The Impact of a Permanent Income Shock on the Situation of Women in the Household: The Case of a Pension Reform in Argentina

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October 10, 2017

Abstract

Income transfers from social programs are often not gender neutral and should, according to the vast literature on intra-household decision making and allocation, affect the distribution of bargaining power within the household. This result, however, was by and large established under the assumption of marriage stability. If this assumption does not hold, then the positive response of bargaining power to income found in the empirical research may be the artefact of sample selection. One may postulate, however, that when restricted to certain groups in the population, such as seniors, the assumption may hold since their probability of divorce is close to zero. In this paper we prove that the assumption is wrong, even when applied to seniors. We use a non-contributory pension reform in Argentina, that resulted in an unexpected and substantial increase in permanent income for around 1.8 million women, to study its effects on outcomes related to both marital stability and women's bargaining power within the household. We find that the reform increased the probability of divorce/separation among senior highly educated women but had no impact on the low-educated. Instead, the latter gained considerable bargaining power within the household by decreasing the probability of being the only one in charge of household chores in tandem with an increase in their husbands' participation in these chores.

1 Introduction

Over the past two decades, several countries, mainly in Latin America, introduced social programs to fight poverty and inequality. Amongst the most expensive programs are non-contributory pensions aimed at extending pension coverage to individuals that did not fully contribute to the social security system (Levy and Schady 2013). These pension programs are often not gender neutral. For example in Latin America, women are particularly affected because of their low attachment to the formal labor market. Thus, these transfer programs do not only redistribute income between households but also alter permanently the distribution of income within households.

The non-contributory pension reform implemented in Argentina in 2007, known as the moratorium, is an ideal setting in which to analyze the effects of exogenous, asymmetric (women were favored), and permanent income effects within the household. Upon the reform, the percentage of pension beneficiaries among elderly women went from 57% in 2005 to 92% in 2009, the highest in South America (Rofman et al. 2014, Benigni et al. 2012), and by 2010 more than 1.8 million women were direct beneficiaries (D'Elia et al. 2011). The reform resulted in a sizable spending of public money which amounted to 2.4% of GDP (Lustig and Pessino 2013).

The vast literature on decision making and allocation within the household predicts that income increases such as those that occurred in Argentina should have a positive effect on female bargaining power.[1] Most of this literature, however, either assumes or is conditional on marriage stability.[2] If this assumption does not hold and income shocks cause some couples to breakup, the positive estimated effects of income shocks on bargaining power (amongst those who remain married) revealed in the literature may simply be a consequence of sample selection. One may postulate that the marriage stability assumption is acceptable when applied to senior couples because of their stable lives and low probability of divorce.[3] In this paper, we prove this is not the case; we find large and signifi-

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[1]Some references are: Becker 1974, Manser and Brown 1980, McElroy and Horney 1981, McElroy 1990, Thomas 1990, Schultz 1990, Lundberg and Pollak 1993, Bourguignon et al. 1993, Lundberg and Pollak 1996, Lundberg et al. 1997, Gray 1998, Browning and Chiappori 1998, Chiappori 1988, Ward-Batts 2008, Attanasio and Lechene 2002, Brown 2009 and Browning et al. 2011 for a thoroughly survey. The first empirical papers in this literature aimed to test 'income pooling' in the household, a necessary condition of the unitary model. By rejecting that model, the link between income and bargaining power is, at least indirectly, established.

[2]Divorce or union dissolution are treated as threat points that are not observed in equilibrium. There are some exceptions such as Schultz 1990—where the possibility of divorce is taken into account empirically although the author conveys the need for more convincing exclusion restrictions—, and Gray 1998 who did not find effects of changes in divorce laws on divorce probabilities.

[3] Using microdata from the 2009 American Community Survey, which provide detailed information regarding changes

cant effects of the Argentinean pension reform on the probability of divorce/separation among senior women. And considering only those women not affected by selection through divorce/separations, i.e. the less educated, we then look for effects of the reform (and of shocks to the female income share in general) on measures of bargaining power. The less educated is a large group, representing 60% of the sample of married/cohabiting women, and it is among them that we find the largest impact on income. In short, the main contribution of our paper is to analyze the effects of permanent income shocks on bargaining power together with the effects on selection through divorce/separation.

The estimated effect on divorce/separation concurs with the 'independence effect´ hypothesis (Ross et al. 1975, also supported by Becker 1974), which predicted an increase in divorce/separation rates with the increase in married women's incomes relative to their husbands'.[4] Our empirical analysis on this topic is most related to Bobonis 2011, who estimates the effect of conditional cash transfers to women from the PROGRESA program in Mexico on union dissolution, although it differs in meaningful ways. First, we look at the effects of transfers on a sample of senior women. Second, we look at the effects of permanent unconditional transfers —as opposed to transitory and conditional transfers. Third, and most important, the total amount transferred exceeds by far the amounts transferred in the case of Bobonis 2011 and in other related papers. More specifically, the net present value received by an Argentinean woman who has made no social security contributions is around 38,135 USD 2009, which represents roughly 5 times the amount transferred by the Mexican program PROGRESA, 4 times the amount of the early 1990s South African pension reform, and 11 times the amount transferred by the Mexican program "70 y más".[5] Finally, presumably because the transferred amounts are so much larger, our results are strikingly different from previous papers:

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to the family structure including divorce, we computed the average 12-month probability of divorce for women aged 60-65 to be around 0.7 percent. Unfortunately, there are no such surveys for Argentina, but from administrative data for the city of Buenos Aires (Institute of Statistics and the Census of Buenos Aires) we calculate a probability of divorce of approximately 0.73 percent for women aged 60-65.

[4]The weight of the evidence in the economic literature favors the 'independence effect' hypothesis (e.g. Becker et al. 1977, Weiss and Willis 1997, Weiss 1997, Jalovaara 2003, Bobonis 2011, and Doiron and Mendolia 2011), although there are notable exceptions (Hoffman and Duncan 1995 and more recently Hankins and Hoekstra 2011).

[5]The net present value figures were computed assuming a conservative 5.3% interest rate and all monetary values were converted to USD 2009 (WDI 2009, World Bank, see footnote 15 for more details on this conversion). We assume a life-expectancy of 15 years i.e. life until 75 years old. The comparison number for the South African pension reform (described for example in Edmonds et al. 2005, Duflo, 2000) is 10,434 USD 2009 and is calculated under the exact same assumption about life-expectancy, although it is an overestimation since life-expectancy for the South African female population is lower. Under the Mexican program "70 y más" described in Galiani et al. 2016, seniors start receiving transfers at the age of 70 years old. Hence, to be consistent we assume a life-expectancy of 5 years, which makes up a total of 3,336 USD 2009. Finally, for the case of the CCT program PROGRESA (e.g. Bobonis 2011), we assume families may be receiving the benefit for at most 7 years, totaling 7,575 USD 2009.

while the literature finds either no impact of transfers on the independence of older women (Edmonds et al. 2005), or a modest impact on marital dissolution of younger women (Bobonis 2011), we find a large impact on divorce/separation of older women.

Contrary to divorce/separation, bargaining power is not directly observable. To circumvent this shortcoming, the empirical literature has focused on variables arguably described as outcomes of intra-household bargaining (e.g. female and children's consumption, children's health and nutrition).[6] We follow the same strategy but focus on outcomes related to household production i.e. household chores. Very little is known about the effect of income (or bargaining power more generally) on the non-market labor in the form of household chores.[7] Importantly, household production or chores may react to income very differently from other outcomes. The reason lies in the potentially different interaction between bargaining power, personal preferences for household chores and domestic goods, and household members' relative productivities in housework and in the labor market (Gupta and Stratton 2010, Browning et al., 2011). It is, thus, a truly empirical issue to know how housework reacts to asymmetric income shocks. An extreme example is the evidence of the "do gender" hypothesis (e.g. Brines 1994, Bittman et al. 2003, and more recently Bertrand et al. 2015) whereby husbands' (wives') participation in housework decreases (increases) with the wives' income share when husbands' income is lower than that of their wives. Because the income share of the average wife in our sample is relatively low (between 26-30%) it is not surprising that we do not find evidence in support of the "do gender" hypothesis. Instead, we find that the Argentinean pension reform led to higher male participation in household chores.

But to what extent can we argue that this finding is a sign of female empowerment? One possibility is that higher male participation in household chores allows women to enjoy more leisure

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[6]Recent papers by Majlesi 2014 and Ambler 2015 interpret direct answers regarding the identity of the decision makers in household surveys as more direct measures of bargaining power. There is at least one potential drawback regarding this interpretation for at least some of the measures considered. For example, the wife may decide on what is for supper but how often does she take her husbands' preferences into account instead of hers? When she cooks her husbands' favorite dish more often than her own then, although formally she is the decision maker, we argue that it is her husband who holds the real bargaining power. In these circumstances, outcomes may be closer to the real bargaining power than the identity of the decision maker. Further criticisms of these measures are pointed out in Attanasio and Lechene 2002.

[7]Sociologists who studied the non-causal relationship between household work and earnings found in general a negative correlation between women's earnings and household work (see for example Bittman et al. 2003 and the references there in). In the Economics literature Friedberg and Webb 2005 estimate the relation between relative (hourly) wages (which they use as a proxy for bargaining power) and hours devoted to household chores. They present a number of specifications that minimize the endogeneity problems but suggest that future research should look for exogenous income sources. Our evaluation of the Argentinean pension reform would fill the void.

and hence increase their well-being. Unfortunately, we neither observe leisure time nor the number of hours devoted to household chores. It is conceivable that an increase in male participation follows an even larger increase in female time devoted to household chores. The other possibility focuses on the relative contribution of husbands and wives to household production (e.g. Cooke 2006, Brown 2009, Kornrich et al. 2013).[8] We follow a similar approach and use indicators of shared housework. Hence, if after an asymmetric income shock we observe a more equal distribution of household chores, we interpret this as evidence of increased female bargaining power.[9,10]

Our paper is also directly related to important empirical literature on the consequences of public transfers. This literature has focused mainly on the labor supply response of the recipients and other household members, on children's nutritional and educational outcomes, and on consumption (e.g. Duflo 2000, Attanasio and Lechene 2002, Bertrand et al. 2003, Edmonds 2006, Posel et al. 2006, Sienaert 2008, Ardington et al. 2009, Ponczek 2011, de Carvalho Filho 2012, Bosch and Guajardo 2012, Juárez and Pfutze 2012, Danzer 2013, Galiani et al. 2016). Instead, we focus on a different set of outcomes directly related to the well-being of female recipients, such as marital stability and cooperation in household chores.

Using the Argentinean Continuous Permanent Household Survey (or EPH from the Spanish acronym for *Encuesta Permanente de Hogares Continua*) for 2004-2009, we estimate the effects of the reform by differences-in-differences (DD here after). As such, the reform increased the probability of receiving a pension by 53 percentage-points (pp), which translated into a 36 pp decrease in the probability of having no income and an average increase in monthly income of 62 USD adjusted for 2009 purchasing power parity.

Likely through its effect on income, the reform brought about an increase in the probability of divorce or separation as well as an increase in the outcomes related to the wife's bargaining power. Concretely, the probability of divorce/separation increased significantly by 2.6 pp, i.e., an increase

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[8]Szinovacz 2000, for example, reports an increase in time devoted to household chores upon retirement.

[9]This result would also be consistent with Lundberg and Pollak (1993)'s "separate spheres" bargaining model, where divorce is not an option, and failure of negotiation between the couple would lead to a non-cooperative equilibrium with an under-provision of the public good that each member of the couple provides according to gender specialization (e.g. the supply of household services in the case of women).

[10]Bittman et al. 2003, for example, find that an increase in the share of women's income in the household is not related to an increase in husbands' participation in housework. In their paper, however, although they have a large set of controls, observed income shares are taken as exogenous variables, while in our case, we have an exogenous income change.

of roughly 18%. This estimate, however, is somewhat misleading as the reform affected women of different education levels very differently. We find that while there is a 5.6 pp increase in the probability of divorce/separation amongst those with at least a high-school diploma, there is no effect on the less educated. Finding an effect of the reform on divorce/separations raises concerns over the validity of the bargaining power regressions on the sample of married/cohabiting women due to sample selection. To avoid contamination from sample selection, we analyse bargaining power effects on the sample of married/cohabiting women who are low-educated. This group of women is of considerable interest for policy-making since it comprises those with no or little attachment to the formal labor market who were therefore the most affected by the reform. In this sample, we find a statistically significant decrease of 7 pp (or 11.7%) in the probability that wives are the only ones in charge of household chores, and a statistically significant increase of 6.1 pp (or 16.5%) in the probability that husbands do some household chores. More broadly, we can estimate the effect of an increase in the wife's income share on their bargaining power by using the reform as an instrumental variable for the female income share. Results from this approach, imply that a 10 pp increase in the wife's income share within the couple leads to a 4.7 pp (or roughly 8%) decrease in the wife's sole participation in household chores and an increase in the husband's participation in household chores of 4 pp (or roughly 10%). We believe such large effects on divorce/separations and bargaining power were possible because transfers were sizable and permanent. Importantly, we confirm that our results are not driven by age differences between the control and the treatment group, by running robustness checks using placebo treatments and different control groups.

We conclude that as a consequence of the increase in income brought about by the Argentinean pension reform, highly-educated women were more likely to opt out of their marriages by increasing their probability of divorce/separation while the low-educated women opt in and gain more bargaining power within their marriages.

The rest of the paper is organized as follows. Section 2 describes the pension reform in Argentina. Section 3 describes the data set. Section 4 describes the empirical strategy. Section 5 describes the main results. Section 6 shows the results of placebo treatments and other robustness checks and Section 7 concludes.

2   The Pension Reform

In Argentina, women can retire at 60 and men at 65 years of age. Besides reaching the retirement age, a worker must have 30 years of social security (hereafter SS) contributions to be entitled to collect a pension. These requisites, together with a traditionally low female participation in the labor market (around 44% in the 80s, ILO 2011) and an increasingly high level of informal jobs[11], resulted in low pension coverage amongst women; by 2004 only 55% of age-eligible women received a pension, and only 35% when excluding widows (see Figure 2). Pension coverage was higher for males (75%) because of their greater participation in the labor force.

In December 2004, the Argentinean Government approved a reform to the pension system (Law 25994), that extended pension and health insurance benefits to people of retirement age, i.e., cohorts 1944 and older if female and cohorts 1939 and older if male, but who did not fulfill the 30-year SS contribution requirement. The reform was implemented through a payment schedule, which was officially named moratorium and popularly known as the housewives pension because housewives were perceived as the group of the population that benefited the most. The payment schedule consisted of paying back to the SS system the amount corresponding to the number of years (up to 30) the individual had failed to contribute subject to a cap. The debt to the SS would be paid in up to 60 monthly installments and was deducted directly from the individual's monthly pension benefit. The law established a maximum deduction of 49% (Lustig and Pessino 2013), which implied that only a fraction of the debt would be paid. Housewives, for example, who had never contributed to SS would receive a pension equivalent to 51% of the minimum pension during the first 5 years, i.e. 304 ARS in 2007 or 191 U.S. dollars PPP 2009 per month, and 596 ARS or 374 U.S. dollars PPP 2009 per month afterward. This minimum transfer was just enough to cover the basket of basic needs for an adult in Argentina in January 2007, which cost 295.89 ARS (Source: INDEC). Assuming a 15-year life expectancy and a conservative 5.3% real interest rate (WDI 2009, World Bank), these figures imply that each woman who claims the full benefits from the moratorium costs the Argentinean State over 38,135 US PPP 2009 in net present value. Importantly, benefits were not automatic and those eligible had to apply in order to benefit from a pension.

Law 25994 expired in April 2007, but this had no effect on the 1944 or older cohorts because

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[11]Tornarolli and Conconi 2007 report a 45% level of informality for all workers, but the value for women is likely to be higher (ILO 2011).

of the prior approval of Decree 1454/05 in December 2005. Decree 1454/05 extended some of the moratorium benefits to younger cohorts as they reached 60 years old, although it established more stringent eligibility criteria.[12] Hence, the pension reform affected different cohorts differently: older cohorts, born in 1944 or earlier, benefited relatively more than younger cohorts. In this paper, we concentrate on the effects of the first law that affected cohorts born in 1944 or earlier.

Law 25994 was unusual in that it was discussed and approved in the Senate and the Congress on the same day, December 16, 2004. This unusual trajectory and the lack of reference to the benefits in the preceding Bill (Bill 1183-D-03 of April 2003), are likely responsible for the scarce media coverage that the law received before December 2004. Despite the unusually fast approval process, the pension reform took more than 2 years to be fully implemented. This delay is documented in D'Elia et al. 2011 and is consistent with the number of pension recipients and income effects observed in our data (see Figure 2). One reason for the late implementation was the delay in regulating the process of accessing pensions under the moratorium, which was completed in July 2006 (BOE 30870, March 21, 2006 and Resolución General Conjunta AFIP 2091/2006, July 2006). Media coverage of the reform understandably peaked during this period. As we show in Figure A.1, news stories about the reform in the two major newspapers in Argentina, *La Nación* and *El Clarin*, appeared mainly in the last months of 2006. Concerns about strategic or biased news coverage are dismissed by the similar pattern shown over time in Google search counts (see Appendix Figure A.2).

3   Data

We use the Argentinean Continuous Permanent Household Survey (EPH) for the years 2004-2009.[13] The EPH is a rotating panel quarterly survey. Approximately 25,000 households are surveyed every quarter. Households are in the panel for four quarters in two alternating periods of two quarters

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[12]Decree 1454/05 only extends the moratorium to the self-employed among the younger cohorts. We could not find evidence as to whether this requirement was effectively monitored or binding in practice. This aspect, however, is not of substance for our analysis.

[13]We cannot consider the years before 2004 since there were important methodological changes to the EPH in 2003. Nor do we include data after 2009 to ensure that women that would turn 60 are not included in the control group. Another important reason to leave years after 2009 out of the sample is the announcement and implementation of the Universal Child Allowance Program (*Asignación Universal por Hijo para Protección Social* Garganta and Gasparini 2015) at the end of 2009. This program paid a monthly amount per child under 18 to parents working in the informal sector or unemployed (Resolution ANSES Nº 393/2009). By 2011 almost 95% of the beneficiaries were women (ANSES, 2012). Data for the third quarter of 2007 is not available because some regions could not be surveyed because of administrative problems in the Statistics Office.

each, spanning one and a half years. Because of the short period that each household is in the survey, we do not explore its panel dimension. The survey covers 32 urban regions representing 62% of the country's population but we must restrict our analysis to the 29 urban areas that were covered by the EPH before 2006.

The survey includes one household questionnaire and individual questionnaires for every person in the household. The questionnaires include questions about housing conditions, household and individual incomes, demographic characteristics, occupation and working conditions, certain types of social benefits, etc. Unfortunately, there is neither information on the number of years individuals have contributed to the SS system, which would have allowed us to identify individuals directly affected by the policy, nor can we identify individuals claiming benefits from the moratorium.

In our empirical analysis below we start by measuring the effect of the reform on different measures of personal income. Concretely, we consider the following income measures: the probability of receiving a pension,[14] the probability of not having any personal income, the amount of monthly personal income, and for married/cohabiting women the wife's share of income within the couple.[15] We transform Argentinean currency (Pesos ARS) to U.S. dollars (USD) using the purchasing power parity (PPP) conversion factor for private consumption in 2009.[16]

Our main outcome variables are: the probability of divorce or separation and measures of sharing housework. Regarding the former, we pool divorcees and separated individuals because the survey question does not allow us to distinguish between them. The measures of sharing housework are analysed for all married/cohabiting women or restricted to those who live only with their husband/partner, to better account for changes in bargaining power within the couple. The information used to construct the housework sharing variables comes from the household module of the survey. The respondent to the household module identifies which household members contribute to household duties, whether they do most of the housework or just help, as well as whether the household

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[14]We constructed a dummy variable for "receives a pension" from the "income from pensions" category of the EPH. Only 2% of pension income was imputed by the Statistics Office.

[15]Because monthly income is sometimes left unreported, the Statistics Office created a twin variable where missing values were imputed. In our sample only 9% percent of the observations had imputed monthly income.

[16]The PPP conversion factor is published by the International Comparison Program database (World Bank). This factor represents the units of ARS required to buy the same amount of goods and services in the domestic market as 1 USD would buy in the United States. We also use the U.S. annual inflation rate to adjust for price changes in the United States throughout our period of analysis. Therefore, 1 U.S. dollar PPP 2009 has the same purchasing power as 1 USD in the United States in 2009.

has domestic service or receives some external help.[17] We constructed the following variables: i) A dummy variable that takes the value 1 if the wife is the *only* person responsible for most of the household chores, i.e., no other household member is identified as in charge of *most* of the household chores; ii) a dummy variable that takes the value 1 if the husband collaborates in housework, whether or not he is the only one responsible; and iii) a dummy variable that takes the value 1 if the household has domestic service or external help with housework.

Other individual characteristics that we include as control variables in our regressions are: cohort dummies; region of residence (dummies for the 29 urban areas, *Aglomerado* in Spanish, where the EPH is conducted); educational level with high and low levels of education dummies, where a low level of education indicates less than a high school diploma; a dummy variable to identify those who were born abroad because most of those women are not able to benefit from the moratorium; and on the personal income regressions we additionally include a dummy that identifies those that belong to the richest one percent of the female sample to capture outliers in the income variable. In the regressions on the sample of married/cohabiting women we also control for variables that reflect age and educational differences between spouses that takes a positive value when the wife is more educated[18] and a dummy that takes value one when the husband is more than 65 years old to account for changes in a husband's behavior as a result of retirement.

Our sample includes women born between 1941-1944 and 1950-1953 (see Section 4 for the details about the sample selection) who are either married/cohabiting or divorced/separated and we use the EPH as a collection of repeated cross sections.[19] The final database contains $34,036$ individual observations of women aged between 51 and 68 years old. In the pre-reform period, 17% of these women were divorced or separated, and the rest were married or cohabiting (see column 5 in the first panel of Table 2). Panels B and C in Table 2 show summary statistics by educational group.

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[17]Specifically, we use the closed-form answers to the following questions on the household questionnaire: 1) *"Who does most of the housework?"* Respondents can indicate up to two household members, domestic service or other help from people who do not live in the household; 2) *"Which other people help in the household chores?"* where respondents indicate which other household members help with these chores, or whether they receive external help or have domestic service.

[18]The maximum level of education attained is a categorical variable available in the EPH. We assign values between 0 and 6 to each category. The maximum education category is "Higher Education (complete)" with the value 6, and the minimum is "no formal education" with the value 0. The other categories are "elementary school (incomplete)", "elementary school (complete)", "high school (incomplete)", "high school (complete)" and "higher education (incomplete)".

[19]For the sake of brevity we do not report results for samples including all women but results hold and are available upon request.

In the subsample of married/cohabiting women living only with their partner analysed in Section 5, their personal income represented on average only 21% of the couple's total income in the pre-reform period, 60% of them were uniquely responsible for doing the household chores, and 37% of the husbands collaborated in housework. Only 1% of this sample had domestic service or external help. In this sample of married/cohabiting women, the average personal income of the treated cohort before treatment was only 98.33 USD PPP 2009. This amount is much lower than the average personal income of a treated divorced woman before treatment (not shown in Table 3), this was 361 USD PPP 2009, which in turn almost matches the long-run pension provided by the moratorium to a woman who had never contributed to the SS.

4 Empirical strategy: Difference-in-Difference Estimation

Although the Argentinean pension reform law was passed in 2004 and 2005, its full implementation started only in 2007, as noted in Section 2. Since our data set covers the period 2004-2009 we can compare outcomes of treated individuals before and after the law came into effect as part of our identification strategy. Accordingly, we define 2004-2006 as the pre-treatment period and 2007-2009 as the post-treatment period.

To complete our identification strategy, we identify as our treatment group those cohorts affected by Law 25994, i.e. those born in 1944 or earlier. To avoid selection due to mortality and to keep the age difference between the treated and the control groups relatively small, we restrict the treated group to women born between 1941 and 1944.[20] Notice that all these women had already turned 60 by 2004 (their ages ranging between 60 and 65 in the pre-treatment period and between 63 and 68 in the post-treatment period), which allows us to isolate the effect of the reform from the effect of changes in individual labor market decisions that occur at retirement age. Note that since we neither observe the number of years individuals contributed to the SS system nor who actually claims benefits resulting from the moratorium, our DD estimates identify intention-to-treat (ITT) effects. Our control group is composed of women born between 1950 and 1953, who, by definition, were not affected by the reform (neither by Law 25994 nor by Decree 1454/05) during our sample period.

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[20]We could have included older cohorts, i.e., born before 1941, in the analysis as well but we were concerned that compositional effects due to mortality would become a problem and the age difference in relation to the control group would render it invalid. This issue is further discussed in Section 5.2.

Their ages range between 51 and 56 in the pre-treatment period and between 54 and 59 in the post-treatment period. Table 1 summarizes the cohort and age composition of the treatment and control groups. Using administrative records, Figure 4.4. from D'Elia et al. 2011 shows that by May 2010 there were roughly 506,000 women from our treated cohorts affected by the moratorium.

Figures 3 and 4 show that after the reform the percentage of women in the treatment group that receives a pension increased from approximately 30% to more than 75% (widows excluded) and that, as a result, the percentage of women without any personal income fell from roughly 40% to 12%. Importantly, Figures 3 and 4 also show that: 1) the increase in pension coverage and personal income of women in the treated cohorts were only effective in 2007; 2) women from the control group were unaffected by the reform.

Table 2 shows the pre- and post-treatment means of a set of relevant variables for the treated and control groups. Differences between the treated and control groups are mostly due to their age difference. For example, because the younger cohorts are typically better educated, there are noticeable differences in education. Placebo runs and robustness checks based on a sample with a different cohort composition in Section 6 prove that this age difference by itself cannot explain the estimated effects of the reform that we obtain in Section 5.

The DD strategy relies on the assumption that the evolution of the outcome of the treatment group in the absence of the reform would have been the same as that of the control group. We check the plausibility of this assumption by comparing the evolution of the unconditional outcomes of interest for the treated and control groups during the pre-treatment period (Figures 6, 8 and 9). The evolution of all our outcomes across treatment and control groups before the reform took place in 2007 is similar.

Our model specification is:

$$y_{it} = \alpha + \beta Treat_i \times Post_t + \delta_i^C + \delta_t + X_i'\gamma + \varepsilon_{i,t} \tag{1}$$

where y_{it} is the outcome of interest for individual i in time t, $Treat_i$ is equal to one when woman i was born in the period 1941-1944, $Post_t$ equals to one when the outcome is observed in the post-treatment period 2007-2009, δ_i^C and δ_t are cohort and period (year-quarter) fixed effects, respectively,

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X_i is a vector of individual characteristics and ε_{it} is the residual. The coefficient β represents the DD estimate of the effect of the reform.

To allow for correlation between the error terms of different cohorts (treated and/or control) in the same geographical area, we cluster the standard errors at the urban area level. Clustering at the urban area level also deals with potential correlation originating from multiple observations (up to four) from the same individual. Because there are only 29 clusters, we show both the cluster robust sandwich standard errors estimates as well as the more conservative p-values based on wild bootstraps-t techniques for a 6-points weight distribution (Cameron et al. 2008, Webb 2013).[21]

We estimate equation 1 using the divorce/separated outcomes on a sample of divorced/separated and married/cohabiting women, as well as using the distribution of household chores outcomes on a sample of women living only with their partners. We restrict the sample of married/cohabiting women to those living only with their partners so that our outcomes on the distribution of household chores can better proxy for bargaining power within the couple.

An important concern in our setting is related to potential anticipation effects amongst women in the control group as they perceive themselves as future beneficiaries of the moratorium. Such anticipation effects, however, would bias downwards our difference-in-difference estimates, reinforcing our results.[22,23]

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[21]When the number of clusters is not too small, it is common practice to cluster the standard errors at the same level as treatment i.e. cohort level in our case (Bertrand et al. 2004, Donald and Lang 2007). Clustering at the cohort level, however, would lead to technical as well as specification problems. The technical problems are due to the very low number of treated cohorts. In a recent paper, MacKinnon and Webb 2015 show that when the number of treated cohorts is equal to or lower than 4—the number of treated cohorts in our case is exactly 4—, the wild bootstrap-t method (Cameron et al. 2008, Webb 2013), which is the most adequate to address the issue of low number of clusters, fails considerably. All our estimates include cohort dummies which should account for some of the correlation that may exist among observations of the same cohort.

[22]Anticipation effects of the treated, for example due to the announcement of the law, also lead to an underestimate of the effects of the reform. These anticipation effects are not very likely, however, given the scarce media coverage before mid-2006 (as shown in Section 2).

[23]An alternative specification would be to exploit the panel structure of the EPH, considering the panel would allow us to include individual fixed effects and even include additional cohorts in the study. However, the approach has important drawbacks because it would reduce the sample size considerably (from $27,157$ observations to just $7,336$) and more importantly, the treated women's exposure to the reform would be very short-lived, between 1 and 5 quarters. Moreover, there is a potential attrition problem related to treatment in the sense that divorcees may be harder to follow up, creating an automatically lower bias in the probability of divorce/separation estimates. We carried out this analysis and realized that the effects on income were considerably smaller in the shorter term. Perhaps due to the lower income effects, we did not find a short-run effect of the reform on outcomes related to the distribution of household chores. One possible explanation is that women may need to accumulate a minimum amount of income before they can bargain with or separate from their partners; another possibility is that there was not enough time since the implementation of the reform for household members to internalize the change in their relative bargaining power.

5 Results

5.1 Effects of the reform on women's income

In this subsection we show DD estimates of the effect of the reform on women's probability of receiving a pension, on the probability of having no personal income, and on their monthly personal income using the specification of equation 1. All these DD effects are statistically significantly different from zero.

In Panel A of Table 4, we report results for the sample of divorced/separated and married/cohabiting women. The implementation of the pension reform successfully increased the percentage of women receiving a pension by 53 pp (313%), which meant a 36-pp reduction in the probability of having no personal income (69%). In column 3 we can see that the reform increased women's monthly personal income by 61.5 USD PPP 2009. Note that since these are intention-to-treat effects, that is, not all women in the treatment group were effectively treated, the real income effect on the treated is much higher than values presented in this table.

In Panels B and C of Table 4 we break the effect on income by education. Groups with both a low and high level of education were affected by the reform, although the latter to a lesser extent. The probability of receiving a pension increased by 61 pp for women with a low level of education and by 38 pp for those with a high level.[24] The results also indicate that the probability of not having any personal income was reduced by 41 pp for those with a low level of education and by 25 pp for the highly educated. Despite the greater attachment to the formal labor market by the highly educated, which should grant them higher pensions conditional on claiming benefits under the reform, the results show that the average increase in the monthly personal income was greater for women with a low level of education (92 USD PPP 2009) than for highly educated women (48 USD PPP 2009). All these effects confirm that highly educated women were less likely to enjoy the benefits from the reform.

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[24]Research using other data for Argentina raises concerns about under-reporting or lack of reporting of information on income (Cruces and Wodon 2003). The income imputation rate in our data varies by education (6% vs 14% for high levels of education). However, it is the estimated effects for women with a lower level of education where imputation is lower which should be regarded as closer to the real treatment values, since this group is more likely to benefit directly from the reform because of the lower attachment to the formal labor market.

5.2 Effects of the reform on the probability of divorce/separation

DD estimates reported in Table 5 show that the implementation of the reform had a positive and statistically significant impact on the probability of divorce or separation, increasing it in 2.6 pp. These effects are large if we take into account that the share of women in the treatment group that were divorced/separated before the reform was only 10% and that the probability of divorce at these ages is low (see footnote 3).[25] However, this average effect is somewhat misleading as it affects women of different educational levels very differently. In columns 2 and 3 of Table 5 we see that the effect on the women with low levels of education is much smaller and not statistically different from zero while the effect on the highly educated is higher (5.6 pp) and statistically significantly different from zero.

We need to address potential compositional effects originating from a different evolution of the proportion of widows and singles in the treated and control groups as well as mortality. For example, women that were married/cohabiting in the pre-treatment but are widows in the post-treatment do not remain in the sample of divorced/separated and married/cohabiting women ("d+m") in the post-treatment period. Hence, a higher probability of widowhood amongst the treated group automatically generates a larger increase in the rate of divorce/separation (d/(d+m)) for the treatment group because the number of married/cohabiting women in the denominator decreases.

Similarly, a higher probability of marriage among single women in the control group, because they are younger than the treated, would lead to an increase in the post-treatment denominator for this group and an upward bias in our estimated impact on divorce/separations. We follow three approaches to check that these compositional effects cannot explain our results: 1) we verify that including widows and single women in the sample does not qualitatively alter our results; 2) using the sample including widows and singles, we estimate equation 1 but using as an outcome variable an indicator for being a widow; 3) with the same sample as in 2), we also estimate equation 1 but using as an outcome variable an indicator for being single. The results of the last two exercises show that the estimated DD parameter is zero, which indicates that the results presented in Table 5 are

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[25]We believe this effect is mainly driven by separations rather than divorces because in Argentina getting a divorce is costly and alimony is not guaranteed. During the period of analysis, there is no unilateral divorce and couples need to be separated for a minimum of 2 years before they can file for divorce. Moreover, if a woman filing for divorce has no income source, it is up to the judge to decide how much and for how long her ex-husband should grant her alimony.

not driven by differences in the evolution of widowhood or singlehood among treated and control groups (results not shown here for the sake of brevity, but available upon request). Mortality is an additional source of compositional effects. Mortality rates increase with age and, therefore, should be higher in the treatment group than in the control group. The restriction of treatment cohorts born after 1940 minimizes this possibility. In addition, mortality would only bias our results upwards if it was disproportionately higher amongst married/cohabiting women (versus divorced/separated women), which research shows is not the case (Manzoli et al. 2007).

5.3 Effects of the reform on the bargaining power of women

To analyse the effects of the reform on the bargaining power around the house we need to restrict the sample to married/cohabiting women. We further restrict the sample to women living only with their partners to better assess the distribution of bargaining power within the couple (results for the whole sample of married/cohabiting women are very similar and are shown in Table A.1 in the Appendix).[26] Because of our results in the previous Section, we analyse only the women with a low level of education where there is no evidence of selection through divorce/separation.

We first show the effects of the reform on the income of married/cohabiting women who live with their partners only (columns 1-4 of Table 6), they are all very statistically significantly different from zero and of the same order of magnitude than those in Panel B of Table 4.[27] Restricting to a sample of married people allow us to show the positive and large effect of the reform on the wife's contribution to the couple's income, which increased by 15 pp or 80%.

Columns 5-7 of Table 6 show the DD estimates of the effects of the reform on different outcomes related to the bargaining power of married/cohabiting women. The regressions include additional controls such as age and educational differences within couples and a dummy variable which takes the value 1 when the husband is above the legal retirement age for men. The differences in age and education in the couple are commonly used in the literature as *distribution factors* (Browning et al. 2011) and proxy for women's bargaining power previous to the reform. Their inclusion does not affect

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[26]We checked that in the sample of married/cohabiting women, the reform did not affect the probability of living only with their partners. Results not shown for the sake of brevity.

[27]In Section 6.3 we show that potential effects on labor market participation cannot account for the effects on bargaining power.

the estimated effects substantially but does slightly reinforce some of our results by making them larger and more precisely estimated. Controlling for the retirement age for men is important not to confound gains in bargaining power that are a result of the reform with gains that are the result of a husband having more time available. In column 5 we show that the reform significantly reduced by 7 pp the probability that the wife is the *only* person responsible for household chores. However, this result would not reflect changes in bargaining power if it were entirely driven by a pure income effect associated with the reform that allowed women to substitute their own time for paid domestic services. This is not the case as we can see from results shown in column 7. On the contrary, we find that husbands or male partners significantly increase their participation in household chores by 6.1 pp. Taken together, these results reflect that gender roles in the household become more alike with the reform.

6 Placebo and Robustness Checks

One concern with our methodology is that different age profiles in the treatment and control groups may blur the identification of the treatment effect. While effective in controlling for age differences within treatment and control groups, the cohort and period dummies included in all the estimations are ineffective in controlling for age differences across groups because age profiles do not overlap. Hence, to discard the possibility that the results obtained so far are driven by different outcome trends associated with the different age profiles across groups, we estimate a placebo treatment effect using data from pre-treatment years with the same age profiles as our main estimations in Section 5. The idea is to show that age differences across groups have zero impact on DD estimates. Placebo runs in Section 6.1. show that placebo treatment effects are not statistically different from zero, which suggests that different age profiles cannot explain our results.

The placebo methodology is not, however, without flaws. First, the lack of information about household chores on the pre-treatment data renders the placebo test incomplete. Secondly, the placebo treatment period may coincide with changes in other laws or overall labor market performance which may impact differently the placebo treated and control groups. To address these concerns, in Section 6.2. we run a different robustness check that uses the same data period as our main specification but employs an alternative methodology. This alternative methodology has the

additional advantage of identifying separately from the treatment effect an extra parameter reflecting the effects of aging and/or retirement. The empirical exercise shown in Section 5 was not able to effectively distinguish between treatment and aging/retirement effects since certain ages were observed only for a certain treatment status and period, e.g. all individuals above age 66 are observed only in the treated group in the post-reform period. In this circumstance, one may wonder if what the estimated effect of the reform is picking up includes the fatigue in marriage due to more intense cohabitation following retirement.

In Section 6.3 we discuss the potential effects on labor force participation.

6.1 Placebo Runs

Using data from pre-treatment years, we estimate a placebo treatment effect. Specifically, as in our original framework, we include 6 years in our analysis (1996-2001), and define 1996-1998 as the placebo pre-treatment period, and 1999-2001 as the placebo post-treatment period.[28] In the placebo treatment group, we include women born between 1933 and 1936, while in the placebo control group we have women born between 1942 and 1945. Therefore, the individuals in these placebo treatment and control groups are of the same age as individuals included in our original groups (see Tables 1 and 7). If an age effect drives our results rather than an income effect, the results of this placebo DD should be similar to those presented in Table 5.

Table 8 presents the results of the placebo DD. First, in columns 1 to 3 we check that the evolution of incomes are similar for the placebo treatment and control groups in the pre- and post-treatment periods. Indeed, all coefficients are close to zero in magnitude and not statistically different from zero. The estimated effects on the probability of being divorced/separated (column 4) have the opposite sign to those in Table 5 and are not statistically significant.

The placebo exercise uses data from the EPH Puntual, a survey that was replaced by the EPH Continua at the end of 2003. Unfortunately, the EPH Puntual does not include questions about sharing housework. Hence, with the placebo runs, we can only confirm that age differences across groups do not drive the results on divorce but we cannot draw any conclusion about the bargaining

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[28]We deliberately avoid including the year 2002 in our placebo sample period because it is the year that follows the severe financial crisis that hit Argentina in December 2001.

power outcomes.

6.2 Alternative Robustness Check

In this Section, we re-estimate the effects of the reform on a different sample with a different cohort composition from that used in Section 5 for the same period as our main specification i.e. 2004-2009. This new sample avoids contamination of the reform effect with other effects coming from aging and retirement, such as the stress arising from the more frequent day-to-day contact of the couple. Looking back at Table 1, one notices that certain ages, both in the control and treatment groups, are only observed in either the pre- or post-reform periods. This means that the cohort and period dummies do not control for age adequately and hence, age effects, if any, may blur the interpretation of the reform effect. To curb this concern, we introduce new cohorts to the pre-reform period and drop post-reform observations of other cohorts so as to achieve a balanced composition of ages before and after the reform.

Table 9 illustrates the difference between the benchmark sample (Section 5) and the sample used in this exercise. The cohort-year observations used in this exercise are those in the shadow area while the cohort-year observations used in Section 5 are those enclosed in a frame, some of which are incorporated in this exercise and some are dropped. We aim to keep the maximum possible number of cohort-year observations used in the main estimations of Section 5 that allows us to find a group of women in the pre-reform period that matches exactly the post-reform age profiles of our treated and control groups. To achieve this purpose, some cohort-year observations had to be deleted and some were added. More specifically, cohort 1941 and 1953 are excluded from the treated and control groups, respectively, while observations from cohorts 1939-1941 and 1947-1949 are added to the pre-reform period.

We can estimate the effect of the reform using the following specification:

$$y_{it} = \alpha + \beta Treat_i \times Post_t + \lambda \mathbf{Treat_i} \times \mathbf{Aging_i} + \delta_i^C + \delta_t + X_i'\gamma + \varepsilon_{i,t} \qquad (2)$$

where $Treat_i$ is a dummy variable that takes value 1 for all cohorts born in 1944 or earlier, implicitly the control group is composed of cohorts 1947-1950. $Post_t$ is defined as a dummy that takes value

1 for observations from periods 2007-2009. δ_i^C and δ_t are cohort and period (year-quarter) fixed effects, respectively, X_i is a vector of individual characteristics and ε_{it} is the residual. We wish to account for an aging effect, a common effect to all those aged more than 60. We would not be able to identify this effect with a dummy for over 60, for example, since that dummy would be identical to $Treat_i$. Instead, we drive a wedge between the two definitions by creating an artificial variable $Aging_i$ that for all women takes value 1 except in the first year their cohort is present in which case it takes the value zero. The interaction of $Treat_i \times Aging_i$ would identify the effect of growing older for the treated group. For example, for all cohorts 1939-1941 $Treat_i \times Aging_i$ is equal to zero in 2003 but equal to one after that, while for all cohorts 1942-1944 it takes value zero in 2006 and value one after that. This wedge is enough to avoid multicollinearity while still being able to identify the common effect of aging.

In Table 10 we show the estimated β coefficients using specification 2. These estimates are alternative to the main specification presented in Section 5 but here we try to purge potential aging and retirement effects. The estimated effects of the reform on income are slightly stronger as expected, and the effects on divorce are strengthened, i.e., the estimated impact on the probability of divorce is now 4.8 pp (versus 2.6pp) and this value is still driven only by the effect on the highly educated. On Table 11 we show the effects on our measures of bargaining power on the sample of women with a low level of education who are living only with their partner. The effects on income are stronger but the results on the sharing housework are identical to those presented in Table 6.

6.3 Effects on Labor Market Participation

Applying the same specification as in 1, we find a statistically significant effect of -6.4 pp (or 26%) of the reform on female labor force participation. Our results on female labor force participation are in line with those of Bosch and Guajardo 2012. Male labor force participation also decreases as a consequence of the reform (-8.9 pp or 17.2%). Despite these effects, average female income share within the couple increases significantly by 15 pp (column 4 of Table 6).

One may worry that the effects on the distribution of household chores could, at least in part, be explained by a reduction in labor market participation of husbands/partners and the consequent increase in the time available to dedicate to household activities. However, if we look at results in

Panel B of Table 12 we see that once we control for potential aging and retirement as in Section 6.2. the effect of the reform on the labor force participation vanishes.

7   Conclusion

What happens inside the household is often not observable to researchers. However, there seems to be a consensus that processes that occur inside the household may have major consequences for economic and social outcomes of individuals and society as a whole. The economic literature has provided evidence that cash transfer programs can, presumably by altering such processes, have large effects on economic outcomes (e.g., labor force participation, child development). In this paper we focus on senior women, all over retirement age, and ask whether cash transfers can affect their marital status and bargaining power within the household. We find surprising effects in this population; both union dissolution and outcomes related to bargaining power are affected by public transfers. Our results on the effects of income shocks on divorce/separation call attention to potential sample selection that may exist in the intra-household bargaining power literature.

Our empirical application comes from a differences-in-differences estimation of the effects of the 2004/2005 Argentinean pension reform. We use data from the Argentinean Continuous Permanent Household Survey (*Encuesta Permanente de Hogares Continua*) for the years 2004-2009. The Argentinean pension reform is an interesting application because it affected the permanent income of almost 2 million women in Argentina; . The large and permanent income shock had an impact of 5.6 pp on the probability of divorce/separation amongst the highest educated seniors (high school diploma or more). Amongst the lowest educated seniors while we find no effects on the probability of divorce/separation, we find a decrease of 7 pp (or 11.7%) in the probability that the wives are the only ones in charge of household chores and a statistically significant increase of 6.1 pp (or 16.5%) in the probability that husbands help with household chores.

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Tables

Table 1: Definition of treated and control groups

	Pre treatment (2004-2006)	Post treatment (2007-2009)
Treated cohorts born 1941-1944	ages 60-65	ages 63-68
Control cohorts born 1950-1953	ages 51-56	ages 54-59

		Pre-treatment			Post-treatment		
		2004	2005	2006	2007	2008	2009
	Cohort			**Age**			
Treated	1941	63	64	65	66	67	68
	1942	62	63	64	65	66	67
	1943	61	62	63	64	65	66
	1944	60	61	62	63	64	65
Control	1950	54	55	56	57	58	59
	1951	53	54	55	56	57	58
	1952	52	53	54	55	56	57
	1953	51	52	53	54	55	56

Table 2: Sample Means: Divorced/separated and married/cohabiting women

	Control cohorts		Treated cohorts		All sample	
	2004-06	2007-09	2004-06	2007-09	2004-06	2007-09
Panel A: All divorced/separated or married/cohabiting women						
Pension recipient	0.04	0.06	0.17	0.72	0.09	0.31
No personal income	0.41	0.39	0.52	0.16	0.45	0.31
Personal income (Argentine Pesos: AR$)	480.59	958.43	332.85	783.39	424.07	893.45
Personal income (2009 PPP USD)	321.27	580.07	222.39	474.11	283.44	540.74
Education (high=1, low=0)	0.46	0.47	0.34	0.33	0.41	0.42
Born abroad	0.07	0.09	0.1	0.11	0.08	0.09
Divorced or separated	0.19	0.18	0.14	0.16	0.17	0.18
Legally married	0.70	0.71	0.76	0.73	0.72	0.72
In union (not legally married)	0.11	0.11	0.1	0.11	0.11	0.11
Married or in union, living only with spouse	0.13	0.2	0.36	0.43	0.22	0.29
Active (in the labor market)	0.58	0.55	0.34	0.22	0.49	0.43
Observations	11051	10433	6460	6092	17511	16525
Panel B: Women with a lower level of education						
Pension recipient	0.03	0.06	0.11	0.74	0.07	0.35
No personal income	0.47	0.45	0.59	0.17	0.52	0.33
Personal income (Argentine Pesos: AR$)	224.21	461.44	165.84	546.38	199.07	497.65
Personal income (2009 PPP USD)	149.97	279.77	110.83	330.47	133.11	301.39
Born abroad	0.08	0.12	0.12	0.12	0.09	0.12
Divorced or separated	0.18	0.17	0.13	0.13	0.16	0.15
Legally married	0.68	0.70	0.76	0.75	0.71	0.72
In union (not legally married)	0.14	0.14	0.11	0.12	0.13	0.13
Married or in union, living only with spouse	0.14	0.21	0.34	0.4	0.23	0.29
Active (in the labor market)	0.52	0.48	0.3	0.18	0.43	0.35
Observations	6189	5609	4382	4071	10571	9680
Panel C: Women with a higher level of education						
Pension recipient	0.05	0.07	0.27	0.67	0.12	0.25
No personal income	0.33	0.32	0.39	0.12	0.35	0.26
Personal income (Argentine Pesos: AR$)	785.54	1523.24	662.36	1263.8	746.95	1446.93
Personal income (2009 PPP USD)	525.01	921.34	442.51	765.28	499.16	875.44
Born abroad	0.06	0.06	0.07	0.09	0.07	0.07
Divorced or separated	0.21	0.21	0.16	0.21	0.2	0.21
Legally married	0.71	0.72	0.76	0.70	0.73	0.71
In union (not legally married)	0.08	0.08	0.07	0.09	0.08	0.08
Married or in union, living only with spouse	0.12	0.2	0.4	0.48	0.21	0.28
Active (in the labor market)	0.66	0.63	0.42	0.3	0.58	0.53
Observations	4862	4824	2078	2021	6940	6845

Table 3: Sample Means: Married/cohabiting women with a lower level of education

	Control cohorts		Treated cohorts		All sample	
	2004-06	2007-09	2004-06	2007-09	2004-06	2007-09
Panel A: All married/cohabiting women with a lower level of education						
Pension recipient	0.03	0.05	0.11	0.74	0.06	0.35
No personal income	0.54	0.52	0.65	0.19	0.59	0.37
Personal income (Argentine Pesos: AR$)	190.53	406.01	141.91	510.42	168.95	451.53
Personal income (2009 PPP USD)	127.33	245.94	94.73	308.98	112.86	273.42
Wife's share of income within couple	0.23	0.23	0.19	0.33	0.21	0.27
Born abroad	0.08	0.11	0.12	0.11	0.1	0.11
Legally married	0.83	0.84	0.87	0.86	0.85	0.85
In union (not legally married)	0.17	0.16	0.13	0.14	0.15	0.15
Married or in union, living only with spouse	0.17	0.25	0.4	0.46	0.27	0.34
Couple's age difference	2.54	2.23	2.37	2.43	2.47	2.32
Couple's education difference	-0.31	-0.33	-0.44	-0.43	-0.36	-0.37
Wife is uniquely responsible for housework	0.4	0.44	0.46	0.43	0.43	0.43
Husband does housework	0.22	0.26	0.28	0.35	0.25	0.3
Have domestic service or external help	0.01	0.01	0.01	0.02	0.01	0.01
Active (in the labor market)	0.46	0.43	0.26	0.15	0.37	0.31
Observations	5160	4613	3808	3527	8968	8140
Panel B: Women with a lower level of education living only with husband/partner						
Pension recipient	0.02	0.05	0.14	0.8	0.1	0.49
No personal income	0.54	0.49	0.64	0.14	0.6	0.28
Personal income (Argentine Pesos: AR$)	207.72	444.84	147.45	546.80	168.7	505.09
Personal income (2009 PPP USD)	138.12	269.6	98.33	330.93	112.36	305.84
Wife's share of income within couple	0.25	0.25	0.19	0.34	0.21	0.31
Born abroad	0.08	0.13	0.14	0.12	0.12	0.12
Legally married	0.73	0.81	0.83	0.85	0.79	0.83
In union (not legally married)	0.27	0.19	0.17	0.15	0.21	0.17
Couple's age difference	3.55	2.89	2.81	3.26	3.07	3.11
Couple's education difference	-0.35	-0.34	-0.44	-0.47	-0.41	-0.42
Wife is uniquely responsible for housework	0.6	0.58	0.6	0.5	0.6	0.54
Husband does housework	0.38	0.39	0.37	0.44	0.37	0.42
Have domestic service or external help	0	0.01	0.01	0.01	0.01	0.01
Active (in the labor market)	0.49	0.47	0.25	0.15	0.33	0.28
Observations	843	1081	1469	1564	2312	2645

Table 4: Effects of the reform on income.

Panel A: All divorced/separated or married/cohabiting women			
	(1) Receive a pension	(2) Without personal income	(3) Personal income (U\$S PPP)
Post*Treated	0.533	-0.358	61.453
SE	(0.038)***	(0.031)***	(13.006)***
p-value from wild bootstrap SE	[0.000]***	[0.002]***	[0.02]**
Observations	34,036	34,036	34,036
Obs. in treatment group	12552	12552	12552
Mean dependent variable	0.166	0.523	222.4

Panel B: Women with a lower level of education			
	(1) Receive a pension	(2) Without personal income	(3) Personal income (U\$S PPP)
Post*Treated	0.610	-0.412	91.533
SE	(0.020)***	(0.028)***	(16.938)***
p-value from wild bootstrap SE	[0.000]***	[0.002]***	[0.002]***
Observations	20,251	20,251	20,251
Obs. in treatment group	8453	8453	8453
Mean dependent variable	0.112	0.593	110.8

Panel C: Women with a higher level of education			
	(1) Receive a pension	(2) Without personal income	(3) Personal income (U\$S PPP)
Post*Treated	0.375	-0.254	47.610
SE	(0.031)***	(0.012)***	(21.850)**
p-value from wild bootstrap SE	[0.000]***	[0.002]***	[0.088]*
Observations	13,785	13,785	13,785
Obs. in treatment group	*4099	4099	4099
Mean dependent variable	0.274	0.386	442.5

Note: The dependent variables are: a dummy that equals one when the woman receives a pension (columns 1); a dummy that equals one when she has no personal income (columns 2); and the woman's monthly personal income in international 2009 PPP dollars (column 3). The coefficient on Post*Treated is the estimated parameter β of equation 1 which is the DD estimates (OLS) of the effect of the reform on each of the outcomes. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1941 and 1944, and the control group women born between 1950 and 1953. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the maximum level of education attained is at least a high school diploma, and a dummy variable that equals one if the woman was born abroad. Regression in column (3) also includes a dummy that indicates whether the woman belongs to the top 1% of the distribution of personal income, to mitigate the impact of extreme outliers. The sample includes all married/cohabiting and divorced/separated women (i.e. it excludes singles and widows). Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Effects of the reform on the probability of being divorced/separated

	Dep. Variable: Women is divorced/separated		
	(1) All divorced and married women	(2) Women with a lower level of education	(3) Women with a higher level of education
Post*Treated	0.026	0.011	0.056
SE	(0.008)***	(0.012)	(0.022)**
p-value from wild bootstrap SE	[0.088]*	[0.616]	[0.098]*
Observations	34,036	20,251	13,785
Obs. in treatment group	12552	8453	4099
Mean dependent variable	0.142	0.132	0.164

Note: The dependent variable is a dummy that equals one if the woman is divorced or separated. The coefficient on Post*Treated is the estimated parameter β of equation 1 which is the DD estimates (OLS) of the effect of the reform on each of the outcomes. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1941 and 1944, and the control group women born between 1950 and 1953. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the maximum level of education attained is at least a high school diploma, and a dummy variable that equals one if the woman was born abroad. The sample includes all married/cohabiting and divorced/separated women (i.e. it excludes singles and widows). Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Effects of the reform on income and on the bargaining power of married/cohabiting women

Sample of women with lower level of education living only with husband/partner

	(1) Receive a pension	(2) Without personal income	(3) Wife's income (U$S PPP)	(4) Wife's share of couple's income	(5) Wife is uniquely responsible for housework	(6) Husband does housework	(7) Domestic service or external help
Post*Treated	0.611	-0.457	118.019	0.150	-0.070	0.061	-0.002
SE	(0.020)***	(0.024)***	(23.747)***	(0.023)***	(0.022)***	(0.015)***	(0.007)
p-value from wild bootstrap SE	[0.000]***	[0.002]***	[0.026]**	[0.006]***	[0.06]*	[0.03]**	[0.844]
Observations	4,957	4,957	4,957	4,866	4,957	4,957	4,957
Obs. in treatment group	3033	3033	3033	2984	3033	3033	3033
Mean dependent variable	0.144	0.638	98.33	0.187	0.597	0.369	0.00823

Note: The dependent variables are: a dummy that equals one when the woman receives a pension (columns 1); a dummy that equals one when she has no personal income (columns 2); the woman's monthly personal income in international 2009 PPP dollars (column 3), the share of wife's income within the couple (columns 4), a dummy that equals one if the wife is the only household member responsible for housework (column 5), a dummy variable that equals one if the husband does housework (column 6), and a dummy variable that equals one if the household has domestic service or external help for housework (column 7). The coefficient on Post*Treated is the estimated parameter β of equation 1 which is the DD estimates (OLS) of the effect of the reform on each of the outcomes. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1941 and 1944, and the control group women born between 1950 and 1953. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the woman was born abroad, an indicator of husband being above retirement age, and differences between spouses' age and level of education attained. Regression in column (3) also includes a dummy that indicates whether the woman belongs to the top 1% of the distribution of personal income, to mitigate the impact of extreme outliers. The sample includes all low-educated married/cohabiting women living only with their partners. Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Placebo DD: Definition of treated and control groups

	Placebo pre treatment (1996-1998)	Placebo post treatment (1999-2001)
Placebo treated cohorts born 1933-1936	ages 60-65	ages 63-68
Placebo control cohorts born 1942-1945	ages 51-56	ages 54-59

		Placebo pre-treatment			**Placebo post-treatment**		
		(t_0)			(t_1)		
		1996	1997	1998	1999	2000	2001
	Cohort			**Age**			
Treated	1933	63	64	65	66	67	68
	1934	62	63	64	65	66	67
	1935	61	62	63	64	65	66
	1936	60	61	62	63	64	65
Control	1942	54	55	56	57	58	59
	1943	53	54	55	56	57	58
	1944	52	53	54	55	56	57
	1945	51	52	53	54	55	56

Table 8: Placebo using pre-treatment data (1996-2001)

All divorced divorced/separated or married/cohabiting women

	(1) Probability of receiving a pension	(2) Probability of not having any personal income	(3) Women's personal income (Dollars PPP)	(4) Probability of being divorced/separated
Panel A: All divorced/separated or married/cohabiting women				
Post*Treated	0.0195	0.00351	-7.394	-0.0127
SE	(0.0115)	(0.0207)	(11.31)	(0.0310)
p-value from wild bootstrap SE	[0.312]	[0.884]	[0.764]	[0.77]
Observations	24,345	24,336	24,345	24,336
Panel B: Women with a lower level of education				
	(1)	(2)	(3)	(4)
Post*Treated	0.0126	0.0135	-7.814	-0.0332
SE	(0.0140)	(0.0228)	(11.89)	(0.0172)*
p-value from wild bootstrap SE	[0.586]	[0.6]	[0.702]	[0.386]
Observations	14,736	14,729	14,729	14,736
Panel C: Women with a higher level of education				
	(1)	(2)	(3)	(4)
Post*Treated	0.0414	-0.0116	-4.602	0.0129
SE	(0.0156)**	(0.0605)	(43.13)	(0.0459)
p-value from wild bootstrap SE	[0.112]	[0.864]	[0.924]	[0.786]
Observations	9,609	9,607	9,607	9,609

Note: The dependent variables are: a dummy that equals one when the women receives a pension (column 1), a dummy that equals one when she has no personal income (column 2), the woman's monthly personal income in international 2009 PPP dollars (column 3), and a dummy that equals one when the woman is divorced or separated (column 4). Concretely, coefficient on Post*Treated is the estimated parameter β of equation (1) for the placebo DD estimates (OLS) specified in Section 4 (see Table 7). Period 1996-1998 is the placebo pre-treatment period and period 1999-2001 post-treatment period. The placebo treatment group includes all women born between 1933 and 1936, and the placebo control group women born between 1942 and 1945. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, regions fixed effects, a dummy variable that equals one if the maximum level of education attained is at least a high school diploma, and a dummy variable that equals one if the woman was born abroad. The sample includes all married/cohabiting and divorced/separated women (i.e. it excludes singles and widows). Data source: Argentine Permanent Household Survey (Encuesta Permanente de Hogares Puntual, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Age of cohorts by years used in exercise of Section 6.2.

Birth cohort	2003	2004	2005	2006	2007	2008	2009
1939 *	64	65	66	67			
1940 *	63	64	65	66			
1941	62	63	64	65	66	67	68
1942		62	63	64	65	66	67
1943		61	62	63	64	65	66
1944		60	61	62	63	64	65
1947 *	56	57	58	59			
1948 *	55	56	57	58			
1949 *	54	55	56	57			
1950		54	55	56	57	58	59
1951		53	54	55	56	57	58
1952		52	53	54	55	56	57
1953		51	52	53	54	55	56

Note: Cells contain the age of each cohort by year. Stars indicate the new cohorts added in the analysis of Section 6.2. All cohort-year observations included in this analysis are in the shadow areas. Cohort-year observations included in the main analysis of Section 4 are enclosed in boxes (observations that cannot be included in the analysis in of Section 6.2. are in gray print).

Table 10: Effects of the reform on income and divorce/separation from alternative specification used in Section 6.2.

Sample of divorced/separated or married/cohabiting women

	Panel A: All divorced/separated or married/cohabiting women			
	(1) Receive a pension	(2) Without personal income	(3) Women's income (U$S PPP)	(4) Divorced or Separated
Post*Treated	0.461	-0.335	114.089	0.048
SE	(0.081)***	(0.039)***	(14.796)***	(0.021)**
p-value wild bootstrap	[0.000]***	[0.002]***	[0.002]***	[0.084]*
Observations	30,545	30,545	30,545	30,545
Obs. in treatment group	11881	11881	11881	11881
Mean dependent variable	0.209	0.509	239.3	0.138
	Panel B: Women with a lower level of education			
	(1) Receive a pension	(2) Without personal income	(3) Women's income (U$S PPP)	(4) Divorced or Separated
Post*Treated	0.548	-0.392	121.915	-0.005
SE	(0.066)***	(0.027)***	(28.248)***	(0.023)
p-value wild bootstrap	[0.000]***	[0.002]***	[0.004]***	[0.85]
Observations	18,819	18,819	18,819	18,819
Obs. in treatment group	8145	8145	8145	8145
Mean dependent variable	0.143	0.586	111.6	0.115
	Panel C: Women with a higher level of education			
	(1) Receive a pension	(2) Without personal income	(3) Women's income (U$S PPP)	(4) Divorced or Separated
Post*Treated	0.269	-0.224	145.652	0.141
SE	(0.075)***	(0.061)***	(28.640)***	(0.055)**
p-value wild bootstrap	[0.004]***	[0.032]**	[0.006]***	[0.072]*
Observations	11,726	11,726	11,726	11,726
Obs. in treatment group	3736	3736	3736	3736
Mean dependent variable	0.347	0.350	503.6	0.183

Note: The dependent variables are: a dummy that equals one when the woman receives a pension (columns 1); a dummy that equals one when she has no personal income (columns 2); the woman's monthly personal income in international 2009 PPP dollars (column 3), and a dummy that equals one if the woman is divorced or separated (column 4). The coefficient on Post*Treated is the estimated parameter β of equation 2. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1939 and 1944, and the control group women born between 1947 and 1952. Table 9 describes the cohort-period composition of the sample. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the maximum level of education attained is at least a high school diploma, and a dummy variable that equals one if the woman was born abroad and the interaction term Post*old. Regression in column (3) also includes a dummy that indicates whether the woman belongs to the top 1% of the distribution of personal income, to mitigate the impact of extreme outliers. The sample includes all married/cohabiting and divorced/separated women (i.e. it excludes singles and widows). Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Effects of the reform on income and on the bargaining power of married/cohabiting women from alternative specification used in Section 6.2.

Sample women with lower level of education living only with husband/partner

	(1) Receive a pension	(2) Without personal income	(3) Wife's income (U\$S PPP)	(4) Wife's share of couple's income	(5) Wife is uniquely responsible for housework	(6) Husband does housework	(7) Domestic service or external help
Post*Treated	0.613	-0.672	188.669	0.271	-0.070	0.072	0.010
SE	(0.089)***	(0.064)***	(43.507)***	(0.076)***	(0.031)**	(0.038)*	(0.012)
p-value wild bootstrap	[0.000]***	[0.002]***	[0.014]**	[0.016]**	[0.038]**	[0.036]**	[0.508]
Observations	5,129	5,129	5,129	5,028	5,129	5,129	5,129
Obs. in treatment group	3070	3070	3070	3016	3070	3070	3070
Mean dependent variable	0.186	0.610	103.2	0.189	0.555	0.396	0.0145

Note: The dependent variables are: a dummy that equals one when the woman receives a pension (columns 1); a dummy that equals one when she has no personal income (columns 2); the woman's monthly personal income in international 2009 PPP dollars (column 3), the share of wife's income within the couple (columns 4), a dummy that equals one if the wife is the only household member responsible for housework (column 5), a dummy variable that equals one if the husband does housework (column 6), and a dummy variable that equals one if the household has domestic service or external help for housework (column 7). The coefficient on Post*Treated is the estimated parameter β of equation 2. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1939 and 1944, and the control group women born between 1947 and 1952. Table 9 describes the cohort-period composition of the sample. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the woman was born abroad, an indicator of husband being above retirement age, and differences between spouses' age and level of education attained. Regression in column (3) also includes a dummy that indicates whether the woman belongs to the top 1% of the distribution of personal income, to mitigate the impact of extreme outliers. The sample includes all low-educated married/cohabiting women living only with their partners. Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

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Table 12: The effect of the reform on the labor status
Sample of women with lower level of education living only with husband/partner

	Panel A: Main specification (equation 1)	
	(1) Wife is in the labor market	(2) Husband is in the labor market
Post*Treated	-0.064	-0.089
SE	(0.028)**	(0.017)***
p-value wild bootstrap	[0.014]**	[0.014]**
Observations	4,956	4,956
Obs. in treatment group	3033	3033
Mean dependent variable	0.248	0.516

	Panel B: Alternative specification of Section 6.2. (equation 2)	
	(1) Wife is in the labor market	(2) Husband is in the labor market
Post*Treated	0.024	-0.099
SE	(0.098)	(0.088)
p-value wild bootstrap	[0.866]	[0.668]
Observations	5,126	5,125
Obs. in treatment group	3069	3069
Mean dependent variable	0.246	0.474

Note: The dependent variable is a dummy that equals one if the wife is economically "active" (column 1) and a dummy that equals one if the husband is economically "active". In the first panel, the coefficient on Post*Treated is the estimated parameter β of equation 1which is the DD estimates (OLS) of the effect of the reform on each of the outcomes. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1941 and 1944, and the control group women born between 1950 and 1953. In the second panel, the coefficient on Post*Treated is the estimated parameter β of equation 2. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1939 and 1944, and the control group women born between 1947 and 1952. Table 9 describes the cohort-period composition of the sample. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the woman was born abroad, an indicator of husband being above retirement age, and differences between spouses' age and level of education attained. Regression in column (3) also includes a dummy that indicates whether the woman belongs to the top 1% of the distribution of personal income, to mitigate the impact of extreme outliers. The sample includes all low-educated married/cohabiting women living only with their partners. Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH.SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.

Figures

Figure 1: Percentage of individuals with personal income = 0
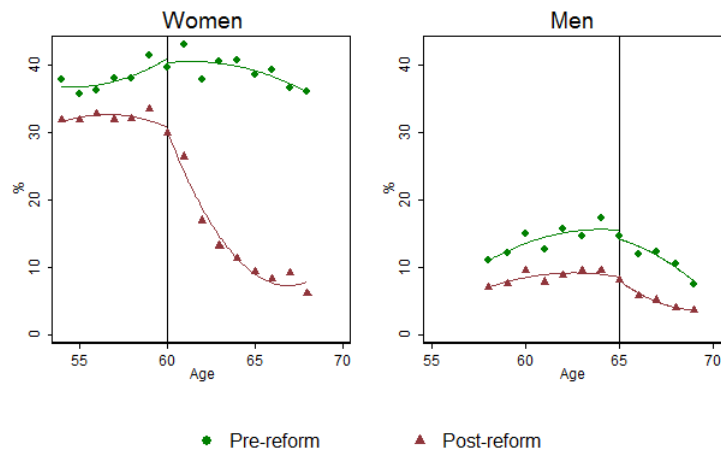
Source: Argentine Continuous Permanent Household Survey (EPH)

Figure 2: Pension recipients (as % of age-eligible individuals)

Source: Argentine Continuous Permanent Household Survey (EPH)

Divorced/separated or married/cohabiting women

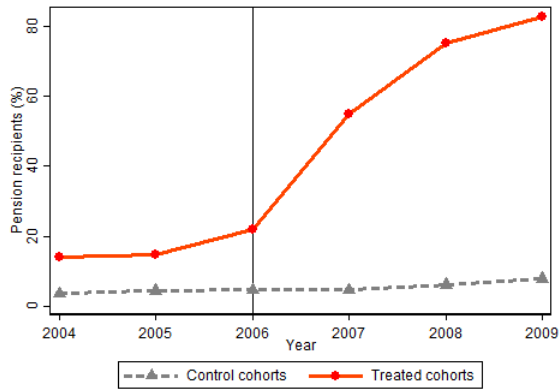<div style="display:flex; justify-content:space-between;">

Figure 3:
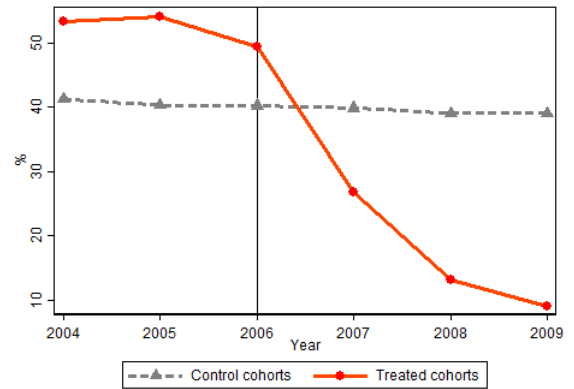Pension recipients

Figure 4:
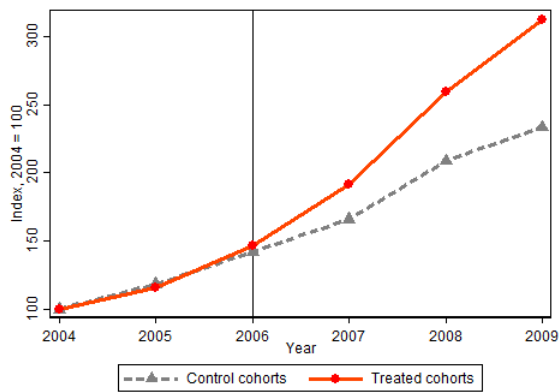Women without personal income

Figure 5:
Evolution of monthly personal income

Figure 6:
Woman is divorced/separated

</div>
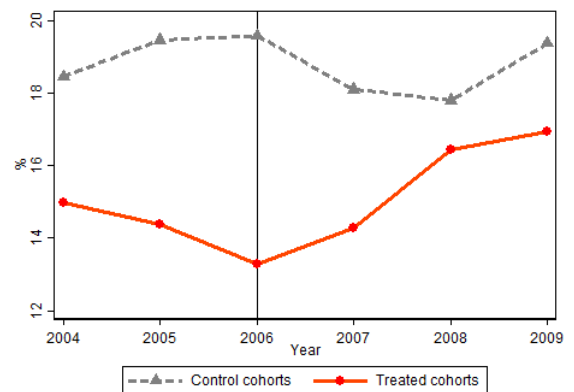
Women with a lower level of education living only with husband/partner

Figure 7: Wife's share of income within the couple
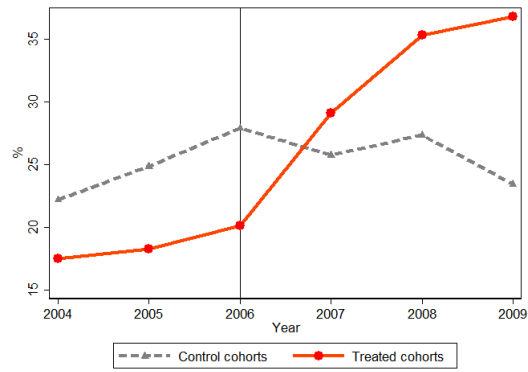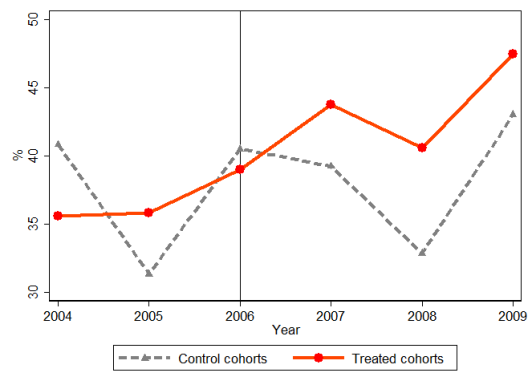

Figure 8: Woman is uniquely responsible for housework
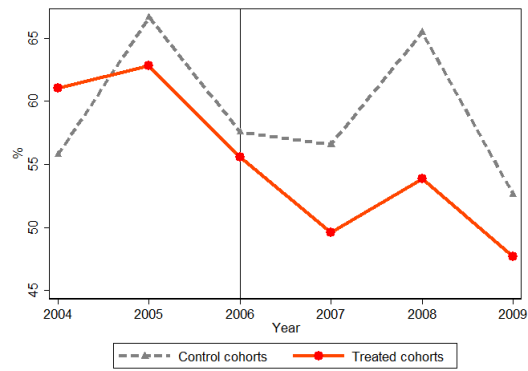
Figure 9: Husband does housework
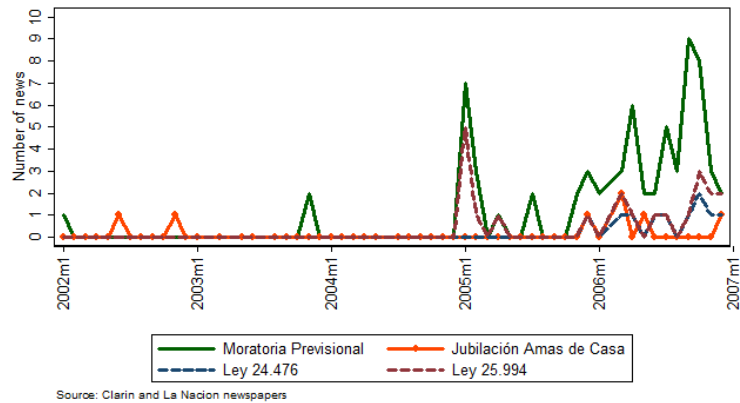
Appendix

Figure A.1: News related to the moratorium

Source: Clarin and La Nacion newspapers

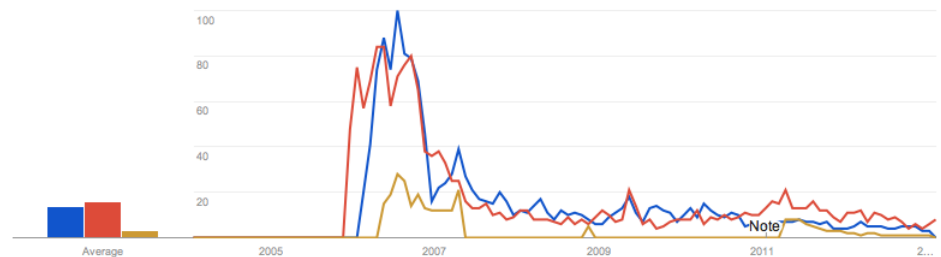Figure A.2: Google searches related to the moratorium

Table A.1: Effects of the reform on income and on the bargaining power of married/cohabiting women

Sample of married/cohabiting women with a lower level of education

	(1) Receive a pension	(2) Without personal income	(3) Wife's income (U$S PPP)	(4) Wife's share of couple's income	(5) Wife is uniquely responsible for housework	(6) Husband does housework	(7) Domestic service or external help
Post*Treated	0.609	-0.443	98.950	0.149	-0.057	0.025	0.008
SE	(0.023)***	(0.027)***	(17.888)***	(0.022)***	(0.015)***	(0.009)***	(0.004)**
p-value from wild bootstrap SE	[0.000]***	[0.002]***	[0.000]***	[0.002]***	[0.056]*	[0.072]*	[0.1]
Observations	16,480	16,480	16,480	16,015	16,480	16,476	16,480
Obs. in treatment group	7335	7335	7335	6868	7335	7144	7335
Mean dependent variable	0.107	0.646	94.73	0.187	0.460	0.285	0.00690

Note: The dependent variables are: a dummy that equals one when the woman receives a pension (columns 1); a dummy that equals one when she has no personal income (columns 2); the woman's monthly personal income in international 2009 PPP dollars (column 3), the share of wife's income within the couple (columns 4), a dummy that equals one if the wife is the only household member responsible for housework (column 5), a dummy variable that equals one if the husband does housework (column 6), and a dummy variable that equals one if the household has domestic service or external help for housework (column 7). The coefficient on Post*Treated is the estimated parameter β of equation 1 which is the DD estimates (OLS) of the effect of the reform on each of the outcomes. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The treatment group includes women born between 1941 and 1944, and the control group women born between 1950 and 1953. All regressions include the following control variables: cohort dummies, year-quarter fixed effects, region fixed effects (29 urban areas), a dummy variable that equals one if the woman was born abroad, an indicator of husband being above retirement age, and differences between spouses' age and level of education attained. Regression in column (3) also includes a dummy that indicates whether the woman belongs to the top 1% of the distribution of personal income, to mitigate the impact of extreme outliers. The sample includes all low-educated married/cohabiting women. Data source: Argentine Continuous Permanent Household Survey (Encuesta Permanente de Hogares Continua, EPH). All results come from regressions using as sample weights the variable pondera in the EPH. SE are clustered at the level of urban area (29 areas). Cluster robust sandwich standard errors are in parentheses, and in squared brackets we show two-tail p-values computed using wild bootstrap-t techniques as in Cameron et al. (2008) with a 6-point distribution as in Webb (2013) and 1000 bootstrap iterations. *** p<0.01, ** p<0.05, * p<0.1.