Stockholm University

Media Influence on Pollution, and Gender Equality

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ISSN 0346-6892
ISBN 978-91-7447-687-3

Cover Picture: Alessandro Campa
Printed in Sweden by PrintCenter US-AB, Stockholm 2013

Distributor: Institute for International Economic Studies

Doctoral Dissertation
Department of Economics
Stockholm University

Abstract

This thesis consists of three self-contained essays.

In the first essay, **"Press and Leaks: Do Newspapers Reduce Toxic Emissions?"**, I investigate whether media coverage induces firms to reduce toxic emissions. I first develop a simple model where newspapers inform consumers about the level of toxic emissions of a firm, potentially lowering demand. Firms react to this "threat of coverage" by proactively adjusting emissions. I test this using data on emissions from 25,523 plants in 2001-2009 from the Toxic Release Inventory of the US Environmental Protection Agency, coupled with data on location and content of newspapers. I find that an increase in *Newspapers Density*, that is the number of newspapers nearby the plant, raises the press coverage of the plant's toxic emissions and reduces the amount of these emissions. If a plant were to move from the twenty-fifth to the seventy-fifth percentile of *Newspapers Density*, its emissions would be 5% lower. I show that this association is unlikely to be driven by selection on unobservables, and is larger in industries exposed to consumer pressure and in counties subjected to extreme negative health outcomes. My estimates suggest that aggregate toxic emissions from plants producing final goods would be 11% larger if there were no newspapers within a 20-mile radius from the plants, a number that doubles for plants located in counties that have experienced an extreme number of cancer deaths.

In the second essay, **"Gender Quotas, Female Politicians and Public Expenditures: Quasi-Experimental Evidence"**, I estimate the effect of gender quotas on the election of female politicians and on public finance decisions in Spanish municipalities, using a Before-After Regression Discontinuity Design. Gender quotas have increased the percentage of female candidates and also, but to a lower extent, the percentage of female councilors. The difference between the two effects is due to the strategic positioning of candidates within lists. The effect of quotas on the election of female mayors and on the size and composition of expenditures is not statistically different from zero, despite there being survey evidence of gender differences in preferences over policy. I advance the hypothesis that gender quotas did not change policy because they failed to promote women to positions with executive and agenda setting power, such as that of mayor. Alternatively, the null effect on can be a "median voter" result.

In the third essay, **"Are attitudes endogenous to political regimes? Beliefs about working women in state-socialist countries "**, co-authored with Michel Serafinelli, we study whether a

policy regime that puts emphasis on gender equality changes people's attitudes toward sex-roles in society. A consensus has emerged that gender attitudes can at least partially explain female labor market outcomes. However, evidence on the causes of the development and evolution of such attitudes is limited. Specifically, it is unclear whether individual beliefs about gender roles are endogenous to political regimes. We address this gap by using a Difference-in-Differences analysis that compares attitudes before and after the advent of state-socialism in Central and Eastern Europe. Between the late 1940s and the early 1990s, state-socialist governments strongly encouraged women's paid employment outside the home. Our results suggest a significant difference in the evolution of attitudes towards gender roles in the labor market between Europeans in state-socialist countries and other Europeans during the period 1947-1991. Central and Eastern Europeans that formed their attitudes during the state-socialist regime seem more likely to hold progressive beliefs regarding working women. No significant difference is found in the evolution of beliefs about the compatibility of work and motherhood.

A mia madre e mio padre

Acknowledgments

This thesis has been a collective work, and this is the time and space to thank everybody who contributed to this.

First and foremost, my supervisor David Strömberg. David has contributed to the writing of this thesis in many ways. First, as a source of inspiration, through his seminal papers in the literature on media-economics; having come across those papers during my Master studies contributed a great deal to my decision to start a PhD, because I saw that research in economics was really a way of addressing questions that arose from my daily experience in society. Second, helping me distinguish between good and "not so good" research ideas, those that can eventually lead to some interesting answers and those that do not have this potential. Third, teaching me how to be as rigorous as possible in pursuing the good ideas: this took discussions, lunches, and his reading of every paper in this thesis, and every paper has been greatly improved by his comments and suggestions. I hope that, in the future, I can reward his work and his trust by writing some good papers, and being a guide for some students as he has been for me.

Other people contributed to this thesis by improving the quality of my research. Torsten Persson is, according to me, the best counterpart for big-picture discussions ever, given his grand knowledge of economics; those discussions have been fundamental to decide in which direction a paper should go, when I felt I was at a crossroads. Torsten has also been able to motivate and encourage me during some difficult phases of writing the thesis.

Emilia Simeonova gave me tons of academic and personal advice in the IIES kitchen at the beginning of my PhD, and continued to do so over the years through Skype-calls and e-mails. Ethan Kaplan is the most generous assistant professor I have ever met, and the time he dedicated to me during my first year gave me an excellent first exposure to research, shaping the way I proceeded in my PhD. Olle Folke has always been up for discussing my ideas and "training" me for the job market.

To all these people goes a big "thank you"!

Every researcher at the IIES provided help to write my papers, through general conversations and specific comments during my presentations. In particular, I would like to thank Philippe Aghion and Peter Nilsson, for providing valuable inputs to my research; Per Krusell for valuable advice during the job market period; Tobias Broer for friendship, advice, and comments on my papers from a "macroeconomist's perspective"; and anyone in the "DSG group": our meetings were an excellent opportunity to discuss research in a friendly and relaxed environment.

I am grateful to the IIES administrative staff: Annika Andreasson, Åsa Storm, Astrid Wåke, Hanna Christiansson and Kalle Eriksson were always there when I asked for their help; Christina Lönnblad knows by herself when one needs help, and anticipates one's requests; my thanks to her also for excellent proof-reading of my papers.

Thanks to my MSc-thesis advisor and co-advisor, Alessandra Casarico and Paola Profeta, who contributed to this thesis by nurturing my interest in gender issues, and encouraging me to purse a PhD in economics.

There are people who contributed to this thesis through excellent comments on my papers, but even more by sharing great moments in Stockholm with me. At a time when my country still did things in its own way, I had already understood that we should do things the way the Germans recommend! Abel Shumann and Alex Schmitt were the best study-mates during my first year; working with them was tremendously important in completing my course work. But above all, they are the best friends I could have met. Abel, I will miss the act that we constantly play when we interact, pretending that we don't get along, whereas we like each other so much! Alex, I will miss your unique kindness, and the feeling of never being alone that I experienced in Stockholm, because you were always there, sharing so much of a phase of our life during which I feel that we both grew by influencing each other a lot. Thanks for your irreplaceable friendship, which I am sure will be a life-lasting one! Everybody else in my PhD year made the first two years very enjoyable, despite the problem-sets, my first experience with darkness at 3 pm and snow in March, and long-distance from people very important to me. My thanks to all of them. Thanks for their friendship to Elena Mattana, who sweetened some bitter days with muffins and cookies; Andrea Guariso, who gave me stomach ache from all the laughs together; Audinga Baltrunaite and Leda Pateli, the most reliable gym buddies ever, and a great company for never-ending dinners and discussions (invariantly around the themes of work-life balance, and health problems); Abdulaziz Shifa and Bei Qin, who shared the fatigues of the job market, always with positive attitude: we worked hard together and we put things in perspective together, and I am so glad that the result is that we will all be happy in our next destination; Nick Sheard, for his provision of entertainment and English lessons. Thanks to my office mates over the years, to Martin Berlin (incidentally, also my first and last teacher of Swedish language), Erik Prawitz and David Seim, with whom I shared the honor and the burden of being an RA, Ettore Panetti and Christina Håkanson, who along with me created the loudest, chattiest, and most visited office at the IIES, and to many more people with whom I shared nice moments over the years: Selva Baziki, Hamid Boustanifar, Nathan Lane, Maria Perrotta, Eric Sjöberg, David von Below and Daniel Spiro are an important but not exhaustive part of this list; special thanks to the badminton crowd and to the climbing crowd for keeping me in shape while I was having fun!

The year in Berkeley has been an unforgettable one, for many reasons, among which the inspiring company of a great group of people; Gioia de Melo and Juliane Parys deserve a special thanks, for the productive time we spent working together at cafés, and for being adorable friends.

There is a part of my life that has not been so visible during these years in Stockholm, but that is in fact behind everything I did and achieved so far. There are my friends back home, whom I

grew up with, learning the sense of being a community. The friends whom I met in Milan, the city that opened up my horizons, and that lit up my desire to know and experience as much as possible. There are my brother Alessandro and my sister-in-law Pamela, my grandparents, and everybody in my big family, that meets every year around a table a couple of times during the Christmas holidays, and the thought of how happy those meetings will be has been another way of dealing with the darkness in November. To all of you, thanks for always making me feel so loved and supported.

There are my parents, who would deserve a PhD for the great job that they did with my brother and me. Mum and dad, you directed and shaped me a great deal until a certain point, teaching me the values that make the person who I am now. Then, when you felt that I knew what was my road, you just stepped back, quietly, and let me do whatever I wanted, trusting me and supporting every decision I made; and you stepped in again to give me advice and direction only when I asked you to do so, and I am sure I will keep on asking for your advice and direction many more times in my life. Thanks for all this, this thesis is for the two of you.

My last thanks, because it is the most full of gratitude, goes to Michel. I find it funny that we both wrote a paper where we show that distance matters; whereas distance did not matter at all for us, because in all these years when we were kept apart by one ocean and one and half continents, I felt you were present in my research and in my life more than everybody else. I will not use many words to tell you how important your reassuring and stimulating presence has been for me to start and complete the PhD, to not give you yet another reason to tease me for being “barocca”. Let me just tell you that, in all these years, you have been my best ally in the enterprise of making the PhD, and above all life, as enjoyable as possible. Thank you clown!

Stockholm, April 2013
Pamela Campa

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Chapter 1

Introduction

This thesis deals with issues that I have been curious about for a long time. It consists of three self-contained essays at the intersection between different fields in economics.

The essays touch on three topics that constitute my research agenda. These are: the effect of mass-media on individual and collective behavior; the determinants of firms' environmental responsiveness; and how public policy can increase gender equality.

The unifying theme across these topics is mainly their source of inspiration. This is the place in Southern Italy where I come from (the heel of the boot).[1] I have always felt the need to explain the reasons for many sources of backwardness in Southern Italy, in order to be able to propose some solutions.

This place inspired my research with the sudden discovery in recent years that its beautiful sunny land and sea are heavily and deadly polluted. Toxic substances, hazardous waste, fumes and leaks have been poisoning the life of thousands of people for decades, and they still do. The map of incidence of cancer in Italy takes the deepest color in the most southern part of the the heel of the boot. Why did people who live, work and raise their children in these places allow this to happen? Why do firms in these territories threaten the environment and human life itself, while *The Economist* in 2008 reported that the general trend worldwide is of an increasing responsibility of firms toward society and the environment? Why do "principles pay" (Heal, 2008) in some places, while in some others they do not?

This question met my interest in media economics, a branch in the field of political economics that studies how mass-media shape decisions mainly made by voters and policy makers. This academic interest is, once more,intertwined with personal experience from a country, like Italy, whose press is classified by *Freedom House* as *Partly Free*.

[1] The cover picture of this thesis, not accidentally, is taken on the extreme eastern point of the heel. I thank my brother Alessandro Campa for fixing the beauty of the "estremo est" in this picture.

It has been shown that access to media and media coverage increase voters' turnout (Gentzkow, Shapiro, and Sinkinson, 2011) and the allocation of public money to informed voters (Stromberg, 2004), improves the selection of politicians (Besley and Prat, 2006; Snyder and Stromberg, 1969), and affects voting behavior (DellaVigna and Kaplan, 2007). A more limited body of literature (Dyck, Volchkova, and Zingales, 2008; Dyck and Zingales, 2002) has tried to establish whether there is also a media effect on other decision-makers, beyond voters and politicians, and in particular on firms.

In the first chapter of my thesis, "**Press and Leaks: Do Newspapers Reduce Toxic Emissions?**", I contribute to this literature, studying whether firms are more environmentally responsible when they are "watched" by newspapers that write, or threaten to write, articles about their environmental performance.

The context of my research is the US, rather than Southern Italy, due to data availability. I use data on toxic emissions from about 25,523 plants in 2001-2009 from the Toxic Release Inventory (TRI), which is the *Pollutant Release and Transfer Register* for the US and is produced by the Environmental Protection Agency. I combine this with data on the location of about 1,500 local newspapers, and data on the newspaper coverage of the TRI announcements of any plant that was ever among the top 20 polluters in its state. I first show that *Newspapers Density*, which is the number of newspapers in 20 mile rings around the plant, weighted by the inverse of their distance to the plant, raises the probability that a plant is given newspaper coverage. Then, I analyze the effect of *Newspapers Density* on toxic emissions. Comparing plants in the same industry group, county and year shows that plants in areas with a higher *Newspapers Density* have lower toxic emissions. An indication that the effect goes through consumer influence is that the effects are largest for plants in subsectors that mostly sell final goods. Moreover, the association of *Newspapers Density* with emissions is larger in counties that have experienced extreme negative health outcomes in the recent past, and where the consumer awareness is likely to be high, which suggests that the constituents pressure firms because of concerns about the health effects of toxic emissions. Under the necessary caution called upon by the limitations of the analysis, the improvement of which is left to future research, this paper shows that newspaper coverage induces firms to reduce their toxic emissions, by solving a problem of asymmetric information between firms and citizens directly affected by the emissions themselves. My research thus suggests that more information might improve the quality of the environment and health outcomes.[2]

Another aspect of life at the heel of the boot that has captured my interest is the dramatic level of gender inequality in the labor market and in politics.

[2]Papers in the economics literature and the epidemiological literature have documented detrimental health effects of the toxic substances that I study in my analysis.

Gender equality has become a goal of many governmental and non-governmental institutions in recent years, not only for fairness but also for economics arguments. As expressed in *The Global Gender Gap Report* (Hausmann, Tyson, and Zahidi, 2008), "the key for the future of any country and any institution is the capability to develop, retain and attract the best talent. Women make up one half of the world's human capital. Empowering and educating girls and women and leveraging their talent and leadership fully in the global economy, politics and society are thus fundamental elements of succeeding and prospering in an ever more competitive world. In particular, with talent shortages projected to become more severe in much of the developed and developing world, maximizing access to female talent is a strategic imperative for business".

What are the policy tools available to a society to increase its levels of gender equality?

In the second chapter of the thesis, "**Gender Quotas, Female Politicians and Public Expenditures: Quasi-Experimental Evidence**", I evaluate one of these tools, namely gender quotas. Gender quotas in politics are often adopted to improve women's opportunities to access legislative and executive positions, and to allow a better representation of the electorate in policy-making, because it is feared that female preferences are not adequately represented by male-dominated legislative bodies.[3] Spain offers a unique opportunity to evaluate the effects of gender quotas on women's access to politics and the representation of their preferences. This is because in 2007, gender quotas were adopted in municipal elections in Spain, but only in municipalities with more than 5,000 inhabitants. In these municipalities, women had to represent at least 40% of the candidates on the list, and also 40% of every five-position block. Then, I compare the evolution of outcomes of interest in municipalities above and below the 5,000 threshold to find that: 1) the gender quotas caused a 20% increase in the percentage of female elected councilors in Spanish municipalities, with respect to the average percentage of female councilors in previous elections; 2) interestingly, the percentage of female candidates increased more than the percentage of female councilors: this difference arises due to a strategic positioning of candidates, where women were systematically placed at the bottom of every five positions; for similar reasons, the gender quota did not significantly affect the share of female mayors; and 3) the gender quota had no effect on policy, measured as the size and composition of local budget.

The Spanish experience thus shows that gender quotas are not always a good answer to the demand for a better representation of women in politics, at least in the short run; they might be more effective under a different design that brings women to positions of real power.

Gender quotas, and any policy that puts an emphasis on gender equality, might also have long-run effects on people's attitude toward women and their role in society.

[3]The seminal work of Chattopadhyay and Duflo (2004) indeed shows that female politicians make decisions that are systematically different than those of men, and more aligned with the preferences of the female electorate.

In the third chapter of my thesis, "**Are Attitudes Endogenous to Political Regimes? Beliefs about Working Women in State-socialist Countries**", co-authored with Michel Serafinelli, we study whether a policy regime that puts emphasis on gender equality changes people's attitudes toward sex-roles in society. We compare the attitudes of individuals in Central and Eastern European countries (CEECs) and in the rest of Europe, before and after the advent of state socialism in CEECs. At the end of World War II, the CEECs under analysis were occupied by the Red Army. The Soviets retained a direct involvement in the internal affairs of these countries after the end of the war. Through a series of coalition governments including Communist parties, and then a forced liquidation of coalition members who were unliked by the Soviets, Stalinist systems were established in each country. Stalinists then gained control of existing governments, police, press and radio outlets in these countries. Between the late 1940s and the early 1990s, state-socialist governments strongly encouraged women's paid employment outside the home (de Haan, 2012). We exploit this background to study the influence of political regimes and social policies on individual attitudes about gender roles. We find suggestive evidence that the political and economic regime in state-socialist countries exerted a noticeable influence on people's beliefs about the appropriateness of a specialization of male and female roles.

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Chapter 2

Press and Leaks: Do Newspapers Reduce Toxic Emissions?*

1 Introduction

This paper investigates how media coverage shapes corporate decisions on environmental issues, above and beyond regulatory compliance. Anecdotal evidence suggests that the diffusion of information through newspapers may substantially influence corporate environmental decisions. For example, in 2003, toxic emissions at a Chevron oil refinery in Contra Costa county, in California, were nearly equal to one million pounds. Emissions in the same year at another Chevron oil refinery in Jackson County, in Mississippi, were slightly larger, all according to the data reported in the Toxic Release Inventory (TRI). The former oil refinery was the second biggest polluter in its county, the latter was the third biggest. Within two days from the publication of the TRI data, five articles were published in local newspapers in California, which featured the Chevron oil refinery in Contra Costa county as one of the top polluters in the Bay Area. Ten more articles covering toxic emissions at this plant were written in local newspapers between 2004 and 2008 in the three days following the release of TRI data. In comparison, there was no press coverage of toxic emissions at the refinery in Jackson county between 2003 and 2008. Emissions at the Chevron plant in

*I am indebted to David Strömberg for advice and guidance at different stages of this project. I am also grateful to Torsten Persson, Emilia Simeonova, Philippe Aghion, Audinga Baltrunaite, Konrad Burchardi, Andrea Guariso, Ruixue Jia, Peter Nilsson, Ettore Panetti, Alex Schmitt, Abel Schumann, Michel Serafinelli, Nicholas Sheard, Abdulaziz Shifa, Eric Sjöberg and Bei Qin for useful comments and suggestions. Timothy Antisdel provided generous help with the Toxic Release Inventory data, and Göran Alm with ArcGIS. I also thank seminar participants at the IIES, SITE, IFN, Universidad Carlos III, Stockholm School of Economics, Universidad de Alicante, University of Barcelona, Universitat Autonoma de Barcelona, Bank of Italy, University of Calgary, University of Alberta and New Economic School, and conference participants at IZA Summer School 2012, Sudswec 2012, Econometric Society Summer Meeting 2012, European Economic Association Meeting 2012 and European Association of Labor Economists Meeting 2012 for comments.

Contra Costa county declined by 50% between 2003 and 2009. Over the same period, emissions at the Jackson county plant went down by only 7%. The Contra Costa plant is located near several newspapers, whereas the Jackson plant has one only newspaper within a 20-mile radius.

To what extent can the differential exposure to newspapers explain the different speed at which emissions decreased at the two Chevron refineries between 2003 and 2009? Does the presence of newspapers nearby a plant make the plant accountable for its environmental performance, solving a problem of asymmetric information on pollution between firms and residents? Answering this question matters, given the effects of pollution on human health (Chay and Greenstone, 2003; Currie and Schmieder, 2009; Agarwal et al., 2010) and on cognitive abilities (Nilsson, 2009; Sanders, 2012).

In this paper, I show that the characteristics of the local newspaper market affect firms' decisions on legal toxic emissions. I first develop a simple model of firm accountability to consumers, mediated by newspaper coverage (accountability through investors' behavior is not modeled, but it is discussed in the implications of the model). Local consumers, the "constituents", gain utility from a firm's local production, through jobs and economic spillovers. However, they also bear the health and environmental costs of local industrial production. The local consumers do not directly observe the level of emissions from the plant, but may get this information from local newspapers. The higher the density of newspapers in the vicinity of the plant, the larger the probability of newspaper coverage. If a newspaper article reports high emissions, consumers decrease their demand for the good produced at the plant to minimize the health impact of local industrial production. The firm trades-off the expected loss of demand following bad press against the cost of using a clean production technology. Since a higher density of newspapers increases the expected loss it lowers the probability that the plant pollutes.

I test these hypotheses using data from the emissions of about 25,523 plants in 2001-2009 from the Toxic Release Inventory (TRI), which is the *Pollutant Release and Transfer Register* for the US, and which is produced by the Environmental Protection Agency. I combine this with data on the location of about 1,500 local newspapers, and data on the newspaper coverage of the TRI announcements of any plant that was ever among the top 20 polluters in its state.

I first analyze the determinants of newspaper coverage of emissions. I provide graphical evidence that the probability that a plant's emissions are featured in a nearby newspaper is approximately inverse to its distance from the newspaper. Total expected coverage in any newspaper is then approximately equal to what I call *Newspapers Density:* the number of newspapers in 20 mile rings around the plant, weighted by the inverse of their distance to the plant. Holding state and industry-subsector fixed, a higher *Newspapers Density* raises the probability that a plant is covered in newspapers located within its respective ring. To guard against any spurious correlation between *Newspapers Density* and a plant's newsworthiness, I control for coverage in newspapers

outside the rings, which should react to a plant's newsworthiness as much as those inside the rings. According to my estimates, if a plant were to move from the 25th to the 75th percentile of *Newspapers Density*, its probability of coverage would increase by 0.004. This is equal to nearly 17% of the average predicted probability of coverage in the sample.

I then analyze the effect of *Newspapers Density* on toxic emissions. Comparing plants in the same industry group, county and year shows that plants in areas with a higher *Newspapers Density* have lower toxic emissions. I show that this relationship is not explained by selection on observables and is unlikely to be explained by selection on unobservables (Altonji et al., 2005). According to my estimates, if a plant were to move from the 25th percentile of *Newspapers Density* to the 75th percentile, its emissions would be 5% lower. An indication that the effect goes through consumer influence is that the effects are largest for plants in subsectors that mostly sell final goods. Moreover, the association of *Newspapers Density* with emissions is larger in counties that have experienced extreme negative health outcomes in the recent past, and where consumer awareness is likely to be high. This is consistent with the model's assumption that the constituents pressure firms because of concerns about the health effects of toxic emissions.

There is no evidence that multi-plant firms move emissions from one plant to another, in response to high *Newspapers Density*. This has relevant implications for the interpretation of the estimates in aggregate terms, analogously to the discussion of leakage in response to carbon taxes.

I finally estimate the rise in aggregate toxic emissions if there were no newspapers close to any plant, so that *Newspapers Density* was zero for all plants. In this counterfactual scenario, aggregate emissions would be 3% higher. Moreover, the emissions from plants in industries that sell final goods would be 11% higher. Finally, emissions from plants located in areas that have experienced extreme infant mortality and mortality from cancer would be, respectively, 17% and 22% higher.

This paper contributes to the literature on the determinants of corporate social responsibility and, more specifically, of corporate environmentalism. Papers in this literature have analyzed both theoretically and empirically what motivates corporate pro-active behavior in terms of social and environmental outcomes (Arora and Cason, 1995; Hamilton, 1995; Konar and Cohen, 1997; Khanna and Damon, 1999; Hamilton, 1999; Maxwell et al., 2000; Harrington et al., 2008). According to Kitzmueller and Shimshack (2012), the empirical evidence points toward a prominent role of public politics and *private politics*.[1] However, the role of exposure to mass media in fostering pro-active behavior has not been investigated. This paper fills this gap, studying whether newspapers create incentives for firms to pollute less than their industry-group counterparts that are less exposed to press coverage, increasing the threat of public and private politics.[2]

[1] An instance of private politics occurs when a situation of conflict is resolved without reliance on the law or the government (Baron, 2001, 2003).

[2] Note that Bui and Mayer (2003) study newspapers and information on pollution, although with a different goal than that in this paper. Their goal is to measure the effect of disclosure of information on toxic emissions on housing

This paper also contributes to the literature on the effects of mass-media on policy and political outcomes (Prat and Strömberg, 2011) and on corporate decisions (Dyck et al., 2008; Guerrero, 2012). In this literature, Dyck and Zingales (2002) show that the cross-country variation in firms' environmental responsiveness is partly explained by the diffusion of the press. With respect to their study, I exploit within-country variation, using plant-level data on emissions, proximity to newspapers, and newspapers coverage of toxic emissions. I directly address the role of the geography of the newspaper market in shaping incentives for newspapers to cover firms, deriving implications on the type of policies that would increase citizens' information on corporate behavior.

The rest of the paper is organized as follows. Section 2 presents the conceptual framework. Section 5 describes the data used. Section 4 presents the estimation strategy and the empirical results. Section 5 investigates the existence of spillovers and computes aggregate effects. Section 6 concludes the paper and draws policy implications.

2 Conceptual Framework

I develop a simple model that studies why the density of newspapers near firms' plants induces firms to decrease toxic emissions. There are two types of agents: a continuum of firms that produce a locally demanded good and a group of consumers-residents (people who consume the local good and live in the vicinity of the firms' plants) that I will refer to as "constituents". In the interaction between a firm and its constituents, moral hazard arises because of an unobservable action, i.e. whether the firm's plant pollutes or not. Local newspapers can mitigate the moral hazard problem.

Each firm sells the locally demanded quantity y_l produced at a fixed cost c_j. The fixed cost varies with the production technology, which can be clean, e_c, or dirty, e_d. The clean technology is more expensive, $c_c > c_d$, but also produces a larger health and environmental cost, h, to the consumers:

$$h(y_l, e_c) = 0,$$
$$h(y_l, e_d) = \overline{h}(y_l) > 0,$$

where $\overline{h}$ increases with y_l, at an increasing rate. The firm decides whether to use the technology e_c or e_d with the objective of maximizing profits.

The constituents demand the quantity y_l. They act as a group.[3] Their utility of y_l is made up

prices. They use newspapers' readership to distinguish between the different degrees at which disclosure of information reaches citizens, depending on their "consumption" of news. Given that they find no effects of information disclosure on housing prices, both in areas with low and large newspapers' readership, they ultimately do not answer the question of the role of mass-media in informing citizens on environmental issues, and the potential implication for the behavior of firms that are exposed to media coverage.

[3] One way of thinking about the constituents acting as a group is to think, as in Aghion et al. (2012), of a rule set by

of three components. First, it is linearly increasing in the consumption of y that they can buy from local producers (y_l) and in the national market (y_{nl}); $y = y_l + y_{nl}$. Second, they get some direct utility from the presence of each local plant, $V(y_l)$, because of the creation of jobs and indirectly through economic spillovers. $V(y_l)$ is increasing in y_l at a decreasing rate. Third, they bear the health and environmental cost h, caused by the production of y_l. h can take the form of increased incidence of certain diseases, dirty local rivers or lakes, high level of dust in the neighborhood etc. The constituents do not observe h, because they do not observe what technology is adopted at the plants located in their neighborhood. They only see the aggregate environmental and health damage H, which can be caused by different plants as well as by mobile sources. Moreover, they know how the choice of technology affects their health, i.e. the function $h\left(y_l, e_j\right)$.

The constituents are endowed with wealth W. I assume that when y_l is equal to W, the marginal utility from the production of y_l is lower than its marginal cost when the dirty technology is used, so that:

Assumption 1 $V^{'}(W) < \overline{h}^{'}(W)$

This occurs when plants are located in relatively rich places, where the marginal value of job creation and economic growth is low, so that it is more than offset by the health and environmental cost of economic activity. I further assume that the value of job creation is higher than the health cost at zero production, $V(0) \geq h(0)$.

The constituents maximize their portion of utility of good y as follows:

$$\begin{aligned} \max_{y_l, y_{nl}} \quad & R = E\left[y_l + y_{nl} + V(y_l) - h(y_l, e_j)\right] \\ s.t. \quad & y_l + y_{nl} \leq W. \end{aligned}$$

The constituents first choose the quantity y^* that they want to consume and then how to allocate this demand between local and non local producers. Their problem involves setting up the optimal "demand strategy" of y_l, once y^* has been chosen. y_{nl} is then residually determined. Therefore, the constituents' problem boils down to maximizing the expected $V(y_l) - h(y_l, e_j)$ subject to the constraint that y_l must be between zero and W. If they observed e_j, they would adopt the following demand strategy:

$$y_l = \begin{cases} W & \text{if } e = e_c \\ \underline{y_l} < W & \text{if } e = e_d. \end{cases} \tag{1}$$

the individuals belonging to the group which, if followed by every group member, maximizes the utility of the group.

where

$$\underline{y_l} = argmax[V(y_l) - \overline{h}(y_l)].$$

See Proof in Appendix A.

However, the constituents cannot directly observe what technology is used at the local plant. They can only learn that the plant uses a dirty technology through reports featured in newspapers. I assume that if one newspaper publishes news about a local plant, all constituents become informed.[4] Even though, in reality, the constituents could access information on the type of technology used at the firm directly, in practice they seem unlikely to do so; collective action problems and ignorance of the availability of data on emissions are likely to explain this behavior.[5]

Newspapers access information on whether or not the plant uses a dirty technology. Then, they write a story, $s = s_d$, with a probability that depends on their location with respect to the plant. If the plant uses a clean technology, there is no coverage in newspapers, i.e. $s = \oslash$. There are N newspapers, located within a distance D to the plant,which face a demand for news about toxic emissions from the plant. I call these "relevant" newspapers.[6]

Let p_i denote the probability that a relevant newspaper i writes an article, given that the plant uses the dirty technology, $p_i = Pr(s_i = s_d|e_d)$. I assume that p_i falls with the distance d_i from the plant. Two aspects motivate this assumption: first, the likely increasing (in distance) cost of coverage, which includes the cost of traveling to the plant to get more information and interview the employees, and the utility cost for the journalist of writing an article;[7] second, the decreasing (in distance) demand for news about the plant, as readers further away from the plant are less affected by its toxic emissions. The probability p_i is assumed to be zero for non relevant newspapers, i.e. newspapers at a distance larger than D from the plant.

The probability P that any newspaper writes a story about the plant after the news $e = e_d$ is out

[4]This simply requires allowing constituents to communicate among themselves. Given that this communication could induce some moral hazard, possibly leading to a situation in which the constituents do not read newspapers, I assume that constituents read newspapers not only to become informed but also because they intrinsically enjoy reading news. Note that the results hold if only a share of constituents becomes informed.

[5]Analyzing the lessons that can be learned from the publication of data on toxic emissions in the US, Hamilton (2005) emphasizes the important role of information intermediaries, given the apparent unwillingness of citizens to gather the information on their own from the direct source of data dissemination (i.e. the Environmental Protection Agency's - EPA - website and publications). Similarly, commenting on the spread of TRI data in the years 1987-1992, Bui and Mayer (2003) observe that the primary source of TRI information for communities was not the raw data release, but rather the media accounts.

[6]Being within a certain distance to the plant makes a newspaper "relevant", because the constituents and the local newspaper's readers substantially overlap. The assumption that newspapers sell in an area nearby their headquarters is especially plausible for local newspapers. In Section 4, the empirical analysis focuses on the US, where local newspapers represent the largest share of the newspaper market.

[7]I assume that the utility cost of coverage is lower if the journalist has a direct interest in shaming a polluting plant, as might be the case if the journalist works and lives in the same area as the plant.

is equal to:

$$P = 1 - \prod_{i=1}^{N}(1 - p_i),$$

which, for very small p_i, can be approximated to:

$$P \approx \sum_{i=1}^{N} \frac{\partial P}{\partial p}\Delta p_i = \sum_{i=1}^{N} p_i = \sum_{i=1}^{N} p(d_i). \tag{2}$$

As in Besley and Prat (2006), three assumptions are implicit in this setup: news cannot be fabricated, only negative stories about a firm are news, and all newspapers have access to the same type of information. The second assumption could easily be relaxed by imposing that newspapers are more likely to write negative rather than positive stories, and the third assumption reflects exactly the type of information flow that I exploit in the empirical application in Section 4.[8]

2.1 Equilibrium

The constituents make their decision only based on s, choosing:

$$y_l^* = argmax[V(y_l) - (1 - P(e_c|s))\overline{h}(y_l)],$$

where the beliefs about the probability that the plant uses the clean technology are computed using Bayes' rule[9],

$$P(e_c|s) = \begin{cases} \approx Pr[(\bar{y}_l - \underline{y}_l)\sum_{i=1}^{N} p(d_i) \geq c_c - c_d] & \text{if } s = \oslash \\ 0 & \text{if } s = s_d. \end{cases} \tag{3}$$

Given these beliefs, the constituents' best response is

$$y_l^* = \begin{cases} \bar{y}_l > \underline{y}_l & \text{if } s = \oslash \\ \underline{y}_l & \text{if } s = s_d. \end{cases} \tag{4}$$

See the Proof in Appendix A. Given this best response, the firm faces expected profits equal to:

[8] In the empirical application, I look at emissions as reported in the EPA's administered Toxic Releases Inventory. This is a database with information on plant-level toxic emissions, which can be freely accessed through different media.

[9] See Appendix A for the derivation of the beliefs with the Bayes' rule; $Pr[(\bar{y}_l - \underline{y}_l)\sum_{i=1}^{N} p(d_i) \geq c_c - c_d]$ is the probability that in equilibrium a firm chooses not to pollute.

$$\pi_c = \bar{y}_l - e_c$$

and

$$\pi_d = (1-P)(\bar{y}_l - e_d) + P(\underline{y}_l - e_d).$$

Therefore, the firm decides what technology to use as follows:

$$e_j = \begin{cases} e_c & \text{if } (\bar{y}_l - \underline{y}_l)\sum_{i=1}^{N} p(d_i) > e_c - e_d \\ e_d & \text{otherwise.} \end{cases} \tag{5}$$

See Proof in Appendix A.

2.2 Testable Implications

In the model, I assume that the probability that a polluting plant is featured in a newspaper is decreasing with the distance of the plant to the newspaper. This implies that the plant's probability of coverage is increasing in $\sum_{i=1}^{N} p(d_i)$, where p is decreasing in d_i. In order to take this relationship to the data, I need to make a functional form assumption on p. In Figure 1, I use data on coverage of toxic emissions and the distance of plants from newspapers in the US, and I show that the relationship between the probability that a plant's toxic emissions are featured in a newspaper and its distance from the newspaper is approximately inverse.[10] Given this evidence, I specifically assume that

Assumption 2 $p_i = \frac{1}{d_i}$.

Assumption 2 and Equation 2 imply that the probability of news coverage for each plant equals

$$P \approx \sum_{i=1}^{N} \frac{1}{d_i} = \textit{Newspaper Density}, \tag{6}$$

where the last equality is a definition. Let $\eta = (\bar{y}_l - \underline{y}_l)/(e_c - e_d)$. We now have the following proposition.

Proposition 1 *If Newspaper Density* $\geq \eta$, *then plants do not pollute, and there is no coverage of toxic emissions. If Newspaper Density* $< \eta$, *then plants pollute and each plant gets featured in the news with probability approximately equal to Newspaper Density.*

[10] In Figure 1 the dots are means, and the line connects the fitted values from the regression of coverage on the inverse of distance.

I will test these implications in the empirical analysis in Section 4.2. Further, note that since plants only pollute when *Newspaper Density* is sufficiently low, the probability that coverage of a polluting plant arises must be close to zero:

Lemma 1 *In equilibrium, coverage of polluting plants is close to zero.*

Before concluding the discussion of the conceptual framework, it is worth emphasizing that, although I focused on pressures made by local consumers, the threat of *private politics* (Baron, 2001, 2003) that affects firms' decisions may come from national or international consumers. For example, local newspapers might "pull the alarm" for national newspapers: once local newspapers have met the initial cost of writing an article, the additional cost of reporting for other newspapers is arguably low; then the news might spread nationwide. Moreover, if consumers value the health of other citizens and environmental conditions at any location, local newspapers could provide input for boycotts that potentially spread nation-wide, or even internationally. Finally, other instances of private or public politics, such as massive sales of stock market shares, or lawsuits and pressures from the civil society for more stringent regulation, can also cause a reduction in expected profits, inducing the pro-active behavior modeled in this Section.[11][12]

[11] Heal (2008) reports that "*in the United States, in particular, most companies live in fear of lawsuits*" (pag. 21). He emphasizes that, since the passage of the Compensation and Liability Act in 1980, companies pay for the harm created by pollution, even if at the time at which the polluting behavior was put in act the company did not violate the law. Moreover, a search that I conducted on Internet documents about 20 lawsuits brought against companies whose emissions received press coverage, according to the data that I collected; these lawsuits were started between 1984 and 2012, and especially in the most recent years, by environmental organizations and citizens group. The ample involvement of environmental organizations in these lawsuits lends support to Baron (2003)'s hypothesis, that environmental organizations benefit from media coverage, because their arguments are deemed more credible when accompanied by newspapers articles about environmental pollution. The cases are often solved with a settlement, that prescribes either that the company installs polluting control technologies, or that it pays civil penalties. Interestingly, lawsuits are often against violation of limits set under the Clean Air Act or the Clean Water Act, which EPA fails to enforce.

[12] In an extension of the simple model presented in this Section, I allow for the possibility that firms "capture" newspapers. The strategic interaction between firms and newspapers is a variant of Besley and Prat (2006), modified to allow for the role of geography (i.e. distance) in determining coverage. As in the model presented in this Section, relevant newspapers have incentives to cover a plant because of their proximity to the plant. However, firms can capture these newspapers through advertisements and bribes (this is what Ellman and Germano (2009) call the "regulatory view" on advertising, as opposed to the "liberal view"; see Gambaro and Puglisi (2010) for empirical evidence on the "regulatory" role of firms' advertising in newspapers). Newspapers thus decide whether to cover a firm depending on their distance from the plant, conditional on not being captured. Distance plays a role in determining the probability that a relevant newspaper is captured through its effect on the cost of coverage: given that the firm has to compensate the newspapers for the forgone net revenues from not publishing the news, the lower the cost of coverage, the higher these foregone net revenues. Therefore, in this model, distance affects the probability of coverage through an additional channel. The firm captures the newspaper only if the cost of capture is lower than the cost of shifting to the clean technology. The cost of capture is increasing in the number of newspapers that are relevant for the firm, because, for the equilibrium of the bargaining game to be robust to deviations, each newspaper has to be compensated as if it were a monopolist in the market, i.e. as if its readers are all the constituents of the plant. The main difference between the extended model and the model presented in this Section is thus that, in the former, the number of relevant newspapers matters because the probability that the firm bribes the newspapers is decreasing in this number. This extension has

3 Data and Variables Description

Toxic Emissions I use data on toxic releases collected by EPA's TRI program. Starting in 1989, every year plants with more than 10 employees that operate in certain sectors (primarily in manufacturing) and that manufacture, process or otherwise use each of about 650 toxic substances above certain thresholds must report the quantity of each toxic substance released into the air, the water and the land.[13] Figure 2 shows the distribution on the US territory of plants reporting their toxic emissions to the TRI. Firms can update their data on emissions when they discover mistakes in previous reporting.[14] However, for the purpose of the current research, I use original data on emissions. These data were provided by EPA for the years 1996 to 2009. In 1998, there was a major change in the program, with new sectors being added among those that were required to report their emissions. Therefore, I limit my sample to plants observed in the years 1998-2009. I use information on total plant-level emissions, the industry in which a plant operates, and its exact geographic location (latitude and longitude).

This is an unbalanced panel; the number of plants is 25,523, for a total of 154,587 plant-by-year observations. All released amounts are reported in pounds except dioxin which is reported in grams. The mean amount of emissions is 164,481, with a standard deviation of 2,465,825.

A potential concern is that TRI data are self-reported. However, this is unlikely to be a major issue for several reasons. First, EPA's Office of Enforcement and Compliance Assistance (OECA), in conjunction with EPA's Regional Offices, conducts inspections and audits, and sanctions plants that misreport their toxic emissions.[15] OECA also queries big polluters and some facilities whose numbers change drastically over a short time period to check that the numbers are correct. EPA provides compliance incentives, i.e. "policies and programs that reduce or waive penalties under certain conditions for business, industry, and government facilities that voluntarily discover, promptly disclose, and expeditiously correct environmental problems".[16] Second, accounts in previous papers that use TRI data are reassuring on data quality. Currie and Schmieder (2009) quote several studies according to which general compliance in the TRI is high. Moreover, they find that reported emissions of TRI chemicals that are expected to impair birth outcomes do indeed have a negative effect on birth weight. Agarwal et al. (2010) point out that, although quality assurance

implications for the relationship between advertisement, pollution and newspaper market that are not testable with US data because of the lack of information on firm-newspaper specific advertisement relationships (and possibly bribes).

[13] If the quantity released is lower than 500 pounds, firms can choose not to report the quantity released, and will just submit a form that certifies that they manufacture some of the listed toxic substances; for these cases, emissions are set to zero. This exception does not hold for Persistent, Bioaccumulative and Toxic (PBT) chemicals.

[14] Updated data on emissions from 1989 to 2009 can be accessed on the EPA website.

[15] Different examples of plants that are fined for failure to report can be found at http://www.epa.gov/tri/stakeholders/enforcement/enforce.html

[16] See http://www.epa.gov/compliance/incentives/index.html. The other information on compliance and enforcement reported here has been acquired through visits to EPA's website and emails with EPA employees.

tests carried out by EPA annually on a very small sample of facilities (< 100) document a large degree of error, there is not currently any evidence of systematic over- or under-reporting; they thus argue that the measurement error is not correlated with their dependent variable, which is infant mortality. In the empirical analysis of this paper, toxic emissions are the dependent variable; therefore, if the measurement error is not correlated with the independent variable of interest, *Newspapers Density*, it would only increase the estimator's variance; if it is instead correlated with *Newspapers Density*, it would be a source of bias, in a direction that cannot be determined a priori. However, given the lack of evidence on systematic over- or under-reporting, and given that EPA inspects plants that have more incentives to under-report in response to high *Newspapers Density*, i.e. big polluters, I assume that the error is not correlated with *Newspapers Density*. Finally, Hamilton (2005) reports the results of an enforcement program launched in Minnesota in 1991 to identify facilities that should have reported. The facilities identified turned out to be very small polluters, and the reason for non compliance was ignorance rather than intentional concealment. Ignorance of the reporting requirements or of the correct estimation method is likely larger in the first years of implementation of the program, and therefore it is less of a concern in my analysis where I look at emissions reported between 2001 and 2009.[17]

Newspapers Density To measure plant level *Newspapers Density,* as defined in equation (6), I use a dataset reporting the name of almost all US newspapers with their city of location, the year they were founded and the year they were closed, if relevant.[18] This dataset is based on information published on the website *Chronicling America*. From the original collection, I extract the sample of English language daily newspapers. In total, there are about 1,500 newspapers every year in my sample. $NewspapersDensity_{pt}$ is computed as the sum of all newspapers within rings of 20 miles radius around plant p at time t, weighted by the inverse of their distance from the plant.

Newspaper Coverage I collected data on coverage of plants' toxic emissions in newspapers as follows. Among the plants that appear in the TRI dataset, I select those that were among the state top-20 polluters at least once in the period analyzed (i.e. 1998-2009). I look for articles covering these plants between 2000 and 2008. These plants reported emissions between 1998 and 2006, since TRI data are released by EPA with a two-year lag.[19] I use a program called Imacros to iteratively search Newslibrary, an archive of editions of a large number of US newspapers, for

[17] Emissions reported in the TRI need not be measured exactly but may be estimated.

[18] Unfortunately, more precise information on newspapers' location is not available; therefore I assume that a newspaper is located at the centroid of its city. While this certainly induces some error in the measure of distance, in a randomly selected sample of twenty newspapers whose location is searched in Google Maps, only three have a distance from the centroid (which presumably nearly coincides with the city center) larger than 1.5 miles, and of these only one is located in one of the three largest cities in this sample.

[19] I limit the search to plants that reported emissions between 1998 and 2006, because the data on emissions (and therefore the articles) are published two years after the reporting year until 2008, whereas in 2009 data for more than one year were released by EPA.

each plant-by-year observation.[20] The algorithm delivers articles published in newspapers within five days from the release of the TRI data, mentioning a short form of the name of the plant, the word EPA, and the name of the city or the county where the plant is located. Figure 3 in Appendix B shows an example of the output of this search. I manually verify that the articles identified cover the plant's yearly toxic emissions recorded in the TRI; hence, while undercounting may be an issue, overcounting is unlikely. I then check whether the articles selected are published in newspapers that are located within or outside the 20 miles rings around the plant. I finally construct the variable C_{psit}, which is a dummy taking the value of 1 if plant p in State s and 3-digit industry i is covered in year t in one of the newspapers in its ring.

Universities Density The variable *Universities Density* is constructed in the same way as *Newspapers Density*. I use data published by the US National Center for Education Statistics, listing all universities offering programs of four years, with the respective zip codes.[21]

Other controls The control variables that I use in the analysis are based on Census data. I download block-group data for each of the variables of interest.[22] Using Geographic Information Systems as ArcGIS and Geospatial Modeling Environment, I split the US territory into cells with areas equal to one square kilometer (≈ 0.39 square miles). Every cell gets the value of the census block group that has its maximum area in the cell itself. Then, I calculate the average across these cells in rings with a radius of five miles from the plant. I use Census data for the years 2000 and 2010, and I linearly interpolate the values in between.

I estimate the effect of *Newspapers Density* on coverage, C_{psit}, in states with some coverage of toxic emissions during the sample period, limiting the sample to plants that were among the state top-20 polluters at least once in the period analyzed. Summary statistics for this sample are shown in Table 2. Figure 4 in Appendix B shows the number of articles by state during the sample period (the names of no coverage states are in capital letters). Around 5.3% of the plant-by-year observations in the sample receive some coverage. Thus, coverage is a relatively rare event. The average number of newspapers in the rings is nearly 2, and, among the newspapers in the rings, the average distance from a plant is about 10 miles.

For the analysis of the effect of *Newspapers Density* on toxic emissions, I consider the entire

[20] Newslibrary archives articles from 4,508 titles. However, these are not all newspapers, as they include TV channels, radio channels, newswires and transcripts. After the articles have been extracted, I check which ones are published in newspapers, and I keep these.

[21] The list of universities is updated to the date at which I accessed the website of the *National Center for Education Statistics* (June 2012). Therefore, measurement error occurs if some universities were opened or closed in recent years. However, I assume that this is not a frequent event. For the purpose of the analysis in Section 4, the mere fact that a college opening or closure is approaching is sufficient to capture the characteristics of the location that I am interested in.

[22] A census block group is a cluster of census blocks that contains between 600 and 3,000 people, with an optimum size of 1,500 people.

sample of plants. The main dependent variable is total toxic emissions, Y_{pcit}, from plant p in county c and 4-digit industry i at time t. In different parts of the analysis, I distinguish between states where there is coverage ("coverage states"), and states where there is no coverage ("no coverage states"). Given that data on emissions are published with a two-year lag from the reporting year, and that I look at the effect of the one-year lag of *Newspapers Density*, in practice I analyze the determinants of emissions between 2001 and 2009.[23] Table 2 shows summary statistics for the estimation sample, distinguishing between "coverage states" and "no coverage states."

As shown in Tables 2 and 2, the distribution of the main variable of interest, *Newspapers Density*, is very skewed to the right. In the sample of state top-20 polluters, where I study the relationship between *Newspapers Density* and coverage, its mean is equal to 0.40, and its standard deviation is equal to 0.67. In the full sample, where I study the relationship between *Newspapers Density* and toxic emissions, its mean is equal to 0.52, and its standard deviation is equal to 0.80.

4 Results

4.1 Newspapers Density and Coverage

I first analyze how the probability that a plant is covered by any newspaper is affected by *Newspapers Density*. I test that implication of Proposition 1 by estimating the following equation:

$$C_{psit} = \alpha_0 + \alpha_1 NewspapersDensity_{pt} + \mu_{st} + \eta_{i3} + \varepsilon_{psit} \tag{7}$$

where C_{psit} is a dummy taking the value of 1 if plant p in State s and industry-subsector i is covered in year t, and φ_{st} and η_{i3} are, respectively, state-by-year and three-digit Primary NAICS (industry-subsector) fixed effects. The parameter estimates are reported in Table 3. *Newspapers Density* is positively correlated with the probability that a plant is covered in newspapers within its respective ring (Column 1).

However, within a state and an industry-subsector, plants that are located in areas with a high concentration of newspapers can be different from those in areas with a low concentration along several dimensions. For example, large plants could be both more well-known and "newsworthy" and located in areas with high *Newspapers Density*. I address this selection issue by separately studying how *Newspapers Density* correlates with coverage in newspapers inside and outside the 20-mile rings.

[23] I drop plants that do not have a match in the respective county of location in the year in which they are observed because I estimate a model with county-by-year effects; plants that do not have a match in the same industry-group (4-digit Primary NAIC), because I estimate industry-group effects; and plants located in counties whose area is above the 90th percentile of the sample distribution, where the heterogeneity across location with different levels of newspapers density is likely to be very large.

Figure 5 shows the newspaper market for plants in Oregon, US. The dots are plants that report to the TRI, the triangles are newspapers, the larger circles are the 20-mile rings in which I measure *Newspapers Density*, and the smaller circles are the rings in which I measure the control variables. *Bhelen Manufacturing Co*. has one newspaper within its ring, whereas *Ash Grove Cement Co.*, *Grant Western Lumber Co.* and *Praire Wood Prods.* have none. If *Bhelen Manufacturing Co* is more likely to be covered because it is more newsworthy, and newsworthiness correlates with *Newspapers Density*, then *Newspapers Density* should also be positively associated with coverage in newspapers outside the *Bhelen Manufacturing Co*'s ring. If *Newspapers Density* is not a proxy for newsworthiness, its effect on coverage in newspapers outside the rings should be smaller. This is indeed what is shown in Table 3, Column (2): the correlation between *Newspapers Density* and probability of coverage outside the rings is positive, but it is lower than the coefficient in Column (1), and not different than zero at conventional levels. [24]

A limitation of the coefficient estimate in Column (1) in Table 3 is that the probability of coverage in newspapers within the ring is mechanically zero for plants with *Newspapers Density* equal to zero. Thus, I restrict the sample to plants in rings where there is at least one newspaper. In Column (3) I show that the effect of *Newspapers Density* on the probability that the plant is covered in newspapers within its ring is not purely mechanical. Moreover, the estimate of α_1 is relatively stable when I control for a measure of plant's newsworthiness, i.e. a dummy for coverage in newspapers outside the rings (Column 4), and when I additionally control for a full set of demographics measured in the area around the plant (Column 5).[25]

According to the estimate in Column (5), if *Newspapers Density* increases by one unit, the probability of coverage of toxic emissions increases by 0.009. If a plant were to move from the twenty-fifth percentile of *Newspapers Density* (0.14) to the seventy-fifth percentile (0.59), then the probability of coverage in the relevant newspapers would increase by 0.004. This is equal to nearly 17% of the average predicted probability of coverage of toxic emissions in the sample. Although the overall probability of coverage of toxic emissions is low, this is a large effect.

[24]The positive association between $Pr(cov\ out)$ and *Newspapers Density* is not surprising because the newspapers within the ring may "pull the alarm" for the other newspapers, which will find it cheaper to write a story once the initial cost of writing an article has been met.

[25]The demographic controls are: log population density (linear and non-linear effect), log income (linear and non-linear effect), share of African-American, share with high school diploma or some college, share with associate diploma or more, share aged below 20, share aged above 65, unemployment in 2000, density of universities.

4.2 Newspapers Density and Toxic Emissions

I now turn to the main question, and test the implication of the model that plants located in areas with higher *Newspapers Density* pollute less. I estimate the following equation:

$$Y_{pcit} = \beta_0 + \beta_1 NewspapersDensity_{pt} + \beta_2 X_{pt} + \mu_{ct} + \eta_{i4} + \varepsilon_{pcit} \quad (8)$$

where Y_{pcit} are toxic emissions from plant p in county c and 4-digit industry i at time t, X_{pt} is a full set of controls measured at the plant level, μ_{ct} are county-by-year effects, η_{i4} are 4-digit Primary NAICS (industry-group) effects, and $NewspapersDensity$ is the measure described in Section 5. In practice, I compare plants that operate in the same county, industry-group and year.[26] In Figure 5, for instance, I would compare emissions at *Behlen Manufacturing* and at *Ash Grove Cement*, provided that they operate in the same industry-group.

I estimate equation (8) using plant-level data on toxic emissions in states where there was some coverage during the sample period. The distribution of emissions is highly skewed to the right, so I perform the analysis on log emissions. Given that emissions can be equal to 0, I add a constant equal to 1 to the original value of emissions.

The estimation results are shown in Table 4. Column (1) reports the baseline estimate of Equation (8). As predicted in Section 2, *Newspapers Density* is negatively correlated with toxic emissions.

The estimate in Column (1) is not necessarily causal. I now discuss robustness to the inclusion of more controls and to sample selection. In Section 4.3, I discuss this issue further.

In order to address selection, I restricted the sample to counties whose area is lower than the 90th percentile in the sample distribution.[27] Moreover, I control for the most likely correlates of emissions and *Newspapers Density*, using demographic variables measured near each plant. The selection of these controls is first of all guided by the historical account reported in Gentzkow et al. (2010), according to which population and income explain most of the variation in newspapers' entry and exit in the US. I thus control for the logarithms of population density (*Log Pop Density*) and of income per capita (*Log Income*) within a five-mile radius from the plant. I also allow for non-linear effects of these two variables, including dummies for realizations of log population density and log income within different percentiles. As shown in Column (2), the coefficient is still negative and statistically significant, although it decreases in magnitude.

Beside population density and income, other local characteristics might be correlated with emissions and *Newspapers Density*. First, emissions and *Newspapers Density* likely correlate with

[26] Notice that there is only a limited time-variation in *Newspapers Density* generated by newspapers entry and exit. Therefore, I do not exploit the panel dimension of the dataset.

[27] Locations within large counties are most likely to be different in ways that cannot be controlled for.

local preferences for environmental amenities. I proxy these local preferences with the share of people with tertiary education (*Education - associate or more*) in the five-mile ring around the plant. Different pieces of evidence suggests that *Education - associate or more* is a reasonable proxy for the unobserved variable.[28] The point estimate of β_1 is remarkably robust to including the proxy in the controls. The share of people with tertiary education is negatively associated with toxic emissions.

In Column (4) I control for a number of additional variables. The literature on environmental justice suggests an association between toxic emission and the share of minorities in a neighborhood of the plant (Bullard, 1990): thus, I control for the share of African-Americans living in the five-mile rings centered at the plant (*Share Black*). Moreover, people who live in the neighborhood of a plant gain utility from the creation of jobs and from the economic spill-overs associated with local production. Consequently, in a context of high unemployment, the trade-off between cost-effectiveness and environmental impact could be more easily resolved in favor of cost-effectiveness, so I control for the unemployment levels in the five-mile rings around a plant in 2000.[29] I also include two measures of the demographic composition of the population, *Share Younger 20* and *Share Older 65*, and the share of people with high school education and some college, *Education - high school or some college*, to account for the observation that a more educated workforce may make it easier to adopt a new clean technology.

The estimate of β_1 is stable to these additional controls. Using a measure developed by Altonji et al. (2005), and adapted to the continuous case by Bellows and Miguel (2009), I conclude that the selection on unobservables should be 2.5 times as large as the selection on observables in order to drive the point estimates for β_1 to zero. This is a large number, if one considers that I included the most powerful predictors of newspaper entry and exit, that the coefficient is remarkably stable to the inclusion of observables, and that I attempted to account for unobserved selection through a proxy variable approach.

The estimated effects are sizable. According to the estimates in Column (4), if we compare a plant that is five miles away from one newspaper, to one that is five miles away from three newspapers, the latter reports 3.8% lower toxic emissions. If a plant moved from the twenty-fifth percentile of *Newspapers Density* (0.14) to the seventy-fifth percentile (0.65), its emissions would

[28] The standard environmental Kuznets Curve argument, according to which the quality of the environment improves with income growth above a certain level of income, has been contested on the ground that income is correlated with other variables that play a role in shaping preferences for environmental outcomes, among which education (Bimonte, 2002). A cross-tabulation of responses of US residents to the World Value Survey shows that higher levels of education are associated with more willingness to take actions in favor of the environment (see Figures 6 - 8 in Appendix B). This is in line with other survey evidence, as in Fleishman-Hillard and the National Consumers League (2007), that "environmental preferences in a society strongly depend on demographic characteristics such as education or technological development" (Kitzmueller and Shimshack, 2012).

[29] Census-block-group data on unemployment are not available for recent years. In a robustness check in Section 4.3.4, I address the potential issue arising from mis-measurement of unemployment in the latest years.

be 5% lower.

In Section 5 I will discuss the implications of this effect in aggregate terms. While it might appear small, in Section 4.4.3 I show that this is the average of very heterogeneous effects across locations and industries, and this effect is much larger for some subsamples of plants. Moreover, accounting for industry-group effects, I might underestimate the effect of *Newspapers Density* on toxic emissions at a given locality, because I do not capture the effect of *Newspapers Density* on the location decision of firms in heavily polluting industry-groups.

4.3 Robustness Checks

The estimated coefficient on *Newspapers Density* in Column (4) of Table 4 is identified under the assumption that *Newspapers Density* is uncorrelated with the error term, conditional on the other included variables. This means that the controls should capture the county-year and industry-group differences that are correlated with emissions and *Newspapers Density*. The fact that the estimate of β_1 in Column (2) is robust to the introduction of additional controls suggests that this assumption is plausible. In this Section, I will provide further evidence to corroborate this claim using different approaches. I first perform two placebo tests. The first tests whether *Universities Density*, computed analogously to *Newspaper Density*, correlates with emissions. The second tests whether *Newspaper Density* correlates with emissions in states which have no actual emissions coverage. Then, I analyze the relationship between emissions and newspaper density using only within-firm variation. Finally, I analyze robustness to using different samples, measures of the dependent variable and estimation methods.

4.3.1 Universities Density and Toxic Emissions

Universities, like newspapers, are arguably more likely located in urban areas, where there are more residential amenities, and where people have better civic attitudes.[30] Therefore, if the negative association of *Newspapers Density* with toxic emissions is due to these non-controlled-for characteristics, a measure of *Universities Density*, constructed as the main independent variable *Newspapers Density*, should be correlated with toxic emissions, when county-by-year and industry-group variation is exploited, and all controls are included.[31] Using the specification in equation (8), but

[30] Dee (2004) argues that the presence of universities might improve civic attitudes not only by rising college attendance, but also by diffusing a "youth-oriented and politically aware culture that promotes the civic engagement of teens independently of its effects on educational attainment". He provides evidence of this spillover, showing, for instance, that "nearness to 4-year colleges is associated with increases in the probability of graduating from high school as well as significant increases in sophomore-year civics knowledge".

[31] The evidence provided in Dee (2004) is specific to universities awarding 4-years degree, versus 2-years colleges; therefore, I measure *Universities Density* only considering institutions that award 4-years degrees.

replacing *Newspapers Density* by *Universities Density*, I get

$$Y_{pcit} = \varphi_0 + \varphi_1 UniversitiesDensity_{pt} + \varphi_2 X_{pt} + \mu_{ct} + \eta_{i4} + \varepsilon_{pcit}. \quad (9)$$

According to the estimate in Table 5, we cannot reject the hypothesis that φ_1 is equal to zero, and the point estimate is substantially smaller than the estimates of β_1 shown in Table 4.[32] If many of the characteristics of a location associated with the presence of newspapers overlap with those associated with the presence of universities, this makes a good case for a causal effect of *Newspapers Density* on emissions.[33]

4.3.2 Newspapers Density and Toxic Emissions in States with no Coverage

Factors such as journalists' distaste for pollution or the occurrence of important events, such as local elections, at the date the EPA data are released, could drive the probability of coverage at some localities to zero, independently of the characteristics of the newspaper market. In these areas, there is no news coverage, no matter how close the newspapers are, and therefore closeness to a newspaper should not have any impact. On the other hand, if the correlation between *Newspaper Density* and pollution is driven by other factors, such as unobserved plant type, then the correlation would still be there also in these non-coverage areas.

I evaluate the identifying assumption estimating the effect of *Newspapers Density* for plants in states where newspapers never cover emissions. In the empirical analysis so far, I focused on states with some coverage during the sample period. However, as shown in Figure 4, for eight states the text search did not find any articles on toxic emissions published between 2000 and 2008.[34] In these states, there should not be any effect of *Newspapers Density* on toxic emissions, because of the lack of coverage and threat of coverage. I test this hypothesis by estimating Equation 8 in "no coverage" states. In Table 5, Column (2), I report the result of this estimation.

The effect of *Newspapers Density* in states where there is no coverage cannot be distinguished from zero. To guard against the possibility that there is not enough variation, in this smaller sample, to estimate a precise coefficient, I also estimate Equation (8) in "no coverage" states with industry sub-sector effects (3-digit Primary NAICS), using the entire sample of counties (i.e. not excluding

[32] According to the estimate of φ_1, if a plant were to move from the 25^{th} to the 75^{th} percentile of *Universities Density*, its toxic emissions would go down by 1%, and the effect is not precisely estimated.

[33] Please notice that the unconditional effect of *Universities Density* on toxic emissions is large and statistically significant; this suggests that the controls included account for selection on the characteristics associated with the presence of universities, which arguably partly overlap with those associated with the presence of newspapers.

[34] Coverage of toxic emissions in these states might be null because readers are not interested in environmental issues. However, only three of these states (i.e. Tennessee, Oklahoma and Mississippi) figure among the ten states with the lowest proportion of citizens organized in Greenpeace, the Sierra Club and the National Wildlife Federation in 1987 and in 2000, according to the account reported in List and Sturm (2006). This suggests that other factors, behind readers' interest in environmental issues, contribute to the lack of coverage in these states.

those with areas above the 90th percentile), or exploiting within state-year variation: also, in these regressions the effect of *Newspapers Density* in states where there is no coverage is virtually zero (results not shown and available upon request), although the large confidence intervals call for cautiousness in interpreting this result. Moreover, if I estimate the same regression as in Table 5, Column (2), limiting the sample to the first eight states for number of articles on toxic emissions in the period studied, the coefficient of *Newspapers Density*, although not significant, is very similar to that estimated on the full sample, being equal to -0.085. If I substitute county-by-year with state-by-year effects, to increase the amount of identifying variation available, the effect for these states is equal to that estimated in the full sample, and statistically significant. This lends support to the hypothesis that there is no clear association between *Newspapers Density* and toxic emissions in states with no coverage, versus the alternative hypothesis that there is not enough variation to estimate this effect in the reduced sample. If the former hypothesis is true, we can rule out an alternative channel of influence between the two variables. Namely, emissions may be lower in areas with larger *Newspapers Density* because non-polluting plants select into these locations, for reasons unrelated to press coverage. However, if this were the case then *Newspapers Density* should be negatively associated with toxic emissions also in states where TRI statistics are not covered in newspapers.

4.3.3 Within-firm variation

A possible concern is that the *Newspapers Density* of plant *p* is correlated with unmodeled characteristics of its parent company, which affect firm-level decisions on toxic emissions. Some of the big firms in the dataset, for example, could be more concerned about reputation because they have been targeted by activists in the past. Coca-Cola, to name one, might pollute less because of these reputation-related concerns, and might locate its plants in areas dense with newspapers so that it can more easily influence journalists with its public relation activities. Similarly, Harrington et al. (2008) find that different technical capabilities, size, and extent of operations explain heterogeneity across firms in environmental responsiveness. If these characteristics are correlated with *Newspapers Density*, this would bias the estimated effect of *Newspapers Density* on toxic emissions. In this Section, I try to address this concern by exploiting within-firm variation in *Newspapers Density* and toxic emissions.

Plants that report their toxic emissions to the TRI can be part of multi-plant firms. The TRI dataset records the parent company name and/or Dun and Bradstreet number (D&B number) for most of the plants in the estimation sample. I identify these plants as linked to the same firm if they are associated with the same parent company name or Dun and Bradstreet number.[35] 80% of the

[35]The match is mostly based on the parent company name, because, in an audit study conducted by D&B for EPA, the submitted TRI D&B numbers matched the D&B U.S Domestic Ultimate Parent Number only in 31% of cases

plants with parent company information are linked to a firm that has more than one plant reporting to the TRI. I use these plants to estimate a variant of Equation (4), which includes firm-by-year fixed-effects (μ_{ft}):

$$Y_{pcit} = \pi_0 + \pi_1 NewspapersDensity + \pi_2 X_{pt} + \mu_{ft} + \eta_{i4} + \varepsilon_{pcit} \quad (10)$$

Table 5, Column (3) shows the point estimate for π_1 in Equation (10). This is the effect of newspapers density on toxic emissions, when comparing plants operated by the same firm in the same 4-digit industry and year. The coefficient is negative, although the standard errors are larger than in the specification with county-by-year effects, due to the smaller sample size. In Column (4), I exclude plants linked to firms that only count two observations in a given year. The effect of *Newspapers Density* for the remaining sample is larger, very similar to that shown in Table 4, Column (4), and marginally significant. The point estimate for π_1 does, in fact, increase as the sample is restricted to larger and larger firms (result not shown).[36] This suggests that the effect of *Newspapers Density* on toxic emissions is increasing with the size of the parent company. Different hypotheses are consistent with this finding: larger firms are more exposed to reputation-related concerns, they are more resourceful and thus more capable of investing in corporate environmentalism, and could reduce emissions at a lower cost, due to economies of scale in the adoption of new technologies. Overall, these results indicate that *Newspapers Density* explains variation in emissions also when we compare plants operated by the same firm, thus addressing the concern that the effect estimated in Section 4.2 is driven by different location decisions across firms.[37]

4.3.4 Additional Robustness Checks

I show that the estimates presented in Table 4 are robust to certain departures from the main sample and estimation method.

Table 6 Column (1) shows that the estimated effect of *Newspapers Density* on toxic emissions is not driven by the higher concentration of newspapers in big cities compared to other areas in the same county. When I exclude the twenty-five most populated cities in the sample, the coefficient

(information provided by EPA employee through email communication). Matching on the parent company name has also some issues, because several variants of a name can be used (for instance, ARCELORMITTAL INC and ARCELORMITTAL USA INC). In order to mitigate this problem, I match firms with different names using a Stata command, *strgroup*, that implements the *Levenshtein algorithm*. However, I set a quite conservative threshold, thus matching only names that are highly similar, and therefore the issue of poor matching partly remains. Given this, the results in this Section, although suggestive, should be interpreted with some caution.

[36] I assume that firms that list more plants in the TRI dataset are larger.

[37] The low precision of the estimates is not surprising, given the noise in the data created by the difficulty in identifying plants linked to the same firm using the parent company name.

is almost the same as that estimated on the main estimation sample.

Another concern is that the bulk of the effect comes from the comparison between plants that are not surrounded by any newspapers and those that are surrounded by some newspapers. The conceptual framework and the analysis in Section 4.1 show that in areas with at least one newspaper, coverage is increasing in the number of newspapers and in their respective proximity to the plant. If *Newspapers Density* affects emissions through coverage, we should then expect this to be true also when comparing plants that have at least one newspaper in their 20-mile rings. This is indeed shown in Column (2) of Table 6.

In estimating equation (8), I use as the dependent variable the natural logarithm of toxic emissions because the distribution of emissions is highly skewed to the right. This has the drawback that I have to add a constant equal to 1 to the original value of emissions due to the presence of plants that have emissions equal to 0. In Column (3) of Table 6, I show that *Newspapers Density* is negatively associated with *log Emissions* when I exclude the observations that have zero emissions and therefore do not have to perform any transformation on the dependent variable. According to the point estimate in Column (3), a one unit increase in *Newspapers Density* would reduce emissions by 6%.[38]

As pointed out in Section 5, when the quantity of a toxic substance released is lower than 500 pounds, firms can choose not to report the quantity released and instead submit a form that certifies that they manufacture some of the listed toxic substances; for these cases, emissions are set to zero. I choose to treat these values as zeros in the main specification because I study the effect of *Newspaper Density* on reported toxic emissions, as they are presented to the public; emissions being a continuous variable, I run a standard OLS regression. However, I also estimate a model that takes into account the fact that whenever the plant reported what is called the "Form A," i.e. a form that only certifies that emissions of a particular substance are below 500 pounds, the quantity actually released could be any value in the interval [0,499]. I thus run an interval regression, using the lower and upper bounds implied by this reporting technique.

I define the true value of emissions as y^*_{pt}. The observed value is:

$$y_{pt} = 0 \text{ if } 0 \leq y^*_{pt} < k,\ y_{pt} = y^*_{pt} \text{ if } y^*_{pt} \geq k$$

Assuming that $y^*|x \sim \mathcal{N}(x\beta, \sigma^2)$, where $\sigma^2 = Var(y^*|x)$ is assumed to be independent of x, the parameters β and σ can be estimated by maximum likelihood, defining the likelihood as follows (Wooldridge, 1995):

[38]The effect estimated in this sample is, not surprisingly, smaller than that estimated on the full sample: the effect of *Newspapers Density* on the decision to not emit toxic substances is not captured in this specification.

$$\log[L(\beta,\sigma)] = \sum_i (1[y_i \geq k])\, log\left\{\sigma^{-1}\phi\left[\frac{(Y_i - X_i\gamma)}{\sigma}\right]\right\} + (1[0 \geq y_i > k])\, log\left\{\Phi\left[\frac{(k - X_i\gamma)}{\sigma}\right] - \Phi\left[\frac{-X_i\gamma}{\sigma}\right]\right\}$$

Due to computational limitations, I estimate a model with county and year fixed effects rather than with county-by-year effects, and 3-digit industry effects rather than 4-digit industry effects. The estimated coefficient is reported in Table 6, Column (4). The point estimate of β_1 is very close to that obtained with the OLS regression.

Given that unemployment is not necessarily persistent over time and that substantial economic shocks could quickly change the economic landscape of a location, the unemployment level in 2000 might not be a good measure for unemployment in recent years. Therefore, I first run the regression estimated in Table 4, Column (4) without including the control for *Unemployment 2000* and limiting the sample to the years 2001-2004. The point estimate for β_1 in equation (8) is -0.084, and it is significant at the 5% level. Then, I run this regression on the same sample, adding a control for unemployment in 2000. As shown in Column (5), the coefficient is stable to the introduction of this further control, even in this sub-sample for which *Unemployment 2000* should be a good measure of the actual level of unemployment.

Finally, in Column (6) I drop from the sample all plants that are located at a linear distance of five or less miles from the border with Canada or Mexico or from the coastline. The independent variables are measured with error for these plants because Census-block-group data are not available for a portion of their ring. If this error is correlated with *Newspapers Density*, the estimated effect of *Newspapers Density* will be downward biased. In Column (6) I show that, excluding these plants, the effect of *Newspapers Density* is larger, but broadly similar to that estimated in the full sample.

4.4 Mechanism

To shed light on the mechanism behind the effect of *Newspapers Density* on toxic emissions, I now analyze heterogeneous effects. I first investigate whether *Newspapers Density* affects more emissions of substances that have strong documented negative effects on humans, which should be the case if the effects go through some public reaction to negative health outcomes (rather than say a correlation between unobserved plant type and *Newspapers Density*). Next, I analyze whether emissions from firms producing final goods respond more to higher *Newspapers Density*, consistently with the model of response to potential consumer reactions. I finally check whether plants in communities that have a prior history of negative health outcomes, and that for this reason are

probably more aware of the problems associated with pollution, react more to higher *Newspapers Density*; this should be the case if the effects go through health concerns and are driven by locals.

4.4.1 Substances with strong health effects

In this Section I focus on *Newspapers Density* and emissions of two substances, dioxin (and dioxin-like compounds) and lead. The TRI is a register of emissions of 650 toxic substances that have different levels of danger for human health and for the environment. Among these substances, EPA pays special attention to the so-called Persistent and Bioaccumulative Substances (PBTs) because they tend to persist in the environment and to bioaccumulate into food chains. Dioxin and lead belong to this category. The reason for focusing on these substances is that their negative effects on humans and the environment have been extensively analyzed, so that it is not implausible that journalists and/or readers are more aware of the danger they pose.[39]

I analyze how coverage and emissions react to *Newspapers Density*, controlling for total plant-level emissions, by estimating the following regressions:

$$C_{psit} = \theta_0 + \theta_1 \text{PBT}_{p,t-2} + \theta_2 emissions_{p,t-2} + \theta_3 NewspapersDensity_{pt} + \theta_4 X_{pt} + \mu_{st} + \eta_{i2} + \varepsilon_{psit}, \tag{11}$$

$$\text{PBT}_{pcit} = \vartheta_0 + \vartheta_1 NewspapersDensity_{pt} + \vartheta_2 emissions_{pt} + \vartheta_3 X_{pt} + \mu_{ct} + \eta_{i4} + \varepsilon_{pcit}, \tag{12}$$

where *PBT* is an index for emissions of one of the PBT's substances on which I focus, and *emissions* is the total level of emissions reported by the plant.[40] [41] If the correlation between emissions and *Newspapers Density* arises because of newspaper coverage, a positive estimate of θ_1 should be associated with a negative estimate of ϑ_1.

As shown in Columns (1) and (2) of Table 7, higher emissions of dioxin trigger coverage,

[39] In the epidemiological literature, many studies on the health effects of dioxin exploit the "natural experiment" created by the Seveso disaster in Italy in 1976. The most recent of these studies (Pesatori et al., 2009) confirmed an excess risk of lymphatic and hematopoietic tissue neoplasms in the most exposed zones, and uncovered a high risk of breast cancer in women contaminated 15 years since the accident. Previous studies established a relationship between exposure to dioxin and, among others, chloracne (Caramaschi et al., 1981), diabetes (Pesatori et al., 1998) and sex ratio (Mocarelli et al., 2000). In the economics literature, Nilsson (2009) shows that early exposure to lead impairs scholastic performance, cognitive abilities and labor market outcomes.

[40] The index takes the value of 0 if the plant-level emissions of the PBT substance are 0, and a number from 1 to 4 depending on which quartile of the positive part of the sample distribution the plant's emissions belong to. The choice of the index is motivated by the fact that the percentage of plants having non-zero emissions of dioxin and lead is very low (5% and 12%, respectively, in the full sample). The categoric variable is used instead of a dummy for emissions above zero to exploit the information from the few plants that emit relatively high quantities of these substances.

[41] The second lag of emissions is relevant for coverage at time t, because, as already explained, data on emissions are published with a two-year lag.

whereas higher emissions of lead do not, controlling for total plant-level emissions. If firms are worried about negative coverage when located in areas with a high concentration of newspapers, the level of dioxin that they emit should then respond to *Newspapers Density* more than the level of lead.

As shown in Table 7, Columns (3) and (4), emissions of dioxin are higher in areas with less *Newspapers Density*, and this negative association does not hold when the dependent variable measures emissions of lead. Although silent on the reasons for which dioxin triggers coverage in newspapers while lead does not, this test lends further support to the thesis that the association between the density of newspapers and toxic emissions is caused by actual and potential coverage of pollution statistics in nearby newspapers.

4.4.2 Consumer Pressure

In the model, when faced with a high concentration of newspapers at their plants' locations, firms have incentives to adopt good environmental practices because they want to avoid drops in demand due to bad press. Gupta and Innes (2011) show that high emitters of toxic substances are more likely to be targeted for a boycott, and that firms are more likely to adopt an environmental management system and an environmental protocol after being subjected to a boycott. Innes and Sam (2008) show that the threat of boycott is positively associated with corporate environmentalism.

Firms in certain industries are more exposed to boycotts than others. Gupta and Innes (2011), for instance, conjecture that firms that sell final goods are more likely to be targeted for a boycott.[42] If newspapers play a substantial role in giving (or threatening to give) relevant information to organized groups of consumers, then the effect of *Newspapers Density* should be larger for firms that sell final goods. In Table 13 I test for this implication by estimating the following equation:

$$\begin{aligned} Y_{pt} = {} & \lambda_0 + \lambda_1 NewspapersDensity_{pt} + \lambda_2 X_{pt} + \lambda_3 NewspapersDensity_{pt} * Final_{i3} \\ & + \mu_{ct} + \eta_{i4} + \varepsilon_{pt} \end{aligned} \tag{13}$$

In practice, I test whether the effect of *Newspapers Density* estimated in Table 4 is larger for plants in industry-subsectors that mainly sell final versus intermediate goods.

I construct the variable *Final* as follows: I look at the 6-digit Primary Naics included in each industry-subsector and classify a subsector as mainly selling final goods if most of its 6-digit Pri-

[42] Although in their data they only find a positive but not significant effect of selling final goods on the probability that a firm is the target of a boycott, Gupta and Innes (2011) do not interpret the result as evidence that selling final goods has no effect on the probability of boycott; they rather think that the insignificant effect is due to the introduction of industry controls, that remove much of the available variation, and to the limitations of their main measure of environmental constituency.

mary Naics sell final goods. Table 8 lists the subsectors classified as selling final goods and those classified as selling intermediate goods.[43] To make some examples of firms in the category "Final", the industry "Utilities" lists companies as American Electric Power and Edison, "Food Manufacturing" lists companies as Kellogg and Kraft, "Beverage and Tobacco Product Manufacturing" includes Coca-Cola and Pepsi. These are all companies with high proximity to consumers.

In Column (1) of Table 9 I show that the effect of *Newspapers Density* on emissions is significantly larger in subsectors that mostly sell final goods.[44] In fact, most of the effect of *Newspapers-Density* shown in Table 4, Column (4), seems to be estimated in these subsectors. This result is robust to excluding subsectors whose composition of Primary Naics in terms of type of good sold is very heterogeneous (result not shown but available upon request).[45]

4.4.3 Local Health Salience

I now test whether firms react more to *Newspapers Density* in places that are more vulnerable to adverse health shocks. The model in Section 2 is based on the assumption that the constituents observe H, i.e. the aggregate environmental and health damage at their place of residence, but they do not know the contribution of each local plant to h. Press coverage of pollution can fill this informational asymmetry. Therefore, the larger is H the more likely it is that constituents both ask for information about toxic emissions and respond to coverage taking action against the firm. In both cases, if the firm also knows H, the presence of newspapers nearby the plant creates larger incentives to cut emissions when the health and environmental outcomes in the area around the plant are more negative.

Given the unavailability of data on local health outcomes, I use county-level data, provided by the *US Department of Health and Human Services* in the *Area Resource Files*. I test the hypothesis that the effect of *Newspapers Density* is larger in counties where there are extreme negative health outcomes by estimating the following equation:

$$Y_{pcit} = \delta_0 + \delta_1 NewspapersDensity_{pt} + \delta_2 X_{pt} + \delta_3 NewspapersDensity_{pt} * \widetilde{H}_c + \mu_{ct} + \eta_{i3} + \varepsilon_{pcit} \tag{14}$$

[43] I excluded plants in the "Public Administration" sector.

[44] Note that the main effect of producing final goods is absorbed by the 4-digit industry fixed effects.

[45] Note that the effect of *Newspapers Density* on toxic emissions could be larger in subsectors selling final goods for a reason different than consumer pressure; namely, plants that are "compliers", i.e. that change their emissions because of *Newspapers Density*, are mostly in subsectors selling final goods. The "compliers" are most likely plants in industries that are traditionally highly polluting. However, while mean emission are slightly larger in the category "final goods", the standard deviation and the 99th percentile of the distribution of emissions is larger in the sample of plants selling intermediate goods; in fact, log emissions are larger in the sample of plants selling intermediate goods. This suggests that there are also "compliers" in this sample.

$\widetilde{H}_c$ is a dummy for extreme negative health outcome, equal to 1 if the level of infant mortality and or mortality from cancer is larger than the 99th percentile in the respective cross-county distributions.[46] I choose to measure the extreme negative health outcome with this dummy because I intend to capture a situation of "alarm" that is most likely perceived by people living in the county. Given this definition, relatively few plants are located in counties that are classified as having experienced extreme health outcomes (see Figure 9). For this reason, I control for industry-subsector, rather than industry-group, fixed effects, to increase the total amount of variation used to estimate δ_3.

The estimates of δ_3 in Table 9 show a pattern that is consistent across the two different measures of $\widetilde{H}_c$. As hypothesized, the effect of *Newspapers Density* on toxic emissions is larger in counties that have experienced some extreme negative health outcomes in the recent past. In light of the theoretical and empirical results presented in this paper, this suggests that polluting firms may face a higher risk from proximity to newspapers where there is a "health alarm", because the "threat of coverage" may be larger and also, holding the "threat of coverage" constant, because the constituents might be more likely to take initiatives against firms spotted as big polluters.[47] [48]

5 Spillovers and Aggregate Effects

The aggregate effect of newspapers on emissions depends crucially on the degree to which multi-plant firms move emissions from one plant to another, in response to high *Newspapers Density*. Emissions at different plants are different inputs, whose cost depends on the level of *Newspapers-Density* of each plant, and the amount of input needed to produce a certain level of output varies with the technology used (clean or dirty). Depending on the degree of substitutability between the

[46] *Infant Mortality* is the number of deaths of infants aged below one per 1,000 live births; *Mortality from Cancer* is the share of deaths caused by cancer. Figure 9 in Appendix B shows the distributions of *Infant Mortality* and *Mortality by Cancer* in the estimation sample.

[47] A concern could be that having extreme health outcomes is correlated with other characteristics of the county that amplify the effect of *Newspapers Density* on toxic emissions. However, the data seem to reject this interpretation. Extreme levels of infant mortality do not overlap with extreme levels of mortality from cancer. Counties with extreme levels of infant mortality appear to be poorer, with lower education and higher unemployment in 2004, but these differences do not hold for counties with extreme mortality from cancer, suggesting that these characteristics are unlikely to explain the larger effect of *Newspapers Density* on toxic emissions.

[48] An example of the interaction between press coverage and alarming health statistics is provided by the following account, which is extracted from the communication of a lawsuit against a polluting firm, given by Bell Legal Group on its website: *Mounting concern over ongoing toxic emissions, water contamination, and noxious odor pollution, coupled with a wave of recently published negative health statistics, triggered Georgetown citizens to question the safety of the facility's operating practices and to demand accountability for environmental abuse. The public outcry began when a study using published EPA data, along with accepted air modeling techniques, brought national attention to the poor air quality surrounding several Georgetown schools in 2008. That sparked numerous reports by local media sources, spotlighting South Carolina Department of Health and Environmental Control (SCDHEC) data showing elevated asthma and cancer rates in Georgetown County* (http://www.belllegalgroup.com/media-spotlight/media-coverage-firm-news/bell-legal-group-files-suit-against-international-paper-ip/).

two inputs, the firm might find it convenient to shift part of the production from a place where the input is more costly (i.e. with large *Newspapers Density*) to one where it is less costly (i.e. with low *Newspapers Density*), or rather to change the technology used where emissions are more costly.

I check for the existence of substitution patterns by regressing emissions on *Newspapers Density* and the aggregate *Newspapers Density* of all other plants belonging to the same firm and 4-digit industry:

$$\begin{aligned} Y_{pcit} =& \sigma_0 + \sigma_1 NewspapersDensity_{pt} + \sigma_2 X_{pt} + \sigma_3 NewspapersDensityFirm_{pt} + \\ & \mu_{ct} + \eta_{i4} + \varepsilon_{pt} \end{aligned} \tag{15}$$

where $NewspapersDensityFirm_{pt}$ is defined as follows. Let me call a *group* the set of plants linked to the same firm and that operate in the same 4-digit industry. Indexing by p' any plant in the same *group* as plant p, I define:

$$NewspapersDensityFirm_{pt} = \sum_{p' \neq p} NewspapersDensity_{p't}$$

This is a measure of the cost of emissions at the other plants in the same firm and industry-group as plant p. Thus, I implicitly assume that firms have an opportunity to substitute emissions if they have more than one plant in the same industry-group. If a firm manages plants operating in different industry-groups, then the mechanism of substitution described above is unlikely to hold, because it is unlikely that the firm treats emissions in different industry-groups as substitutes. In estimating Equation 15, I limit the sample to plants belonging to a *group* that has at least two elements. If a firm substitutes emissions across plants, depending on the relative levels of *Newspapers Density*, then σ_3 should be positive.

Moreover, if a firm shifts a part of its emissions to the plant with the minimum relative cost of pollution, i.e. with the minimum realization of *Newspapers Density*, then emissions should be increasing in a dummy (*Min Density*) that takes the value of one if plant p has the lowest level of *Newspapers Density* across the other plants in its *group*. Having the minimum value of *Newspapers Density* is correlated with the number of plants in the same *group*, which might also be correlated with emissions, and therefore this variable needs to be controlled for. I test these hypotheses as follows:

$$Y_{pcit} = \rho_0 + \rho_1 NewspapersDensity_{pt} + \rho_2 X_{pt} + \rho_3 MinDensity_{pt} + \rho_4 Nplants_{fit} + \mu_{ct} + \eta_{i4} + \varepsilon_{pcit} \quad (16)$$

where

$$MinDensity_{pt} = 1 \quad if \quad NewspapersDensity_{pt} = min(NewspapersDensity_{pfit})$$

and $Nplants$ is the number of plants in the same *group* as plant p. The average value of $MinDensity_{pt}$ in the sample is 0.12.

In equations (15) and (16), I exploit variation across plants in the same county and industry-group. Thus, I answer the following question: when we compare plants in the same county, industry-group and year, and once *Newspapers Density* and other characteristics of the area where the plant is located are accounted for, does the relative level of *Newspapers Density* at plant p, with respect to other plants in the same *group*, explain its toxic emissions?

Table 10, Columns (1) and (2), provide no evidence for the existence of substitution patterns within firms. The estimates of σ_3 and ρ_3 are highly imprecise and negative. The coefficient of *Newspapers Density* is the same as in previous specifications, although not precisely estimated due to the smaller sample size. The lack of within-firm substitution has important implications for computing the aggregate effect of newspapers on total emissions. In order to compute these aggregate effects, I conduct a counterfactual analysis, based on that in Yanagizawa-Drott (2010).

I use different estimates from the previous sections: the effect of *Newspapers Density* on toxic emissions estimated in equation (4), the effect of *Newspapers Density* on toxic emissions for plants in industries that sell final goods estimated in equation (13), and the effect of *Newspapers Density* on toxic emissions in counties that have experienced extreme negative health outcomes in the recent past estimated in equation (14). As in Yanagizawa-Drott (2010), I define the notion of counterfactual outcome y_c as the outcome that we would observe had *Newspapers Density* been zero for any observation in the sample. For example, this means that in equation (4),

$$y_c = exp(ln(y_{pt} + 1) - \beta_1 NewspapersDensity_{pt}) - 1 \quad (17)$$

To account for the variance in the estimated parameters, I first generate a normal random variable for each coefficient of interest, with a mean equal to the point estimate and a standard deviation equal to the standard error. [49] Then, I extract a value from these distributions for each coefficient

[49] The point estimates in the interaction models are given by the linear combination of the point estimate for *News-*

of interest and I calculate the y_c's. Summing over all the y_c's in the sample of interest I get Y_c, which is the aggregate counterfactual. This is a measure of what the total level of toxic emissions would be in the sample of interest had *Newspapers Density* been zero for any observation. For each coefficient of interest, I repeat this procedure 500 times, deriving a mean and a standard deviation for Y_c. Comparing the mean of Y_c and the true aggregate emissions Y in the sample of interest, we can interpret the estimates in this paper in aggregate terms.

The results of this exercise are reported in Table 11. In the full sample, the total lower emissions accounted for by *Newspapers Density* are 3% of the actual emissions. Once we focus on samples where we expect the effect to bite the most, newspapers seem to have an economically important impact on the environmental outcome studied. Without any newspapers in the 20-mile radius around the plants, toxic emissions from plants that produce final goods would have been 11% larger, toxic emissions in counties with extreme infant mortality in the recent past would have been 17% larger, and toxic emissions in counties with extreme mortality from cancer would have been 22% larger.

6 Conclusion and Policy Implications

This paper assesses how newspapers induce firms to reduce toxic emissions at their plants. I show that the probability that a newspaper covers a plant's toxic emissions falls approximately inversely to its distance from the plant. Specifically, using newly-collected data on the coverage of toxic emissions in US newspapers, I show that the higher is a plant's *Newspapers Density*, the more likely it is that some articles about its toxic emissions are written in nearby newspapers. A simple model predicts that plants located in areas with a higher *Newspapers Density* would pollute less. The empirical evidence in this paper is consistent with this prediction: if a plant moved from the 25th percentile of *Newspapers Density* to the 75th percentile, its toxic emissions would be 5% lower.

I show that this association is unlikely to be explained by selection on unobservables, and that it is only valid in states where some coverage of toxic emissions has been observed during the sample period. Moreover, this effect is larger in industries mainly selling final goods, revealing an interaction of newspaper coverage and consumer pressure in shaping corporate environmental decisions. The effect is also larger in counties that have recently experienced extreme negative health outcomes, suggesting that the probability that citizens take action against firms shamed as polluters is higher under such circumstances. A counterfactual exercise suggests that, if there were no newspapers within a 20-mile radius from the plants that report emissions to the TRI, these

papers Density and that for the interaction term; the standard errors are calculated using the estimated variance and covariance matrix for the vector of estimates in the respective equation.

emissions would be 3% larger. For plants in industry-sectors that mainly sell final goods, emissions in the counterfactual scenario would be 11% larger, a number that doubles when looking at plants in counties exposed to an extreme incidence of mortality from cancer.

The substantial effect of the presence of newspapers on reported emissions has several policy implications. First, it gives an important insight into the effectiveness of the principle of "Regulation through Revelation " (Hamilton, 2005). Pollutant Release and Transfer Registers are currently used in several countries and regions as an environmental policy tool aimed at reducing pollution. The Kiev Protocol on Pollutant Release and Transfer Registers states that "although regulating information on pollution, rather than pollution directly, the Protocol is expected to exert a significant downward pressure on levels of pollution, as no company will want to be identified as among the biggest polluters (UNECE, 2003)."[50] The findings in this paper show that the availability of information is not a sufficient condition to make people informed. The lower levels of emissions reported by plants located nearby newspapers, paired with the documented effect of *Newspapers Density* on the probability of receiving bad press, reveals that people are more likely to be informed when there are some *intermediaries* that *lower the costs to the public of public information* (Hamilton, 2005). Absent these intermediaries, information provision programs might not reap their potential benefits, *de facto* depriving public policy of a potentially successful policy tool. While the widespread adoption of Pollutant Release and Transfer Registers addresses the problem of informational asymmetries as a possible determinant of polluting behavior on the firms'side, it does not provide a sufficient regulatory framework to overcome these asymmetries. A more appropriate approach would require a focus on the incentives for information providers to disseminate the information in the Registers.

More generally, these findings provide further evidence, beyond that summarized in Prat and Strömberg (2011), on the large set of outcomes that mass-media can affect. Some of the pollutants reported to the TRI are carcinogens that cause an excess risk of mortality from cancer if released in large quantity. Moreover, Currie and Schmieder (2009) show that specific chemicals in the TRI impair birth outcomes, and Agarwal et al. (2010) estimate a negative effect of TRI releases on infant mortality.[51] These facts, and the magnitude of the aggregate effects for some categories of plants, as presented in Table 11, suggest that it is not implausible that newspapers ultimately affect human health. In the words of Hamilton (2005), "if the scrutiny generated by the TRI results in fewer emissions of air carcinogens or lower releases of toxics into waterways, the TRI can (for a set of people who may never realize it) improve human health." The policy implications

[50]The Protocol was adopted on May 2003, following the 1998 *Convention on Access to Information, Public Participation in Decision-making and Access to Justice in Environmental Matters*, and establishes the first Pollutant Release and Transfer Register with international scope

[51]They estimate that the average county-level decrease in the concentration of different TRI toxics observed between 1998 and 2002 saved 13,800 infant lives.

following from this are tied to the other finding in this paper, that geography creates incentives for newspapers to write about a plant. This finding is similar in spirit to that in Snyder Jr and Strömberg (2010), that congruence between newspapers readers and a congressman's constituents increases the coverage given to the congressman. In the case of physical plants, the constituents are identified as the people who live nearby the plant. As in Snyder Jr and Strömberg (2010), the relationship between the geography of the newspaper market and coverage has policy implications in terms of press regulation.

Given the importance of citizens' information for firms' environmental performance, it is crucial to understand the implications of the spread of Internet for environmental outcomes. As observed in Snyder Jr and Strömberg (2010), the potential Internet audience being global, the incentives to cover local plants might be low especially when it comes to small firms that produce at few localities relative to global firms. Given the potential high costs of bad reputation for firms coming from consumer boycotts, loss of brand value, risk of litigation, employee productivity and cost of capital (Heal, 2008), this could make the incentives of big corporations increasingly aligned to those of their constituents whereas local firms would not internalize the external costs of their decisions. As suggested in Snyder Jr and Strömberg (2010), the same reasoning also applies when it comes to the trend in newspaper concentration, and in broadcast media substituting newspapers. The findings in this paper suggest that these trends could decrease residents' information about local plants' environmental performance, ultimately removing the incentives for many firms to control toxic emissions that are detrimental for human health.

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A Appendix

A.1 Proofs of Results in Section 2

A.1.1 Proof of result in Equation 1

Trivially, the utility of the consumer is maximized at $y_l = W$ when $e = e_c$, because the consumer bears no cost from industrial production.

When $e = e_d$, the utility is maximized at $\underline{y}_l$ s.t.:

$$V'(\underline{y}_l) = \overline{h}'(\underline{y}_l)$$

$\underline{y}_l < W$ from Assumption 1

A.1.2 Proof of results in equations 3 and 4

Define γ as $Pr[(\overline{y} - \underline{y}) \sum_{i=1}^{N} p(d_i) \geq c_c - c_d]$. The beliefs are formed following Bayes' rule as follows:

$$\begin{aligned} Pr(e_c|s = \oslash) &= \frac{Pr(s = \oslash|e_c) Pr(e_c)}{Pr(s = \oslash)} \\ &= \frac{1 * \gamma}{\gamma + (1-\gamma)(1-P)} \approx \gamma \end{aligned}$$

where the approximation comes from the fact that, when a plant chooses $e = e_d$, it is because P is very small.

and

$$\begin{aligned} Pr(e_c|s = s_d) &= \frac{Pr(s = s_d|e_c) Pr(e_c)}{Pr(s = s_d)} \\ &= \frac{0 * \gamma}{(1-\gamma)(P)} = 0 \end{aligned}$$

Given these beliefs, the constituents choose:

$$\underline{y}_l = argmax[V(y_l) - \overline{h}(y_l)] \quad \text{if } s = s_d \tag{18}$$

and

$$\bar{y}_l = argmax[V(y_l) - (1-\gamma)\overline{h}(y_l)] \quad \text{if } s = \oslash \tag{19}$$

I prove that $\bar{y}_l > \underline{y}_l$ by Contradiction.

Suppose that $\bar{y}_l \leq \underline{y}_l$. Then $V^{'}(\bar{y}_l) \geq V^{'}(\underline{y}_l) = \overline{h'}(\underline{y}_l) \geq \overline{h'}(\bar{y}_l)$.

Therefore,

$$V^{'}(\bar{y}_l) \geq \overline{h'}(\bar{y}_l)$$

but also, from FOC of the optimization problem in 19,

$$V'(\bar{y}_l) = (1-\gamma)\overline{h'}(\bar{y}_l) \qquad \Longrightarrow V'(\bar{y}_l) < \overline{h'}(\bar{y}_l) \quad \text{for } \gamma > 0$$

Contradiction.

A.1.3 Proof of result in equation 5

Given the consumer demand strategy, the manager chooses e_c if:

$$\bar{y}_l - e_c \geq (1-P)(\bar{y}_l - e_d) - P(\underline{y}_l - e_d)$$

Rearranging:

$$(\bar{y}_l - \underline{y}_l)\sum_{i=1}^{N} p(d_i) \geq e_c - e_d.$$

A.2 Tables and Figures

Figure 1: ***Distance from plant and probability of coverage in newspaper***

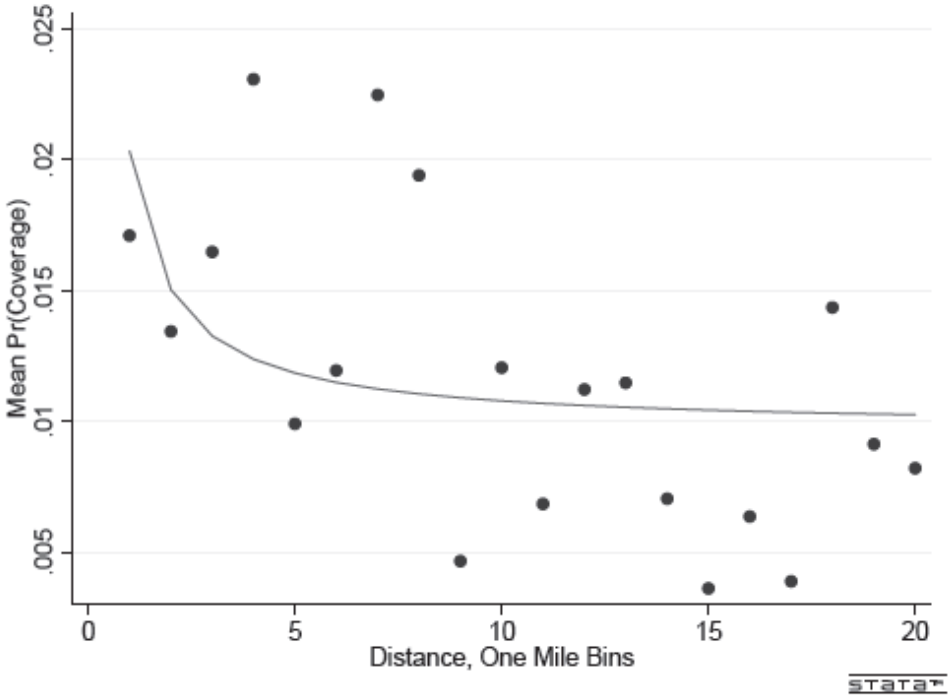


Figure 2: ***Location of TRI plants in the US territory, 2005***

Figure 3: ***An example of article search output in Newslibrary***

Figure 4: ***Articles Published by State, 2000-2008***

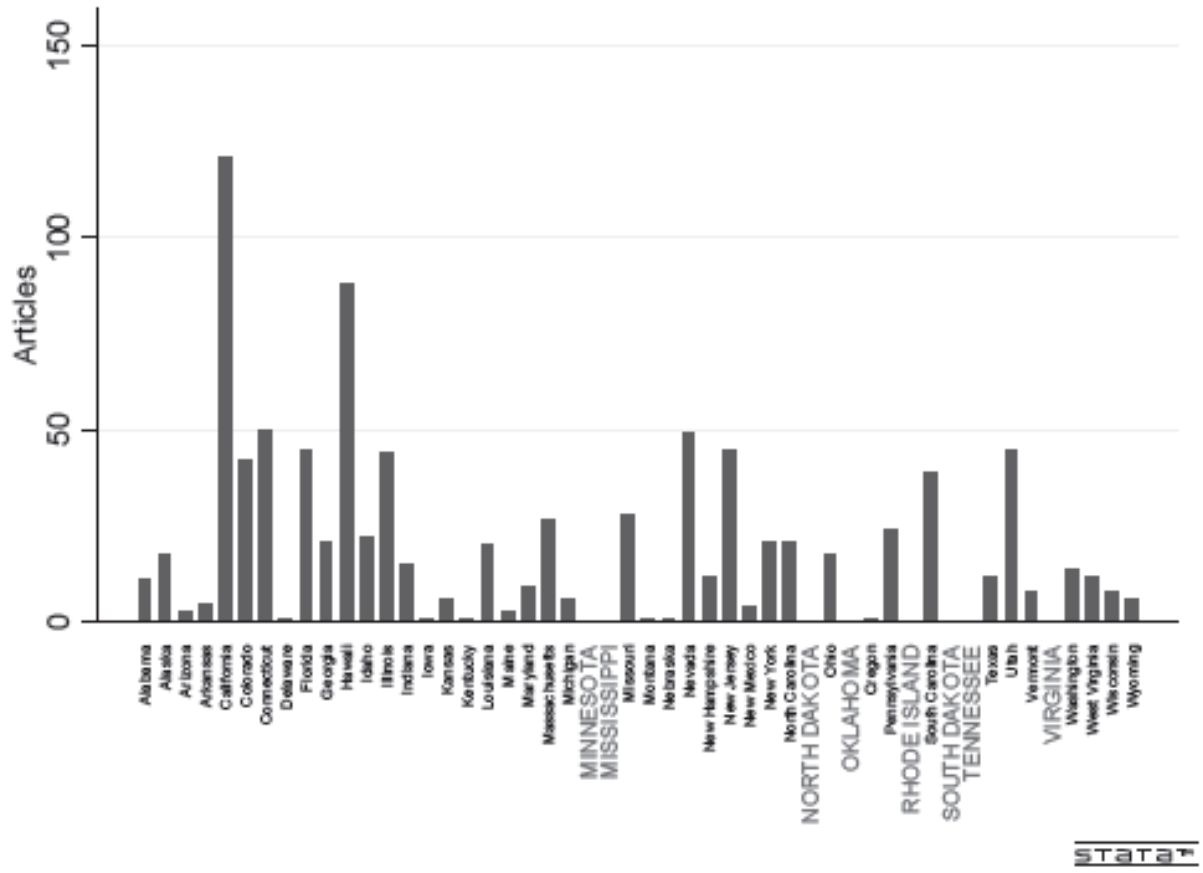
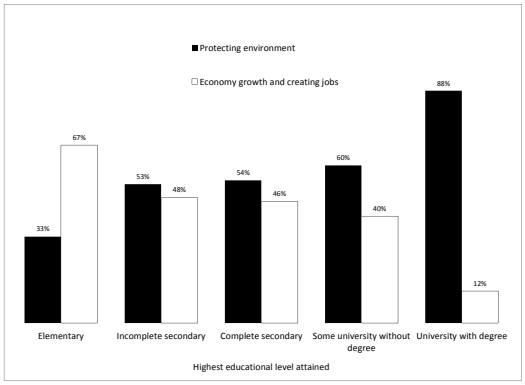


Figure 5: ***Newspapers Density in Oregon, US***

Figure 6: ***Education and Preferences for Clean Environment, I***



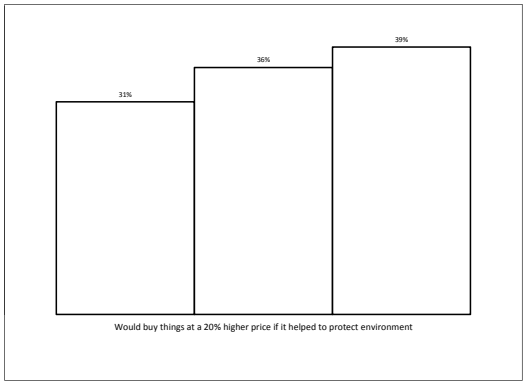
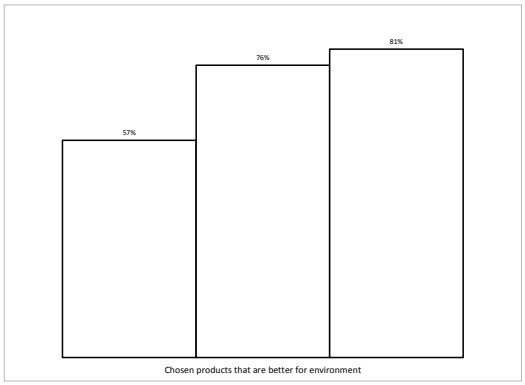


Figure 7: ***Education and Preferences for Clean Environment, II***



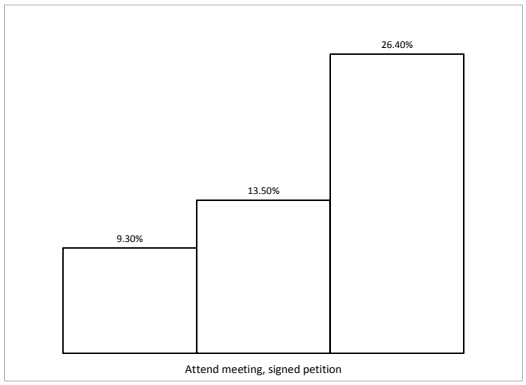
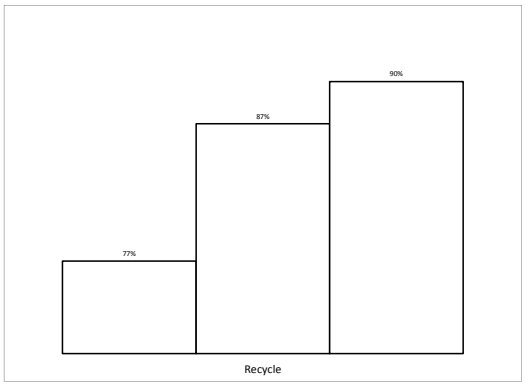


Figure 8: ***Education and Preferences for Clean Environment, III***



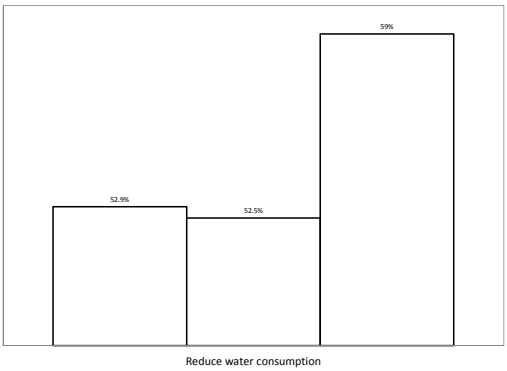


Figure 9: ***Distribution of county-level Health Outcomes across plants***

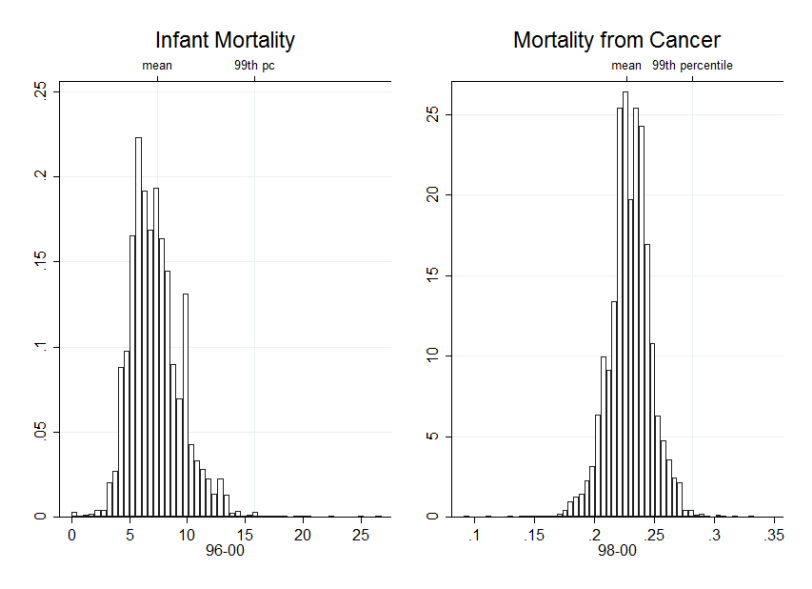


Table 1: ***Summary statistics, state top 20 polluters***

Variable	Mean	Std. Dev.	N
Newspapers Density	0.402	0.665	12772
Covered	0.053	0.224	12772
Number Newsp in Rings	2.215	2.866	12772
Avg Distance Newsp in Rings	9.837	4.863	10064

Table 2: ***Summary statistics, by coverage in State***

	Coverage in State	No Coverage in State
Toxic Emission (in 000)	166 (2586)	154 (1232)
Lag 1 Newspapers Density	0.53 (0.80)	0.41 (0.59)
Pop Density	1378 (2003)	897 (1100)
Income pc	24444 (7055)	23052 (6241)
Education - high school or some college	53 (8)	53 (6)
Education - associate or more	29 (11)	28 (11)
Share Black	11 (14)	14 (18)
Share Younger 20	28 (3)	28 (3)
Share Older 65	13 (3)	13 (3)
Universities Density	3 (4)	2 (3)
Unemployment 2000	5 (3)	5 (2)

Means. Standard Deviations in parenthesis

Table 3: ***Newspapers Density and probability of coverage of toxic emissions***

Dep. var.	(1) Pr(cov ring)	(2) Pr(cov out)	(3) Pr(cov ring) Density>0	(4) Pr(cov ring) Density>0	(5) Pr(cov ring) Density>0
Newspapers Density	0.012**	0.005	0.009*	0.008*	0.009*
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Covered Out				0.077***	0.079***
				(0.025)	(0.025)
Observations	12,772	12,772	10,064	10,064	9,967
Adjusted R-squared	0.084	0.137	0.104	0.112	0.117
$Pr(\overline{X}\widehat{\beta})$	0.0186	0.0387	0.0232	0.0232	0.0234

Standard errors clustered by county. State-by-year and Industry-subsector FE included in all the regressions. The controls included in Column (5) are: log pop density (linear and non-linear effect), log income (linear and non-linear effect), share black, share with high school education or some college, share with college education ore more, share aged less than 20, share aged more than 65, share unemployed in 2000, density of universities.

Table 4: ***Newspapers Density and toxic emissions***

Dep Variable: ln *Emissions*	(1)	(2)	(3)	(4)
Lag 1 Newspapers Density	-0.136***	-0.092**	-0.095**	-0.097**
	(0.041)	(0.041)	(0.041)	(0.041)
Log Pop Density		-0.233***	-0.186**	-0.192**
		(0.078)	(0.080)	(0.087)
Log Income		-0.895***	0.191	0.282
		(0.279)	(0.448)	(0.486)
Education - associate or more			-0.027***	-0.028**
			(0.010)	(0.014)
Share Black				-0.002
				(0.005)
Share Younger 20				-0.029
				(0.018)
Share Older 65				-0.031
				(0.022)
Education - high school or some college				-0.000
				(0.012)
Unemployment 2000				0.014
				(0.028)
Observations	154,587	154,582	154,582	154,582
Adjusted R-squared	0.272	0.273	0.273	0.274

Standard Errors clustered by County. County-year and industry-group fixed effects included in all the regressions. Dummies for values of ln pop density and ln income < the 10th, between the 10th and the 25th,between the 25th and the 50th, and between the 50th and the 75th percentile included in Columns (2)-(4).

Table 5: ***Universities Density, states with no coverage and within-firm variation***

Dep Variable: ln *Emissions*	(1)	(2)	(3)	(4)
		No coverage states	*Within-firm*	
			> 1 plant	> 2 plants
Lag 1 Newspapers Density		0.027	-0.075	-0.087*
		(0.171)	(0.047)	(0.048)
Universities Density	-0.006			
	(0.009)			
Observations	154,582	20,661	90,662	76,364
Adjusted R-squared	0.273	0.281	0.498	0.504

Standard Errors clustered by County. Industry-group fixed effects included. Controls for demographics and dummies for values of ln pop density and ln income < the 10th, between the 10th and the 25th,between the 25th and the 50th, and between the 50th and the 75th percentile included.

Table 6: ***Alternative samples and estimation methods***

Dep Var: ln *Emissions*	(1) No Large Cities	(2) Density >0	(3) Emissions>0	(4) Interv Reg	(5) 2001-2004	(6) Not at border
Lag 1 Newspapers Density	-0.106**	-0.100**	-0.066*	-0.095***	-0.085*	-0.121***
	(0.043)	(0.042)	(0.040)	(0.015)	(0.043)	(0.044)
Unempl 2000					0.007	
					(0.031)	
Observations	146,512	138,439	120,569	154,475	72,145	131,998
Adjusted R-squared	0.277	0.263	0.280		0.256	0.270

Standard Errors clustered by County. County-year and industry-group fixed effects included in Columns (1), (2), (3), (5) and (6). County, industry-subsector and year fixed effects included in Column (4). Controls for demographics and dummies for values of ln pop density and ln income $<$ the 10th, between the 10th and the 25th,between the 25th and the 50th, and between the 50th and the 75th percentile included in all the regressions.

Table 7: ***Heterogeneous effects by substance***

	(1)	(2)	(3)	(4)
Dep. var.	Pr(cov ring)	Pr(cov ring)	emission dioxin	emissions lead
emissions dioxin	0.003*			
	(0.002)			
emissions lead		-0.004		
		(0.003)		
Lag 1 Newspapers Density			-0.010**	0.003
			(0.004)	(0.007)
State-by-year FE	X	X		
3-digit industry FE	X	X		
County-by-year FE			X	X
4-digit industry FE			X	X
Observations	9,967	9,967	154,582	154,582
Adjusted R-squared	0.118	0.118	0.364	0.117

Total emissions, demographics, and dummies for values of ln pop density and ln income lower than the 10th, between the 10th and the 25th,between the 25th and the 50th, and between the 50th and the 75th percentile are included in all the regressions. Control for *Newspapers Density* included in Columns (1) and (2). *emissions dioxin* and *emissions lead* are categoric variables that take value 0 if the respective emissions are zero, and values from 1 to 4 depending if emissions are, respectively, before the 1th, between the 2nd and the 3rd, between the 3rd and the 4th, and above the 4th quartile, in the part of the distribution for which emissions are larger than zero.

Table 8: ***Industry subsectors, by type of good or service***

Final
Utilities
Construction of Buildings
Food Manufacturing
Beverage and Tobacco Product Manufacturing
Textile Product Mills
Apparel Manufacturing
Petroleum and Coal Products Manufacturing
Computer and Electronic Product Manufacturing
Furniture and Related Product Manufacturing
Miscellaneous Manufacturing
Building Material and Garden Equipment and Supplies Dealers
Food and Beverage Stores
Health and Personal Care Stores
Nonstore Retailers
Air Transportation
Publishing Industries (except Internet)
Rental and Leasing Services
Educational Services
Hospitals
Repair and Maintenance
Personal and Laundry Services
Intermediate
Crop production
Animal farming
Forestry and Lodging
Support Activities for Agriculture and Forestry
Oil and Gas Extraction
Mining
Support Activities for Mining
Specialty Trade Contractors
Textile Mills
Leather and Allied Product Manufacturing
Wood Product Manufacturing
Paper Manufacturing
Printing and Related Support Activities
Chemical Manufacturing
Plastics and Rubber Products Manufacturing
Nonmetallic Mineral Product Manufacturing
Primary Metal Manufacturing
Fabricated Metal Product Manufacturing
Machinery Manufacturing
Electrical Equipment, Appliance, and Component Manufacturing
Transportation Equipment Manufacturing
Merchant Wholesalers, Durable Goods
Merchant Wholesalers, Nondurable Goods
Wholesale Electronic Markets and Agents and Brokers
Support Activities for Transportation
Warehousing and Storage
Real Estate
Professional, Scientific, and Technical Services
Management of Companies and Enterprises
Administrative and Support Services
Waste Management and Remediation Services

The industries classified as "mixed" are: Textile Product Mills, Specialty Trade Contractors, Textile Mills, Petroleum and Coal Products Manufacturing, Building Material and Garden Equipment and Supplies Dealers, Repair and Maintenance, Computer and Electronic Product Manufacturing

Table 9: ***Heterogeneous effects by consumer pressure and incidence of extreme negative health outcomes***

Dep Var: ln *Emissions*	(1)	(2)	(3)
	By Consumer Pressure	By Incidence of Negative Health Outcomes	
Lag 1 Newspapers Density	-0.057	-0.084**	-0.085**
	(0.044)	(0.042)	(0.042)
Lag 1 Newspapers Density*Final Good	-0.260**		
	(0.104)		
Lag 1 Newspapers Density*Extreme Infant Mortality 97-01		-0.564***	
		(0.144)	
Lag 1 Newspapers Density*Extreme Mortality from Cancer 98-00			-3.549***
			(1.067)
Industry Effects	4-digit	3-digit	3-digit
Observations	153,479	154,582	154,548
Adjusted R-squared	0.275	0.180	0.181

Standard Errors clustered by County. County-year fixed effects included. Controls for demographics and dummies for values of ln pop density and ln income $<$ the 10th, between the 10th and the 25th,between the 25th and the 50th, and between the 50th and the 75th percentile included.

Table 10: ***Within-firm substitution***

Dep Var: ln *Emissions*	(1)	(2)
	> 1 plant per firm-ind group	
Lag 1 Newspapers Density	-0.102	-0.105
	(0.089)	(0.089)
Min Density		-0.035
		(0.128)
N plants Firm-Industry		-0.005
		(0.005)
Newspapers Density Firm	-0.081	
	(0.121)	
Observations	66,505	66,060
Adjusted R-squared	0.384	0.383

Standard Errors clustered by County. County-by-year and Industry-group effects included. Controls for demographics and dummies for values of ln pop density and ln income $<$ the 10th, between the 10th and the 25th,between the 25th and the 50th, and between the 50th and the 75th percentile included.

Table 11: ***Aggregate effects***

	Total Emissions, Actual	Total Emissions, Counterfactual	Lower Emissions due to Newspapers Density	Lower Emissions due to Newspapers Density, %
Full Sample	25,600,000	26,400,000	800,000	3%
Industries selling final goods	9,460,000	10,500,000	1,040,000	11%
Counties with extreme infant mortality	31,300	36,500	5,200	17%
Counties with extreme mortality from cancer	79,800	97,800	18,000	22%

Means, in thousands. Standard deviations in parenthesis. See Section 5 for a description of how the counterfactuals are calculated.

Chapter 3

Gender Quotas, Female Politicians and Public Expenditures: Quasi-Experimental Evidence *

1 Introduction

It is a well established fact that, despite the large and persistent increase in female labor market participation in the last twenty years, women have failed to achieve comparable levels of participation in politics. According to the 2011 Edition of the Global Gender Gap Report, in the US the female to male ratio of labor force participation is equal to 0.85, whereas the female to male ratio of presence in parliament is equal to 0.20. In Spain, the country I am going to focus on in this paper, these numbers are 0.77 and 0.58, respectively.

The use of electoral quotas has become a popular way of improving women's access to governing bodies and around half of the countries in the world today use some type of electoral quota when electing their parliaments (Dahlerup, 2005). Among the arguments for the introduction of gender quotas, a prominent one is that the preferences of wome may be given less weight than those of men in male-dominated policy making. Chattopadhyay and Duflo (2004) indeed show that female politicians in India make systematically different decisions than male ones, reflecting gender differences in preferences over policies. However,to date,there is not any credible evidence on the effect of gender quotas on policy-making in developed countries.

This paper attempts to estimate the causal effect of gender quotas on both the number of elected

*I am grateful to David Strömberg for many comments and suggestions on this paper. I also thank Miguel Almunia, Manuel Bagues, Gioia de Melo, Frederico Finan, Olle Folke, Ethan Kaplan, Torsten Persson, Michel Serafinelli and Emilia Simeonova for many useful discussions. I benefited from the inputs from seminar participants at the Development Lunch at UC Berkeley and at the 4th COSME-FEDEA Annual Workshop in Gender Economics.

female politicians and on government policy in Spain, using a Before-After Regression Discontinuity Design (Before-After RDD henceforth). In general, these effects are hard to identify, because the settings where quotas are adopted and where they are not often differ in ways that are correlated with the outcomes studied. Gender quotas are, for example, likely to be adopted when there is a trend favoring female political participation, and this trend may directly affect the number of elected female politicians and policy. The Law 3/2007, which introduced legislated candidate gender quotas in Spain, offers a unique source of exogenous variation in gender quotas. This law prescribes that both genders be represented in at least 40% of every five positions on municipal electoral lists. Municipalities with less than 5,000 inhabitants are exempt from the law, allowing me to exploit an RDD. The empirical strategy applied in this paper compares differences between pre-reform and post-reform outcomes, examining municipalities just below and just above the 5,000 population threshold.

The first part of the paper investigates the effect of gender quotas on the percentage of female candidates and elected politicians. According to my estimates, the gender quotas increased the percentage of female elected councilors in Spanish municipalities by 6 percentage points; this is about a 20% increase with respect to the average percentage of female councilors in 2003. Interestingly, the percentage of female candidates increased more than the percentage of female councilors. This difference arises due to a strategic positioning of candidates, where women were systematically placed at the bottom of every five positions on voting lists. For similar reasons, the gender quota did not significantly affect the share of female mayors, since mayors are indirectly elected and are in practice always the first name on each list. These results are consistent with recent empirical findings in the political economy literature on strategic positioning of candidates (Esteve-Volart and Bagues, 2012).

The second part of the paper analyzes the effect of gender quotas on policy making. To what extent can legislated candidate gender quotas allow a better representation of women's preferences? There are different reasons to suspect that their effect could be null. Parties change the strategic positioning of women on electoral lists, in order to minimize the probability that female councilors are elected, thus diluting the effect of the reform and signaling a discriminatory behavior toward women. Moreover, there is no increase in the probability that women are elected to mayoral positions; this is particular relevant in the Spanish institutional setting, characterized by the prominent role of municipal mayors (Sweeting, 2009). Following Acemoglu and Robinson (2006) and Acemoglu and Robinson (2008), one can also imagine that male elites invest to increase the *de facto* power in the hands of the mayor and of the party leaders, given that their de jure power to control the pool of candidates is undermined by gender quotas; this would minimize the influence of female councilors on policy making.

Consistent with the above arguments, I find no evidence that the gender quota made municipal

policy more aligned with female preferences. This is in contrast to Chattopadhyay and Duflo (2004), who, however, study the effect on the policy of reserving the most important seat in Indian villages for a woman. While the null effect on policy could also be caused by similar preferences among women and men, I actually find evidence of differences across gender in the response of Spanish citizens to surveys on values and attitudes. Finally, it could be that the competition for political power is so strong that the preferences of politicians are of no importance for policy making, only the preferences of voters matter (as in the Downsian model of electoral competition).

My evaluation of the Equality Law parallels that in Casas-Arce and Saiz (2011). We started working on our respective papers independently. They exploit the introduction of gender quotas in Spain to infer the causes of low female participation in politics. Both papers document the effect of gender quotas on female representation, and the strategy adopted by the party-machine to dilute this effect; however, I also analyze the effect of gender quotas on policy outcomes. I contend that it is relevant to focus on policy outcomes, since one of the reasons for adopting gender quotas is to give a larger weight to female preferences in policy-making.

The rest of the paper is organized as follows. In Section 2,I provide the background to the research questions investigated. Section 3 describes the policy analyzed and the Spanish institutional context in terms of electoral law and government responsibilities at the local level. Section 4 presents the empirical strategy and Section 5 presents the data. Section 6 analyzes the results. Section 7 discusses the assumption needed for a causal interpretation of the estimates and Section 8 concludes the paper.

2 Conceptual Framework and Background

The investigation of the effect of gender quotas on the election of female politicians relates to a number of existing studies. The paper that is most closely related to mine is De Paola, Scoppa, and Lombardo (2010); they estimate a positive effect of legislated candidate gender quotas on the percentage of female councilors in Italian municipalities. The institutional context of their study is, however,different than mine, which may be of importance for the functioning of quotas, as emphasized in Norris (2006). First, the so-called preference vote (*voto di preferenza*) is allowed in Italy, which means that voters can express an explicit preference for a candidate on a list; Spain instead has a closed list system, which allows me to investigate how party leaders reoptimize their placement strategies in response to the introduction of gender quotas. Second, I estimate what impact gender quotas have on the indirect election of mayors, whereas De Paola, Scoppa, and Lombardo (2010) look at direct elections.

In more recent work, Baltrunaite, Bello, Casarico, and Profeta (2012) analyze the effect of

the introduction of gender quotas on the quality of elected politicians in Italy; they find that their average educational level increases because more female politicians, who are on average more educated than men, are elected, and because men at the lowest tail of the educational distribution are no longer elected. Besley et al. (2012) show that a "zipper" candidate gender quota voluntarily introduced by the Social Democratic party in Sweden raised the competence of men in municipalities where the initial share of women was low.

Given its focus on the effect of gender quotas on policy, this paper is also closely related to the work of Chattopadhyay and Duflo (2004). However, there are some important differences between these two studies. Chattopadhyay and Duflo (2004) estimate the effect on policy of the reservation of seats for heads of Village Councils; these reserved positions are defined as the only powerful ones at the village level. Clearly, the effects of legislated candidate quotas for the election of a legislature that makes decisions with majority rule can be very different, as opposed to the effects of seat reservation. As noted by Ferreira and Gyourko (2011), legislators negotiate with colleagues over the passing of legislation, so a modest increase in the number of legislators may not be sufficient to change policy, and large shocks to the gender composition of the legislature would be needed.

More broadly, this work is related to the literature on the effect of female politicians on policy outcomes. However, the introduction of gender quotas has different implications for policy formation with respect to the close election of female legislators, which is the focus of most of this literature. Effects of gender quotas on policy could arise through changes in the gender composition of the legislature, but also by inducing general changes in women's participation in politics: women's presence in political parties, female turnout, involvement in public debates and lobbying. How these changes map into policy changes depends on the political process.

Starting with the effects through the gender composition of the legislature, two useful benchmarks are the Citizen Candidate Model (Osborne and Slivinski, 1996; Besley and Coate, 1997) and the Downsian Electoral Competition Model (Downs, 1957). The former predicts that the preferences of politicians are of importance for policy making and the latter that they do not. The empirical evidence regarding which model is a better description of the actual policy choice is mixed. A number of papers find that the preferences of politicians are of importance in developing countries (Chattopadhyay and Duflo, 2004; Clots-Figueras, 2011), whereas Rehavi (2007), Svaleryd (2009) and Ferreira and Gyourko (2011) present mixed evidence in developed countries. This framework also applies to research on the effects on policy of other legislator characteristics, such as the party of affiliation in Lee, Moretti, and Butler (2004) and female children in Washington (2008).

The effect of legislators' identity on policy making also depends on the type of legislative bargaining model that is implied; see Baron and Ferejohn (1989). Female politicians may have a larger effect on policy outcomes if they can access positions with agenda setting power, such as

mayor, deputy-mayor, or member of the Cabinet. They may also have a larger bargaining power if the agenda setters (i.e. the mayor and the party leaders) did not have the power to end their political career by placing them outside a closed list.

Finally, the effect of the increase in the number of female legislators on policy outcomes depends on the ability of the male elite to invest in *de facto* power, in order to offset the partial loss in *de jure* power caused by the gender quota (see (Acemoglu and Robinson, 2006, 2008)).

Now turning to changes in the female voter turnout, this would have an impact in most models of policy choice. For example, it may change the gender of the median voter in the Downsian Competition Model.

Summarizing, although related to a number of theoretical and empirical studies on gender quotas and on legislators' identity and policy outcomes, this paper is the first to investigate whether candidate gender quotas change policy in a direction which is more aligned to female preferences; it also provides new evidence on the effect of gender quotas on women's access to positions of power, in an institutional context different from that in previous work.

Understanding the potential of legal candidate quotas to change the policy outcomes is crucial given that policy change is at the core of the pro-quota arguments, and given that legal candidate quotas are increasingly being applied in both developing and developed countries.[1]

3 Institutional Context

In March 2007, the Equality Law (Ley de Igualdad) modified the Spanish electoral law and introduced the "principle of balanced presence". Party electoral lists are required to have a minimum of 40% of each sex among their candidates in all elections (general, regional, European, local). More specifically, Spanish parties have to adopt the "double quota": the principle of a balanced presence does not only apply to the entire party list, but also to every fifth position. This "double quota" is to prevent political parties from adopting a strategic placement of candidates, with the under-represented sex occupying the positions at the bottom of the list. This is particularly relevant in Spain because municipal councils are elected through proportional elections with closed lists: voters can only express their preference for a list, and the seats won by each list are allocated to its members according to their order on the list.

[1] According to *Quota Project, Global Database of Quotas for Women*, the following countries have adopted legislated candidate quotas at the sub-national level: Argentina, Belgium, Bolivia, Bosnia and Herzegovina, Brazil, Burkina Faso, Costa Rica, Dominican Republic, Ecuador, Macedonia, France, Greece, Honduras, Republic of Korea, Mauritania, Mexico, Namibia, Nepal, Paraguay, Peru, Portugal, Senegal, Serbia, Slovenia, South Africa, Spain, Uruguay, Uzbekistan. The Italian Council of Ministry did in March 2011 approve a Legislative Decree that prescribes the introduction of candidate gender quotas in municipal elections in Italy; in particular, the decree prescribes that each gender cannot represent more than two thirds of the candidate lists.

The law was first applied in the local and regional elections in May 2007 and in the general elections in March 2008. Towns with less than 5,000 inhabitants do not have to comply with the quota.

It is important to understand the organization of local government in Spain, in order to study the effect of gender quotas on policy making at the municipal level. This is mainly disciplined by the Law 7/1985 (*Ley Reguladora de las Bases de Regimen Local*). According to this law, municipalities are administered and governed by the municipal council, which is composed of the municipal councilors, elected in free elections every four years, and the mayor, usually also the leader of the majority coalition, who is elected by the municipal councilors. The division of power between municipal councilors and the mayor is so that the latter can more effectively affect policy making.

Municipal councilors are entitled to the control and supervision of the governing bodies, the approval of instruments of general planning, regulations and ordinances, the approval of the budget, the disposal of expenditure in different fields of competence, and the roll-call vote of confidence on the mayor.

Nevertheless, the mayor appears to be the dominant figure in local politics. She convenes and chairs the meetings of the plenary assembly of municipal councilors and of the cabinet and casts the deciding vote in the case of ties, disposes expenditures within the limits of her competence, heads the municipal staff and as such approves appointments, sanctions and dismissals, heads the municipal police, approves instruments of development planning not explicitly attributed to the municipal councilors and instruments of urban management and urbanization projects, approves construction projects if they are part of the municipal budget, grants licenses, and orders the publication and enforcement of the decisions of the municipal council. Moreover, she appoints mayoral deputies and cabinet members, and formal rules allow her to remove them at any point in time.[2] This last feature confers a great deal of power to the mayor, because mayoral deputies and cabinet members prepare the general plan of the council and the municipal budget under the mayor's guidance; the general plan and the budget are then subject to the municipal council for approval. This means that the mayor and the other people whom she appoints are the agenda setters for the determination and allocation of expenditures. Sweeting (2009) analyzes formal and informal rules that regulate the decision-making process at the municipality level in Spain and he concludes that the mayor remains dominant over the Cabinet.[3]

[2]In municipalities with more than 5,000 inhabitants, the mayor appoints not more than one third of the councilors to the cabinet; in municipalities with less than 5,000 inhabitants, a cabine can be appointed if agreed through organic regulation or by the municipal council.

[3]Sweeting reports some statements by people involved in local politics in Spain whom he directly interviewed, as the followings: "The mayor has all the powers, all of them...by law, only the first Deputy mayor has the power to substitute for the mayor through illness or incapacity. But the mayor has the power to sack the first Deputy mayor"; "All the powers are with the mayor. If the mayor delegates, to a councilor say, citizen security, at any moment he

An important feature of the decision-making process described by Sweeting (2009) is the strict party discipline that seems to be imposed on party members who vote in the Municipal Council and in the Cabinet. A mayor interviewed by Sweeting states:

"*Legally they could [vote against the group] but they would be reprimanded by the party. We could not remove them from the Town Hall, but we could separate them from the government. I could take their competences away from them*"

Interestingly, Sweeting (2009) argues that the closed list system strengthens this practice, because councilors feel that they have been elected due to their party membership, rather than due to their individual characteristics, and therefore they vote following the party directives rather than their individual attitudes. Moreover, the degree of authority attributed to the mayor by the formal rules on decision making is enriched by the informal rules about the formation of the lists. The first person on the list (running for the mayoral position) is chosen first; after that, the other candidates and their order are determined, and it is reported that the mayor "always gets what he wants". Therefore, Sweeting observes that "the mayor can reward loyal members by moving them up the list, and penalize others".

From the institutional details provided above, it can be seen that the mayor has a crucial role in decision-making, due to both formal and informal rules; about the former, the power to set the agenda in decisions over the allocation of expenditures and the power to appoint the other agenda setters are the most relevant; about the latter, the practice that gives a great deal of authority to the mayor in deciding the positions on the list is also extremely important.

Given this, one would not expect a substantial change in policy resulting from the introduction of legislated candidate gender quotas, unless they promote more women to the position of mayor, or unless a large shock to the gender composition of parties and Municipal councilors is brought about, changing the informal rules within parties.[4] Moreover, given that the male elite of local party leaders (from the pool of which the mayoral candidate is likely to be selected) sees its *de jure* power being diluted by the introduction of gender quotas, it may increase its investment in *de facto* power, exercising more authority in decisions over the positioning of candidates, and increasing the influence of the (usually male) mayor on voting decisions in Municipal Councils and Cabinet meetings. This process, very similar to that modeled by Acemoglu and Robinson (2006, 2008), though in reference to a different context, would reinforce the mechanism through which gender quotas fail to promote policy change.

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can revoke it. And that has nothing to do with the full council; "Municipalities are presidential. Central government Ministers have weight. That does not happen locally".

[4]This would be more a long-term process anyway.

Table 1: Services provided by Town-Halls in Spain

Population Size	Service
Any Size	public lighting, cemeteries, waste collection, street cleaning, household water supply, sewerage, access to villages, paving roads, food and beverage control
More than 5000	public park, public library, waste treatment, organization of markets
More than 20000	civil protection, social services, prevention and fire fighting, sports facilities for public use
More than 50000	urban public passenger transport and environmental protection

4 Empirical Strategy

In the absence of randomization, it is hard to identify the causal effect of gender quotas on the outcomes of interest. Municipalities that self-select into an electoral system with quotas are systematically different from those that do not, and these differences are likely to be correlated with the outcomes that I want to investigate. Ideally, I would prefer to randomly assign gender quotas to different municipalities. While this is not possible in the context studied here, the RDD combined with the Before-After strategy that I present below provides a close approximation to the ideal experiment, allowing me to credibly claim that the allocation of gender quotas is "as good as randomly assigned".

The standard RDD allows the identification of the effect of gender quotas if other variables do not change sharply at the 5,000 population threshold, given that, in Spain, gender quotas only apply to municipalities with more than 5,000 inhabitants. However, this is not the case for Spanish municipalities. The 5,000 threshold is indeed relevant for the application of other laws. Most importantly, the services provided by the Municipal Council vary depending on the size of the population (see Table 1).[5] [6] However, these laws existed before the Equality Law that was passed in 2007. For this reason, I combine the RDD with a before-after comparison.

Let Y_{it} be the outcome of interest. I estimate the following equation:

$$\begin{aligned} Y_{it} &= \alpha + \beta_1 After_{it} + \beta_2 Pop5000_{it} + \beta_3 Quota_{it} + \\ &\varphi_N(\gamma_n Pop^n_{it}) + \varphi_N(\delta_n Pop5000_{it} Pop^n_{it}) + \varepsilon_{it} \end{aligned} \tag{1}$$

where Pop_{it} is population in the pre-elections year, $Pop5000_{it}$ is a dummy for municipalities with

[5] Municipal Councils are obliged to provide the services listed in Table 1, but can also provide any other good or service that they will deem useful to the community and which falls within their competences. On the revenue side, Spanish municipalities rely pretty heavily on funding from other levels of government. In numbers, about 55% of the municipal budget come from taxes and charges levied by the municipality itself (Sweeting, 2009)

[6] Besides this difference and that discussed in Section 3, municipalities with more than 5,000 inhabitants elect a minimum of 13 councilors while those with fewer inhabitants elect a maximum of 11 councilors.

$Pop_{it} > 5000$, $After_{it}$ is a dummy for the post-reform period and $Quota_{it}$ is the interaction term between $Pop5000_{it}$ and $After_{it}$, i.e. $Quota_{it} = 1$ if $Pop5000_{it} = 1$ and $After_{it} = 1$. The function φ_N is defined as follows: let $\sigma = \{\gamma, \delta\}$ and $k = \{1, Pop5000_{it}\}$; $\varphi_N(\sigma, k, Pop_{it}) = \Sigma_{n=1}^{N} \sigma_n k Pop_{it}^n$.

β_3 is the Before-After RD parameter, estimating the effect of gender quotas on the outcomes of interest.

I estimate equation (1) for N=$\{1,2,3,4\}$ on bandwidths equal to $\pm$4000 (0.80), $\pm$1000 (0.20), and $\pm$500 (0.10). This deals with Lee and Lemieux (2010) concern that an estimation on the whole sample controlling for a polynomial in the running variable would provide global estimates of the regression function over all values of X, while the RD identification is based on estimates of the regression function at the threshold; Lee and Lemieux suggest a polynomial approach only as a robustness check for local linear estimates.

In equation (1), I allow the slope of the polynomial to be different on the two sides of the threshold, but I constrain it to be identical on each side before and after the reform. An inspection of the data for some outcome variables shows that this is a plausible constraint. However, following Grembi, Nannicini, and Troiano (2011), I also estimate a more flexible version of equation (1), allowing the slope of the polynomials to be different on the two sides of the threshold and also across the two periods; in practice, I fit four polynomials. The estimated equation is:

$$\begin{aligned} Y_{it} =& \alpha + \beta_1 After_{it} + \beta_2 Pop5000_{it} + \beta_3 Quota_{it} + \varphi_N(\gamma_n Pop_{it}^n) + \varphi_N(\delta_n Pop5000_{it} Pop_{it}^n) \\ &+ \varphi_N(\eta_n After_{it} Pop_{it}^n) + \varphi_N(\lambda_n After_{it} Pop5000_{it} Pop_{it}^n) + \varepsilon_{it} \end{aligned} \tag{2}$$

where the function φ_N is defined as for equation (1) with:

$$\sigma = \{\gamma, \delta, \eta, \lambda\}$$

and

$$k = \{1, Pop5000_{it}, After_{it}, Pop5000_{it} After_{it}\}$$

Given equation (1), and defining:

$$\begin{aligned} \Gamma_N(Pop_{it}) = & \varphi_N(\gamma_n Pop_{it}^n) + \varphi_N(\delta_n Pop5000_{it} Pop_{it}^n) + \varphi_N(\eta_n After_{it} Pop_{it}^n) \\ &+ \varphi_N(\lambda_n After_{it} Pop5000_{it} Pop_{it}^n) \end{aligned}$$

β_3 is equal to

$$\begin{aligned}
&\{E[Y_{it}|Pop5000_{it}=1, After_{it}=1, \Gamma_N(Pop_{it})] \\
&- E[Y_{it}|Pop5000_{it}=0, After_{it}=1, \Gamma_N(Pop_{it})]\} \\
&- \{E[Y_{it}|Pop5000_{it}=1, After_{it}=0, \Gamma_N(Pop_{it})] \\
&- E[Y_{it}|Pop5000_{it}=0, After_{it}=0, \Gamma_N(Pop_{it})]\}
\end{aligned}$$

which is the average causal effect of gender quotas on the dependent variable, $E[Y_{i1} - Y_{i0}]$, under the assumptions that the effect of gender quotas is homogeneous across different population levels and $E[\varepsilon_{it}|, Pop5000_{it}=1, After_{it}=1, \Gamma_N(Pop_{it})] = 0$. The latter condition requires that there are no time-varying, non-controlled for characteristics specific to municipalities with a population just above 5,000 and correlated with the outcome of interest.

In Section 7 I present some robustness checks to show that the necessary conditions for the validity of this assumption are met.

5 Data

5.1 Municipal Councils Data

I apply the empirical strategy described above to a sample of Spanish municipalities (5,538 in 2003, 5,502 in 2007). [7]

Data on the results for the 2003 and 2007 municipal elections were made available by the *Subdirección General de Política Interior y Procesos Electorales*. Data on the 2007 elections include candidates' names, gender, municipality, party affiliation, order on the list and a variable coded "YES" if the candidate was elected.[8]

The variable *gender* is missing from the 2003 data; I therefore merged this dataset with another that contains information on gender.[9]

Based on these sources, I measure the municipality-level percentage of female candidates and female councilors resulting from the 2003 and 2007 elections. Table 2 contains summary statistics

[7] Spanish law prescribes a special electoral rule for municipalities with less than 250 inhabitants: these municipalities are excluded from the analysis.

[8] The names of 180 candidates are not known and all were non-elected; these observations are dropped; none of the candidates dropped ran in the municipalities used for the restricted sample estimation; in three municipalities the ratio of candidates dropped to the total number of candidates running was rather high (between 20% and 30%), therefore these municipalities were dropped from the sample.

[9] I am especially grateful to Manuel Bagues for proving these data, where candidates' gender is inferred on the basis of names.

for these variables, by year. The summary statistics for each variable are calculated for both the whole sample and within the 20% bandwidth ($\pm$1000 inhabitants). Both the percentage of female candidates and the percentage of female councilors increased in 2007 with respect to 2003, and the size of the increases was higher in the restricted sample around the threshold. In both years, the average population was larger (although not hugely) than 5,000; moreover, in 2007 the 75th percentile in the distribution of the population was equal to 4,344 (data not shown), but the municipalities with more than 5,000 inhabitants contain the largest share of the Spanish population.

I also collected information on the gender of mayors elected in 2003 and 2007. The *Ministerio de Política Territorial y Administración Pública* has published a list of the mayors elected between 2007 and 2010 in each Spanish municipality, with the respective date of election. I only used information on mayors elected in 2007 and who were in office for the entire four-year period. Data on the name of the list supporing each mayor elected before 2007 are provided by the same institution. Spanish mayors are elected indirectly by the municipal council, and the person elected mayor is the first candidate on the list that gets the majority of votes, or on one of the lists that constitutes the basis for the majority coalition. I merge the dataset with the electoral list of the mayor to that with the names of candidates running for each list in the 2003 municipal elections. [10]

Given this, I can use data on the gender of the mayor for 4,644 observations in 2003 and 4,061 observations in 2007.

The percentage of female mayors in municipalities with a population between 4,000 and 6,000 inhabitants increased from 12% to 16% between the pre-reform and the post-reform period (see Table 2).

5.2 Local Budget Data

Data on budget size and the composition of expenditures in Spanish municipalities are available at the *Ministerio de Economia y Hacienda* website. I use two variables for government size, measuring the revenue and the expenditure side, respectively. I follow the classification in the official data which breaks down expenditure into functional categories, reflecting the responsibilities of the Town Council listed in Law 7/1985. Budget data for 2008 and 2009 are merged with election data for 2007; budget data for 2004 and 2005 are merged with election data for 2003.[11] I measure

[10] The match between these two datasets is not perfect, as it is done based on the name of the town and on the list. This implies that some observations are lost, as some names could not be found in both datasets, and also that there could be some error, which is however hardly correlated with the adoption of gender quotas, thus inducing only a higher variance of the estimator when I use the gender of the mayors as the dependent variable.

[11] Of the 11,040 municipality-by-legislature observations in the dataset with electoral results, 600 are not matched with the expenditures dataset, because expenditure data are not available for a number of municipalities every year; of these, only 16 observations are in the 0.20 bandwidth.

expenditures after the introduction of gender quotas as average expenditures in 2008 and 2009, and expenditures before the introduction of gender quotas as average expenditures in 2004 and 2005.

Table 2 presents summary local budget statistics. *General services* and *social goods* constitute the bulk of Municipal Council expenditures. *General services* include expenditures for government departments and general administration. *Social goods* include health, pre-school and school education, water supply, sanitation and distribution, housing and urbanism, refuse collection and street cleaning, other social welfare enhancing services, cemeteries and funeral services, cultural promotion, sport and entertainment and protection of the historical and artistic heritage.

6 Results

6.1 Gender Quotas and Female Politicians

The research design presented in Section 4 allows estimation of the causal effect of the gender quotas introduced in 2007 on the percentage of female councilors elected to Spanish municipalities. The results of the estimation are reported in Table 3. In all tables with regression results, I report the estimates of Equation (1) in columns (1)-(2), for $N = \{1,2,3,4\}$; in columns (3) and (4) I report the coefficients from the estimation of the augmented version of Equation (1) described in Equation (2).[12]

The Akaike criterion suggests that a linear polynomial provides the best fit for a bandwidth of 0.20 and a fourth-order polynomial provides the best fit for bandwidths of 0.80 and 0.10. The goodness of fit test rejects any estimation on the 0.80 bandwidth and the quadratic polynomial estimation on the 0.20 bandwidth.[13] Given this, the effect of gender quotas on the percentage of female councilors is between 5.85 and 5.99 points, and is very precisely estimated. The coefficient drops dramatically in column (6) and it is not statistically significant; the effect of gender quotas on the election of female councilors is too small and noisy to be precisely estimated with a very flexible model using a relatively low number of observations.

In Table 4, I present the estimates for the same equations but with the percentage of female candidates as the dependent variable. Using the Akaike criterion and the goodness of fit test, as above, to select the most reliable coefficients, I estimate a positive effect of gender quotas on the percentage of female candidates, which falls between 7.59 and 8.89 points.

[12] I do not restrict the bandwidth below 0.80 when I allow the polynomial to differ on the two sides of the threshold and across periods, because the number of observations may be too low to efficiently fit this model; I only report the linear estimates on the 0.10 bandwidth, as a robustness check for the estimates on the 0.80 bandwidth.

[13] The goodness of fit test I use is proposed in Lee and Lemieux (2010): I run the regressions estimated in Table 3 controlling for 200 inhabitants bin dummies. Then, I interpret joint significance of the dummies as a rejection of the model.

Gender quotas caused an increase in the percentage of female candidates and in the percentage of female councilors in Spanish municipalities. This shows that gender quotas are binding, and they reach one of the objectives they are adopted for, which is a larger presence of women in politics.

Figures 1 and 2 plot the bin averages of the after-before difference for the two outcomes against population in 2006, using bin sizes of 100 inhabitants, and fitting a second-order polynomial on the two sides of the threshold.[14] These figures confirm that gender quotas result in an increase in both the percentage of female councilors and the percentage of female candidates, but the increase in the former is both smaller and more noisy. This suggests that the allocation of women and men to every five positions may not be random: if it were, we would observe an increase in the percentage of female councilors very close to the increase in the percentage of female candidates in municipalities where gender quotas are applied.

More specifically, this evidence suggests that women are systematically placed in less desirable positions on the electoral list. Esteve-Volart and Bagues (2012) indeed provide evidence of strategic manipulation on party lists aimed at women being appointed to positions with relatively lower chances of victory. Given the design of the law, I would expect to see more women at the bottom of every five positions within each party list, and more men at the top. I check whether this is happening by running t-tests for the probabilities, by gender, of being in each of the first ten positions.[15] Table 5 displays the results of this test for the 2007 elections in municipalities with more than 5,000 inhabitants. Men are more likely than women to be in the first two positions in each of the two five-position blocks. For example, 14% of the male candidates in the first ten positions are at the head of the list, and 4% of the female candidates, whereas 7% of the male candidates are in the fifth position and 13% of the female candidates. When I repeat the exercise for municipalities with less than 5,000 inhabitants in 2007 and for municipalities with more than 5,000 inhabitants in 2003, men are statistically significantly more likely to be in the first two positions and women are more likely to be in any other position (these results are available upon request).

This analysis shows that gender quotas are effective in changing the gender composition of legislatures only as long as they put constraints on certain behaviors, but they do not change the attitudes toward the under-represented group or raise the awareness of the importance of its better representation. If such an effect were in place, I would observe a more gender-balanced placement of candidates within the list.[16]

[14] Figures with a bin size of 50 and 200 inhabitants deliver the same general picture as those with bin size of 100 inhabitants.

[15] I drop candidates in positions higher than 10 from the sample, and I estimate the female and male probabilities of being in each position conditional on being positioned in the first 10.

[16] I cannot exclude that there will be a long-term effect on attitudes, as is shown in Beaman et al. (2009) and in De Paola et al. (2010).

Table 6 reports the t-tests in Table 5 by party, selecting the two main parties on the Spanish political arena, i.e. the Socialist Party (*Partido Socialista Obrero Espanol*) and the People's Party (*Partido Popular*). Although both parties are more likely to place male candidates in the first positions, the tendency is more pronounced for the People's Party, i.e. the main opposition party in the national government in 2007. This evidence hints at two possibilities: either that more experienced women self-select into the Socialist Party, or, if the quality of female candidates on average is the same across parties, preferences for female politicians are heterogeneous across parties.[17]

Overall, the fact that, although to different extents, the two major parties in the Spanish political arena use strategic positioning to increase the probability that male candidates will be elected shows that voters do not sanction this behavior; otherwise it would be optimal for the parties to propose a more balanced distribution of female and male candidates within the five position blocks.

What is the effect of gender quotas on the election of female mayors? Spanish mayors are elected indirectly by the Municipal councilors and the person elected is the first candidate on the list of the party with an absolute majority, or the first candidate on one of the lists of the parties forming a majority coalition. Given the strategic positioning documented in Table 5, gender quotas most likely have a null effect on the election of female mayors. I estimate Equations (1) and (2) with the dependent variable being a dummy for female mayor. The regression results are presented in Table 7 and graphical evidence is also provided in Figure 3. The coefficient of *Quota* is not precisely estimated and switches signs across different specifications; the evidence in Figure 3 suggests that there is no effect of gender quotas on the election of female mayors.

This is consistent with the evidence on strategic positioning and with the claim in Chattopadhyay and Duflo (2004) that India's initial implementation of a reservation policy which assigned one-third of the seats on Village Councils to women was not successful because it did not substantially increase the proportion of female pradhans, the most important position in the local government.

This finding suggests that the Spanish experience with gender quotas can partly be regarded as disappointing. As argued in Section 3, Spanish law gives agenda setting powers to the mayor, which suggests that a female mayor could ensure representation of female preferences in policy decisions more than female councilors; also the position of mayor in Spain is the most prestigious one in the local government, which means that the increase in the number of female mayors may be more effective in breaking down the negative stereotypes related to women in politics than the increase in the number of female councilors (see the case of the election of female Pradhans in India in Beaman et al., 2009). Therefore, gender quotas in Spain are most likely a missed opportunity

[17]The Socialist Party has had internal quotas since the 1990s, which may in part explain the observed difference across the two parties.

for a change in policy and in attitudes toward female policy-makers.

In the next section, I will directly study the effect on policy, looking at local budget data.

6.2 Gender Quotas and Policy Outcomes

6.2.1 Estimation Results

Tables 8, 9 and 10 present the estimates of Equations (1) (columns 1 to 3) and (2) (columns 4 and 5). The dependent variable in each regression is the average of the respective policy outcome over 2004 and 2005 in the pre-quota period, and over 2008 and 2009 in the after-quota period. Measures of the size of government are in logarithms, while measures of the composition of expenditures are the fraction of each voice of expenditures to total expenditures per capita.

Using the Akaike criterion and the goodness of fit test to select the most preferred models, I estimate that municipalities with quotas experienced between a 1% decrease and a 0.3% increase in expenditures per capita, and between a 3% and a 4.6% decrease in revenues per capita.[18] A null of zero effect can thus not be rejected, which is confirmed by Figure 4. Figure 4 plots the differences in the outcome variables against population, after and before the reform. A second-order polynomial is fit to the data on the two sides of the threshold. The same procedure is applied in Figures 5 and 6.

The coefficients of expenditure composition are in general small and not statistically significant, as is also shown in Figures 5 and 6. Two sets of estimates deserve further analysis. First, I find a positive effect of gender quotas on expenditures for social promotion when I estimate Equation (1) on the 0.10 bandwidth and Equation (2) on the 0.80 bandwidth for $N = \{2,3\}$. However, this result is not robust to alternative specifications, as shown in Table 9. Moreover, there is no evidence of a jump at the threshold in Figure 5; the estimated effect is thus likely to be driven by noisiness in the data. Second, I estimate a negative effect of gender quotas on the fraction of expenditures for the regulation of the production sector, in a subset of specifications. According to my estimates, the introduction of gender quotas caused a decrease in expenditures for the regulation of the production sector equal to 0.8 percentage points. Given that, in 2003, the expenditures per capita were on average equal to 935 euros, and that on average about 1% of the total expenditures is allocated to the regulation of the production sector, this coefficient translates into a negligible number in terms of euros per capita. It is also hard to detect a sizeable decrease in this category of expenditures from the Before-After RD graph in Figure 6.

Thus, I find no evidence that the introduction of gender quotas in Spain in 2007 has triggered any changes in the local public finance decisions. This result is different than the findings in

[18] The results are not precisely estimated, if not on the largest bandwidths, which should be used as a robustness check rather than to make causal statements on the relationship between two variables (see Lee and Lemieux 2010).

Chattopadhyay and Duflo (2004), but in line with the predictions of my paper. Chattopadhyay and Duflo (2004) studied the effect of reserving the most effective seat in Indian local politics , the one with executive power. [19], while the reform studied in this paper has no statistically significant effect on the percentage of women holding mayoral office. Moreover, female legislators could have more autonomy of decision over policy if their future political career did not depend on the agenda setter, who can decide to put them at the bottom of the list in the next elections; this suggests that the change in the gender composition of the legislature may have failed to improve the representation of female preferences because the closed list system attributes a great deal of authority to party leaders, and gender quotas do not limit their influence on policy-making. Moreover, possibly a critical percentage of women in the Municipal Council or among the decision-makers at the party level is necessary to change the informal rules that regulate politics in Spanish municipalities. Finally, party leaders in the 2007 municipal elections decided on the order of the candidates on the list so as to minimize the probability that a woman was elected, given the constraints imposed by gender quotas. This documents a discriminatory behavior toward female candidates (that can be either statistical or taste-based), and it shows that the male elite disposes of and uses its *de facto* power to dominate municipal politics, despite the introduction of gender quotas.

Beside these features of the institutional setting that made me predict no effect of gender quotas on policy, the difference between the result in this paper and that in Chattopadhyay and Duflo (2004) can be justified in two alternative ways. First, the socio-economic context of the policy change in this paper is different than that in Chattopadhyay and Duflo (2004), suggesting that there may be differences in terms of politicians' incentives and characteristics and in terms of the process of policy formation which, in Spain, may be better described by the "median voter model" than by the "citizens candidates model". If this hypothesis is true, the argument that gender quotas improve representation because male dominated legislative bodies do not properly represent women's points of view is not supported in the case of Spain. Second, preferences over policy across genders may be more aligned in Spain than in rural India.

While I am not able to rule out that the null effect is a "median voter" result, I can exclude that policy preferences in Spain are perfectly homogeneous across genders, because I document gender differences by looking at survey data on values and attitudes; the results of this analysis are shown in the next section. [20]

[19] Chattopadhyay and Duflo (2004) quote Kanago's (1998) statement that the position of Pradhan reserved to women in one third of the villages is "the only one that yields effective power".

[20] A further explanation for the null effect could be an issue of data quality, meaning that the classification of expenditures is too coarse to capture changes resulting from the gender composition of the Municipal Council. However, the rather broad categories used are dominated by a small set of areas of expenditures, as shown by the aggregated data at the national level. General expenditures consist of expenditures for the general administration for 85% of their value in municipalities below 20,000 inhabitants; social promotion only includes educational promotion and promotion of employment; 78% of the expenditures on infrastructure go to highways, roads and urban public roads; the different voices under social goods can be reconducted to mainly three categories of expenditures: health and education

6.2.2 Gender Preferences in Spain

I rely on two surveys that ask questions that can be directly or indirectly linked to policy outcomes. The first is the Spanish *Barometro*, which is administered by the *Centro de Investigationes Sociologicas* and covers 2,500 people across Spain concerning their opinions on various topics.

I use the responses to some of the questions in the June 2008 *Barometro* survey, and I find evidence of gender differences in preferences over specific issues. The first question asks respondents to rank the three problems that they considered to have the largest effect on Spain at the time of the questionnaire. Respondents were given 31 options. I compare the probabilities that women and men rank some of the issues proposed in the first place, selecting among those of the greatest concern to the majority of the people surveyed, and those where we would expect gender differences given the findings in the literature.[21] Table 11 shows that there are statistically significant gender differences in the degree of concern over quality of housing, healthcare and violence against women. Moreover, when asked what problem affects them most, men are significantly more likely than women to indicate "unemployment" (17% vs. 14%), while women are significantly more likely than men to answer "housing" (9% vs 11%). This may reflect the differences in how men and women spend their time. Respondents were asked to indicate their main working day activity from seven alternatives; the two-sample Kolmogorov-Smirnov test rejects the null of equality of the empirical distribution of the answers across gender with men being more likely to work, and women being more likely to be housewives and to look after children, a fairly standard division of roles in Southern European societies, and one that might explain women's greater concern about housing quality. Another remarkable difference emerges in the work-life balance. Respondents were asked to express their agreement with one of two statements: "Conciliation of work and family life is an issue that has to be solved between employers and employees, and where public intervention would be useless" and "Governments should adopt measures such that firms are sympathetic to work-family life balance issues". Women are significantly more likely to agree with the second statement (1 is agreement with the first statement; 2 agreement with the second; the average score for men is 1.72, while for women it is 1.78). Moreover, men favor more traditional divisions of tasks in the family, with a larger preference for models where one of the two parents works - or works more, and the other stays at home - or works less and dedicates his/her time to the children (on a scale of 1 to 3, where 1 is the most equal division and 3 the most unequal, men score 1.53 on average and women score 1.43).[22]

(14%), expenditures for housing and urbanism (30%) and interventions for the cleanliness, organization and beauty of towns (38%). Therefore, the functional division of expenditures should be narrow enough to capture the effect of changes in the gender composition of the Municipal Council. Nevertheless, I am currently working on collecting more disaggregated expenditure data.

[21] See Rehavi (2008) for a review of the findings on gender differences in policy preferences.

[22] All differences are statistically significant at 1%.

I also drew on the responses of Spanish women and men to the 2005-2008 edition of the World Value Survey.[23] One of the questions asked respondents to choose the first and second goal for Spain in the following ten years, from four alternatives. While the distribution of women's and men's opinion on the first goal was the same (see Table 12), a two sample Kolmogorov-Smirnov test shows statistically significant differences for the second ranked goal. These differences seem to mainly be driven by women's higher preferences for "try to make our cities more beautiful" (see Table 13). Also in answer to the question "If you had to choose, which one of the things on this card would you say is most important?", from the four choices, women are significantly more likely than men to choose "fighting inflation" (39% vs 34%). More generally, women and men display different preferences in relation to politics: women are statistically significantly less interested in politics, are significantly less likely to report participation in political activity, but have more confidence in women's movement, are more likely to think that discrimination against women is the most serious problem in the world (9% vs 13%) and are less likely to think that income is the most important characteristic of a good job (32% vs 27%).

7 Further Discussion of the Identification Strategy

The RDD estimates on the reduced sample have a causal interpretation, if there are no not-accounted for differences across municipalities on either side of the threshold, which are correlated with the outcome being explained. McCrary (2008) shows that this is equivalent to requiring that the forcing variable is not discontinuous at the cutoff as a consequence of manipulation, i.e. that every town has the same ex-ante probability of being on one side or the other of the cutoff.

In my two-period setting, this condition can be relaxed, as it is sufficient to require that the ex-ante probability of being at either side of the cutoff does not change differentially across municipalities above and below the threshold in 2006.

McCrary (2008) proposes a test for the plausibility of the assumption of non-manipulation. The idea behind this test is that continuity of the conditional density of the running variable X at the cutoff implies continuity of the density of X, under the assumption of monotonicity of the benefits from the treatment.

Figure 7 shows the result of the McCrary test with the discontinuity at 5,000; the test is run on municipalities with a population below the 99th percentile. Using a bandwidth equal to 1,000 inhabitants to fit the polynomial and bins of 200 inhabitants, I cannot reject the null of no discontinuity at the threshold. However, this result is not robust to alternative specifications, as shown in Figure 8, which uses a bandwidth equal to 2,000. Larger bandwidths produce similar results

[23] The survey was conducted in 2007 in Spain.

to those depicted in Figure 8. Thus, the McCrary test is not conclusive. Moreover, the presence of the discontinuity is not sufficient to claim that there was manipulation at the threshold, because a precisely estimated jump may result randomly from noisy data. However, when I run the McCrary test on different cutoffs (points along a grid with a minimum value equal to 200, a maximum value equal to 10,000 and delta equal to 200) there are only four points where I estimate a statistically significant difference (see Figure 9). This suggests that it is unlikely that the discontinuity estimated at 5,000 is random. [24]

If there were manipulation, it should be noted that it is not related to the application of the legislated quota, which was approved in March 2007, while the data refer to the population in January 2006. The discontinuity indeed appears also if I run the McCrary test on 2002 population data (see Figure 7 and Figure 8).[25] This is relevant because, as anticipated, the condition that needs to be satisfied in a dynamic setting is that the discontinuity does not change at the time when the quota is introduced. Table 14 shows that the population data I use for my analysis satisfy this requirement.[26]

As a further check of the identification strategy, I run placebo regressions where I estimate Equations (1) and (2) on data for 2001-2002 (before) and 2005-2006 (after). There is no evidence of systematic differential trends across municipalities above and below the threshold before gender quotas were introduced. While some estimates could point toward a (non robust) positive effect of being above the threshold on expenditures and revenues per capita, before-after RD graphs do not corroborate this result (tables and graphs are available upon request).

Finally, in the spirit of randomized experiments, I test whether covariates are balanced at either side of the threshold within the 20% bandwidth, choosing variables and time-lags depending on data availability. Table 15 shows that there are no statistically significant differences in the means of these covariates.

Overall, while the reliability of my empirical strategy depends on a fundamentally untestable assumption, the tests presented in this section, and knowledge of the particular institutional context allow me to argue that I have estimated the causal effect of gender quotas on the election of female

[24] I contacted the Spanish Statistical Office to ask for a clarification of this observed discontinuity. In addition to ruling out manipulation of data by Municipal Councils, the Spanish Statistical Office provided detailed documents describing the procedure it follows before population data are declared to be official. However, some manipulation could occur completely within the rules if, for instance, a mayor were to encourage relatives and/or friends to strategically change their place of residence. For instance, it was reported in the newspaper ElPais on April 1th 2011, that the mayor of a town in the *comarca* of Xinzo had admitted that some citizens had moved their place of residence to his town for electoral purposes.

[25] It is unlikely that mayors or Municipal councilors manipulate population data for their municipality to qualify for gender quotas, given the evidence in this paper on the strategies used by parties to limit the effects of the quotas.

[26] I assume that the covariance of the estimated discontinuities after and before the reform is equal to 0; under this assumption I derive an estimate for the standard error of the difference between the estimated discontinuities, but it is likely that this estimate is upward biased.

politicians and public finance decisions.

8 Conclusion

In this paper, I used a Before-After RD design to estimate the effect of legislated candidate gender quotas on the election of female politicians and on public finance decisions in Spanish municipalities.

The identification strategy is based on the application of a double-quota only in municipalities with more than 5,000 inhabitants in the 2007 municipal elections. I conducted a graphical and regression based analysis of the effect of gender quotas on the outcomes of interest.

According to my estimates, gender quotas caused an increase in female councilors in Spain of 6 percentage points, while the increase in female candidates was 8-9 percentage points. Gender quotas had no significant effect on the percentage of female mayors.

The difference between the increase in the percentage of female candidates and that in the percentage of female councilors is due to strategic positioning. I find that men are significantly more likely to run in the first or second of every five positions wherever quotas are applied, while they are simply more likely to be in the first and second position if there are no legislated gender quotas.

I estimate a null effect of legislated gender quotas on local public finance decisions, both in terms of the size of the local government and the composition of expenditures. This could be interpreted as a Median Voter result. Alternatively, I would advance the hypothesis that gender quotas do not cause a shift in priorities in municipal-level policy, because they do not cause a relevant increase in the percentage of women in municipal councils, a legislature that makes decisions based on majority rule. Moreover, candidate quotas do not increase the percentage of women that achieve the position of mayor, who has agenda setting power and makes a crucial input to decision making. Another likely explanation is that, being responsible for the formation of the closed lists, party leaders impose party discipline in decisions over policy, as they can credibly punish legislators who deviate, ending their political carrier in the next elections.

Overall, the results in this paper warn against seeing gender quotas as a "panacea" for the low participation of women in politics. One of the main arguments for increasing female participation in politics is that the preferences of women, who constitute 50% of the electorate, are not adequately represented in male-dominated legislatures. However, while gender quotas that reserve important offices have proven successful in improving the representation of female preferences, the evidence from this paper shows that legislated candidate gender quotas cannot achieve as good a result. If the goal of the introduction of gender quotas is a better representation of female preferences, a stronger focus on the percentage of women elected and the number of female mayors,

rather than simply on female candidates, is needed; moreover, it would be relevant to limit the ability of the (usually male) elite of party leaders to influence policy decisions made by elected legislators, particularly in a proportional system with a closed list, which is the norm in Spain, where it is not the voter but the party leader who decides who will govern.

An understanding of the effect of candidate gender quotas on female promotion to legislative and executive roles and on policy outcomes is fundamental, to balance the benefits and costs of gender quotas themselves in a rigorous way, and indirectly to understand the evolution of a society where, although slowly, women are catching up in terms of accessing roles of responsibility.

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A Appendix

A.1 Figures and Tables

Figure 1: ***Difference between after-reform and pre-reform percentage of female councilors by population: local averages and polynomial fit.***

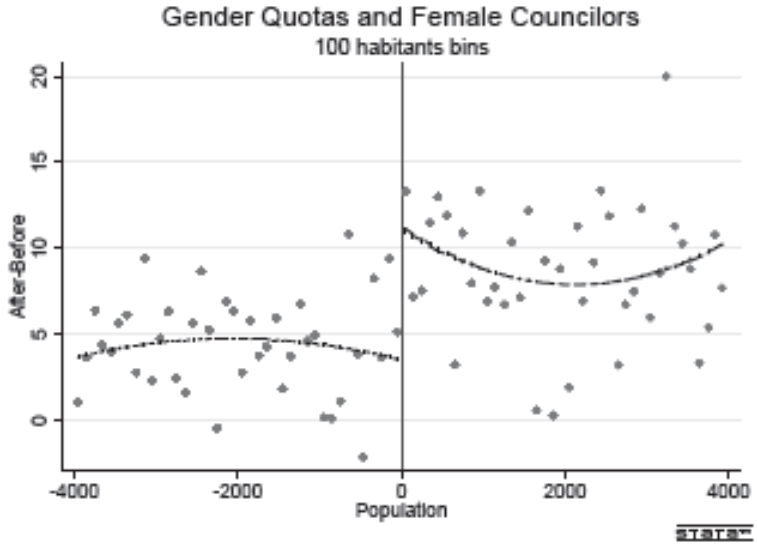

Figure 2: ***Difference between after-reform and pre-reform percentage of female candidates by population: local averages and polynomial fit.***

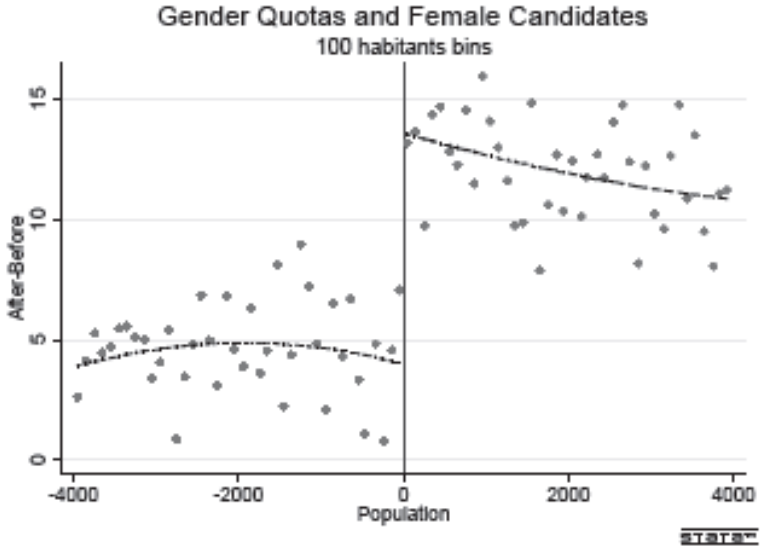

Figure 3: ***Difference between after-reform and pre-reform probability of female mayors by population: local averages and polynomial fit.***

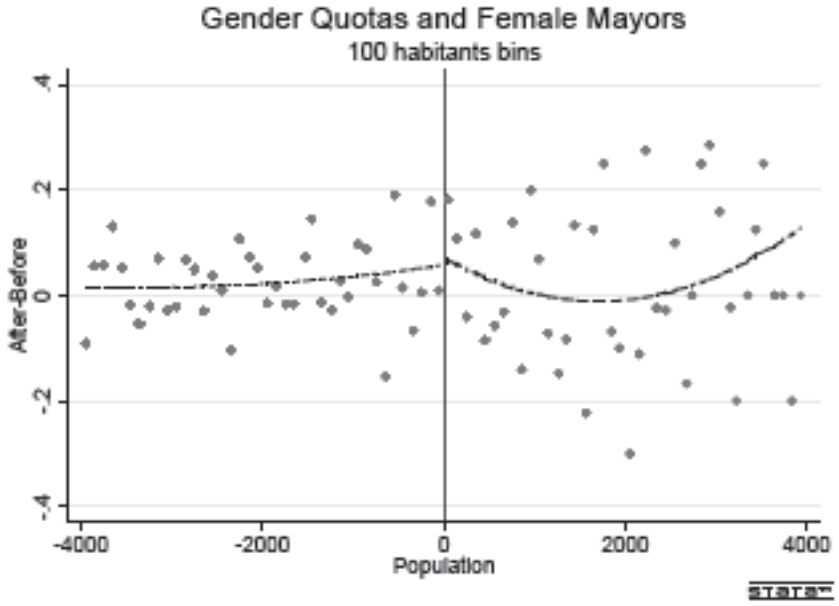

Figure 4: ***Difference between after-reform and pre-reform measures of size of local government by population: local averages and polynomial fit.***

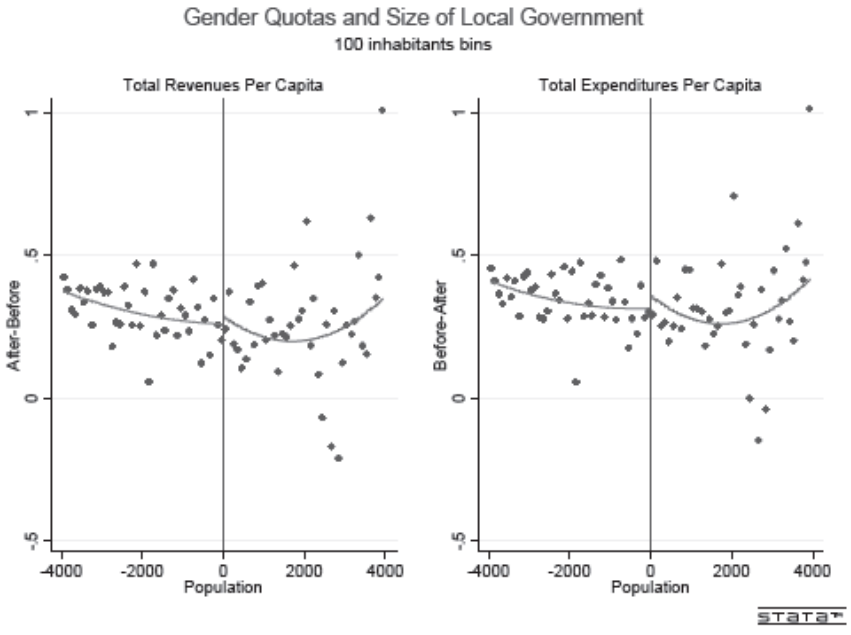

Figure 5: ***Difference between after-reform and pre-reform local government composition by population: local averages and polynomial fit.***

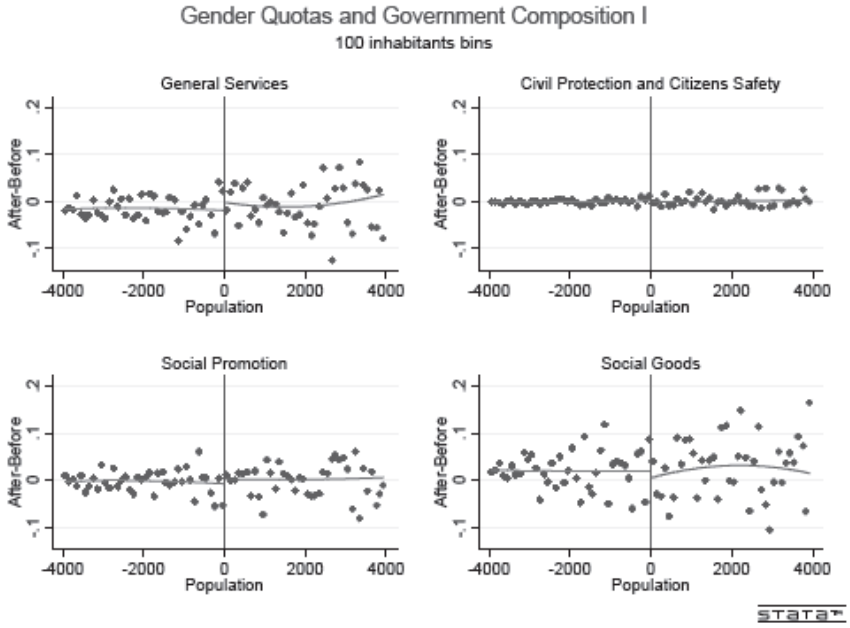

Figure 6: ***Difference between after-reform and pre-reform local government composition by population: local averages and polynomial fit.***

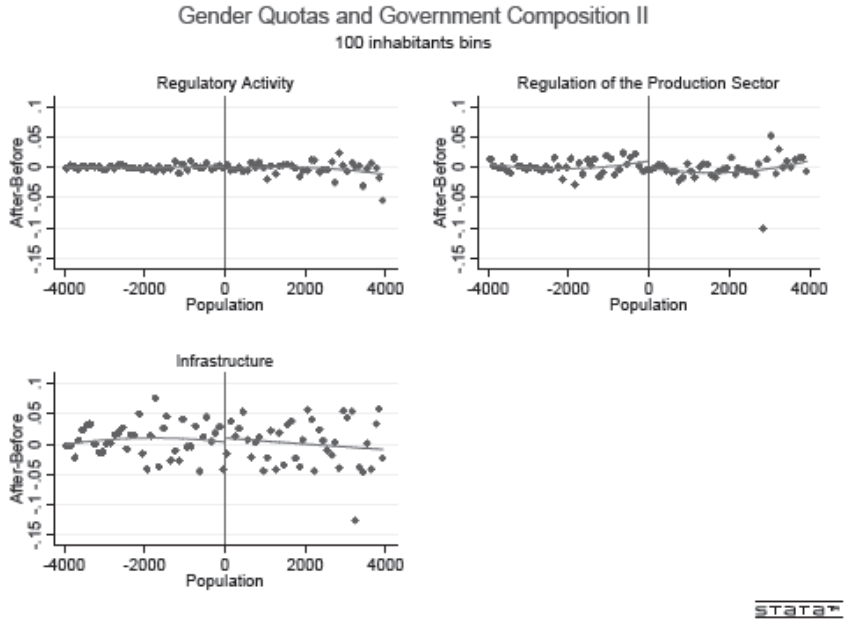

Figure 7: ***Estimated density of the running variable after and before the reform.***

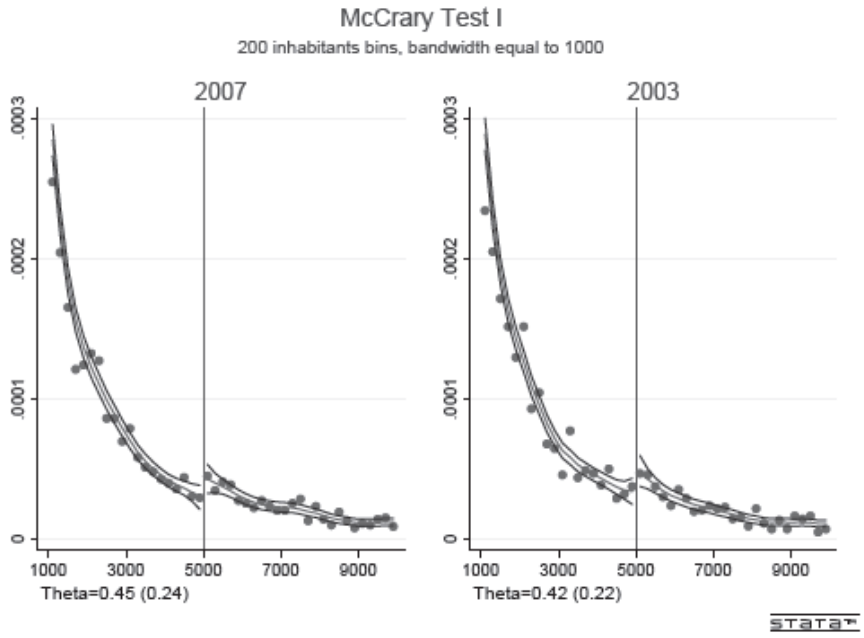

Figure 8: ***Estimated density of the running variable after and before the reform.***

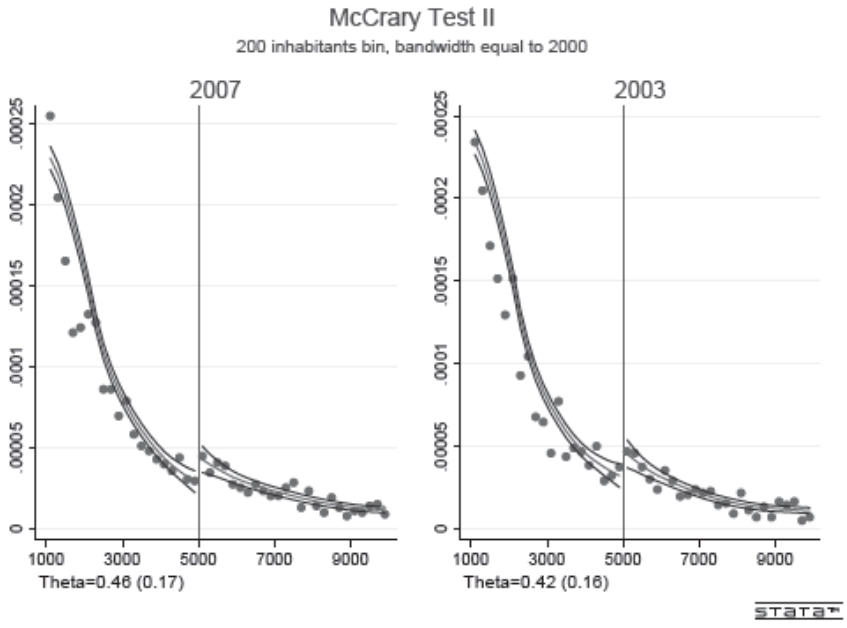

Figure 9: ***T-Statistics from McCrary Test and cutoff: the test estimates the discontinuity of the density at each hypothetical cutoff.***

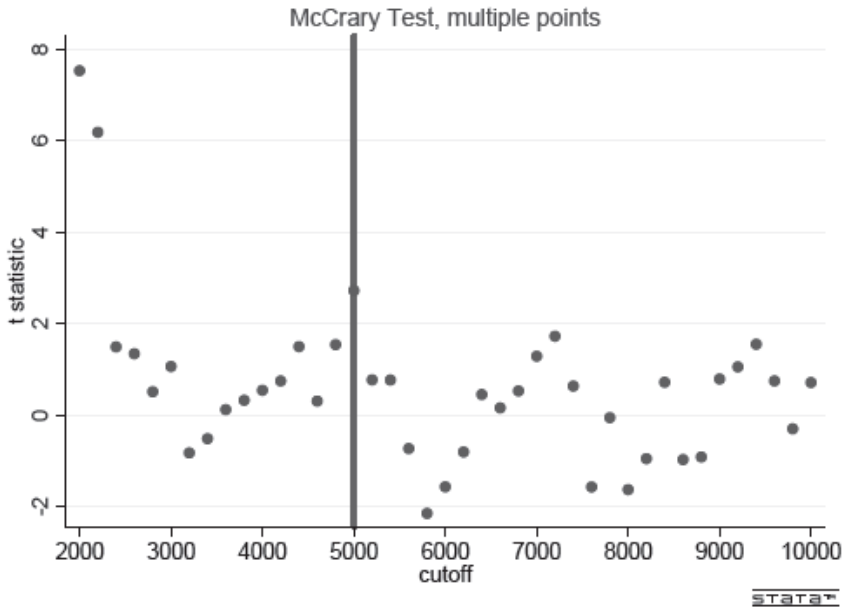

Table 2: ***Summary Statistics, Full and Restricted Sample***

	Pre-Quotas		**After-Quotas**	
Bandwidth	1.00	0.20	1.00	0.20
population	7861	4934	8492	4964
	(54719)	(556)	(57312)	(575)
% fem councilors	26	29	31	35
	(14)	(12)	(15)	(11)
% fem candidates	30	33	36	42
	(11)	(8)	(11)	(7)
% fem mayor	0.13	0.12	0.15	0.16
	(0.34)	(0.32)	(0.36)	(0.37)
N	5538	385	5502	381
total revenues per capita	990	1046	945	1180
	(674)	(492)	(642)	(414)
total expenditure per capita	935	880	1328	1174
	575	419	634	401
general services	0.23	0.18	0.21	0.17
	(0.14)	(0.10)	(0.13)	(0.10)
civil protection and citizens' safety	0.02	0.04	0.02	0.04
	(0.03)	(0.02)	(0.03)	(0.03)
social promotion	0.10	0.13	0.09	0.13
	(0.08)	(0.08)	(0.07)	(0.08)
social goods	0.45	0.45	0.48	0.47
	(0.17)	(0.14)	(0.17)	(0.14)
infrastructure	0.13	0.10	0.12	0.11
	(0.13)	(0.10)	(0.13)	(0.10)
regulatory activity	0.02	0.02	0.01	0.02
	(0.03)	(0.02)	(0.03)	(0.03)
regulation of the production sector	0.01	0.02	0.01	0.02
	(0.03)	(0.03)	(0.04)	(0.03)
N	5219	379	5161	371

Standard Errors in parenthesis

Table 3: ***Gender Quotas and Female Councilors***

	(1)	(2)	(3)	(4)	(5)
Bandwidth	0.80	0.20	0.10	0.80	0.10
	Two polynomials			Four polynomials	
Polynomial					
One	4.30***	5.99***	5.23**	4.56***	-0.54
	(0.63	(1.42)	(2.13)	(1.30)	(4.18)
Two	4.30***	6.04***	5.49**	6.90***	
	(0.62)	(1.43)	(2.13)	(2.03)	
Three	4.31***	5.95***	5.76***	4.68*	
	(0.62)	(1.43)	(2.12)	(2.82)	
Four	4.31***	6.03***	5.85***	3.54	
	(0.62)	(1.43)	(2.11)	(3.59)	
Observations	4,887	766	385	4,887	385
Optimal order of the polynomial	4	1	4	4	
Models rejected by the goodness of fit test	1-2-3-4	2	–	1-2-3-4	–

*** p<0.01, ** p<0.05, * p<0.1
Standard Errors clustered by municipality

Table 4: ***Gender Quotas and Female Candidates***

	(1)	(2)	(3)	(4)	(5)
Bandwidth	0.80	0.20	0.10	0.80	0.10
	Two polynomials			Four polynomials	
Polynomial					
One	7.59***	8.76***	9.28***	8.01***	6.82***
	(0.40)	(0.83)	(1.24)	(0.81)	(1.54)
Two	7.59***	8.81***	9.57***	8.88***	
	(0.40)	(0.84)	(1.24)	(1.24)	
Three	7.58***	8.80***	9.77***	9.23***	
	(0.40)	(0.84)	(1.24)	(1.75)	
Four	7.58***	8.89***	9.77***	9.43***	
	(0.40)	(0.84)	(1.24)	(2.30)	
Sample Avg Before	31.31	32.98	33.52	31.31	33.52
Observations	4,887	766	385	4,887	385
Optimal order of the polynomial	1	4	1	1	
Models rejected by the goodness of fit test	–	2	1	–	1

*** p<0.01, ** p<0.05, * p<0.1
Standard Errors clustered by municipality

Table 5: ***Positions in Electoral Lists, Municipalities with Quotas***

Position	Women	Men	Diff	p-value
1	0.04	0.14	-0.10	0.00
2	0.09	0.11	-0.02	0.00
3	0.10	0.10	0.00	0.08
4	0.12	0.09	0.03	0.00
5	0.13	0.07	0.06	0.00
6	0.08	0.12	-0.04	0.00
7	0.09	0.11	-0.02	0.00
8	0.11	0.09	0.02	0.00
9	0.11	0.09	0.02	0.00
10	0.13	0.08	0.05	0.00

P-values for the test of equality of means

Table 6: ***Positions in Electoral Lists by Party, Municipalities with Quotas***

PSOE	Difference	p-value	PP	Difference	p-value
1	-0.10	0.00	1	-0.08	0.00
2	0.02	0.00	2	-0.02	0.00
3	0.00	0.55	3	0.01	0.05
4	0.02	0.00	4	0.02	0.00
5	0.01	0.21	5	0.05	0.00
6	-0.01	0.08	6	-0.04	0.00
7	-0.02	0.01	7	-0.03	0.00
8	0.02	0.00	8	0.01	0.00
9	0.00	0.55	9	0.02	0.01
10	0.03	0.00	10	0.06	0.00

P-values for the test of equality of means

Table 7: ***Gender Quotas and Female Mayors***

	(1)	(2)	(3)	(4)	(5)
Bandwidth	0.80	0.20	0.10	0.80	0.10
Polynomial					
First	0.00	-0.01	0.02	-0.00	0.06
	(0.02)	(0.06)	(0.08)	(0.05)	(0.16)
Second	0.00	-0.01	0.02	0.03	
	(0.02)	(0.06)	(0.08)	(0.08)	
Third	0.00	-0.01	0.03	0.05	
	(0.02)	(0.06)	(0.08)	(0.10)	
Fourth	0.00	-0.01	0.03	0.12	
	(0.02)	(0.06)	(0.08)	(0.13)	
Observations	3,871	598	300	3871	300
Optimal order of the polynomial	1	1	1	1	
Models rejected by the goodness of fit test	1-2-3-4	–	–	1-2-3-4	–

*** p<0.01, ** p<0.05, * p<0.1

Standard Errors are clustered by municipality

Table 8: ***Gender Quotas and Size of Government***

	(1)	(2)	(3)	(4)	(5)
Bandwidth	0.80	0.20	0.10	0.80	0.10
	Two polynomials			Four polynomials	
	Expenditures per capita (logarithms)				
Polynomial					
First	-0.050***	-0.008	-0.011	-0.016	0.007
	(0.018)	(0.047)	(0.077)	(0.042)	(0.138)
Second	-0.050***	-0.006	-0.001	0.016	
	(0.018)	(0.047)	(0.078)	(0.070)	
Third	-0.051***	-0.002	-0.008	-0.023	
	(0.018)	(0.047)	(0.078)	(0.098)	
Fourth	-0.051***	0.003	-0.010	0.032	
	(0.018)	(0.048)	(0.078)	(0.119)	
Optimal order of the polynomial	4	4	1	4	
Models rejected	1-2-3-4	–	–	1-2-3-4	–
	Revenues per capita (logarithms)				
Polynomial					
First	-0.074***	-0.030	-0.046	-0.038	0.038
	(0.019)	(0.049)	(0.080)	(0.044)	(0.139)
Second	-0.074***	-0.028	-0.038	-0.003	
	(0.019)	(0.049)	(0.081)	(0.074)	
Third	-0.075***	-0.025	-0.047	-0.060	
	(0.019)	(0.050)	(0.081)	(0.010)	
Fourth	-0.075***	-0.018	-0.051	0.011	
	(0.019)	(0.050)	(0.081)	(0.120)	
Optimal order of the polynomial	4	1	1	4	
Models rejected	1-2-3-4	–	–	1-2-3-4	–
Observations	4,028	656	328	4,028	328

*** p<0.01, ** p<0.05, * p<0.1

Standar errors clustered by municipality

Table 9: ***Gender Quotas and Government Composition I***

Bandwidth	0.80	0.20	0.10	0.80	0.10
	Two polynomials			Four polynomials	
Polynomial					
			General Services		
First	0.006	0.011	0.006	0.005	-0.037
	(0.005)	(0.014)	(0.019)	(0.012)	(0.041)
Second	0.006	0.011	0.006	0.010	
	(0.005)	(0.014)	(0.019)	(0.019)	
Third	0.006	0.013	0.006	0.008	
	(0.005)	(0.014)	(0.019)	(0.027)	
Fourth	0.006	0.012	0.006	-0.027	
	(0.005)	(0.014)	(0.019)	(0.034)	
Optimal order of the polynomial	4	4	1	4	
Models rejected	1-2-3-4	–	–	1-2-3-4	–
			Civil Protection and Citizens Safety		
First	0.001	-0.003	-0.006	-0.003	-0.010
	(0.001)	(0.004)	(0.007)	(0.003)	(0.016)
Second	0.001	-0.003	-0.005	-0.004	
	(0.001)	(0.004)	(0.007)	(0.006)	
Third	0.001	-0.003	-0.005	-0.004	
	(0.001)	(0.004)	(0.007)	(0.006)	
Fourth	0.001	-0.003	-0.005	-0.014	
	(0.001)	(0.004)	(0.007)	(0.012)	
Optimal order of the polynomial	1	1	1	1	
Models rejected	1-2-4	–	–	1-2-4	–
			Social Promotion		
First	0.004	0.004	0.031**	0.011	0.049
	(0.004)	(0.010)	(0.015)	(0.008)	(0.031)
Second	0.004	0.005	0.032**	0.015*	
	(0.004)	(0.010)	(0.015)	(0.014)	
Third	0.004	0.004	0.033**	0.043**	
	(0.004)	(0.010)	(0.015)	(0.020)	
Fourth	0.004	0.004	0.033**	0.041	
	(0.004)	(0.010)	(0.015)	(0.025)	
Optimal order of the polynomial	4	1	1	4	
Models rejected	1-2-3-4	–		1-2-3	–
			Social Goods		
First	0.002	-0.008	-0.024	-0.011	-0.012
	(0.007)	(0.019)	(0.028)	(0.016)	(0.060)
Second	0.002	-0.008	-0.023	-0.014	
	(0.007)	(0.019)	(0.029)	(0.027)	
Third	0.003	-0.009	-0.021	-0.036	
	(0.007)	(0.019)	(0.029)	(0.039)	
Fourth	0.003	-0.007	-0.022	-0.008	
	(0.007)	(0.019)	(0.029)	(0.050)	
Optimal order of the polynomial	4	4	1	4	
Models rejected	1-2-3-4	–	–	1-2-3-4	–
Observations					

*** p<0.01, ** p<0.05, * p<0.1

Standar errors clustered by municipality

Table 10: ***Gender Quotas and Government Composition II***

Bandwidth	0.80	0.20	0.10	0.80	0.10
	Two polynomials			Four polynomials	
Polynomial					
			Infrastructure		
First	-0.002	0.004	0.005	0.001	-0.003
	(0.005)	(0.012)	(0.019)	(0.011)	(0.036)
Second	-0.002	0.003	0.006	0.005	
	(0.005)	(0.012)	(0.019)	(0.017)	
Third	-0.002	0.003	0.003	-0.000	
	(0.005)	(0.012)	(0.019)	(0.025)	
Fourth	-0.002	0.002	0.003	0.010	
	(0.005)	(0.012)	(0.019)	(0.032)	
Optimal order of the polynomial	4	4	1	4	
Models rejected	1-2-3-4	–	–	1-2-3-4	–
			Regulatory Activity		
First	-0.002	-0.002	-0.004	-0.001	0.004
	(0.001)	(0.003)	(0.004)	(0.002)	(0.007)
Second	-0.002	-0.002	-0.003	-0.005	
	(0.001)	(0.003)	(0.004)	(0.004)	
Third	-0.001	-0.002	-0.003	-0.002	
	(0.001)	(0.003)	(0.004)	(0.005)	
Fourth	-0.002	-0.002	-0.003	-0.002	
	(0.001)	(0.003)	(0.004)	(0.006)	
Optimal order of the polynomial	4	4	4	4	
Models rejected	1-2-3-4	–	–	1-2-3-4	–
			Regulation of the production sector		
First	-0.003**	-0.008*	-0.005	-0.007*	0.010
	(0.002)	(0.004)	(0.007)	(0.004)	(0.013)
Second	-0.003**	-0.008*	-0.008	-0.009	
	(0.002)	(0.004)	(0.008)	(0.006)	
Third	-0.004**	-0.008*	-0.008	-0.001	
	(0.002)	(0.004)	(0.008)	(0.009)	
Fourth	-0.004**	-0.008*	-0.008	-0.004	
	(0.002)	(0.004)	(0.008)	(0.011)	
Optimal order of the polynomial	4	4	4	2	
Models rejected	1-2-3-4	–	3-4	1-2-3-4	–
Observations	4,028	656	328	4,028	328

*** p<0.01, ** p<0.05, * p<0.1

Standar errors clustered by municipality

Table 11: ***What is currently the most serious issue for Spain?***

	Men	Women	p-value
Economy	0.33	0.34	0.64
Unemployment	0.29	0.28	0.77
Immigration	0.09	0.08	0.14
Housing	0.05	0.08	0.03
Terrorism	0.07	0.05	0.02
Education	0.01	0.01	0.59
Health	0.00	0.01	0.10
Violence Against Women	0.00	0.01	0.02
N	1173	1206	

Table 12: ***People sometimes talk about what the aims of this country should be for the next ten years. Would you please say which one of these you consider the most important? First choice***

	Men	Women	p-value
economic growth	0.46	0.45	0.72
strong defense force	0.09	0.08	0.77
people have more say	0.38	0.38	0.94
cities more beautiful	0.07	0.08	0.26
N	582	580	

Table 13: ***People sometimes talk about what the aims of this country should be for the next ten years. Would you please say which one of these you consider the most important? Second choice***

	Men	Women	p-value
economic growth	0.26	0.24	0.43
strong defense force	0.13	0.19	0.14
people have more say	0.36	0.34	0.43
cities more beautiful	0.25	0.32	0.01
N	582	580	

Table 14: ***McCrary Test, Difference in Discontinuities***

	2007	2003	Diff
Discontinuity	0.46	0.42	0.04
Standard Error	0.17	0.16	0.23

Bandwidth size is 2000, 200 inhabitants bins

Table 15: ***Test of Balanced Covariates, 0.20 Bandwidth***

Dep. Var.	Coeff	Mean
% pop change 04-09	-1.62 (0.64)	11.39
% pop change 96-01	-2.07 (0.55)	5.59
% pop change 91-96	4.90 (0.41)	9.22
pop change 04-09 (Spanish) 04-09	-25.17 (0.83)	251.07
pop change (Foreigners) 04-09	-39.83 (0.43)	243.98
annual pop growth rate 04-09	-0.30 (0.60)	2.01
annual pop growth rate 96-01	-0.47 (0.45)	0.91
annual pop growth rate 91-96	0.42 (0.53)	1.35
% registered unemployed 06	0.36 (0.32)	4.11
% registered unemployed 05	0.42 (0.26)	4.18
% registered unemployed 04	0.42 (0.19)	3.47
% registered unemployed 03	0.27 (0.37)	3.58
% registered unemployed 02	0.16 (0.58)	3.30
% registered unemployed 01	0.13 (0.63)	3.11
% registered unemployed 00	0.27 (0.36)	3.22
unemployment rate men 2001	0.46 (0.34)	6.87
unemployment rate women 2001	0.34 (0.56)	6.93
unemployment rate (15-24) 2001	0.80 (0.21)	8.02
unemployment rate (25-49) 2001	0.75 (0.35)	11.39
unemployment rate (+50) 2001	0.23 (0.53)	4.45
banks offices 2006	0.14 (0.72)	4.66
% change industrial activity 04-09	-2.99 (0.39)	3.4
% change commercial activity 04-09	-6.33 (0.34)	16.68
% change small commercial activity 04-09	-8.91 (0.41)	-6.19
voters 2007	8.90 (0.90)	2283.76
white ballots 2007	-3.92 (0.50)	47.25
null ballots 2007	11.36 (0.56)	87.60

Coefficients from second-order polynomial regression of Dep. Var. on Quota, p-values in parenthesis.

Chapter 4

Are Attitudes Endogenous to Political Regimes? Beliefs about Working Women in State-Socialist Countries*

1 Introduction

Gender attitudes are important determinants of labor market outcomes. For instance, Fortin (2009) shows a substantial effect of culture on an individual's decision to join the labor market in the United States. In this vein, exploiting the evolution of cultural attitudes over time within country as well as across countries, Giavazzi, Schiantarelli, and Serafinelli (2013) show that attitudes towards gender influence the employment rates of women and hours worked.

Despite the evidence that beliefs and values matter for labor market outcomes, very little is known about their development and evolution. Fortin (2009) shows, mostly using the 1972-2006 General Social Survey, that there are large cohort effects: U.S. women tend to become more outward oriented, but the progression stalled in the mid-1990s when the AIDS crisis peaked. Giavazzi, Schiantarelli, and Serafinelli (2013) also observe an evolution in attitudes towards the role of women in the labor market over the last twenty-five years in a panel of OECD countries. However, the identity and relative importance of alternative mechanisms that can account for this evolution are still unclear. Specifically, it is unknown to what extent political regimes and social policies affect gender attitudes.

In this paper, we employ a Difference-in-Differences analysis that compares attitudes of individuals in Central and Eastern European countries (CEECs) and in the rest of Europe, before and

*This paper is coauthored with Michel Serafinelli, University of California Berkeley. We thank Audinga Baltrunaite, David Stromberg, Jan deVries, GérardRoland and Fabio Schiantarelli for helpful discussions and comments on the issues at stake.

after the advent of state socialism in CEECs.[1]

The main attitude variables that we investigate are those concerning the role of women in the labor force. We maintain that the state-socialist experience in CEECs constitutes a quasi-experimental setting that can be exploited to study whether attitudes are endogenous to policy regimes. The argument for employing this strategy runs as follows. At the end of World War II, the CEECs under analysis were occupied by the Red Army. The Soviets retained a direct involvement in the internal affairs of these countries after the end of the war. Through a series of coalition governments including Communist parties, and then a forced liquidation of coalition members who were unliked by the Soviets, Stalinist systems were established in each country. Stalinists then gained control of existing governments, police, press and radio outlets in these countries. Therefore, the advent of state-socialism can be seen as an event exogenous to the evolution in gender attitudes. Prior to the imposition of the new political and economic regime, as we shall show, the attitudes of interest in state-socialist countries and the rest of Europe were quite similar.

Between the late 1940s and the early 1990s, state-socialist governments strongly encouraged women's paid employment outside the home (de Haan, 2012). The state-socialist governments used the wage system (keeping the wages low, thus requiring two-income earners to make a reasonable family income) as well as centralized political pressure and propaganda to their ends. Against this background, we study the influence of political regimes and social policies on individual attitudes on gender roles.

Our research involves using various data sets, such as the General Social Survey (henceforth GSS) and the World Value Survey (henceforth WVS). We cope with the lack of survey data on gender attitudes before the advent of state-socialism in CEECs using attitudes inherited by second, third and fourth generation immigrants in the USA from the GSS. The use of attitudes inherited by immigrants in the USA allows us to go back in time, proxying the gender attitudes of people living in country c at time t with the gender attitudes of the descendants of immigrants who arrived in the US from country c at time t. (Algan and Cahuc, 2010)

The state-socialist countries that we consider in our core analysis are: Czechoslovakia, East Germany, Hungary, Lithuania, Poland and Romania.[2] Our results suggest a significant difference in the evolution of attitudes between Europeans in state-socialist countries and other Europeans during the period 1947-1991. We perform several tests and show that the results do not appear to be driven by a non-random selection of immigrants. Central and Eastern Europeans that formed their attitudes during the state-socialist regime seem more likely to hold progressive beliefs regarding gender roles in the labor market. In particular, we consider the statements: *Do you approve or*

[1] We use the rest of Europe as the control in order to account for a general trend in gender attitudes that might have been in place, for instance following WWII (Fernández, Fogli, and Olivetti, 2004).

[2] In some specification we shall also include Yugoslavia, which was a Soviet satellite between 1943 and 1948.

disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?, and *Do you agree or disagree: Husband and wife should both contribute to income.* Having experienced state-socialism increases the probability of approving/agreeing by approximately 5 percentage points (6%) and 13 percentage points (18%), respectively. We interpret this as evidence that the political and economic regime in state-socialist countries exerted a noticeable influence on people's beliefs about the appropriateness of a specialization of male and female roles.[3] Overall, we interpret the evidence in this paper as suggestive of the political and economic regime in state-socialist countries exerting a noticeable influence on people's beliefs about the appropriateness of a specialization of male and female roles. No significant difference between CEECs and the rest of Europe is found in the evolution of beliefs about the compatibility of work and motherhood using the GSS. The remainder of this paper is organized as follows. Section 2 discusses the related literature. Section 3 discusses some background to the state-socialist regime in satellite states. Section 4 describes the measures of attitudes and data. Section 5 discusses the econometric model. Section 6 presents the findings and Section 7 concludes the paper and lists directions for future research.

2 Relation to Previous Research

This paper adds to a growing literature on attitudes and labor market outcomes. The body of work on the evolution and transmission of attitudes toward gender roles is the most closely related to this paper. The paper by Fortin (2009) mentioned in the introduction is a major contribution. Fernández (2013) develops a dynamic model of culture where individuals hold heterogeneous beliefs regarding the relative long-run payoffs for women who work in the market versus in their home. These beliefs evolve rationally via an intergenerational learning process. Women are assumed to learn about the long-term payoffs of working by observing (noisy) private and public signals. They then make a work decision. This process generically generates an S-shaped figure for female labor force participation, which is what is found in the data. The S-shape results from the dynamics of learning. When either small or large shares of women work, learning is very slow and the changes in female labor force participation are also small. When the proportion of working women is close to 50%, rapid learning and rapid changes in female LFP take place.[4] Fernández, Fogli, and

[3] We also look at the statement: *It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of home and family*. The estimates return a positive and economically significant effect of state-socialism on the disagreement with this statement. While in principle this is consistent with the other evidence on the evolution of beliefs about gender roles, in practice the standard errors are very large and prevent us from drawing any definitive conclusions.

[4] The author calibrates the model to several key statistics and shows that it does a very good job in replicating the quantitative evolution of female labor force participation in the US over the last 120 years.

Olivetti (2004) focus on the role of the family in the transmission of attitudes. In particular, using GSS data, they show that the working status of the husband's mother affects the employment status of the wife. Using US census data for various years from the 1940s to the 1980s, they also show that the male mobilization rate in WWII has a positive effect on women's employment status in later years.

We contribute to this literature by analyzing whether and how political regimes contribute to shaping the attitudes towards gender roles. Our findings concerning the evolution of attitudes on the appropriateness of specialization of male and female roles are similar to those of Bauernschuster and Rainer (2011) who exploit the German separation and five waves of ALLBUS, the German equivalent to the GSS, during the period 1991-2008. During the years when the country was divided, East German institutions encouraged female employment, while the West German system deterred women, in particular mothers, from full-time employment. Their estimates show that East Germans are significantly more likely to hold egalitarian sex-role attitudes than West Germans. Their estimates also show a significant difference in the evolution of beliefs about the compatibility of work and motherhood, something we do not find in our comparison of CEECs and the rest of Europe.

Our paper also contributes to a more general research program on culture and economics. There are two strands in this literature: one emphasizes the persistence and slow moving nature of cultural traits and how they often originate in political economy and technological features of the distant past (Tabellini, 2010; Alesina, Giuliano, and Nunn, 2013; Durante, 2009). Another emphasizes how cultural attitudes can change relatively quickly in response to changes in economic opportunities, technology and institutions (Di Tella, Galiant, and Schargrodsky, 2007; Giuliano and Spilimbergo, 2009). Our paper contributes to this debate by showing that attitudes are not set in stone and evolve with the policy regime experienced directly or indirectly, through intra-family transmission.

Further, our work is related to a literature analyzing the effect of state-socialism in shaping individual attitudes. We summarized the study by Bauernschuster and Rainer (2011) above. Blanchflower and Freeman (1997) focus on the legacy of communism on individual attitudes and on how this changed during the transition to a market system. Looking at opinion polls over the years 1987-1993, they find substantial differences between former communist countries and western countries: citizens in CEECs turned out to be more egalitarian, more satisfied with their jobs and more in favour of state intervention in the economy, compared to their western counterparts. Fuchs-Schundeln and Alesina (2007) exploit the German separation to analyze the evolution of attitudes towards redistribution.

Finally, our paper relates to a body of work that has noted and exploited the correlation between the behavior of immigrants and that of residents in the country of origin. For instance, Giuliano

(2007) documents and studies the similarity in the living arrangements of children of immigrants to those in the country of origin. Fernández (2007) uses both female labor force participation and attitudes in the women's country of origin as cultural proxies, and shows that both proxies have significant effects on women's work outcomes. Antecol (2000) also uses such an epidemiological approach. Algan and Cahuc (2010) use the attitudes of American immigrants towards trust to study the effect of trust on the growth rate of a country's per capita income in the long run. Fernandez and Fogli (2009) analyze fertility outcomes and labor market outcomes for US women, and instrument culture with past female labor force participation and total fertility rates in the woman's country of origin.

3 Institutional Background

This Section discusses some background to state-socialism in CEECs. It draws heavily on the in-depth discussion in de Haan (2012).

State-socialist governments strongly encouraged women's paid employment outside the home for two reasons. First, the rapid industrialization which started in Eastern Europe in 1945 was based on an intensive use of labor and was therefore dependent on a large-scale inclusion of women in the labor force. Second, women's economic independence was seen as a necessary precondition for women's equality, a principle to which state-socialist governments were committed (Buckley, 1984, argues that the first reason was by far more important).

The state-socialist governments used the wage system (keeping the wages low, thus requiring two-income earners to make a reasonable family income) as well as centralized political pressure and propaganda to their ends (Fodor, 2002). Following the ideas of Lenin and Engels, the initial ideological assumption was that women should not be differentiated from men in the workforce in any way.[5] All state-socialist countries adopted the principle of equality between men and women in their new constitutions. For instance, the Hungarian Constitution of 1949 proclaimed that women were entitled to the same work under the same working conditions as men. "The new family laws in 1952 – preceding the revision of the Austrian family law by almost two-and-a half decades – supported the independence of women" (Fodor, 2002).

Throughout the region, the effect of the state-socialist agenda towards women was a large-scale entry of women into paid employment outside the home (Wolchik, 1992).[6] Table 1 shows the number of female workers as a percentage of the labor force in state-socialist countries and OECD

[5] As reported by Ilic (1999:pg 28), in his speech at the Non-Party conference of Women Workers in 1919, Lenin states: 'As long as women are engaged in housework their position is a restricted one. In order to achieve the complete emancipation of women and to make them really equal with men we must have social economy, and the participation of women in general productive labor. Then women will occupy the same position as men'.

[6] A similar picture emerges regarding female education.

countries, for the period 1950-1988. The numbers reached in East Germany and Czecholsovakia in 1960 (more than 40 per cent) and by most other state-socialist countries in 1970, were reached in the OECD countries only in 1989.[7]

It is clear that the lives of women in Eastern Europe during state-socialism were fundamentally different from those of their mothers and grandmothers. In contemporary sources as well as in later interviews, some women emphasize the way in which socialism liberated them. Others foreground their sacrifices and suffering (having to work longer hours than men because of the amount of work done at home); whereas yet others convey both dimensions. Most women were workers as well as mothers, but without sufficient social services or an equal sharing of the paid and unpaid work with men (Alpern Engel and Posadskaya-Vanderbeck, 1998). However, despite being structurally overburdened, women made it very clear that they did not want to give up their paid work. In 1960 in Hungary for example, only 13 percent of 4,828 working women said they would have preferred to work solely at home (Völgyes and Volgyes, 1977).[8]

Many women acknowledged the possibilities that state-socialism had created for them; they only had to look at their own family history. Looking at socialist legislation, propaganda and oral history interviews, Massino (2009) analyzes how women's identities and roles - as well as gender relations - were reformulated as a result of women's participation in paid labor in socialist Romania. Although some women regarded work as burdensome and unsatisfying, others found it intellectually fulfilling, personally rewarding and, in certain respects, empowering. For example, work improved women's economic position and offered them an array of social services, which, although inadequate in a number of ways, were welcomed by many women. Moreover, work increased women's physical and social mobility which, in turn, provided them with greater freedom in directing their own lives and in choosing a partner. Finally, the experience of being harassed by male co-workers and of combining work outside the home with domestic responsibilities motivated some women to rethink their status both within the workplace and the family, and renegotiate their relationships with male colleagues and partners. Although women never achieved full equality in socialist Romania, Massino (2009) concludes, by creating the conditions for women's full-time engagement in the workforce, state socialism decisively shaped the course of women's lives, their self-identities and their conception of gender roles, often in positive ways. The importance of a workplace collective for working-class women also comes through in material about the German Democratic Republic and Hungary (Toth, 2009; Watson-Franke, 1981).

However, other studies emphasize that women were not fully equal to men in the sphere of paid work. A wage gap of more than 30 percent existed everywhere in the state-socialist world

[7]Data for Lithuania are not available.

[8]According to du Plessix-Gray (1990) in the second half of the 1980s, only one out of five Soviet women would willingly give up work if they could afford to.

(Molyneux, 2001). There was a number of reasons for this. Women, by and large, remained concentrated in branches of the economy (education, services, retail, medicine, light industry) that had lower wages than the high-priority branches of the economy (mining and heavy industry). The differences in occupations and wages were also due to the fact that many more men than women chose a technical education, which was more highly valued because of the focus on industrialization. Other facts that explain the significant wage gap were outright discrimination against women (Bardasi and Monfaldini 2005, Lane 1987), the fact that few women held leading economic positions, and women's willingness to accept positions for which they were overqualified in order to be nearer to their homes. The reason for the latter was that although women had been proclaimed "equal", domestic work and care for children remained, as already mentioned, the responsibility of women, in all socioeconomic groups.

Another relevant fact as a background to our research is that from the early 1960s, the birthrates dropped significantly across Eastern Europe. Political leaders interpreted this as a reaction by women to their "double burden" (though as noted by de Haan (2012), there is almost always a decline in the birth rate when there is an increase in women's education), and thus took steps to make the combination of paid work and motherhood somewhat easier. Paid maternity leave and mothers' allowances, in some cases allowing women with more than one child to remain at home until their children were three, did alleviate women's burden. However, they also reinforced the identification of women as a group with family and home. In the 1970s and 1980s, women's paid employment rates continued to be high, or increased even further. But in that period of economic and political crises, the so-called women's issues remained low on the list of politicians' priorities (LaFont, 2001). For instance in Poland in the 1970s, the language centered on "equal rights" which was prominent in the early 1950s gave way to a vocabulary focused on the "appreciation" of women's multiple roles and sacrifices, which seemed to be inevitable and independent of the political context (Fidelis, 2010).

4 Measure of Attitudes and Data

In this Section, we first explain how we use immigrants in the US to recover attitudes before the advent of state-socialism in CEECs, and then we describe the datasets and variables that we use in the empirical analysis.

4.1 Attitudes

Since we study the change in attitudes during the communist domination in CEECs, a measure of attitudes at different points in time is needed. More specifically, since CEECs became state-

socialist regimes between the late 1940s and the early 1950s, we need to measure attitudes before this period. We cope with the lack of survey data on gender attitudes before the 1980s using attitudes inherited by second, third and fourth generation immigrants in the US. As explained in Section 2, the correlation between the behaviors and attitudes of immigrants and those residents in the country of origin has been noted and exploited by several authors. The use of attitudes inherited by immigrants in the US allows us to go back in time, proxying the gender attitudes of people living in country c at time t with the gender attitudes of the descendants of immigrants to the US from country c at time t. For this purpose, we use the GSS, which contains individual answers to questions on values and attitudes, and individual data on the country of birth of the respondent, her parents and of her grandparents. More precisely, following Algan and Cahuc (2010), we measure the attitudes before the advent of state-socialism (before 1949) as compared to the attitudes of second generation Americans born before 1940 (whose parents left the country before 1940), third generation Americans born before 1965 (whose grandparents left the country before 1940), and fourth generation Americans born before 1990 (whose grand-grandparents left the country before 1940). We call this group of individuals the 1940 cohort. We assume that parents on average give birth when they are 25.[9] Then, we measure attitudes before the fall of the state-socialist regimes (before 1990) with the attitudes of second generation Americans born before 1990 and after 1940, third generation Americans born before 2005 and after 1965, and fourth generation Americans born after 1990. We call this group of individuals the 1990 cohort.[10] In part of the analysis, we shall measure attitudes after the state-socialist experience in CEECs using responses to the 1990-wave of the World Value Survey (WVS) of individuals in their country of residence.

The state-socialist countries that we consider in our core analysis using GSS are: Czechoslovakia, Hungary, Lithuania, Poland and Romania.[11] Yugoslavia was a Soviet satellite between 1943 and 1948. After this period, this country remained a state-socialist state, since the policies implemented in Yugoslavia were not dictated by the Soviet influence from 1948 and onwards, and therefore they might be considered endogenous to preferences and values in the country. For this reason, we omit Yugoslavia from our sample for the baseline specification. However, we will

[9] If they did, in fact, give birth earlier, they left the country before the year of interest; if they gave birth at the age of26-28, we are still measuring attitudes before 1943; given the period studied, it is quite unlikely that they gave birth at a later age.

[10] Notice that, as in Algan and Cahuc (2010), we make sure that the same individual does not belong to two cohorts. Moreover, the *1990 cohort* contains individuals who inherited attitudes formed in the country of origin at any time before 1990. This means that some of these individuals inherited attitudes formed before 1940, and therefore we underestimate the evolution of attitudes during the period 1940-1990, when looking at differences between the *1940 cohort* and the *1990 cohort*.

[11] Notice that, according to the Russian government, Lithuania has been a Soviet Republic, whereas the official position of Lithuania, the US and the European Union, among others, is that Lithuania was an independent state, subjected to the Russian occupation. This distinction is virtually irrelevant for the purpose of this study, since, regardless of its formal status, Lithuanian people experienced a state-socialist regime and policies were mainly dictated by Moscow.

include Yugoslavia in some specifications. For our analysis using the GSS we drop Germany, because immigrants who report Germany as their country of origin may come from East or West Germany, and therefore they may or may not be "treated". However, when we use the 1990-wave of the World Value Survey (WVS), we also include East Germany (in the state-socialist group) and West Germany (in the non state-socialist group) since German residents who respond to the WVS report state whether they come from East or West Germany. There are no available data on any other Soviet satellites or Soviet Republics.

4.2 Data

The main source of data for our analysis is the GSS, which collects answers to various questions on values and attitudes by US residents between 1972 and 2010. For each of the immigrants belonging to the cohorts described above, we look at answers to questions on attitudes toward working women. In total, we count 1,507 immigrants from CEECs in our sample, of which 1,394 belong to the 1940 cohort and 113 belong to the 1990 cohort. The immigrants from the rest of the world are 14,132, of which 13,556 belong to the 1940 cohort, and 576 belong to the 1990 cohort. Unfortunately, not all of them answered the questions that we consider in our analysis, because every question is only asked in certain years. Of all questions related to gender attitudes and family values, we select those answered by at least 30 persons in each group and period. Table 2 lists the questions studied, with summary statistics and number of respondents, by group and period.[12] The variables are coded in such a way that increasing values denote more progressive attitudes about the appropriateness of a specialization of male and female roles and the compatibility of work and motherhood.

The countries in the non-state socialist group are: Austria, Belgium, Denmark, Finland, France, Greece, Ireland, Italy, Norway, Portugal, Spain, Sweden, and the UK. We drop Germany from the GSS sample for the reasons specified in Section 4.1. As mentioned above, in some of the analysis we will measure attitudes after the state-socialist experience in CEECs using responses of CEEC residents to the 1990-wave of the World Value Survey (WVS). We will also use WVS data when we document the representativeness of immigrants with respect to attitudes in their home country.

5 Econometric Model and Identification

We compare changes in gender attitudes before and after the advent of state-socialism in CEECs. As argued in the introduction, the advent of state-socialism in CEECs can be seen as an exogenous

[12] We only count individuals for which a full set of control variables, which we use in our most preferred estimation, is available.

event, at least with respect to attitudes toward gender equality in the late 1940's. Therefore, in principle, the before-after difference in attitudes (where "after" means "following the advent of state-socialism") could be interpreted as the effect of state-socialism itself. A concern arises, however, that at that time, a general trend in gender attitudes might have been in place, due for instance to WWII.[13] In order to account for such a trend, we estimate a Differences-in-Differences equation, where we compare the evolution of attitudes for immigrants from countries that experienced state-socialism versus the evolution for immigrants from other countries in Europe. In Figures 1-4, we document parallel trends in the average attitudes of these two groups at two points in time before the advent of state socialism in CEECs.

The identifying assumption is thus that, absent the state-socialist regime, the evolution of gender attitudes in CEECs countries would have followed a path that cannot, on average, be distinguished from that in other countries, as was the case in the decades preceding 1940.

Thus, we estimate the following equation:

$$\begin{aligned} Y_{icrp} =& \beta_0 + \beta_1 \text{state\_socialism}_c + \beta_2 \text{post-1940}_p + \beta_{DiD} \text{state\_socialism} \cdot \text{post-1940}_{c,p} \\ &+ \beta_4 X_{icrp} + \eta_r + \varepsilon_{icrp} \end{aligned} \tag{1}$$

where Y_{icrp} are gender attitudes of individual i from country c residing in US region r in period p; state_socialism_c is a dummy taking the value of one if country c was ever state-socialist, post-1940_p is a dummy taking the value of one if the individual inherited culture formed in the country of origin before 1990, X_i are individual-level characteristics and η_r are regional dummies.[14]

In interpreting Equation (1), three points need to be highlighted. First, our data allow us to include a rich set of individual-level characteristics that control for changes in the demographic composition of the population in the country, but also for changes in the sample, if the latter are not representative of the former. These controls include age, education, income, marital status, political views, religion, number of kids and employment status.

Second, a possible source of concern is that immigrants are selected in a non-random way, in the sense that people who decided to leave their country of origin may be those who are more independent and less attached to the values of their country of origin (Algan and Cahuc, 2010; Alesina and Giuliano, 2010). However, the evidence in the body of work surveyed in Section 2 suggests that cultural traits of the country of origin are maintained by immigrants. Moreover, below we will show that the attitudes of residents in country c in the 1990 wave of the World

[13] As mentioned above, Fernández, Fogli, and Olivetti (2004) show that the male mobilization rate in WW2 has a positive effect on women's employment status in later years.

[14] We include region fixed effects, since the information about the state of residence is not available in the publicly available version of GSS.

Values Survey are correlated with the inherited attitudes before 1990 for the same country (see Section 6.3). More specifically, the correlation between inherited attitudes toward gender roles in the United States and attitudes toward gender roles in the home country is statistically significant at the 1 percent level. Nevertheless, since the correlation between inherited attitudes and attitudes among residents is not perfect, there might be a concern that a possible change in attitudes after the advent of state-socialism is due to a change in the selection of immigrants. We do not expect this to be a major factor in our context given the rich set of individual-level characteristics that we add in Equation 1.

To further explore this possibility, we regress each of the individual controls in X_i on state_socialism, post-1940, state_socialism · post-1940, and regional dummies. This allows us to check whether the characteristics of immigrants change differentially in state-socialist and non-state socialist countries, between 1940 and 1990. The results of this analysis are shown in Table 3. The characteristics of immigrants from CEECs do indeed change along some dimensions. With respect to other countries, immigrants from CEECs become younger (by two years on average); the share of Protestant and Jewish immigrants increases, while the share of Catholics decreases. For what concerns the political dimension, immigrants from CEEC countries become more leftist as compared to those from other countries. Finally, they are more likely to be separated and less likely to report "other marriage status". The selection does not change in terms of income, education, gender, satisfaction with financial situation, number of kids, share of people married, divorced or widowed, share of people who report no religion or who report other religion with respect to those explicitly classified, and the share of Orthodox. Since we have no reason to suspect that state-socialism caused the observed decrease of Catholics, increse of Protestants and Jews, and increase in the average age of the population, we think that the change in these variables is rather due changing selection of immigrants. Therefore, these variables are important controls to be included in our analysis. A different consideration is in order for the documented changes in political leaning and marriage status: these are most likely a direct effect of state-socialism. Specifically, the average political views of CEECs residents might have changed, as suggested by the evidence in the body of work on the effect of state-socialism on individual beliefs toward redistribution surveyed in Section 2. Therefore, we consider the variables measuring political views and marriage status as "bad controls", and we exclude them from the baseline regression. Overall, although we are not able to completely rule out that our results are partly determined by selection on unobservables, the richness of controls we can access, and the fact that immigrants do not appear to be selected on most individual characteristics, is quite reassuring.

Third, the composition of immigrants might also change over time in terms of country of origin. Country of origin, as emphasized in Section 2, matters for attitudes toward gender equality. Our estimates would be biased if the share of immigrants arriving, for instance, from a very conserva-

tive country, increases dramatically, whereas that of immigrants from a more progressive country decreases. In Table 4, we show the share of immigrants by country, for the 1940 cohort and the 1990 cohort. The share of immigrants who report Italy, UK and Ireland as their country of origin changes quite substantially, and the share from other countries changes to a lower extent.[15] In Section 6.1.2, we focus on a specification with country-fixed effects, which compares the evolution of attitudes in a given country versus that in other countries, thus addressing the issue of a changed selection of immigrants by country of origin.

6 Results

6.1 Attitudes about the appropriateness of specialization of male and female roles

6.1.1 Estimates using GSS

We study the effect of state-socialism on attitudes about the appropriateness of specialization of male and female roles by estimating Equation (1), where the dependent variables are answers to the questions: *Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her?* and *It is much better for everyone involved if the man is the achiever outside the home and the woman takes care of the home and family*. The answers to the first question are *"Disapprove"*, *"Don't Know"*, or *"Approve"*, which we recode, respectively, as 0, 0.5 and 1. We call the resulting variable *"Should Women Work"*: the higher its value, the more progressive are an individual's attitudes toward working women. The answers to the second questions are *"Strongly Agree"*, *"Agree"*, *"Don't Know"*, *"Disagree"*, and *"Strongly Disagree"*, which we recode, respectively, as 1, 2, 2.5, 3, and 4. We call the resulting variable *Better for Men to Work, Woman to Tend to the Home*. Once more, the higher its value, the more progressive is the individual's attitude toward working women.

The results from estimating Equation (1), having these two measures of attitudes as the dependent variable, are shown in Table 5. Central and Eastern Europeans that formed their attitudes during the state-socialist regime seem more likely to hold progressive beliefs regarding gender roles in the labor market, even when the general trend in gender attitudes during this period is accounted for (see columns 1 and 2). The coefficient on state_socialism · post-1940 means that having experienced state-socialism increases the probability of approving of a married woman working if her husband is capable of supporting her by 4.6 percentage points (5.7%), once a full set of individual characteristics has been controlled for. To put the number in context, being a man versus being a

[15] Notice that the decrease in Catholics from state-socialist countries versus other countries in Europe, shown in Table 3, is mainly determined by the increase in the share of Italians in the control group.

woman and reporting being Catholic versus reporting "no religion" are associated with a decrease in the probability of approving by approximately 2.8 and 4 percentage points, respectively.

The coefficient of state_socialism · post-1940 is significant at the 5% level in Column 1, and the 10% level in Column 2, probably due to the low number of observation in the 1990 cohort. However, with few clusters, serial or cross-sectional correlation among errors is likely to be underestimated. To further explore this aspect, we pursue several directions. First, we cluster the standard errors by country-wave, assuming that the cross-sectional correlation among the errors is more serious than the serial correlation; this doubles the number of clusters. Second, we follow Donald and Lang (2007), and calculate the p-value for the coefficient on State-socialist x post-1940 under the assumption that the t-statistic has a t-distribution with $g-k$ degrees of freedom, where g is the number of clusters and k is the number of regressors.[16] [17] Third, we bootstrap the standard errors following the procedure developed by Cameron et al. (2008) to improve the inference with clustered standard errors. We report the p-values based on these standard errors at the bottom of Table 5. The coefficient of state_socialism · post-1940 is significant at conventional levels when we cluster by country-wave, and when we assume a t-distribution with fewer degrees of freedom. When we use the Cameron et al. (2008) procedure, the p-value is 0.162 in Column 1 and 0.128 in Column 2. In column (3) we add as additional controls a measure of political views, and indicators for marriage status. We consider these variables as "bad controls", and for this reason we do not include them in the baseline regession (see Section 5). Finally, in column (4) we add Yugoslavia to the sample. This inclusion increases the number of individuals in the state-socialist group. The conclusions regarding the effect of state-socialism are very similar to those from Columns 1 and 2.[18] Overall, these estimates suggest a significant difference in the evolution of attitudes between Europeans in state-socialist countries and other Europeans during the period 1947-1991.

In columns (4), (5) and (6), we look at the question *Better for Men to Work, Woman Tend Home.* The estimates give a positive and economically significant $\widehat{\beta}_{DiD}$. When we include the controls, the estimated coefficient is 0.163, with a semi-elasticity of 0.061. While, in principle, this is consistent with the idea that the political and economic regime in state-socialist countries exerted a noticeable influence on people's beliefs about sex- roles, in practice the standard errors are very large (0.114), due to the low number of individual answers to the GSS that we can exploit, and this prevents us from drawing any definitive conclusions. Another tentative explanation for the estimate in columns (4) - (6) is that the question *Better for Men to Work, Woman to Tend Home,* unlike the question *Should Women Work*, poses a choice between women working in the labor market and women taking care of home and family. As discussed in Section 3, most women in CEECs were

[16] This option is not available in the specification which includes controls, because k is larger than g.

[17] The standard routine implemented in Stata with the *cluster* option assumes a t-distribution with g-1 degrees of freedom.

[18] The reasons why we prefer not to include Yugoslavia in the baseline specification are discussed in Section 4.1.

workers as well as mothers, but without sufficient social services or an equal sharing of the paid and unpaid work with men. Although women had been proclaimed "equal", in all socioeconomic groups, domestic work and care for children remained women's responsibility. Starting from the early 1960s, political leaders took steps to make the combination of paid work and motherhood somewhat easier but they also reinforced the identification of women as a group with family and home. In the 1970s and 1980s, women's issues remained low on the list of politicians' priorities. In Section 6.4, we further discuss these issues and we show that no significant difference between CEECs and the rest of Europe is found in the evolution of beliefs about the compatibility of work and motherhood. As anticipated in Section 5, one concern is that the estimated effect is, at least partly, due to a changing composition of the treatment and control groups. While we only use immigrants from countries that are represented before and after, the share of individuals from each of these countries changes, as shown in Table 4. One way of addressing this issue is to include a country fixed-effect, which would weigh every country in the sample equally, regardless of its number of immigrants in the US. If we replace the dummy $state\_socialism_c$ with a country fixed effect in equation 1, the estimated semi-elasticity is 4%, and it is marginally significant (the result is available upon request). However, given the very low number of observations in the *post-1990* cohort, this estimate is based on the assumption that very few individuals are representative of the attitudes formed in a country (either in CEECs or in the rest of Europe) during the state-socialist regime in CEECs. Thus, we try to improve on this analysis exploiting the information in the WVS, where we can measure attitudes in the *1990 cohort* looking at residents and hence, substantially increase the number of individuals for each country.

6.1.2 Estimates using WVS

In this Section, we address the concerns related to the Difference-in-Differences estimation in Section 6.1.1, by improving our measure of attitudes in the *1990 cohort* using information in the WVS. From the WWS, we select a question that is comparable to *Should Women Work*. More specifically, to measure attitudes towards gender-roles in the WVS, we consider the following question in the survey: *Do you agree or disagree: husband and wife should both contribute to income*. We regard as egalitarian those respondents who answer *"Agree"* and *"Strongly Agree"* (the other possible answers are *"Disagree"* and *"Strongly Disagree"*). We combine these data with GSS data for immigrants in the *1940 cohort*. We regard as egalitarian those respondents who answer *"Agree"* and *"Strongly Agree"* (the other possible answers are *"Strongy Disagree"*, *"Disagree"*, *"Neither Agree nor Disagree"*, *"Don't Know"*). The resulting variable is called *Should Husband and Wife Contribute to Income*. Notice that we did not use *Should Husband and Wife Contribute to Income* in the GSS analysis, because there are not at least 30 individuals answering this question in each group for the *1990 cohort*. We make the same choices as before

in terms of countries to be excluded from the sample. However, we now use data from East and West Germany. This is because the 1990 wave of the WVS distinguishes between respondents from the two parts of Germany. Therefore, we can identify East and West Germans in the new *1990 cohort*, which consists of residents of country c, rather than immigrants from country c.[19] As concerns the *1940 cohort*, we classify immigrants from Germany as both East and West Germans. In practice, they appear in the sample twice, representing the *before* counterpart of both East and West Germans in the *1990 cohort*. This is based on the assumption that, before the split of the country, the two parts of Germany were, on average, indistinguishable from each other. In any case, we will show results excluding East and West Germans from the estimation sample. In the 1990 wave of the WVS, there are no respondents from Yugoslavia.[20] From the initial sample, we only keep individuals representing a country-wave for which there are at least fifteen respondents to the question of interest.[21] In Table 2, we report summary statistics for *Should Husband and Wife Contribute to Income*, with the number of individuals in the estimation sample by group and wave. In Figure 5, we document parallel trends in the evolution of these attitudes before the exposure of CEECs to the state-socialist regime. The advantage of using the WVS to measure attitudes in the post-communist period is that we rely on a larger number of observations. This allows us to estimate a country fixed-effect model, as follows:

$$Y_{icp} = \beta_0 + \alpha_1 \textit{post-1940}_p + \beta^{wvs}_{DiD} \textit{state\_socialism} \cdot \textit{post-1940}_{c,p} + \phi_c + \alpha_3 X_{icrp} + \varepsilon_{icrp} \qquad (2)$$

where ϕ_c is a country fixed-effect, which remains for country of origin in the *1940 cohort*, and country of residence in the *1990 cohort*. The other variables are defined as in Equation (1).[22] The estimated parameters of Equation (2) are shown in Table 6. Column 1 suggests that people who lived in CEECs until 1990 became more progressive than US immigrants who inherited attitudes from the same countries before 1940, once the general trend in attitudes over this period is accounted for.[23] A potential concern is that the estimated effect is biased if US immigrants who

[19] The "Treaty on the Final Settlement with Respect to Germany" between the Federal Republic of Germany, the German Democratic Republic, and the Four Powers which occupied Germany at the end of World War II (France, the Soviet Union, the United Kingdom, and the United States of America) was signed on 12 September 1990. In the treaty, the Four Powers renounced all rights they held in Germany, allowing a united Germany to become fully sovereign in the following year. The Treaty became effective on 15 March 1991.

[20] While there are respondents from Slovenia; including or not Slovenians respondents in the *State-socialist* group does not matter, because there are no observations in the *1940 cohort* for this country, and therefore Slovenians do not provide any identifying variation.

[21] This follows Algan and Cahuc (2010). As before, we only consider individuals for which the full set of control variables, which we will use in our most preferred estimation, is available.

[22] In this specification, we cannot include dummies for region or residence, as we compare immigrants in the US to European residents.

[23] In this setting, the control for the dummy *post-1940* is especially important, to account for the fact that attitudes are measured with two different strategies before and after.

left their home country at any date before 1940 were selected in a systematically different way in CEECs and other European countries. We do not expect this to be a major factor in our context given that CEECs countries before 1940 were not a bloc clearly distinguishable from any other European country. To further explore this possibility, we try to account for possible selection including a full set of controls, the introduction of which leaves the coefficient virtually unchanged. Having being exposed to state-socialism increases the probability of believing that husband and wife should both contribute to their income by 14 p.p. (19%). In column (3) we show that the semi-elasticity is unchanged when we add controls for political views and marriage status. It instead decreases substantially when we drop East and West Germany from the sample (column 4). [24] In column (5) we estimate the model in column (2), keeping only individuals from countries represented both before and after; not surprisingly, the estimated coefficient does not change, since only these countries are used to identify the effect of interest in column (2). Finally, in column (6) we only keep countries that we also use in the GSS analysis; in practice, with respect to column (2), we drop East and West Germans from the sample used to identify the effect of interest, and countries that are represented only in one wave; for this reason, the estimated coefficient is identical to that estimated in column (3).[25] We cluster the standard errors by country-wave, because with the country fixed effect, the part of attitudes which is common to a country across two waves is taken into account; the number of clusters is acceptable or borderline in some specifications, (39 in column (1), 34 in column (2), 30 in column (3)), and low in others (24 in column (4), 20 in column (5)). As before, we assume a t-distribution with g-k degrees of freedom in the regression without controls, showing that the precision of the estimates is highly unaffected. However, we emphasize that the quality of the inference with standar errors clustered by country-wave should be satisfactory in columns (1) and (2), given the acceptable number of clusters. A further concern is that the estimated effect is simply due to a cross-sectional difference in attitudes in 1990 between CEEC countries and other European countries; this difference might have existed also before the communist period, but we fail to pick it up because immigrants in the US are assimilated. This once more brings up the issue of the representativeness of the immigrants, which we show not to be a major concern in Section 6.3.

6.2 Discussion

Overall, the evidence shown in the Sections 6.1.1 and 6.1.2 suggests that the political and economic regime in state-socialist countries exerted a noticeable influence on people's beliefs about sex-roles

[24] As discussed in Section 7, in future work we want to look more closely at Germany and see how our estimates compare to the Bauernschuster and Rainer (2011) estimates using the ALLBUS dataset.

[25] The countries that we drop are Sweden (before), and Austria, Belgium, Bulgaria, Finland, Hungary, Iceland, Lithuania, Portugal and Romania (after).

in the labor market. In interpreting our estimates, two further points need to be highlighted. First, our estimates should be interpreted as a general equilibrium reduced-form effect that combines both the direct impact of state-socialist ideology and policies (which encompass propaganda for gender equality, measures to bring women into the labor force, quotas for the presence of women in representative bodies, provision of childcare, passage of laws on abortion and reproductive rights) and all other associated changes, such as increases in the female participation rate to the labor market. Conditional on the parallel trend assumption being verified, this total effect of state-socialism on gender attitudes can be identified.[26] Second, while Section 3 documents the focus of state-socialist governments on gender equality, a similar focus, although within liberal-democratic systems, might have emerged in some countries in the control group but not in others. In other words, in our current framework, we are estimating the effect of state-socialism on gender equality versus the effect of any other policy regime that, we argue, put less emphasis on gender equality.

6.3 Are Immigrants Representative?

In this Section, we document that attitudes toward gender equality among immigrants up to the fourth generation mirror those in their country of origin. In Section 2, we referred to the large body of literature that documents the persistence of cultural traits from the country of origin among US immigrants. Especially relevant for this paper, given that they also look at second, third and fourth generation immigrants, is the evidence in Algan and Cahuc (2010). Here we follow their approach, using the variable *Husband and Wife Contribute to Income*, which is available in both the WVS and the GSS. Using GSS data, we identify a cohort of immigrants, which we call *cohort 1990 long*, and includes first, second and fourth generation immigrants whose ancestors left their country of origin before 1990. This is to increase the number of observations in the *1990 cohort*, which only includes immigrants whose ancestors left their country of origin before *1990* and after *1940*.[27] We only keep countries with at least fifteen observation than can be used in the estimated equation.

[26] Attitudes toward working women might also change because people in low-wage countries see female work as necessary for the sustenance of the family. In particular, low wages might be an important channel through which state-socialism affects attitudes toward working women. If this is reflected in the attitudes of US immigrants from CEEC countries, who face the same economic conditions as immigrants from other countries, it can be interpreted as the effect that we are interested in estimating. This is especially relevant in our setting since, as discussed in Section 3, governments used the wage system (keeping the wages low, thus requiring two-income earners to make a reasonable family income) to increase female participation. However, we acknowledge that low wages in the CEECs are not exclusively the result of the focus on gender equality, and therefore a possible change in gender attitudes might be a by-product of economic policy in general, rather than of a specific policy to foster gender equality.

[27] If we do not make this adjustment, only 2 countries have at least 15 individuals in the *1990 cohort,* making inference unreliable. Recall that the paucity of observations is also the reason why we do not use *Should Husband and Wife Contribute to Income* from the GSS in Section 6.1.1.

Then, we estimate the following:

$$Y_{icr} = \gamma_0 + \delta_1 \textit{attitudes in home country}_c + \gamma_2 X_{icr} + \eta_r + \varepsilon_{icr} \quad (3)$$

where *attitudes in home country*$_c$ is the average of Y_{icr} in the country of origin of individual i, obtained using the answers to *Husband and Wife Should Contribute to Income* in the 1990 wave of the WVS. The other variables are defined as in Equation (1). We report the estimate of γ_1 in Table 8, where individual characteristics and region of residence in the US have been accounted for. The correlation between inherited attitudes toward gender roles in the United States and attitudes toward gender roles in the home country is statistically significant at the 1 percent level. Since we only have fifteen clusters, we bootstrap the standard errors following the procedure in Cameron, Gelbach, and Miller (2008), and the correlation is still highly significant, as shown at the bottom of the table. We also document immigrants' representativeness plotting country fixed-effects against each other for answers to *Should Husband and Wife Contribute to Income*, estimated on the WVS sample and on the *cohort 1990 long*, respectively. Thus,we run the following regression:

$$Y_{icr} = \delta_0 + \delta_c + \gamma_1 X_{icr} + \varepsilon_{icr} \quad (4)$$

on the WVS and the *1990 cohort long* samples. We estimate, respectively, the country of residence fixed-effects and the country of origin fixed-effects, once all individual controls are taken into account, and omitting the category "UK". We plot these fixed-effects against each other in Figures 6 and 7. The latter excludes the category "Netherlands", given that this might appear as an outlier in terms of extremely low fixed-effects in the two samples. In both figures, the correlation between the fixed effects in the two samples is pretty striking, providing further evidence in favour of immigrants' representativeness of attitudes toward gender roles in home countries.

6.4 Attitudes about the compatibility of work and motherhood

In this Section, we study the effect of state-socialism on attitudes on the compatibility of work and motherhood, by estimating Equation (1), where the dependent variables are answers to the questions: *A preschool child is likely to suffer if his or her mother works*; and *A working mother can establish just as warm and secure a relationship with her children as a mother who does not work*. We recode the answers to the first question as follows: 1 if *"Agree Strongly"*, 2 if *"Agree"*, 2.5 if *"Don't Know"*, 3 if *"Disagree"*, and 4 if *"Disagree Strongly"*. We call the resulting variable *Preschool Kids Suffer if Mother Works*. We recode the answers to the second question as follows: 1 if *"Agree Strongly"*, 2 if *"Agree"*, 2.5 if *"Don't Know"*, 3 if *"Disagree"*, and 4 if *"Disagree*

Strongly". We call the resulting variable *Mother Working Hurts Kids.* The higher the value of these variables, the less the individual agrees with this statement, and therefore the more progressive are the individual's beliefs about the compatibility of work and motherhood. The results from estimating Equation (1), having these two measures of attitudes as the dependent variable, are shown in Table 7. In Columns (1) - (4), we look at the question *Preschool Kids Suffer if Mother Works.*: $\widehat{\beta}_{DiD}$ is never significant. When we include the controls, the estimated coefficient is 0.098, with the standard error being equal to 0.082. In Columns (5) - (8), we look at the question *Mother Working Hurts Kids*: $\widehat{\beta}_{DiD}$ is once more never significant. In the specification without individual controls - Column (5) - the estimated coefficient is negative (-0.047) with a standard error of 0.101. When we include the controls in Columns (6) and (7), the estimated coefficient is 0.032 and -0.009, with standard errors equal to 0.120 and 0.127 respectively. The coefficient is positive not precisely estimated also when we include Yugoslavia in the sample. We conclude that, using the GSS, no significant difference between CEECs and the rest of Europe is found in the evolution of beliefs about the compatibility of work and motherhood.

7 Conclusion and Future Directions

A consensus has emerged that gender attitudes can at least partially explain some labor market outcomes. However, evidence on the causes of the development and the evolution of such attitudes is limited. In this paper, we have analyzed how attitudes evolve over time as a result of changes in policy regimes and social policies. We showed that the state-socialist regime had an impact on the sex-role attitudes of individuals in CEECs. In interpreting our results, two points need to be highlighted. First, our estimates should be interpreted as a general equilibrium reduced-form effect that combines both the direct impact of state-socialist ideology and policies (which encompass propaganda for gender equality, measures to bring women into the labor force, quotas for the presence of women in representative bodies, provision of childcare, passage of laws on abortion and reproductive rights) and all other associated changes, such as the increases in the female participation rate to the labor market. Conditional on the parallel trend assumption being verified, this total effect of state-socialism on gender attitudes can be identified. Second, while Section 3 documents the focus of state-socialist governments on gender equality, a similar focus, although within liberal-democratic systems, might have emerged in some countries in the control group, but not in others. In other words, in our current framework, we estimate the effect of state-socialism on gender equality versus the effect of any other policy regime that, we argue, put less emphasis on gender equality. There are several directions that this work could take. First, using the 1995–97, 1999–2001 and 2005–07 waves of WVS, we would like to investigate whether individuals in CEECs hold more progressive beliefs and values about the appropriateness of specialization of

male and female roles, after the fall of the Berlin Wall. Second, we would like to further explore the relation between inherited attitudes toward gender roles in the United States and attitudes toward gender roles in the home country. Specifically, we want to (a) select questions from WVS that are comparable to *Should Women Work* and *Better for Men to Work, Woman Tend Home*, in order to overcome the issue of the low number of individuals within most countries in the *1990 cohort*, when using the question *Should Husband and Wife Contribute to Income.* We also want to analyze the correlation between the attitudes of residents in country c in the 1981 wave of World Values Survey and the inherited attitudes before 1981 for the same country. Third, we would like to shed some more light on the evolution of beliefs about the compatibility of work and motherhood in state-socialist countries and the rest of Europe during the period 1947-1991. In particular, we want to select questions from WVS that can be combined with *Preschool Kids Suffer if Mother Works and Mother Working Hurts Kid* in order to check whether we obtain more precise estimates once we measure attitudes in the *1990 cohort* looking at residents and hence, substantially increasing the number of individuals for each country. Exploiting the information on WVS, we also want to look at Germany and see how our estimates compare to the Bauernschuster and Rainer (2011) estimates using the ALLBUS dataset.

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A Appendix

A.1 Figures and Tables

Figure 1: ***Should Women Work: Evolution of Unconditional Mean Attitudes among Immigrants in the US***

Figure 2: ***Better for Men to Work, Women Tend Home: Evolution of Unconditional Mean Attitudes among Immigrants in the US***

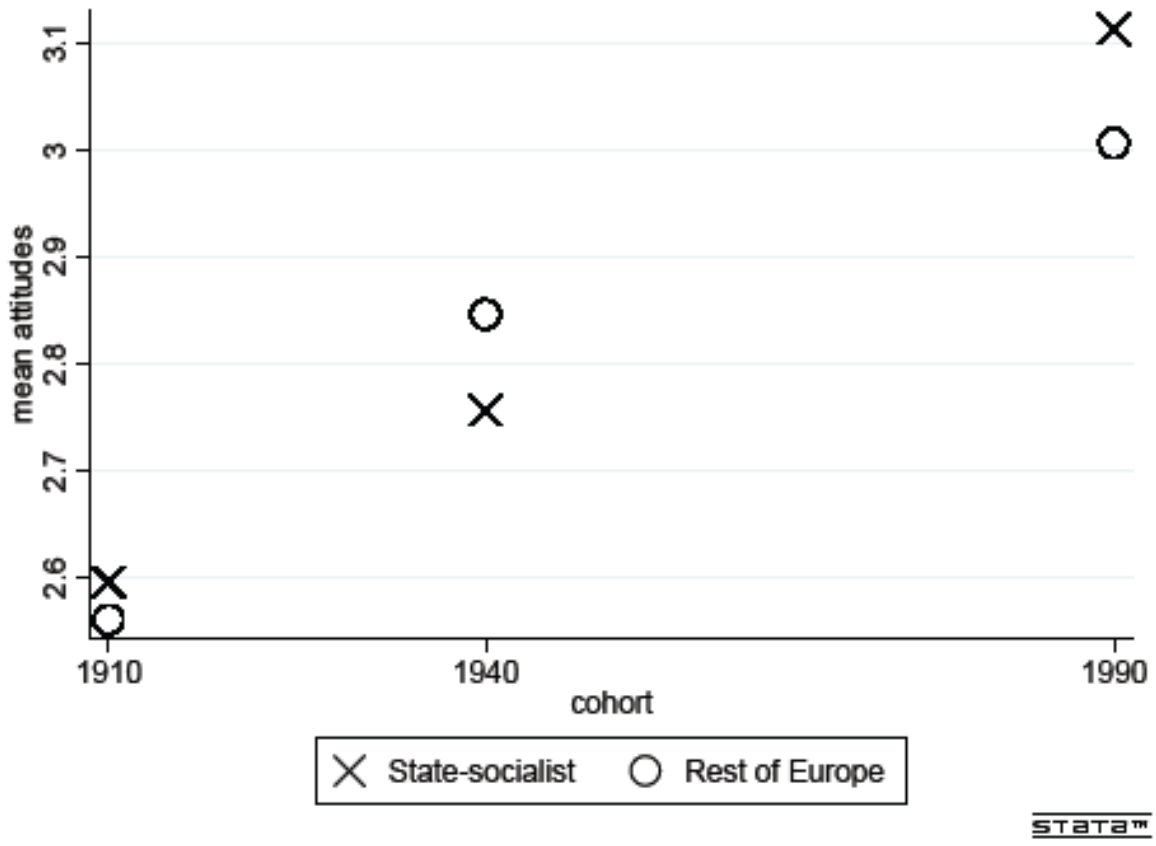

Figure 3: ***Preschool Kids Suffer if Mother Works: Evolution of Unconditional Mean Attitudes among Immigrants in the US***

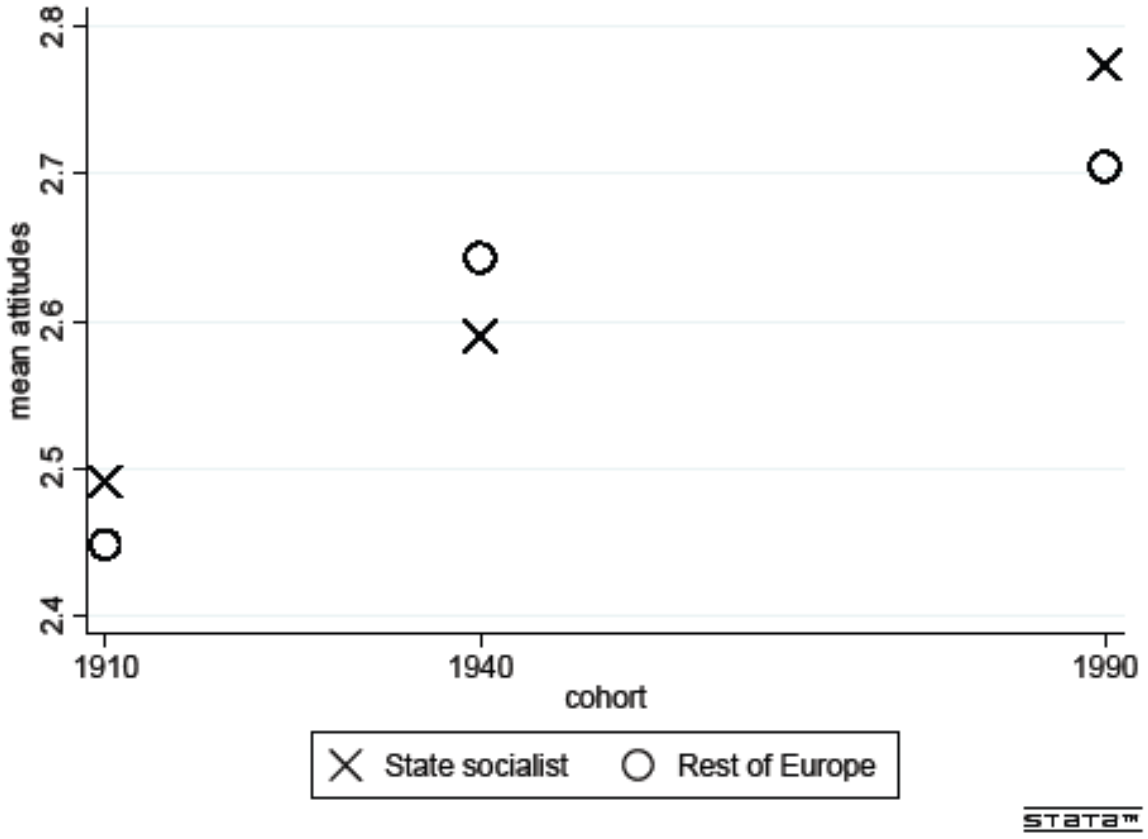

Figure 4: ***Mother Working Hurts Kids: Evolution of Unconditional Mean Attitudes among Immigrants in the US***

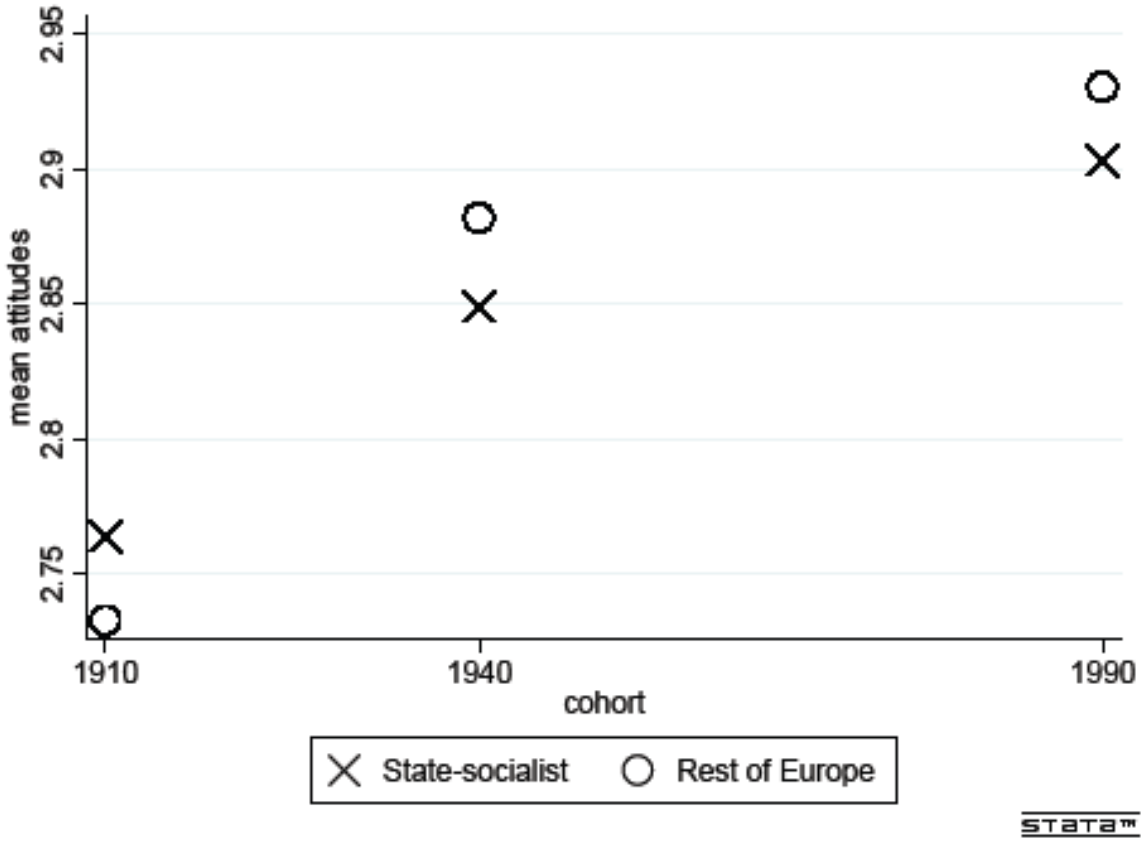

Figure 5: ***Husband and Wife Contribute to Income: Evolution of Unconditional Mean Attitudes among Immigrants in the US (1910 and 1940) and Residents (1990)***

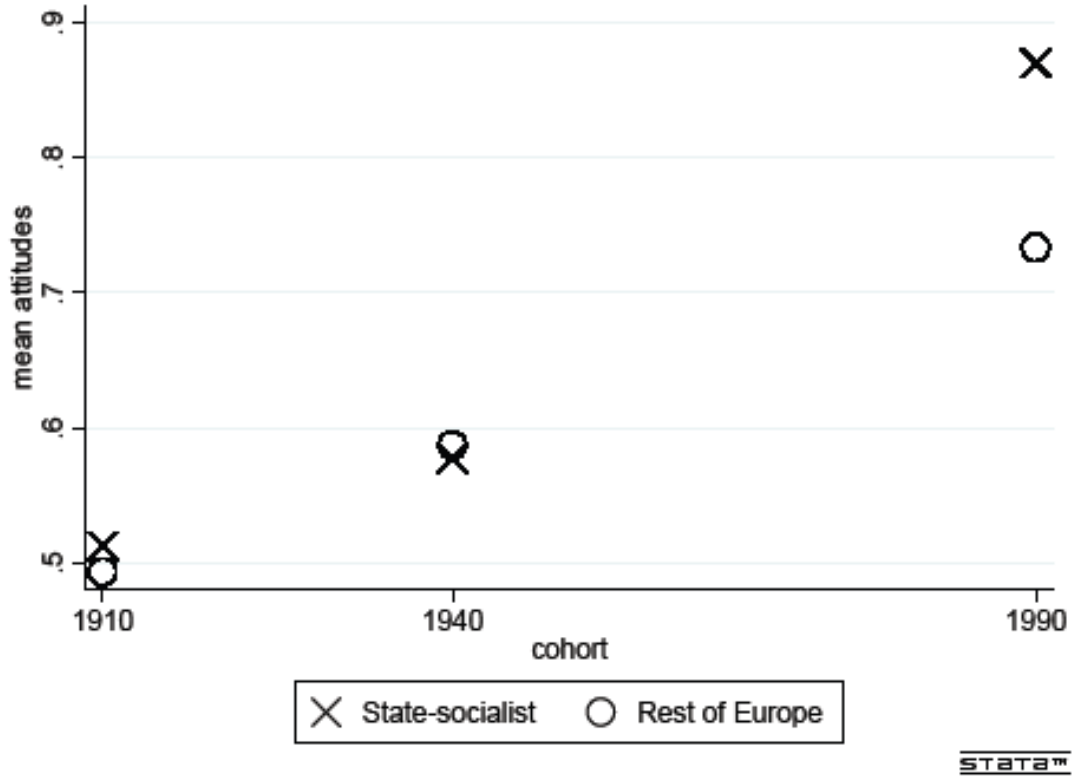

Figure 6: ***Immigrants Representativeness***

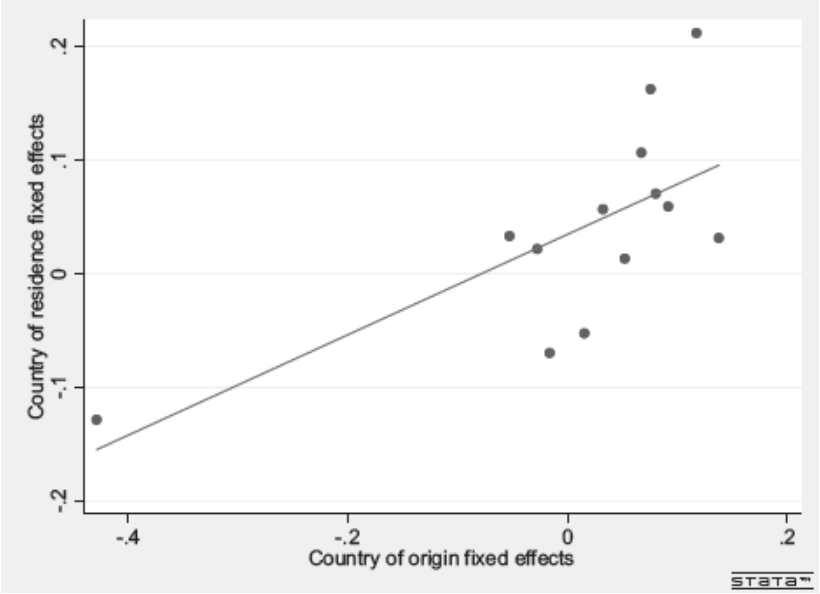

Figure 7: ***Immigrants Representativeness, no Netherlands***

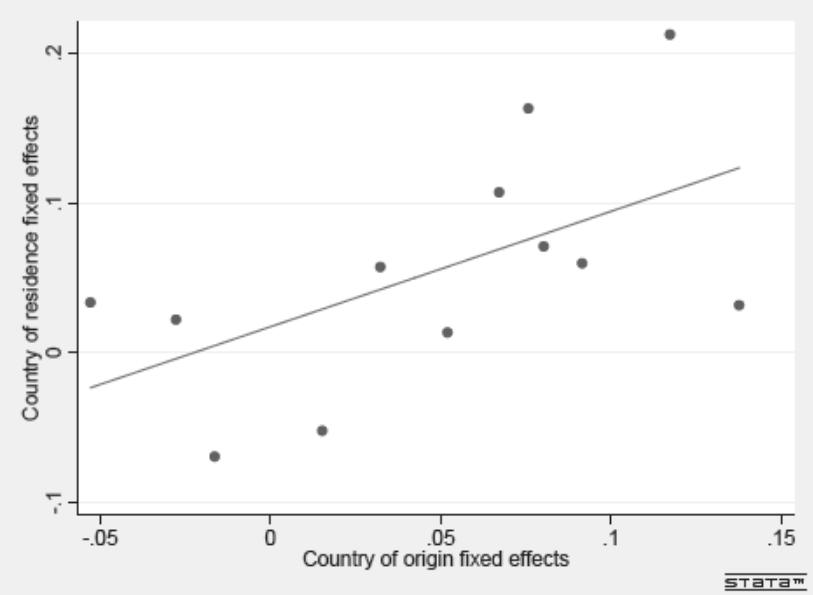

Table 1: ***Female Workers as a Percentage of the Labor Force, in Eight State-Socialist Countries and the OECD, 1950-1988***

Year	Albania	Bulgaria	Chzecosl.	GDR	Hungary	Poland	Romania	Yugoslavia	OECD
1950		27.4(a)	38.4	38.4		33(b)		23.2(c)	31.4
1960	25.1	33.5	42.8	44.3	32.5	32.8	27.1	27	33.6
1970	38.7	41	46.7	47.7	40.6	40	30.1	31	35.2(c)
1980		47.1	45.4	51.0	45.7(d)	44.5		35.5	38.7
1988		49.5(e)	46	50.3	46.0(e)	46.8		38.3(e)	41.6(f)

(a) 1951;(b) 1955;(c) 1971;(d) 1985;(e) 1986;(f) 1989. Source: LaFont (2001)

Table 2: ***Measures of Attitudes***

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**Observations**			
					St-Soc, Before	*St-Soc, After*	*Non St-Soc, Before*	*Non St-Soc, After*
Should Women Work	0.81	0.38	0	1	566	31	5499	173
Better for Men to Work, Woman Tend Home	2.7	0.84	1	4	615	47	6452	312
Preschool Kids Suffer if Mother Works	2.54	0.8	1	4	616	47	6456	311
Mother Working Hurts Kids	2.8	0.87	1	4	615	47	6461	312
Husband and Wife Contribute to Income (a)	0.77	0.42	0	1	590	6979	1634	14594

(a) After is from WVS

Table 3: ***Selection of Immigrants on Observables: Difference in Changes between State-Socialist and non State-Socialist Group***

Dep. variable	**Male**	**Age**	**Income cat**	**Satisfied with Financial Situation**	**Children**	**Education cat**	**Politically Conservative**
State-socialist country x post-1940	0.00823	2.305*	-0.0133	-0.0939	0.0900	-0.00744	-0.280**
	(0.0406)	(1.145)	(0.136)	(0.0798)	(0.0789)	(0.111)	(0.100)

Dep. variable	**Catholic**	**Protestant**	**Jew**	**No Religion**	**Orthodox**	**Other Religion**
State-socialist country x post-1940	-0.254***	0.187***	0.0856**	0.0186	-0.0162	-0.0210
	(0.0541)	(0.0519)	(0.0329)	(0.0457)	(0.0159)	(0.0197)

Dep. variable	**Married**	**Widowed**	**Divorced**	**Separated**	**Other Marriage Status**
State-socialist country x post-1940	0.0730	-0.0125	0.00451	0.0444**	-0.109*
	(0.0440)	(0.0115)	(0.0267)	(0.0198)	(0.0537)

Coefficient and Standard Errors from regression of *Dep. var* on state-socialism, post-1940, state-socialism x post-1940 and regional dummies. Standard Errors clustered by country.

Table 4: ***Country of Family Origin by Cohort*** (%)

Country of family origin	*Cohort 1940*	*Cohort 1990*	Total
Austria	0.92	1.60	0.95
Belgium	0.37	0.29	0.36
Czechoslovakia	2.52	2.18	2.50
Denmark	1.45	1.31	1.45
Finland	0.86	1.16	0.87
France	4.02	3.77	4.01
Greece	0.52	3.77	0.67
Hungary	0.86	2.90	0.95
Ireland	25.60	15.38	25.15
Italy	9.85	30.33	10.75
Lithuania	0.53	1.31	0.56
Netherlands	3.08	3.77	3.11
Norway	3.78	2.03	3.70
Poland	5.26	9.00	5.43
Portugal	0.43	2.03	0.51
Romania	0.16	1.02	0.20
Spain	1.23	4.50	1.37
Sweden	3.46	2.03	3.40
UK	35.11	11.61	34.08

Table 5: ***State-socialism and Attitudes Toward Working Women, Diff-in-Diff Estimation***

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.		**Should Women Work**				**Better for Men to Work, Women Tend Home**		
				with Yugoslavia				with Yugoslavia
Age		0.003	0.001	0.003		0.001	0.002	0.000
		(0.002)	(0.002)	(0.002)		(0.003)	(0.003)	(0.003)
Age squared		-0.000***	-0.000*	-0.000***		-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Male		-0.023***	-0.023***	-0.021***		-0.237***	-0.206***	-0.237***
		(0.006)	(0.006)	(0.006)		(0.017)	(0.018)	(0.017)
Household Income Cat		0.015***	0.015***	0.015***		0.023***	0.032***	0.023***
		(0.002)	(0.002)	(0.002)		(0.005)	(0.005)	(0.005)
Satisfied with financial situation		-0.005	-0.004	-0.005		-0.029**	-0.011	-0.030**
		(0.005)	(0.006)	(0.005)		(0.012)	(0.014)	(0.012)
Children		-0.008**	-0.007**	-0.007**		-0.042***	-0.029***	-0.042***
		(0.003)	(0.003)	(0.003)		(0.007)	(0.008)	(0.007)
Catholic		-0.032**	-0.031	-0.030*		-0.281***	-0.192***	-0.283***
		(0.015)	(0.018)	(0.015)		(0.039)	(0.043)	(0.039)
Protestant		-0.028***	-0.021**	-0.027***		-0.369***	-0.248***	-0.370***
		(0.008)	(0.010)	(0.008)		(0.025)	(0.028)	(0.025)
Jew		0.035*	0.035	0.039*		0.029	0.051	0.023
		(0.020)	(0.021)	(0.020)		(0.087)	(0.073)	(0.085)
Orthodox		0.044**	0.059**	0.014		-0.381*	-0.219	-0.336*
		(0.021)	(0.022)	(0.037)		(0.185)	(0.184)	(0.161)
Other Religion		-0.007	-0.017	-0.007		-0.167**	-0.122*	-0.180**
		(0.042)	(0.045)	(0.041)		(0.068)	(0.064)	(0.070)
Education Cat		0.048***	0.047***	0.047***		0.153***	0.150***	0.153***

		(0.004)	(0.004)	(0.003)		(0.004)	(0.004)	(0.005)
Politically Conservative			-0.012***				-0.112***	
			(0.004)				(0.010)	
Married			-0.001				-0.086***	
			(0.010)				(0.027)	
Widowed			-0.053**				-0.042	
			(0.024)				(0.042)	
Divorced			0.030**				0.024	
			(0.012)				(0.019)	
Separated			0.057*				0.140*	
			(0.030)				(0.076)	
State-socialist country	0.005	-0.005	-0.012	-0.012	0.028	-0.002	0.002	-0.009
	(0.019)	(0.014)	(0.015)	(0.015)	(0.023)	(0.029)	(0.026)	(0.030)
Post-1940	0.095***	0.019	0.018	0.019	0.297***	0.009	0.011	0.008
	(0.010)	(0.015)	(0.016)	(0.015)	(0.031)	(0.033)	(0.035)	(0.033)
State-socialist country x post-1940	**0.060****	**0.046***	**0.046***	**0.065***	**0.093**	**0.163**	**0.122**	**0.078**
	(0.026)	**(0.023)**	**(0.026)**	**(0.033)**	**(0.127)**	**(0.114)**	**(0.118)**	**(0.139)**
Observations	7,206	6,591	6,302	6,658	8,278	7,471	7,461	7,530
Adjusted R-squared	0.004	0.086	0.088	0.085	0.015	0.201	0.235	0.200
Regional Dummies	YES	YES	YES	YES	YES	YES	YES	YES
P-value cluster country-wave	0.018	0.050	0.057	0.025	0.503	0.180	0.322	0.602
P-value t[g-k]	0.048	.	.	.	0.481	.	.	.
P-value CGM	0.162	0.128	0.148	0.170	0.526	0.314	0.432	0.643
Eydx	**0.074**	**0.057**	**0.058**	**0.081**	**0.035**	**0.061**	**0.046**	**0.029**

SE clustered by country in parenthesis; number of clusters by country is 19, which doubles when the cluster is done by country-wave; P-value cluster country-wave is the p-value for the coefficient on *State-socialist x post-1940* when the standard errors are clustered by country-wave. P-value t[g-k] is the p-value for the coefficient on *State-socialist x post-1940*. When deriving this p-value, a t-distribution with a g-k degree of freedom is assumed for the test statistic, where g is the number of clusters and k the number of regressors, as opposed to the t-distribution with g-1 degrees of freedom assumed in the standard Stata routine. This p-value is not calculated in the regression with controls, because k > g. The CGM P-value is the p-value for the coefficient *Communist bloc x post-1940* derived from the wild cluster bootstrap described in Cameron, Gelbach and Miller (2008) and implemented using the Stata command *cgmwildboot*. Regional dummies cannot be included when the Standard Errors are bootstrapped. Eydx is the semi-elasticity of the dependent variable to *State-socialist x post-1940*

Table 6: ***State-Socialism and Attitudes Toward Working Women, Diff-in-Diff Estimation GSS plus WVS***

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.			**Should Husband and Wife Contribute to Income**			
				No Germany	Countries represented before and after	Countries in GSS analysis
Age		0.002	0.002	0.002	-0.000	0.001
		(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
Age squared		-0.000	-0.000	-0.000	0.000	-0.000
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Male		-0.048***	-0.044***	-0.047***	-0.053***	-0.051***
		(0.006)	(0.006)	(0.007)	(0.008)	(0.009)
Satisfied with financial situation		-0.010	-0.008	-0.009	-0.016*	-0.012
		(0.006)	(0.006)	(0.007)	(0.009)	(0.009)
income_cat		-0.019**	-0.017**	-0.024**	-0.024**	-0.032**
		(0.007)	(0.008)	(0.009)	(0.010)	(0.013)
Children		-0.020***	-0.015***	-0.018***	-0.023***	-0.021***
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Catholic		-0.029**	-0.015	-0.020	-0.020	-0.003
		(0.014)	(0.012)	(0.013)	(0.016)	(0.008)
Protestant		-0.038**	-0.035**	-0.038**	-0.038*	-0.032
		(0.014)	(0.013)	(0.016)	(0.017)	(0.018)
Jew		0.025	0.032	0.052	0.058	0.074
		(0.047)	(0.041)	(0.059)	(0.059)	(0.085)
Orthodox		0.007	-0.007	0.008	-0.000	0.005
		(0.012)	(0.018)	(0.016)	(0.030)	(0.027)
Other Religion		-0.051**	-0.057***	-0.055***	-0.088***	-0.083***
		(0.019)	(0.019)	(0.018)	(0.023)	(0.020)
Education Cat		-0.013**	-0.013**	-0.011	-0.017*	-0.014

		(0.006)	(0.006)	(0.007)	(0.008)	(0.010)
Politically Conservative			-0.018***			
			(0.003)			
Married			-0.047***			
			(0.012)			
Widowed			-0.049**			
			(0.018)			
Divorced			0.017			
			(0.019)			
Separated			0.013			
			(0.026)			
Post-1940	0.179***	0.146***	0.145***	0.178***	0.141***	0.176***
	(0.035)	(0.030)	(0.031)	(0.022)	(0.031)	(0.022)
State-socialist x Post-1940	**0.149*** **	**0.141*** **	**0.148*** **	**0.073** **	**0.140*** **	**0.069***
	(0.036)	**(0.033)**	**(0.034)**	**(0.033)**	**(0.034)**	**(0.037)**
Constant	0.555***	0.720***	0.773***	0.712***	0.798***	0.762***
	(0.023)	(0.048)	(0.055)	(0.041)	(0.059)	(0.056)
Observations	36,828	29,101	23,450	23,537	18,814	14,927
Adjusted R-squared	0.092	0.103	0.120	0.106	0.124	0.126
Country FE	YES	YES	YES	YES	YES	YES
P-value cluster country-wave	0.000	0.000	0.000	0.017	0.000	0.028
P-value t[g-k]	0.000	.	.	.	.	.
Eydx	**0.197**	**0.189**	**0.201**	**0.099**	**0.192**	**0.093**

SE clustered by country in parenthesis; number of clusters by country varies from 12 to 25, depending on the specification. P-value cluster country-wave is the p-value for the coefficient on *State-socialist x post-1940* when the standard errors are clustered by country-wave. P-value t[g-k] is the p-value for the coefficient on *State-socialist x post-1940*. When deriving this p-value, a t-distribution with g-k degree of freedom is assumed for the test statistic, where g is the number of clusters and k the number of regressors, as opposed to the t-distribution with g-1 degrees of freedom assumed in the standard Stata routine. This p-value is not calculated in the regression with controls, because k > g. Eydx is the semi-elasticity of the dependent variable to *State-soviet x post-1940*

Table 7: State-socialism and attitudes about the compatibility of work and motherhood, Diff-in-Diff estimates using GSS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.		**Preschool Kids Suffer if Mother Works**				**Mother Working Hurts Kids**		
				with Yugoslavia				with Yugoslavia
Age		-0.004	-0.006**	-0.004*		0.007**	0.005	0.006**
		(0.002)	(0.003)	(0.002)		(0.003)	(0.003)	(0.003)
Age squared		-0.000	-0.000	-0.000		-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)		(0.000)	(0.000)	(0.000)
Male		-0.307***	-0.274***	-0.307***		-0.385***	-0.355***	-0.383***
		(0.014)	(0.013)	(0.014)		(0.012)	(0.013)	(0.012)
Household Income Cat		0.019***	0.024***	0.019***		0.026***	0.033***	0.026***
		(0.004)	(0.005)	(0.004)		(0.004)	(0.004)	(0.004)
Satisfied with financial situation		0.015	0.028*	0.014		-0.017	-0.001	-0.019
		(0.012)	(0.014)	(0.012)		(0.020)	(0.022)	(0.020)
Children		-0.016***	-0.017***	-0.016***		-0.019***	-0.013*	-0.018***
		(0.005)	(0.006)	(0.005)		(0.005)	(0.006)	(0.005)
Catholic		-0.100***	-0.034	-0.098***		-0.112***	-0.042	-0.110***
		(0.030)	(0.030)	(0.029)		(0.026)	(0.031)	(0.026)
Protestant		-0.188***	-0.105***	-0.184***		-0.179***	-0.086***	-0.177***
		(0.016)	(0.013)	(0.017)		(0.017)	(0.017)	(0.017)
Jew		-0.030	-0.013	-0.023		-0.011	0.011	0.004
		(0.098)	(0.095)	(0.097)		(0.105)	(0.108)	(0.103)
Orthodox		-0.353*	-0.214	-0.389**		-0.400**	-0.266	-0.388**
		(0.170)	(0.183)	(0.150)		(0.154)	(0.164)	(0.138)
Other Religion		-0.035	-0.003	-0.044		-0.056	-0.025	-0.062
		(0.046)	(0.044)	(0.047)		(0.058)	(0.057)	(0.058)

Education Cat		0.068***	0.069***	0.069***		0.088***	0.087***	0.089***
		(0.012)	(0.012)	(0.012)		(0.010)	(0.009)	(0.010)
Politically Conservative			-0.085***				-0.086***	
			(0.005)				(0.006)	
Married			0.061*				-0.017	
			(0.033)				(0.021)	
Widowed			0.141***				0.022	
			(0.038)				(0.038)	
Divorced			0.149***				0.124***	
			(0.024)				(0.035)	
Separated			0.208***				0.170***	
			(0.062)				(0.045)	
State-socialist country	0.039	0.013	0.011	-0.007	0.038	0.028	0.033	0.014
	(0.032)	(0.035)	(0.037)	(0.038)	(0.040)	(0.036)	(0.034)	(0.039)
Post-1940	0.167***	0.033	0.043	0.033	0.124***	-0.004	0.002	-0.005
	(0.035)	(0.040)	(0.041)	(0.040)	(0.041)	(0.048)	(0.049)	(0.048)
State-socialist country x post-1940	**0.046**	**0.098**	**0.060**	**0.053**	**-0.047**	**0.032**	**-0.009**	**0.039**
	(0.076)	**(0.082)**	**(0.090)**	**(0.076)**	**(0.101)**	**(0.120)**	**(0.127)**	**(0.110)**
Observations	8,281	7,475	7,465	7,533	8,289	7,480	7,470	7,539
Adjusted R-squared	0.004	0.089	0.110	0.088	0.003	0.097	0.117	0.097
Regional Dummies	YES	YES	YES	YES	YES	YES	YES	YES
P-value cluster country-wave	0.549				0.581		0.322	
P-value t[g-k]	0.559	0.286	0.542	0.561	0.658	0.753	0.936	0.669
Eydx	**0.018**	**0.039**	**0.024**	**0.021**	**-0.017**	**0.011**	**-0.003**	**0.014**

SE clustered by country in parenthesis; the number of clusters by country is 19. P-value cluster country-wave is the p-value for the coefficient on *State-socialist x post-1940* when the standard errors are clustered by country-wave. P-value t[g-k] is the p-value for the coefficient on *State-socialist x post-1940*. When deriving this p-value, a t-distribution with g-k degree of freedom is assumed for the test statistic, where g is the number of clusters and k the number of regressors, as opposed to the t-distribution with g-1 degrees of freedom assumed in the standard Stata routine. This p-value is not calculated in the regression with controls, because k > g. Eydx is the semi-elasticity of the dependent variable to *State-soviet x post-1940*

Table 8: Immigrants' representativeness

	(1)
Dep. var.	**Husband and Wife Should Contribute to the Income**
Average attitudes home country	0.377***
	(0.0916)
Observations	1,929
Adjusted R-squared	0.070
Controls	YES
Region Dummies	YES
P-value CGM	0.000
Eyex	**0.552**

SE clustered by country in parenthesis; number of clusters is 15. The CGM P-value is the p-value for the coefficient *Average attitudes home country*, derived from the wild cluster bootstrap described in Cameron, Gelbach and Miller (2008) and implemented using the Stata command *cgmwildboot* Eydx is the elasticity of the dependent variable to *Average attitudes home country*.

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