert}$ | -0.049 | 0.196 | 0.141 | -0.140 | -0.081 | -1.599 | 0.094 | 0.087 | -9.225*** |
| | (0.152) | (0.162) | (0.831) | (0.086) | (0.319) | (2.102) | (0.068) | (0.121) | (1.998) |
| Observations | 121 | 120 | 120 | 121 | 121 | 121 | 121 | 121 | 95 |
| R-squared | 0.215 | 0.103 | 0.184 | 0.170 | 0.140 | 0.193 | 0.062 | 0.237 | 0.436 |
| pval diff | 0.335 | 0.071 | 0.796 | 0.125 | 0.882 | 0.232 | 0.889 | 0.015 | 0.046 |
| Time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Ind cov | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with <2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for births occurring between October-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 4.10: Robustness -Moldova as control

| A: Conceptions per 1000 | (1)<br>DD | (2)<br>DD | (3)<br>DD Non-high | (4)<br>DD High |
|---|---|---|---|---|
| $Treat * T^{Cert}$ | -0.043 | -0.043 | -0.084 | 0.047 |
| | (0.451) | (0.456) | (0.551) | (0.728) |
| $Treat$ | 0.553* | 0.553* | 0.316 | 1.073** |
| | (0.326) | (0.327) | (0.422) | (0.450) |
| $T^{Cert}$ | 0.161 | 0.038 | -0.077 | 0.291 |
| | (0.557) | (0.594) | (0.654) | (0.915) |
| Observations | 62 | 62 | 62 | 62 |
| R-squared | 0.131 | 0.151 | 0.073 | 0.220 |
| Time trend | Yes | Yes | Yes | Yes |
| Quarter FE | No | Yes | Yes | Yes |

| B: Abortion | (1)<br>DD | (2)<br>DD | (3)<br>DD | (4)<br>DD Nonhigh | (5)<br>DD High |
|---|---|---|---|---|---|
| $Treat * T^{Cert}$ | -0.062 | -0.061 | -0.059 | -0.089 | -0.003 |
| | (0.061) | (0.057) | (0.056) | (0.065) | (0.081) |
| $Treat$ | -0.045 | -0.097** | -0.097** | -0.079 | -0.160** |
| | (0.045) | (0.043) | (0.043) | (0.049) | (0.063) |
| $T^{Cert}$ | -0.160** | -0.149** | -0.172** | -0.140** | -0.204*** |
| | (0.072) | (0.067) | (0.068) | (0.071) | (0.075) |
| Observations | 1,217 | 1,217 | 1,217 | 1,040 | 901 |
| R-squared | 0.022 | 0.173 | 0.176 | 0.177 | 0.182 |
| Time trend | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | No | No | Yes | Yes | Yes |
| Ind.cov. | No | Yes | Yes | Yes | Yes |

Notes: Dependent variable: Panel A: monthly number of conceptions per 1000 women. Panel B:1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.11: Robustness: Outcomes conditional on live birth-Moldova as control

| VARIABLES | (1) Prenatal vitamins | (2) Mth 1st control | (3) Low bewight | (4) Postnatal consult | (5) Breast feeding | (6) Months breastf. |
|---|---|---|---|---|---|---|
| **Panel A: All women** | | | | | | |
| $Treat * T^{Cert}$ | 0.014 | -1.209 | -0.079 | 0.021 | -0.028 | -4.764*** |
| | (0.079) | (1.361) | (0.054) | (0.087) | (0.050) | (1.073) |
| $Treat$ | 0.335*** | -0.827*** | 0.097*** | -0.383*** | -0.092*** | -2.838*** |
| | (0.051) | (0.221) | (0.034) | (0.056) | (0.034) | (0.882) |
| $T^{Cert}$ | 0.033 | 2.623 | -0.072 | 0.023 | -0.015 | 2.412 |
| | (0.141) | (2.601) | (0.084) | (0.128) | (0.070) | (2.252) |
| Observations | 640 | 562 | 647 | 647 | 578 | 481 |
| R-squared | 0.235 | 0.042 | 0.029 | 0.154 | 0.068 | 0.171 |
| **Panel B: Non-high hh assets index** | | | | | | |
| $Treat * T^{Cert}$ | 0.065 | -1.226 | -0.076 | 0.051 | -0.060 | -5.293*** |
| | (0.087) | (1.360) | (0.070) | (0.102) | (0.070) | (1.263) |
| $Treat$ | 0.336*** | -0.832*** | 0.123*** | -0.461*** | -3.084*** | -5.336*** |
| | (0.059) | (0.307) | (0.046) | (0.064) | (0.046) | (1.074) |
| $T^{Cert}$ | 0.017 | 2.490 | 0.001 | 0.030 | 0.010 | 3.097 |
| | (0.156) | (2.885) | (0.087) | (0.133) | (0.079) | (2.514) |
| Observations | 538 | 460 | 544 | 544 | 475 | 402 |
| R-squared | 0.207 | 0.039 | 0.030 | 0.206 | 0.105 | 0.119 |
| **Panel C: High hh assets index** | | | | | | |
| $Treat * T^{Cert}$ | -0.075 | -1.268 | -0.082 | -0.036 | 0.021 | -4.596*** |
| | (0.099) | (1.503) | (0.060) | (0.130) | (0.051) | (1.250) |
| $Treat*$ | 0.349*** | -1.002** | 0.055 | -0.242*** | -0.052 | -2.806** |
| | (0.062) | (0.388) | (0.043) | (0.087) | (0.042) | (1.011) |
| $T^{Cert}$ | -0.030 | 3.177 | -0.097 | -0.023 | 0.008 | 3.131 |
| | (0.164) | (3.397) | (0.080) | (0.143) | (0.051) | (2.617) |
| Observations | 473 | 395 | 474 | 474 | 405 | 371 |
| R-squared | 0.209 | 0.046 | 0.029 | 0.068 | 0.101 | 0.147 |
| pval diff | 0.224 | 0.521 | 0.714 | 0.208 | 0.739 | 0.703 |
| Time trend | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Ind. cov. | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Dependent variable: (1) 1 if mother took prenatal vitamins; (2) month of first prenatal control; (3) 1 if child is born with <2500 g; (4) 1 if mother and child had a postnatal control; (5) 1 if infant was breastfed; (6) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for births occurring between April-December 2003, and the interaction term $Treat * T^{Pot}$."Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.12: Robustness -Mother fixed effects

| VARIABLES | (1) Baseline | (2) Non-high | (3) High |
|---|---|---|---|
| **Panel A: Conception** | | | |
| $Treat * T^{Cert}$ | 0.013 | 0.009 | 0.024 |
| | (0.017) | (0.020) | (0.045) |
| $T^{Cert}$ | -0.010 | -0.009 | -0.019 |
| | (0.015) | (0.015) | (0.043) |
| Observations | 6,774 | 4,748 | 2,026 |
| R-squared | 0.561 | 0.559 | 0.563 |
| Ind cov | Yes | Yes | Yes |
| **Panel B: Abortion** | | | |
| $Treat * T^{Cert}$ | -0.208 | -0.276 | -0.121 |
| | (0.144) | (0.186) | (0.625) |
| $T^{Cert}$ | -0.203 | -0.186 | -0.432 |
| | (0.151) | (0.162) | (0.576) |
| Observations | 529 | 445 | 84 |
| R-squared | 0.812 | 0.821 | 0.827 |
| Ind cov | Yes | Yes | Yes |

Notes: Panel A: dependent variable is 1 if women conceived in period "t" (t=0 for January 2000-March 2003; t=1 for April 2003-July 2004). Controls: number of previous abortions at pregnancy i,number of children at pregnancy i. Panel B: sample of women with at leat 2 pregnancies in the interval January 2000-July 2004. Dependent variable is 1 if pregnancy i was terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003. Controls: number of previous abortions at pregnancy i, number of children at pregnancy i. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003."Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.13: Sensitivity to specification

| | | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|
| I | Concept per1000 | 0.464 | 0.464 | 0.435 | 0.464 | 0.464 | 0.464 |
| | | (0.480) | (0.482) | (0.496) | (0.466) | (0.484) | (0.485) |
| II | Prob abortion | -0.047 | -0.052 | -0.048 | -0.046 | -0.046 | -0.046 |
| | | (0.059) | (0.059) | (0.060) | (0.060) | (0.059) | (0.059) |
| III | Alcohol, smoke | 0.136 | 0.128 | 0.132 | 0.134 | 0.136 | 0.136 |
| | | (0.084) | (0.084) | (0.087) | (0.086) | (0.084) | (0.084) |
| | Prenatal vitamins | -0.060 | -0.062 | -0.037 | -0.069 | -0.060 | -0.060 |
| | | (0.083) | (0.084) | (0.088) | (0.086) | (0.084) | (0.083) |
| | Mth 1st control | -0.012 | -0.004 | -0.109 | 0.079 | -0.020 | -0.012 |
| | | (0.315) | (0.316) | (0.334) | (0.320) | (0.314) | (0.315) |
| | Low birth weight | -0.112 | -0.123* | -0.120 | -0.117 | -0.112 | -0.112 |
| | | (0.072) | (0.072) | (0.077) | (0.074) | (0.072) | (0.072) |
| | Postnatal consult | 0.107 | 0.095 | 0.065 | 0.089 | 0.107 | 0.106 |
| | | (0.099) | (0.100) | (0.105) | (0.101) | (0.100) | (0.100) |
| | Days in hosp | 1.497 | 1.482 | 1.781 | 1.592 | 1.497 | 1.505 |
| | | (1.255) | (1.276) | (1.389) | (1.296) | (1.256) | (1.259) |
| | Vitamins to infant | 0.002 | 0.010 | 0.025 | -0.016 | 0.002 | -0.000 |
| | | (0.076) | (0.075) | (0.082) | (0.075) | (0.076) | (0.075) |
| | Breastfeeding | -0.015 | -0.019 | -0.006 | 0.003 | -0.015 | -0.013 |
| | | (0.065) | (0.066) | (0.067) | (0.066) | (0.065) | (0.065) |
| | Mths breastfeed | 1.219 | 1.145 | 1.359 | 1.084 | 1.165 | 1.092 |
| | | (1.104) | (1.120) | (1.182) | (1.126) | (1.081) | (1.091) |

Notes: (1) Baseline specification; (2)Exclude March 2003; (3) Exclude March and April 2003; (4) Conception month fixed effects; (5) Quadratic trend; (6) Split trend.
Panel I: Regression dos not control for any individual level characteristics. Panel II and III: Regressions include individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Panel I and II: $T^{Cert}$ is 1 for conceptions occurring after April 2003. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. Panel III: $T^{Cert}$ is 1 for births occurring after January 2004. Regressions include $T^{Pot}$ that is 1 for births occurring between April-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.14: Probability of conception: conceptions per 1000 women, Weighted Least Squares

| VARIABLES | (1)<br>DD | (2)<br>DD | (3)<br>DD Non-high | (4)<br>DD High |
|---|---|---|---|---|
| $Treat * T^{Cert}$ | 0.460 | 0.460 | 0.673 | -0.043 |
| | (0.471) | (0.480) | (0.663) | (0.697) |
| $Treat$ | -0.454 | -0.454 | -2.233*** | 3.411*** |
| | (0.366) | (0.366) | (0.527) | (0.458) |
| $T^{Cert}$ | 0.048 | 0.087 | -0.010 | 0.894 |
| | (0.538) | (0.589) | (0.790) | (1.131) |
| Observations | 62 | 62 | 62 | 62 |
| R-squared | 0.171 | 0.181 | 0.404 | 0.586 |

Notes: Dependent variable: monthly number of conceptions per 1000 females. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Estimation using Weighted Least Squares. *** p<0.01, ** p<0.05, * p<0.1

Table 4.15: Conditional on conception: Probability of abortion, Weighted Least Squares

| VARIABLES | (1) DD | (2) DD | (3) DD | (4) DD Non-high | (5) DD High |
|---|---|---|---|---|---|
| $Treat * T^{Cert}$ | -0.050 | -0.069 | -0.069 | -0.103* | 0.001 |
| | (0.071) | (0.050) | (0.050) | (0.058) | (0.159) |
| $Treat$ | -0.164*** | 0.004 | 0.005 | 0.006 | 0.087 |
| | (0.049) | (0.037) | (0.037) | (0.041) | (0.077) |
| $T^{Cert}$ | -0.123 | -0.146*** | -0.163*** | -0.140** | -0.361** |
| | (0.084) | (0.054) | (0.061) | (0.065) | (0.181) |
| Observations | 1,008 | 1,008 | 1,008 | 801 | 207 |
| R-squared | 0.049 | 0.367 | 0.369 | 0.373 | 0.404 |

Notes: Dependent variable: 1 if pregnancy terminated using abortion. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Estimation using Weighted Least Squares. *** p<0.01, ** p<0.05, * p<0.1

Table 4.16: Outcomes conditional on live birth- Weighted Least Squares

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | Alcohol& smoking | Prenatal vitamins | Mth 1st control | Low bweight | Postnatal consult | Days in hospital | Infant vitamins | Breast feeding | Months breastf. |
| **Panel A: All women** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.097 | -0.095 | 0.067 | -0.090 | 0.123 | 1.954 | -0.016 | 0.001 | 1.012 |
| | (0.078) | (0.077) | (0.289) | (0.069) | (0.099) | (1.452) | (0.071) | (0.063) | (1.036) |
| $Treat$ | -0.061 | 0.074 | -0.495** | 0.039 | 0.047 | -0.865 | 0.014 | -0.019 | -1.038 |
| | (0.045) | (0.058) | (0.210) | (0.046) | (0.062) | (0.996) | (0.049) | (0.042) | (0.947) |
| $Treat$ | -0.030 | 0.188 | 1.080** | -0.011 | 0.170 | -0.196 | -0.059 | -0.125 | -5.832*** |
| | (0.134) | (0.137) | (0.501) | (0.125) | (0.143) | (1.432) | (0.101) | (0.096) | (2.217) |
| Observations | 520 | 486 | 486 | 520 | 520 | 517 | 520 | 520 | 344 |
| R-squared | 0.103 | 0.089 | 0.235 | 0.026 | 0.103 | 0.080 | 0.044 | 0.017 | 0.209 |
| **Panel B: Non-high hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.090 | -0.013 | 0.038 | -0.115 | 0.145 | 2.166 | -0.015 | -0.023 | -0.296 |
| | (0.088) | (0.088) | (0.344) | (0.087) | (0.114) | (1.658) | (0.090) | (0.079) | (1.388) |
| $Treat$ | -0.123*** | 0.031 | -0.562** | 0.038 | 0.067 | -0.878 | 0.016 | -0.013 | -0.308 |
| | (0.047) | (0.066) | (0.247) | (0.053) | (0.066) | (1.047) | (0.054) | (0.050) | (1.085) |
| $T^{Cert}$ | -0.043 | 0.180 | 0.842 | 0.148 | 0.177 | -0.441 | -0.128 | -0.138 | -4.865** |
| Observations | 399 | 366 | 366 | 399 | 399 | 396 | 399 | 399 | 249 |
| R-squared | 0.118 | 0.108 | 0.203 | 0.032 | 0.063 | 0.081 | 0.042 | 0.031 | 0.184 |
| **Panel C: High hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.054 | -0.285 | -0.042 | 0.055 | 0.410 | 2.118 | -0.038 | 0.133* | 5.984*** |
| | (0.055) | (0.176) | (0.686) | (0.055) | (0.306) | (1.897) | (0.066) | (0.078) | (2.081) |
| $Treat$ | 0.018 | 0.172 | -0.072 | 0.016 | -0.353 | -1.712 | -0.016 | -0.114 | -5.164*** |
| | (0.037) | (0.163) | (0.486) | (0.037) | (0.229) | (1.484) | (0.059) | (0.069) | (1.915) |
| $T^{Cert}$ | -0.633** | 0.196 | 1.682* | -0.646** | -0.394 | -2.199 | 0.097 | -0.224 | -11.026*** |
| | (0.286) | (0.308) | (0.898) | (0.286) | (0.510) | (3.804) | (0.152) | (0.136) | (4.034) |
| Observations | 121 | 120 | 120 | 121 | 121 | 121 | 121 | 121 | 95 |
| R-squared | 0.265 | 0.096 | 0.150 | 0.272 | 0.137 | 0.169 | 0.065 | 0.130 | 0.504 |
| Time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Ind cov | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

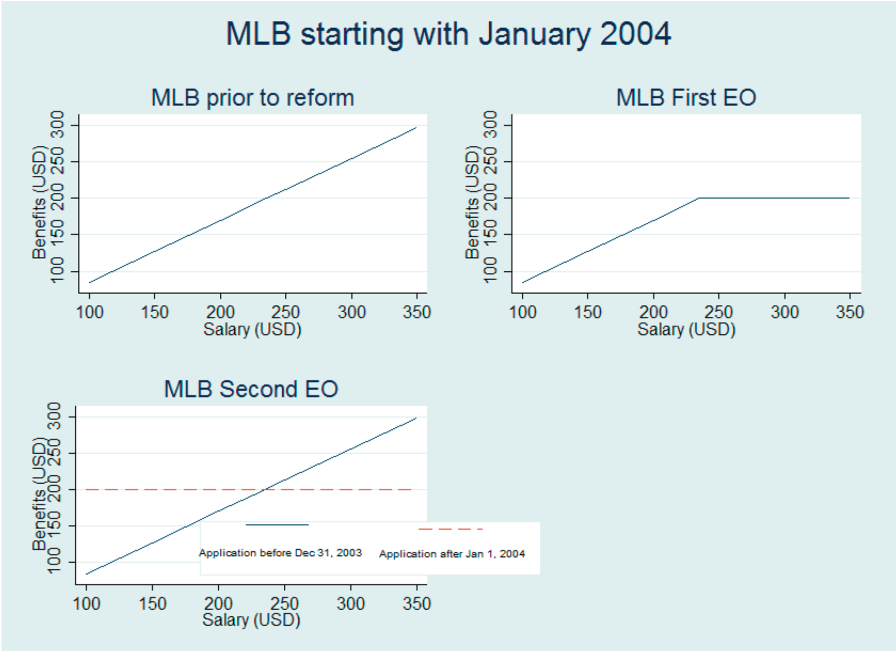
Notes: Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with <2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for births occurring after January 2004. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Regressions include $T^{Pot}$ that is 1 for births occurring between October-December 2003, and the interaction term $Treat * T^{Pot}$. "Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Estimation using Weighted Least Squares. *** p<0.01, ** p<0.05, * p<0.1

Table 4.17: Heterogenous effects

| | | (1) Baseline | (2) Low i wage | (3) Married | (4) Unmarried | (5) Younger | (6) Older | (7) Rural | (8) Urban |
|---|---|---|---|---|---|---|---|---|---|
| I | Conceptions | 0.464 | 0.484 | 0.318 | 0.754 | 1.341 | 0.381 | 0.352 | 0.553 |
| | | (0.480) | (2.873) | (0.675) | (0.554) | (1.333) | (0.329) | (0.884) | (0.478) |
| II | Abortion | -0.047 | -0.461 | -0.072 | -0.044 | -0.070 | -0.045 | -0.110 | 0.018 |
| | | (0.059) | (0.304) | (0.064) | (0.182) | (0.116) | (0.072) | (0.081) | (0.112) |
| III | Alcohol&smoking | 0.136 | 0.126 | 0.128 | 0.618** | 0.164 | 0.177 | 0.144 | 0.069 |
| | | (0.084) | (0.089) | (0.087) | (0.270) | (0.139) | (0.109) | (0.108) | (0.129) |
| | Vitamins | -0.060 | -0.051 | -0.122 | -0.059 | 0.055 | -0.178* | 0.014 | -0.081 |
| | | (0.083) | (0.089) | (0.085) | (0.353) | (0.151) | (0.093) | (0.109) | (0.146) |
| | Mth 1st control | -0.012 | -0.006 | 0.137 | -0.994 | -0.415 | 0.216 | -0.147 | 0.119 |
| | | (0.315) | (0.335) | (0.339) | (1.190) | (0.545) | (0.403) | (0.490) | (0.443) |
| | Lowbw | -0.112 | -0.097 | -0.084 | -0.562** | -0.100 | -0.083 | -0.074 | -0.038 |
| | | (0.072) | (0.078) | (0.079) | (0.222) | (0.113) | (0.096) | (0.100) | (0.121) |
| | Postnat consult | 0.107 | 0.117 | 0.125 | -0.571** | 0.122 | 0.093 | 0.430*** | -0.177 |
| | | (0.099) | (0.106) | (0.110) | (0.285) | (0.169) | (0.131) | (0.131) | (0.187) |
| | Days in hosp | 1.497 | 1.774 | 1.916 | -0.312 | 1.318 | 2.713 | 3.887 | -0.430 |
| | | (1.255) | (1.445) | (1.356) | (1.673) | (1.373) | (1.767) | (2.513) | (1.639) |
| | Infant vitamins | 0.002 | -0.009 | -0.023 | 0.241 | -0.074 | -0.018 | -0.008 | -0.014 |
| | | (0.076) | (0.082) | (0.079) | (0.192) | (0.136) | (0.087) | (0.113) | (0.146) |
| | Breastfeeding | -0.015 | -0.051 | -0.040 | 0.074 | 0.114 | -0.090 | 0.012 | 0.003 |
| | | (0.065) | (0.072) | (0.069) | (0.316) | (0.093) | (0.093) | (0.086) | (0.117) |
| | Mths breastfeeding | 1.219 | 1.727 | 0.801 | -11.527* | 1.476 | 0.550 | -0.184 | 1.847 |
| | | (1.104) | (1.244) | (1.152) | (6.176) | (2.398) | (1.376) | (1.846) | (1.712) |

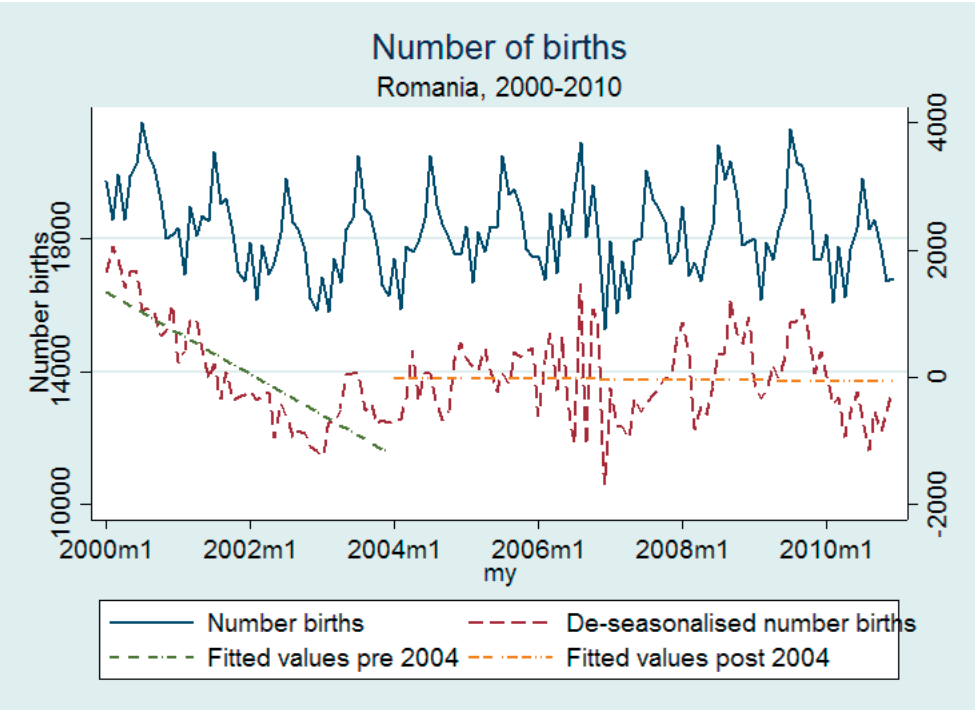
Notes: (1) Baseline specification; (2)Subsample of employed women with imputed wages below the threshold; (3) Subsample: married women; (4) Subsample: unmarried women; (5) Subsample: younger than 24 at pregnancy; (6)Subsample: older than 24 at pregnancy; (7) Subsamle: rural residence; (8) Subsample: urban residence. Only showing estimated coefficient on $Treat * T^{Cert}$, where $Treat$ is 1 for employed women and 0 for housewives. $T^{Cert}$ is 1 for conceptions occurring after April 2003.
Panel I: Regression dos not control for any individual level characteristics. Panel II and III: Regressions include individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Panel I and II: $T^{Cert}$ is 1 for conceptions occurring after April 2003. Regressions include $T^{Pot}$ that is 1 for conceptions occurring between January-March 2003, and the interaction term $Treat * T^{Pot}$. Panel III: $T^{Cert}$ is 1 for births occurring after January 2004. Regressions include $T^{Pot}$ that is 1 for births occurring between April-December 2003, and the interaction term $Treat * T^{Pot}$. All regressions include a linear time trend and conception quarter fixed effects."Non-high" refers to households with low and medium household assets levels. "High" refers to households with high household assets level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 4.1: Maternity Leave Benefits



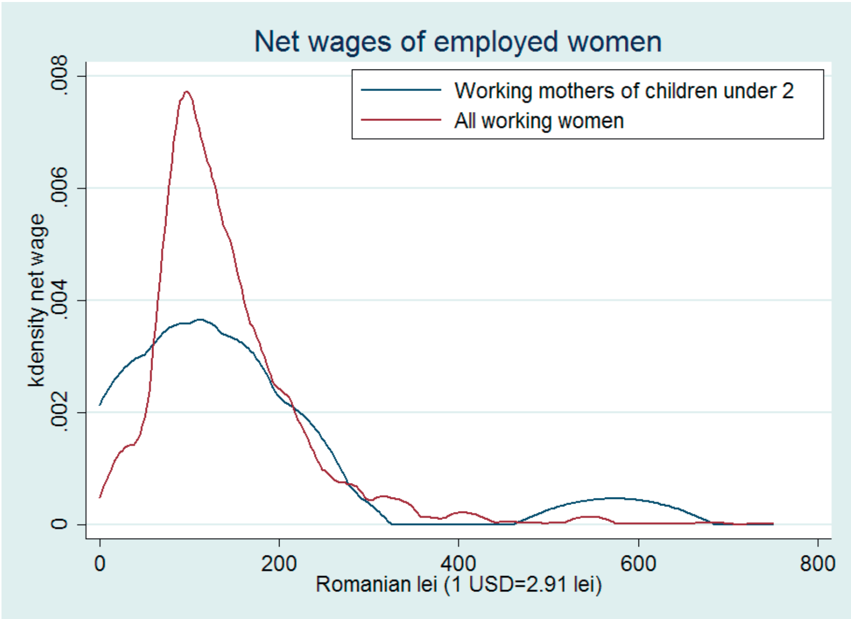

Notes: Figure illustrates the monthly benefits which would have been received by employed mothers starting with January 2004 under the alternative regimes, depending on the application date to the benefits. **Upper left panel** presents the benefits which would be received under the previous regime: proportional benefits for all income levels, irrespective of the application date. **Upper right panel** presents the proposed changes in the first EO: up to an income of 235 USD, women would receive a proportional benefit, irrespective of the application date, and above 235 USD they would receive a fixed benefit, irrespective of the application date. **Lower left panel** presents the final provisions of the changed law: women applying before December 31, 2003 would receive a proportional benefit, for all income levels; women applying after January 1, 2004 would receive a fixed benefit, irrespective of their income.

Figure 4.2: Number of births



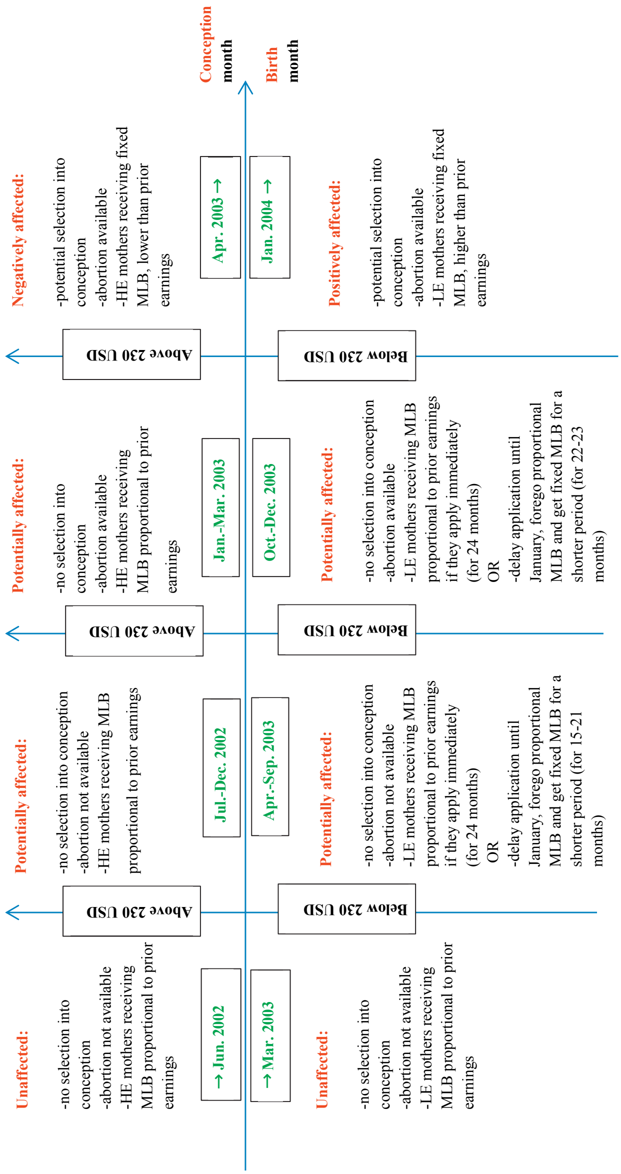

Notes: Series of monthly number of live birth on left axis. De-seasonalized monthly series of live births (residual series after controlling for month dummy variables) on the right axis.

Figure 4.3: Net wages of employed women, GGS data



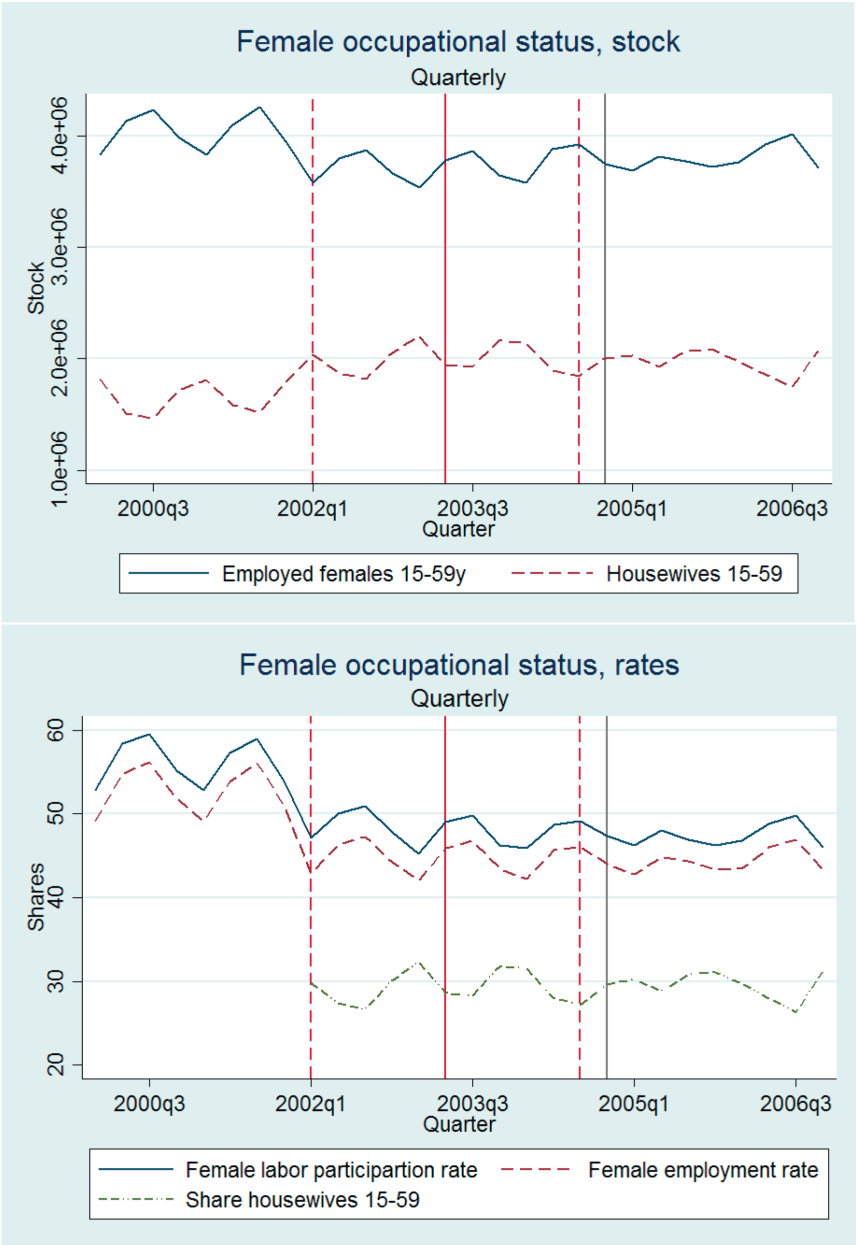

Notes: The distribution of net wages (thousand lei) of employed women (all and wimen with children under 2 who are working). Source: Generations and Gender Survey 2005.

Figure 4.4: Potential selection effects of the policy change announcement



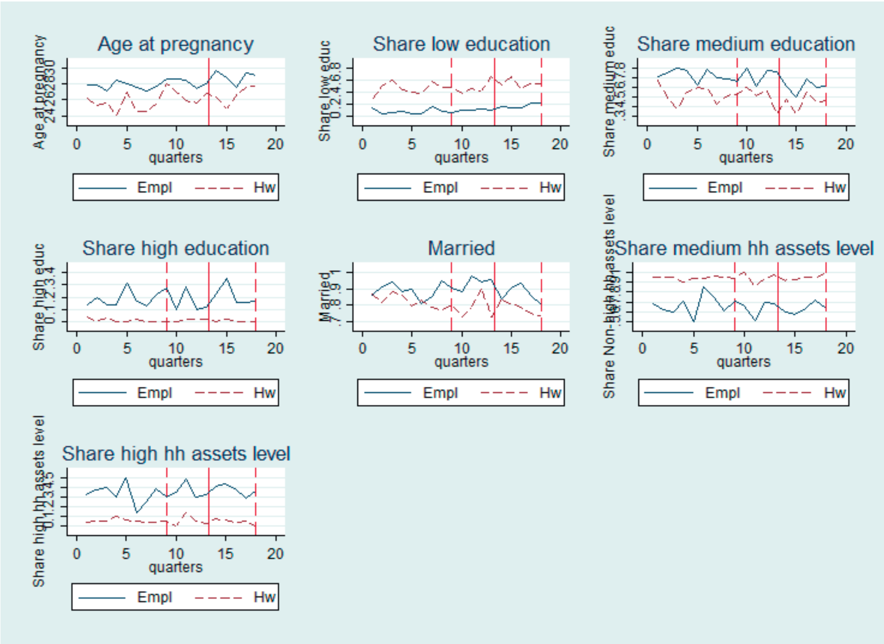

Notes: On the horizontal axis there are the conception (birth) months. On the vertical axis there are the pre-birth average income categories of the mother: above 230 USD per month (women whose benefits would be smaller under the fixed scheme than under the proportional scheme), and below 230 USD per month (women whose benefits would be larger under the fixed scheme than under the proportional scheme).

Figure 4.5: Occupational status of women



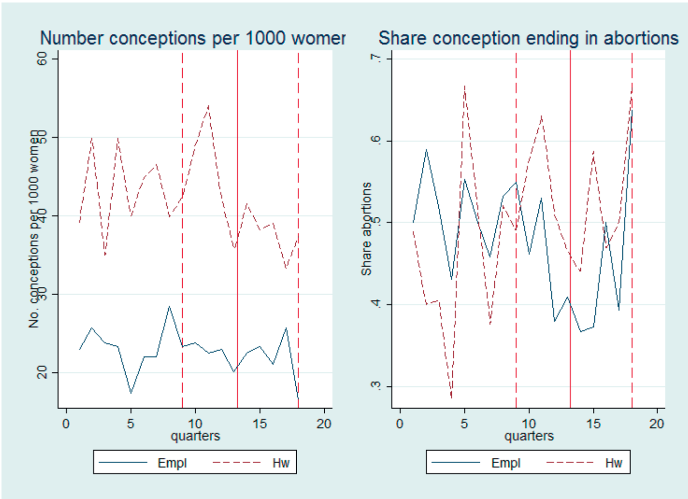

Notes: Quarterly stock women (unbroken line) and housewives (dashed line), 2000-2006. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change announcement month. vertical gray unbroken line marks the time when RHS-Ro survey was conducted. Source: Statistics Romania.

Figure 4.6: Average maternal characteristics by quarter of conception



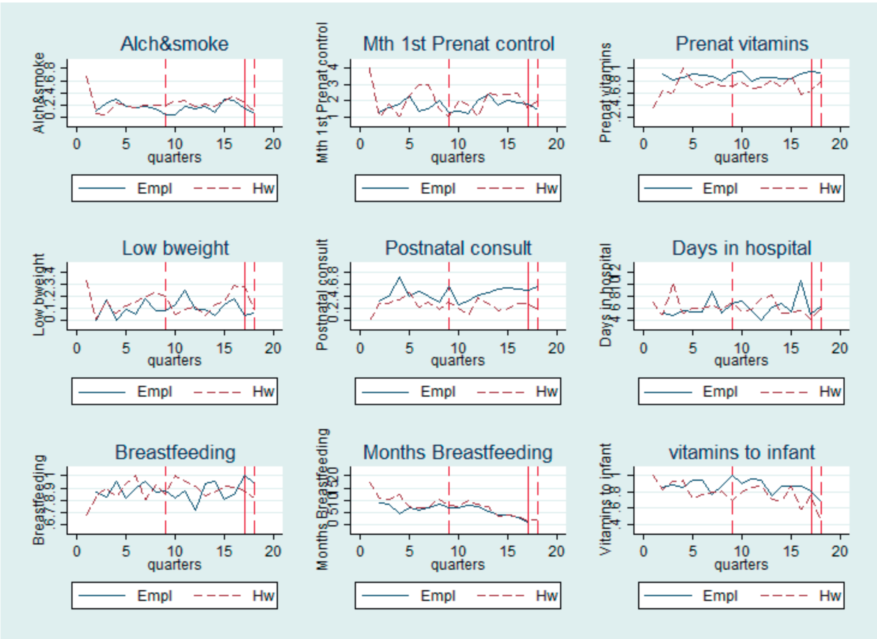

Notes: Average maternal characteristics by quarter of conception. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change announcement month.

Figure 4.7: Number of conceptions and probability of abortion by quarter of conception




Notes: Average number of conceptions per 1000 women and average abortion rates by quarter of conception. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change announcement month.

Figure 4.8: Early investments in child health by quarter of conception




Notes: Average outcomes conditional on live birth, by quarter of birth. Vertical dotted lines delimit analysis sample. Vertical unbroken line marks the policy change month.

# Appendix

## Imputed wages

As a second robustness check, I employ a synthetic matching (synthetic linkage) procedure in which I use a donor dataset to impute the wage income, to my primary dataset, the RHS-Ro. As donor dataset I use the Barometer of Public Opinion (BOP), a survey conducted in May 2003 (multiple waves available). BOP includes a representative sample of individuals and records the after tax wage income for April 2003, in addition to a series of socio-economic characteristics similar to those in RHS (e.g. education, occupational status, household assets, marital status, and household size) -the matching variables. These matching variables have the same marginal and joint distribution in both datasets as revealed by Kolmogorov-Smirnov tests, hence they are suitable for use in a synthetic matching procedure. As matching methods I use 1) a mixed method using propensity scores, and 2) hot deck method using Mahalanobis distance. In the mixed method, I use a propensity score to determine for each employed woman in RHS her n (n = $\overline{1,4}$) nearest "neighbours" from the BOP, and obtain her predicted/imputed wage income as an average of the wage income of her closest "neighbours" (as calculated by psmatch2). For the hot deck method, I define the distance as Mahalanobis distance and hence the method retrieves "live values" from the donor dataset. I use both methods and vary the number of nearest neighbors to be able to verify the robustness of the results to this crucial definition of earnings level. After having obtained the predicted wage income, I use a 5 mil. ROL threshold, a conservative bound for 5.8 mil. ROL threshold, which the net wage corresponding to a pre-tax wage of 7.6 mil. ROL, to divide the women in the RHS sample in high earners (HE) and low earners (LE).

Table 4.18: 3 months window, Probability of abortion

| VARIABLES | (2) DD | (3) DD | (4) DD | (5) DD Non-high | (6) DD High |
|---|---|---|---|---|---|
| $Treat * T^{Cert}$ | -0.084 | -0.076 | -0.068 | -0.143 | 0.195 |
| | (0.093) | (0.085) | (0.085) | (0.095) | (0.311) |
| $Treat$ | 0.022 | 0.053 | 0.048 | 0.077 | -0.125 |
| | (0.056) | (0.056) | (0.057) | (0.061) | (0.205) |
| $T^{Cert}$ | -0.087 | -0.061 | -0.059 | -0.099 | -0.175 |
| | (0.099) | (0.088) | (0.088) | (0.094) | (0.329) |
| Observations | 509 | 509 | 509 | 420 | 89 |
| R-squared | 0.008 | 0.216 | 0.218 | 0.246 | 0.298 |

Notes:Dependent variable: 1 if pregnancy terminated using abortion. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4.19: 3 months window, Outcomes conditional on live birth

| VARIABLES | (1) Alcohol& smoking | (2) Prenatal vitamins | (3) Mth 1st control | (4) Low bweight | (5) Postnatal consult | (6) Days in hospital | (7) Infant vitamins | (8) Breast feeding | (9) Months breastf. |
|---|---|---|---|---|---|---|---|---|---|
| **Panel A: All women** | | | | | | | | | |
| $Treat * T^{Cert}$ | -0.014 | -0.223* | 0.145 | -0.143 | 0.203 | 1.878* | 0.025 | -0.117 | 1.379 |
| | (0.129) | (0.126) | (0.502) | (0.108) | (0.151) | (1.079) | (0.102) | (0.107) | (1.891) |
| $Treat$ | 0.077 | 0.075 | -0.647* | 0.069 | 0.028 | -1.124 | -0.033 | -0.025 | -0.555 |
| | (0.081) | (0.083) | (0.333) | (0.071) | (0.083) | (0.696) | (0.071) | (0.068) | (1.536) |
| $Treat$ | 0.041 | 0.237* | 0.005 | 0.014 | 0.118 | -1.128 | -0.014 | -0.014 | -5.021** |
| | (0.134) | (0.141) | (0.658) | (0.113) | (0.138) | (0.989) | (0.100) | (0.101) | (2.120) |
| Observations | 192 | 179 | 178 | 192 | 192 | 189 | 192 | 192 | 137 |
| R-squared | 0.197 | 0.139 | 0.135 | 0.036 | 0.141 | 0.049 | 0.027 | 0.077 | 0.168 |
| **Panel B: Non-high hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | -0.033 | -0.174 | 0.074 | -0.170 | 0.235 | 1.391 | 0.091 | -0.205 | 1.192 |
| | (0.152) | (0.151) | (0.605) | (0.137) | (0.171) | (1.320) | (0.126) | (0.137) | (2.592) |
| $Treat$ | 0.036 | 0.015 | -0.632* | 0.098 | 0.009 | -0.571 | -0.057 | -0.022 | -0.893 |
| | (0.086) | (0.096) | (0.378) | (0.079) | (0.083) | (0.630) | (0.085) | (0.079) | (1.771) |
| $T^{Cert}$ | 0.084 | 0.184 | -0.034 | 0.040 | 0.198 | -1.102 | -0.035 | 0.012 | -3.366 |
| | (0.154) | (0.162) | (0.764) | (0.125) | (0.142) | (1.035) | (0.115) | (0.115) | (2.524) |
| Observations | 155 | 142 | 141 | 155 | 155 | 152 | 155 | 155 | 104 |
| R-squared | 0.172 | 0.150 | 0.087 | 0.047 | 0.134 | 0.052 | 0.028 | 0.112 | 0.160 |
| **Panel C: High hh assets index** | | | | | | | | | |
| $Treat * T^{Cert}$ | 0.088 | -0.351 | -0.290 | 0.320 | -0.210 | 9.102*** | 0.037 | 0.165 | -1.421 |
| | (0.336) | (0.328) | (1.339) | (0.253) | (0.567) | (3.221) | (0.090) | (0.174) | (4.424) |
| $Treat$ | 0.283 | 0.374 | -0.953** | -0.193 | -0.030 | -6.572*** | -0.045 | -0.096 | 4.074 |
| | (0.229) | (0.306) | (0.381) | (0.184) | (0.494) | (2.046) | (0.061) | (0.120) | (3.642) |
| $T^{Cert}$ | -0.121 | 0.474 | 1.095 | -0.526 | -0.002 | -9.903*** | -0.117 | -0.259 | -6.841 |
| | (0.271) | (0.374) | (1.393) | (0.322) | (0.598) | (3.527) | (0.144) | (0.261) | (5.124) |
| Observations | 37 | 37 | 37 | 37 | 37 | 37 | 37 | 37 | 33 |
| R-squared | 0.496 | 0.424 | 0.447 | 0.350 | 0.304 | 0.573 | 0.224 | 0.261 | 0.621 |
| Time trend | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Ind cov | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Dependent variable: (1) 1 if mother consumed alcohol or smoked during pregnancy; (2) 1 if mother took prenatal vitamins; (3) month of first prenatal control; (4) 1 if child is born with <2500 g; (5) 1 if mother and child had a postnatal control; (6) number of days in hospital after birth; (7) 1 if infant was given vitamins; (8) 1 if infant was breastfed; (9) number of months of breastfeeding. Controls: linear time trend, quarter of conception fixed effects; individual controls(number of previous abortions at pregnancy i, age at pregnancy i, educational level, marital status, rural dummy, number of members in household). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

***Previous doctoral theses in the Department of Economics, Gothenburg***

Avhandlingar publicerade innan serien Ekonomiska Studier startades
(Theses published before the series Ekonomiska Studier was started):

**Östman, Hugo** (1911), Norrlands ekonomiska utveckling
**Moritz, Marcus** (1911), Den svenska tobaksindustrien
**Sundbom, I.** (1933), Prisbildning och ändamålsenlighet
**Gerhard, I.** (1948), Problem rörande Sveriges utrikeshandel 1936/38
**Hegeland, Hugo** (1951), The Quantity Theory of Money
**Mattsson, Bengt** (1970), Cost-Benefit analys
**Rosengren, Björn** (1975), Valutareglering och nationell ekonomisk politik
**Hjalmarsson, Lennart** (1975), Studies in a Dynamic Theory of Production and its Applications
**Örtendahl, Per-Anders** (1975), Substitutionsaspekter på produktionsprocessen vid massaframställning
**Anderson, Arne M.** (1976), Produktion, kapacitet och kostnader vid ett helautomatiskt emballageglasbruk
**Ohlsson, Olle** (1976), Substitution och odelbarheter i produktionsprocessen vid massaframställning
**Gunnarsson, Jan** (1976), Produktionssystem och tätortshierarki – om sambandet mellan rumslig och ekonomisk struktur
**Köstner, Evert** (1976), Optimal allokering av tid mellan utbildning och arbete
**Wigren, Rune** (1976), Analys av regionala effektivitetsskillnader inom industribranscher
**Wästlund, Jan** (1976), Skattning och analys av regionala effektivitetsskillnader inom industribranscher
**Flöjstad, Gunnar** (1976), Studies in Distortions, Trade and Allocation Problems
**Sandelin, Bo** (1977), Prisutveckling och kapitalvinster på bostadsfastigheter
**Dahlberg, Lars** (1977), Empirical Studies in Public Planning
**Lönnroth, Johan** (1977), Marxism som matematisk ekonomi
**Johansson, Börje** (1978), Contributions to Sequential Analysis of Oligopolistic Competition

Ekonomiska Studier, utgivna av Nationalekonomiska institutionen vid Göteborgs Universitet. Nr 1 och 4 var inte doktorsavhandlingar. (The contributions to the department series 'Ekonomiska Studier' where no. 1 and 4 were no doctoral theses):

2. **Ambjörn, Erik** (1959), Svenskt importberoende 1926-1956: en ekonomisk-statistisk kartläggning med kommentarer
3. **Landgren, K-G.** (1960), Den "Nya ekonomien" i Sverige: J.M. Keynes, E. Wigfors och utecklingen 1927-39
5. **Bigsten, Arne** (1979), Regional Inequality and Development: A Case Study of Kenya
6. **Andersson, Lars** (1979), Statens styrning av de kommunala budgetarnas struktur (Central Government Influence on the Structure of the Municipal Budget)
7. **Gustafsson, Björn** (1979), Inkomst- och uppväxtförhållanden (Income and Family Background)

8. **Granholm, Arne** (1981), Interregional Planning Models for the Allocation of Private and Public Investments
9. **Lundborg, Per** (1982), Trade Policy and Development: Income Distributional Effects in the Less Developed Countries of the US and EEC Policies for Agricultural Commodities
10. **Juås, Birgitta** (1982), Värdering av risken för personskador. En jämförande studie av implicita och explicita värden. (Valuation of Personal Injuries. A comparison of Explicit and Implicit Values)
11. **Bergendahl, Per-Anders** (1982), Energi och ekonomi - tillämpningar av input-output analys (Energy and the Economy - Applications of Input-Output Analysis)
12. **Blomström, Magnus** (1983), Foreign Investment, Technical Efficiency and Structural Change - Evidence from the Mexican Manufacturing Industry
13. **Larsson, Lars-Göran** (1983), Comparative Statics on the Basis of Optimization Methods
14. **Persson, Håkan** (1983), Theory and Applications of Multisectoral Growth Models
15. **Sterner, Thomas** (1986), Energy Use in Mexican Industry.
16. **Flood, Lennart** (1986), On the Application of Time Use and Expenditure Allocation Models.
17. **Schuller, Bernd-Joachim** (1986), Ekonomi och kriminalitet - en empirisk undersökning av brottsligheten i Sverige (Economics of crime - an empirical analysis of crime in Sweden)
18. **Walfridson, Bo** (1987), Dynamic Models of Factor Demand. An Application to Swedish Industry.
19. **Stålhammar, Nils-Olov** (1987), Strukturomvandling, företagsbeteende och förväntningsbildning inom den svenska tillverkningsindustrin (Structural Change, Firm Behaviour and Expectation Formation in Swedish Manufactury)
20. **Anxo, Dominique** (1988), Sysselsättningseffekter av en allmän arbetstidsför-kortning (Employment effects of a general shortage of the working time)
21. **Mbelle, Ammon** (1988), Foreign Exchange and Industrial Development: A Study of Tanzania.
22. **Ongaro, Wilfred** (1988), Adoption of New Farming Technology: A Case Study of Maize Production in Western Kenya.
23. **Zejan, Mario** (1988), Studies in the Behavior of Swedish Multinationals.
24. **Görling, Anders** (1988), Ekonomisk tillväxt och miljö. Förorenings-struktur och ekonomiska effekter av olika miljövårdsprogram. (Economic Growth and Environment. Pollution Structure and Economic Effects of Some Environmental Programs).
25. **Aguilar, Renato** (1988), Efficiency in Production: Theory and an Application on Kenyan Smallholders.
26. **Kayizzi-Mugerwa, Steve** (1988), External Shocks and Adjustment in Zambia.

27. **Bornmalm-Jardelöw, Gunilla** (1988), Högre utbildning och arbetsmarknad (Higher Education and the Labour Market)
28. **Tansini, Ruben** (1989), Technology Transfer: Dairy Industries in Sweden and Uruguay.
29. **Andersson, Irene** (1989), Familjebeskattning, konsumtion och arbetsutbud - En ekonometrisk analys av löne- och inkomstelasticiteter samt policysimuleringar för svenska hushåll (Family Taxation, Consumption and Labour Supply - An Econometric Analysis of Wage and Income Elasticities and Policy Simulations for Swedish Households)
30. **Henrekson, Magnus** (1990), An Economic Analysis of Swedish Government Expenditure
31. **Sjöö, Boo** (1990), Monetary Policy in a Continuous Time Dynamic Model for Sweden
32. **Rosén, Åsa** (1991), Contributions to the Theory of Labour Contracts.
33. **Loureiro, Joao M. de Matos** (1992), Foreign Exchange Intervention, Sterilization and Credibility in the EMS: An Empirical Study
34. **Irandoust, Manuchehr** (1993), Essays on the Behavior and Performance of the Car Industry
35. **Tasiran, Ali Cevat** (1993), Wage and Income Effects on the Timing and Spacing of Births in Sweden and the United States
36. **Milopoulos, Christos** (1993), Investment Behaviour under Uncertainty: An Econometric Analysis of Swedish Panel Data
37. **Andersson, Per-Åke** (1993), Labour Market Structure in a Controlled Economy: The Case of Zambia
38. **Storrie, Donald W.** (1993), The Anatomy of a Large Swedish Plant Closure
39. **Semboja, Haji Hatibu Haji** (1993), Energy and Development in Kenya
40. **Makonnen, Negatu** (1993), Labor Supply and the Distribution of Economic Well-Being: A Case Study of Lesotho
41. **Julin, Eva** (1993), Structural Change in Rural Kenya
42. **Durevall, Dick** (1993), Essays on Chronic Inflation: The Brazilian Experience
43. **Veiderpass, Ann** (1993), Swedish Retail Electricity Distribution: A Non-Parametric Approach to Efficiency and Productivity Change
44. **Odeck, James** (1993), Measuring Productivity Growth and Efficiency with Data Envelopment Analysis: An Application on the Norwegian Road Sector
45. **Mwenda, Abraham** (1993), Credit Rationing and Investment Behaviour under Market Imperfections: Evidence from Commercial Agriculture in Zambia
46. **Mlambo, Kupukile** (1993), Total Factor Productivity Growth: An Empirical Analysis of Zimbabwe's Manufacturing Sector Based on Factor Demand Modelling
47. **Ndung'u, Njuguna** (1993), Dynamics of the Inflationary Process in Kenya
48. **Modén, Karl-Markus** (1993), Tax Incentives of Corporate Mergers and Foreign Direct Investments
49. **Franzén, Mikael** (1994), Gasoline Demand - A Comparison of Models
50. **Heshmati, Almas** (1994), Estimating Technical Efficiency, Productivity Growth And Selectivity Bias Using Rotating Panel Data: An Application to Swedish Agriculture

51. **Salas, Osvaldo** (1994), Efficiency and Productivity Change: A Micro Data Case Study of the Colombian Cement Industry
52. **Bjurek, Hans** (1994), Essays on Efficiency and Productivity Change with Applications to Public Service Production
53. **Cabezas Vega, Luis** (1994), Factor Substitution, Capacity Utilization and Total Factor Productivity Growth in the Peruvian Manufacturing Industry
54. **Katz, Katarina** (1994), Gender Differentiation and Discrimination. A Study of Soviet Wages
55. **Asal, Maher** (1995), Real Exchange Rate Determination and the Adjustment Process: An Empirical Study in the Cases of Sweden and Egypt
56. **Kjulin, Urban** (1995), Economic Perspectives on Child Care
57. **Andersson, Göran** (1995), Volatility Forecasting and Efficiency of the Swedish Call Options Market
58. **Forteza, Alvaro** (1996), Credibility, Inflation and Incentive Distortions in the Welfare State
59. **Locking, Håkan** (1996), Essays on Swedish Wage Formation
60. **Välilä, Timo** (1996), Essays on the Credibility of Central Bank Independence
61. **Yilma, Mulugeta** (1996), Measuring Smallholder Efficiency: Ugandan Coffee and Food-Crop Production
62. **Mabugu, Ramos E.** (1996), Tax Policy Analysis in Zimbabwe Applying General Equilibrium Models
63. **Johansson, Olof** (1996), Welfare, Externalities, and Taxation; Theory and Some Road Transport Applications.
64. **Chitiga, Margaret** (1996), Computable General Equilibrium Analysis of Income Distribution Policies in Zimbabwe
65. **Leander, Per** (1996), Foreign Exchange Market Behavior Expectations and Chaos
66. **Hansen, Jörgen** (1997), Essays on Earnings and Labor Supply
67. **Cotfas, Mihai** (1997), Essays on Productivity and Efficiency in the Romanian Cement Industry
68. **Horgby, Per-Johan** (1997), Essays on Sharing, Management and Evaluation of Health Risks
69. **Nafar, Nosratollah** (1997), Efficiency and Productivity in Iranian Manufacturing Industries
70. **Zheng, Jinghai** (1997), Essays on Industrial Structure, Technical Change, Employment Adjustment, and Technical Efficiency
71. **Isaksson, Anders** (1997), Essays on Financial Liberalisation in Developing Countries: Capital mobility, price stability, and savings
72. **Gerdin, Anders** (1997), On Productivity and Growth in Kenya, 1964-94
73. **Sharifi, Alimorad** (1998), The Electricity Supply Industry in Iran: Organization, performance and future development
74. **Zamanian, Max** (1997), Methods for Mutual Fund Portfolio Evaluation: An application to the Swedish market
75. **Manda, Damiano Kulundu** (1997), Labour Supply, Returns to Education, and the Effect of Firm Size on Wages: The case of Kenya
76. **Holmén, Martin** (1998), Essays on Corporate Acquisitions and Stock Market Introductions

77. **Pan, Kelvin** (1998), Essays on Enforcement in Money and Banking
78. **Rogat, Jorge** (1998), The Value of Improved Air Quality in Santiago de Chile
79. **Peterson, Stefan** (1998), Essays on Large Shareholders and Corporate Control
80. **Belhaj, Mohammed** (1998), Energy, Transportation and Urban Environment in Africa: The Case of Rabat-Salé, Morocco
81. **Mekonnen, Alemu** (1998), Rural Energy and Afforestation: Case Studies from Ethiopia
82. **Johansson, Anders** (1998), Empirical Essays on Financial and Real Investment Behavior
83. **Köhlin, Gunnar** (1998), The Value of Social Forestry in Orissa, India
84. **Levin, Jörgen** (1998), Structural Adjustment and Poverty: The Case of Kenya
85. **Ncube, Mkhululi** (1998), Analysis of Employment Behaviour in Zimbabwe
86. **Mwansa, Ladslous** (1998), Determinants of Inflation in Zambia
87. **Agnarsson, Sveinn** (1998), Of Men and Machines: Essays in Applied Labour and Production Economics
88. **Kadenge, Phineas** (1998), Essays on Macroeconomic Adjustment in Zimbabwe: Inflation, Money Demand, and the Real Exchange Rate
89. **Nyman, Håkan** (1998), An Economic Analysis of Lone Motherhood in Sweden
90. **Carlsson, Fredrik** (1999), Essays on Externalities and Transport
91. **Johansson, Mats** (1999), Empirical Studies of Income Distribution
92. **Alemu, Tekie** (1999), Land Tenure and Soil Conservation: Evidence from Ethiopia
93. **Lundvall, Karl** (1999), Essays on Manufacturing Production in a Developing Economy: Kenya 1992-94
94. **Zhang, Jianhua** (1999), Essays on Emerging Market Finance
95. **Mlima, Aziz Ponary** (1999), Four Essays on Efficiency and Productivity in Swedish Banking
96. **Davidsen, Björn-Ivar** (2000), Bidrag til den økonomisk-metodologiske tenkningen (Contributions to the Economic Methodological Thinking)
97. **Ericson, Peter** (2000), Essays on Labor Supply
98. **Söderbom, Måns** (2000), Investment in African Manufacturing: A Microeconomic Analysis
99. **Höglund, Lena** (2000), Essays on Environmental Regulation with Applications to Sweden
100. **Olsson, Ola** (2000), Perspectives on Knowledge and Growth
101. **Meuller, Lars** (2000), Essays on Money and Credit
102. **Österberg, Torun** (2000), Economic Perspectives on Immigrants and Intergenerational Transmissions
103. **Kalinda Mkenda, Beatrice** (2001), Essays on Purchasing Power Parity, RealExchange Rate, and Optimum Currency Areas
104. **Nerhagen, Lena** (2001), Travel Demand and Value of Time - Towards an Understanding of Individuals Choice Behavior

105. **Mkenda, Adolf** (2001), Fishery Resources and Welfare in Rural Zanzibar
106. **Eggert, Håkan** (2001), Essays on Fisheries Economics
107. **Andrén, Daniela** (2001), Work, Sickness, Earnings, and Early Exits from the Labor Market. An Empirical Analysis Using Swedish Longitudinal Data
108. **Nivorozhkin, Eugene** (2001), Essays on Capital Structure
109. **Hammar, Henrik** (2001), Essays on Policy Instruments: Applications to Smoking and the Environment
110. **Nannyonjo, Justine** (2002), Financial Sector Reforms in Uganda (1990-2000): Interest Rate Spreads, Market Structure, Bank Performance and Monetary Policy
111. **Wu, Hong** (2002), Essays on Insurance Economics
112. **Linde-Rahr, Martin** (2002), Household Economics of Agriculture and Forestry in Rural Vienam
113. **Maneschiöld, Per-Ola** (2002), Essays on Exchange Rates and Central Bank Credibility
114. **Andrén, Thomas** (2002), Essays on Training, Welfare and Labor Supply
115. **Granér, Mats** (2002), Essays on Trade and Productivity: Case Studies of Manufacturing in Chile and Kenya
116. **Jaldell, Henrik** (2002), Essays on the Performance of Fire and Rescue Services
117. **Alpizar, Francisco, R.** (2002), Essays on Environmental Policy-Making in Developing Countries: Applications to Costa Rica
118. **Wahlberg, Roger** (2002), Essays on Discrimination, Welfare and Labor Supply
119. **Piculescu, Violeta** (2002), Studies on the Post-Communist Transition
120. **Pylkkänen, Elina** (2003), Studies on Household Labor Supply and Home Production
121. **Löfgren, Åsa** (2003), Environmental Taxation – Empirical and Theoretical Applications
122. **Ivaschenko, Oleksiy** (2003), Essays on Poverty, Income Inequality and Health in Transition Economies
123. **Lundström, Susanna** (2003), On Institutions, Economic Growth and the Environment
124. **Wambugu, Anthony** (2003), Essays on Earnings and Human Capital in Kenya
125. **Adler, Johan** (2003), Aspects of Macroeconomic Saving
126. **Erlandsson, Mattias** (2003), On Monetary Integration and Macroeconomic Policy
127. **Brink, Anna** (2003), On the Political Economy of Municipality Break-Ups
128. **Ljungwall, Christer** (2003), Essays on China's Economic Performance During the Reform Period
129. **Chifamba, Ronald** (2003), Analysis of Mining Investments in Zimbabwe
130. **Muchapondwa, Edwin** (2003), The Economics of Community-Based Wildlife Conservation in Zimbabwe
131. **Hammes, Klaus** (2003), Essays on Capital Structure and Trade Financing
132. **Abou-Ali, Hala** (2003), Water and Health in Egypt: An Empirical Analysis
133. **Simatele, Munacinga** (2004), Financial Sector Reforms and Monetary Policy in Zambia
134. **Tezic, Kerem** (2004), Essays on Immigrants' Economic Integration
135. INSTÄLLD

136. **Gjirja, Matilda** (2004), Efficiency and Productivity in Swedish Banking
137. **Andersson, Jessica** (2004), Welfare Environment and Tourism in Developing Countries
138. **Chen, Yinghong** (2004), Essays on Voting Power, Corporate Governance and Capital Structure
139. **Yesuf, Mahmud** (2004), Risk, Time and Land Management under Market Imperfections: Applications to Ethiopia
140. **Kateregga, Eseza** (2005), Essays on the Infestation of Lake Victoria by the Water Hyacinth
141. **Edvardsen, Dag Fjeld** (2004), Four Essays on the Measurement of Productive Efficiency
142. **Lidén, Erik** (2005), Essays on Information and Conflicts of Interest in Stock Recommendations
143. **Dieden, Sten** (2005), Income Generation in the African and Coloured Population – Three Essays on the Origins of Household Incomes in South Africa
144. **Eliasson, Marcus** (2005), Individual and Family Consequences of Involuntary Job Loss
145. **Mahmud, Minhaj** (2005), Measuring Trust and the Value of Statistical Lives: Evidence from Bangladesh
146. **Lokina, Razack Bakari** (2005), Efficiency, Risk and Regulation Compliance: Applications to Lake Victoria Fisheries in Tanzania
147. **Jussila Hammes, Johanna** (2005), Essays on the Political Economy of Land Use Change
148. **Nyangena, Wilfred** (2006), Essays on Soil Conservation, Social Capital and Technology Adoption
149. **Nivorozhkin, Anton** (2006), Essays on Unemployment Duration and Programme Evaluation
150. **Sandén, Klas** (2006), Essays on the Skill Premium
151. **Deng, Daniel** (2006), Three Essays on Electricity Spot and Financial Derivative Prices at the Nordic Power Exchange
152. **Gebreeyesus, Mulu** (2006), Essays on Firm Turnover, Growth, and Investment Behavior in Ethiopian Manufacturing
153. **Islam, Nizamul Md.** (2006), Essays on Labor Supply and Poverty: A Microeconometric Application
154. **Kjaer, Mats** (2006), Pricing of Some Path-Dependent Options on Equities and Commodities
155. **Shimeles, Abebe** (2006), Essays on Poverty, Risk and Consumption Dynamics in Ethiopia
156. **Larsson, Jan** (2006), Four Essays on Technology, Productivity and Environment
157. **Congdon Fors, Heather** (2006), Essays in Institutional and Development Economics
158. **Akpalu, Wisdom** (2006), Essays on Economics of Natural Resource Management and Experiments
159. **Daruvala, Dinky** (2006), Experimental Studies on Risk, Inequality and Relative Standing
160. **García, Jorge** (2007), Essays on Asymmetric Information and Environmental Regulation through Disclosure

161. **Bezabih, Mintewab** (2007), Essays on Land Lease Markets, Productivity, Biodiversity, and Environmental Variability
162. **Visser, Martine** (2007), Fairness, Reciprocity and Inequality: Experimental Evidence from South Africa
163. **Holm, Louise** (2007), A Non-Stationary Perspective on the European and Swedish Business Cycle
164. **Herbertsson, Alexander** (2007), Pricing Portfolio Credit Derivatives
165. **Johansson, Anders C.** (2007), Essays in Empirical Finance: Volatility, Interdependencies, and Risk in Emerging Markets
166. **Ibáñez Díaz, Marcela** (2007), Social Dilemmas: The Role of Incentives, Norms and Institutions
167. **Ekbom, Anders** (2007), Economic Analysis of Soil Capital, Land Use and Agricultural Production in Kenya
168. **Sjöberg, Pål** (2007), Essays on Performance and Growth in Swedish Banking
169. **Palma Aguirre, Grisha Alexis** (2008), Explaining Earnings and Income Inequality in Chile
170. **Akay, Alpaslan** (2008), Essays on Microeconometrics and Immigrant Assimilation
171. **Carlsson, Evert** (2008), After Work – Investing for Retirement
172. **Munshi, Farzana** (2008), Essays on Globalization and Occupational Wages
173. **Tsakas, Elias** (2008), Essays on Epistemology and Evolutionary Game Theory
174. **Erlandzon, Karl** (2008), Retirement Planning: Portfolio Choice for Long-Term Investors
175. **Lampi, Elina** (2008), Individual Preferences, Choices, and Risk Perceptions – Survey Based Evidence
176. **Mitrut, Andreea** (2008), Four Essays on Interhousehold Transfers and Institutions in Post-Communist Romania
177. **Hansson, Gustav** (2008), Essays on Social Distance, Institutions, and Economic Growth
178. **Zikhali, Precious** (2008), Land Reform, Trust and Natural Resource Management in Africa
179. **Tengstam, Sven** (2008), Essays on Smallholder Diversification, Industry Location, Debt Relief, and Disability and Utility
180. **Boman, Anders** (2009), Geographic Labour Mobility – Causes and Consequences
181. **Qin, Ping** (2009), Risk, Relative Standing and Property Rights: Rural Household Decision-Making in China
182. **Wei, Jiegen** (2009), Essays in Climate Change and Forest Management
183. **Belu, Constantin** (2009), Essays on Efficiency Measurement and Corporate Social Responsibility
184. **Ahlerup, Pelle** (2009), Essays on Conflict, Institutions, and Ethnic Diversity
185. **Quiroga, Miguel** (2009), Microeconomic Policy for Development: Essays on Trade and Environment, Poverty and Education
186. **Zerfu, Daniel** (2010), Essays on Institutions and Economic Outcomes
187. **Wollbrant, Conny** (2010), Self-Control and Altruism
188. **Villegas Palacio, Clara** (2010), Formal and Informal Regulations: Enforcement and Compliance

189. **Maican, Florin** (2010), Essays in Industry Dynamics on Imperfectly Competitive Markets
190. **Jakobsson, Niklas** (2010), Laws, Attitudes and Public Policy
191. **Manescu, Cristiana** (2010), Economic Implications of Corporate Social Responsibility and Responsible Investments
192. **He, Haoran** (2010), Environmental and Behavioral Economics – Applications to China
193. **Andersson, Fredrik W.** (2011), Essays on Social Comparison
194. **Isaksson, Ann-Sofie** (2011), Essays on Institutions, Inequality and Development
195. **Pham, Khanh Nam** (2011), Prosocial Behavior, Social Interaction and Development: Experimental Evidence from Vietnam
196. **Lindskog, Annika** (2011), Essays on Economic Behaviour: HIV/AIDS, Schooling, and Inequality
197. **Kotsadam, Andreas** (2011), Gender, Work, and Attitudes
198. **Alem, Yonas** (2011), Essays on Shocks, Welfare, and Poverty Dynamics: Microeconometric Evidence from Ethiopia
199. **Köksal-Ayhan, Miyase Yesim** (2011), Parallel Trade, Reference Pricing and Competition in the Pharmaceutical Market: Theory and Evidence
200. **Vondolia, Godwin Kofi** (2011), Essays on Natural Resource Economics
201. **Widerberg, Anna** (2011), Essays on Energy and Climate Policy – Green Certificates, Emissions Trading and Electricity Prices
202. **Siba, Eyerusalem** (2011), Essays on Industrial Development and Political Economy of Africa
203. **Orth, Matilda** (2012), Entry, Competition and Productivity in Retail
204. **Nerman, Måns** (2012), Essays on Development: Household Income, Education, and Female Participation and Representation
205. **Wicks, Rick** (2012), The Place of Conventional Economics in a World with Communities and Social Goods
206. **Sato, Yoshihiro** (2012), Dynamic Investment Models, Employment Generation and Productivity – Evidence from Swedish Data
207. **Valsecchi, Michele** (2012), Essays in Political Economy of Development
208. **Teklewold Belayneh, Hailemariam** (2012), Essays on the Economics of Sustainable Agricultural Technologies in Ethiopia
209. **Wagura Ndiritu, Simon** (2013), Essays on Gender Issues, Food Security, and Technology Adoption in East Africa
210. **Ruist, Joakim** (2013), Immigration, Work, and Welfare
211. **Nordén, Anna** (2013), Essays on Behavioral Economics and Policies for Provision of Ecosystem Services
212. **Yang, Xiaojun** (2013), Household Decision Making, Time Preferences, and Positional Concern: Experimental Evidence from Rural China
213. **Bonilla Londoño, Jorge Alexander** (2013), Essays on the Economics of Air Quality Control
214. **Mohlin, Kristina** (2013), Essays on Environmental Taxation and Climate Policy
215. **Medhin, Haileselassie** (2013), The Poor and Their Neighbors: Essays on Behavioral and Experimental Economics

216. **Andersson, Lisa** (2013), Essays on Development and Experimental Economics: Migration, Discrimination and Positional Concerns
217. **Weng, Qian** (2014), Essays on Team Cooperation and Firm Performance
218. **Zhang, Xiao-Bing** (2015), Cooperation and Paradoxes in Climate Economics
219. **Jaime Torres, Monica Marcela** (2015) Essays on Behavioral Economics and Policy Design
220. **Bejenariu, Simona** (2015) Determinants of Health Capital at Birth: Evidence from Policy Interventions