The Impact of a Permanent Income Shock on the Situation of Women in the Household: the case of a pension reform in Argentina[1]

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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 (because of divorce or separation), then the positive response of bargaining power to income found in the empirical research may be the artifact of sample selection. In this paper we prove that the marriage stability 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 at least 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.

Keywords: intra-household bargaining power, marital disruption, divorce, collective models, non-contributory pensions, public transfers, household chores, permanent income shock

JEL Classification: J12, J16, J26, H55

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 2014).

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

[1]The literature on decision making and allocation within the household typically takes household income as given while income shares may vary. This is not the approach of the more recent (and empirical) literature on (public) transfers —discussed in the main text further below—where both household income and income shares change. The pension reform in Argentina changed both income shares within the household as well as household total income conferring to our bargaining power results the interpretation of a combination of income and bargaining power effects. Some important references of the former literature 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, Attanasio and Lechene 2002, Ward-Batts 2008, Brown 2009 and Browning et al. 2014 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

shocks cause some couples to breakup, the positive estimated effects of income shocks on bargaining power revealed in the literature amongst those who remain married 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 significant 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. We motivate the split by education with the help of a model of bargaining power within the household that incorporates the possibility of divorce. The model shows that initial conditions which are likely correlated with education, such as pre-reform bargaining power, lead some women to divorce and others to gain bargaining power upon a positive income shock. 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, interesting on its own as an outcome of the bargaining process, concurs with the 'independence effect' hypothesis (Ross et al. 1975, also supported by Becker 1974), which predicts an increase in divorce/separation rates with the increase in married women's incomes.[4] Our empirical analysis on this topic is most related to Bobonis 2011, who estimates the effect of Mexico's PROGRESA conditional cash transfers (CCT) to women on union dissolution, although it differs in meaningful ways. First, we look at the effects of transfers on a sample of senior women.

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 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).

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 38,135-47,488 USD 2009, which represents roughly 5-6 times the amount transferred by the Mexican program PROGRESA, 4 times the amount of the early 1990s South African pension reform, and 8-11 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: 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

[5]For purposes of comparing the different transfers, all monetary values were converted to USD 2009 (WDI 2009, World Bank, see footnote 18 for more details on this conversion). The net present value figures were computed assuming a conservative 5.3% interest rate and two different values for life-expectancy at age 60, 15 and 20 years. The life-expectancy values of 15-20 years are close to the real female life-expectancy at age 60 in Mexico (22.4 years), South Africa (17.1 years), and Argentina (23 years) in 2005 according to the World Health Organization. The net present value of the South African pensions after the reform (described for example in Edmonds et al. 2005, Duflo, 2000) are 10, 434–12,464 USD 2009. Under the Mexican program "70 y más" described in Galiani et al. 2016, seniors start receiving transfers at the age of 70 years old, which makes up a total of 3,336-5,912 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.

[6]Recent papers by Majlesi 2016 and Ambler 2016 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 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

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., 2014). 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 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

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.

[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 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 2015, 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.

By focusing on senior women—permanent and exogenous income shocks are unlikely at younger ages—we contribute to the literature analyzing the impact of income shocks on marriage stability and household bargaining power in a population previously understudied. Our results also derive important policy implications for similar poverty-relief measures, which have either become common in developing countries such as non-contributive pensions, or are currently under debate in many developed countries such as the guaranteed minimum income, as long as these policies are not gender neutral. For example, since the year 2000, at least 13 other Latin American countries increased pension coverage of senior citizens (Berniell, 2012, Rofman et al., 2014). If these policies, just as the Argentinean pension reform, also increase divorce/separations, which is known to reduce household wealth (e.g. Knowles et al., 2007, Ananat and Michaels, 2008) and increase mortality, specially for men (e.g. Yuanreng and Goldman, 1990 and Lillard and Panis, 1996), then policy makers should worry about the overall effects of such policies.[11] On the positive side, by not being gender neutral, policies such as the Argentinean pension reform may have positive consequences not only on senior women but also on health and schooling outcomes of co-residing adults and children by increasing the bargaining power of women in the household (e.g. Duflo, 2000, de Carvalho Filho, 2012, Duflo, 2012, Majlesi, 2016).

[11]Moreover, although, to the best of our knowledge, the effects of divorce/separations on (co-residing) adult children and grandchildren have not been studied thoroughly (there are a few studies analysing the effects of late life divorce on interpersonal relationships and intergenerational financial support e.g. Aquilino, 1994), there is a large literature documenting the negative effects of parental divorce on children (e.g. Gruber, 2004, Page and Stevens, 2004, Ginther and Pollak, 2004, Antecol and Bedard, 2007, Tartari, 2015). One of the drivers identified in this literature is the negative income shock produced at the time of separation. This mechanism is likely to work as well upon the breakup of a senior couple despite the pension reform.

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 personal 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 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. In Appendix 2 we present a simple model of bargaining inside the household, which provides insights on how divorce would lead to empirical bias due to sample selection.

To avoid contamination from sample selection, we analyze 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 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 4.6 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 3.7 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. Appendix 1 contains extra figures and Tables and Appendix 2 contains a model of sample selection caused by divorce/separations.

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[12], 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

[12]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).

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.[13] 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 2014), 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 conservative 15-year life expectancy (see footnote 5) 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 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.[14] 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.

[13] All income shocks have potential effects on health. The provision of health insurance could further increase that effect. We interpret the availability of health insurance as an additional component of the income shock although we do not attach a dollar value to it.

[14] 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.

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 1 Figure A.2).

In summary, the implementation of law 25994 was delayed until 2007. Both the scarce media coverage of the law before the end of 2006 and the low take up of pensions before 2007 suggest that anticipation effects were not present. Were these effects present before 2007, our estimates would underestimate the true effect as we discuss at the end of Section 4.

3 Data

We use the Argentinean Continuous Permanent Household Survey (EPH) for the years 2004-2009.[15] 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

[15]We start our analysis in 2004 because in the middle of 2003 there was an important methodological change in the EPH. Additionally, we do not 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 due to administrative problems in the Statistics Office.

two alternating periods of two quarters 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,[16] 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.[17] We transform Argentinean currency (Pesos ARS) to U.S. dollars (USD) using the purchasing power parity (PPP) conversion factor for private consumption in 2009.[18]

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 analyzed for all married/cohabiting women or restricted to those who live only with their husband/partner. The latter allows to better account for changes in bargaining power within the couple. The information used to construct the housework

[16]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.

[17]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.

[18]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.

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 has domestic service or receives some external help.[19] 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[20] 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 di-

[19]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.

[20]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)".

vorced/separated and we use the EPH as a collection of repeated cross sections.[21] 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 1). Panels B and C in Table 1 show summary statistics by educational group.

As we explain in Section 5.3, to analyze the effects of the reform on bargaining power we restrict our sample to married/cohabiting low educated women living only with their partner. Panel B of Table 2 shows that for this group 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 2), 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

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

relatively small, we restrict the treated group to women born between 1941 and 1944. 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. Their ages range between 51 and 56 in the pre-treatment period and between 54 and 59 in the post-treatment period. Table 3 summarizes the cohort and age composition of the treatment and control groups.[22] 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 1 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.[23]

[22] Eissa and Hoynes, 2004 and Eissa and Hoynes, 2006 studies of the labor supply responses of married couples to the expansions of the earned income tax credit (EITC) are examples of relatively recent papers that have used different non-overlapping age groups in the context of diff-in-diff analysis.

[23] Moreover, as suggested by a Referee, we run the main specification 1 using as dependent variables also the education dummy and the born abroad dummy. Results show that none of the effects on these characteristics are statistically different from zero.

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:

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

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, 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.[24]

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

[24]We do not exploit the panel structure of the EPH because of two important limitations: 1) Small sample size. The panel subsample would be restricted to women who were sampled both before and after the reform i.e. women interviewed for the first time between 2005-IV and 2006-IV. 2) Short exposure to the policy. The observations in the panel subsample would span the period between 2005-IV and 2008-I, restricting to only 21% the percentage of treated women who would be exposed to the policy for 5 quarters, to 19% the percentage exposed for 4 quarters, to 36% the percentage exposed for 2 quarters and as much as 25% would be exposed to a single quarter. Figures 3 and 5—as well as estimates by year using the overall sample (not shown)—show that although most of the income effects are already present in 2007, they grow considerably over time. The divorce/separation and bargaining power results also take time to produce (estimates, not shown, are consistent with Figure 6), for example, for the overall sample the effect on divorce/separation is not even statistically significant in 2007. Hence, it is unlikely that the panel estimation would find any statistically significant effects since the last quarters, where most of the change in outcomes occurs, are missing from the panel subsample. The evidence that these household processes may take time is also given in Bobonis (2011) who finds that cash transfers do not affect union dissolution until the second year of the implementation of the program. The small sample size combined with a very short exposure to the reform and the evidence that effects take time to produce lead to finding zero or non-statistically different from zero effects when using the data as a panel.

weight distribution (Cameron et al. 2008, Webb 2013).[25]

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. We checked that the reform did not affect the probability of living only with partner and, hence, we are confident that there are no compositional effects for this subsample of women. As a robustness check, we rerun all regressions by including linear and quadratic trends at the urban geographical level (results now shown for sake of brevity). All results for income, divorce/separations and bargaining power are robust to the inclusion of these trends.

We also break all our results by educational group (high and low educated) similarly to Bobonis 2011. This break is particularly interesting in the case of the Argentinean reform, because educational attainment may also proxy for the different income shocks across individuals. Note that income shocks brought about by the moratorium were heterogeneous across the population. They depended on the number of years (if any) each individual had contributed to the SS system in the past and, naturally, on the value of those individual contributions. The latter were linked to past wages and, therefore, likely linked to individual educational attainment.[26] Since we can neither identify who is receiving benefits from the moratorium nor observe the individual contributions to the SS, we use educational attainment as the best proxy for the differences in the benefits from the moratorium. Note, however, that even if the moratorium benefits were homogeneous,

[25]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 (2017) 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.

[26]To guarantee that the different responses to the income shock work through the education level channel and not through other mechanisms that might be correlated to education, such as cultural norms, we repeat our estimation by education for large cities only (i.e. larger than 500,000 inhabitants), where cultural norms regarding divorce or separation are presumably more relaxed. The differential impact of the reform on high and low educated women remains and is similar in magnitude to the our main results. We thank a Referee for suggesting this potential mechanism.

we should expect very different effects of the reform due to large differences in the initial income and in-household income shares, across educational groups. Table 1 shows that the high educated treated women had a pre-reform average income four times larger than that of the low educated treated women. And, in a simple model of bargaining within the household (see Appendix 2), we show how different initial conditions, such as in-household income share, may affect the response of the household to an income shock. For ease of exposition, the model depicts the situation of two different women, L and H, with L having a lower pre-reform share of household income and, hence, also bargaining power lower than H. The model shows that when faced with a positive income shock, woman L gains bargaining power and remains married while woman H decides to divorce. In panels B and C of Table 2 we show the initial conditions—i.e. before the reform—of low and high educated treated women. As woman H in the model of Appendix 2, women with a high level of education have a relatively high share of household income (31%) compared to those with low education (19%). Moreover, as the model predicts, they enjoy more bargaining power within the household as shown by the lower likelihood of being the only responsible of household chores (44%) compared to women of low educational level (60%) and the higher likelihood that their husbands help in the house (40%) compared to those with a lower level of education (37%).

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.[27]

[27] 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). Moreover, before the implementation of the law in 2007, many of these women were financially constrained and, hence, were not able to act on the announcement of the law by divorcing or separating.

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.[28] 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

[28]Research using other data for Argentina raises concerns about under-reporting or lack of reporting of information on income (Cruces and Wodon, 2007). 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.

less likely to enjoy the benefits from the reform.[29]

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

Difference-in-difference 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 14% and that the probability of divorce at these ages is low (see footnote 3).[30] 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.[31]

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

[29] All regressions presented in Section 5 were also done without covariates at the request of a Referee. All results carry through except the income of the high-educated. The latter is explained by the presence of very high income outliers in the sample, which are captured in all regressions by a dummy for the "1% richest women." These outliers represent only 312 observations, 285 of those are high-educated women and most belong to the control group. When we exclude all covariates but the "1% richest women" dummy the income effect for the high-educated and for the overall sample is again identical to the results with covariates. Lastly, we rerun all the regressions in the paper dropping these outliers and we find no impact whatsoever on the results with or without covariates.

[30] 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.

[31] As suggested by a referee, we checked whether the different result by educational group could be due to an underlying correlation between women's education and the likelihood that their husbands are beneficiaries of the reform. Because we do not observe any characteristic of the husband for those who divorce/separate, we decided to look at the relationship between husband's characteristics and women's education before the reform. We looked at the correlation with the two conditions a man needs to meet to benefit from the moratorium: 1) being older than 65; 2) not receiving any other pension. We do not find a statistically different from zero correlation between any of these characteristics of the husband and his wife's education in the pre-reform period.

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 (see results of 2) and 3) in Table A.1 in Appendix 1). 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 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 analyze 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.3 in Appendix 1). Because of our results in the previous Section, we analyze 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.[32] 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. 2014) and proxy for women's bargaining power previous to the reform. Their inclusion does not affect 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.[33] 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.

In Table A.2 in the Appendix we provide estimates of 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. A 10 pp increase in the wife's income share within

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

[33] Because the dummy "husband older than 65" acts as a proxy for the (endogenous) pensioner status of the husband, it may fail to control adequately for greater time availability coming from the retirement of husbands/partners. We dismiss this possibility in the discussion of Section 6.3.

the couple decreases 4.6 pp (or roughly 8%) the wife's sole participation in household chores and increases the husband's participation in household chores by 3.7 pp (or roughly 10%). Taken together, these results reflect that gender roles in the household become more alike with the reform.

6 Robustness

One concern with our methodology is that different age profiles in the treatment and control groups may blur the identification of the effect of the reform. The cohort and period dummies, included in all the estimations of our main specification (1), are ineffective in controlling for age differences across groups because age profiles do not overlap and certain age profiles are only observed either in the pre- or post-reform period. Hence, the empirical results of Section 5 capture treatment effects but potentially also effects due to aging.

To discard the possibility that the results obtained so far are driven by the inadequate control for age, we perform two exercises. First, in Section 6.1., 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. Indeed, placebo runs in Section 6.1. show that placebo treatment effects are not statistically different from zero, strongly suggesting that different age profiles cannot explain our results.

Second, in Section 6.2., we control for potential aging effects with a methodology similar to a triple difference approach. This alternative methodology can be thought of as a merge between a placebo and our main DD analysis of Section 5. Relative to the DD analysis of Section 5, it has the advantage of estimating the effect of the reform net of any aging effects although, as we explain below, at the expense of a lower number of observations from our benchmark sample from Section 5.

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.[34] 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 3 and 7). In this placebo we use data from the EPH Puntual, a survey that was replaced by the EPH Continua at the end of 2003.

The interpretation of the placebo runs is valid under two assumptions: a) The placebo treatment does not coincide with changes in the labor market performance, which may impact differently the placebo treated and control groups. b) The aging effects are similar for different cohorts.

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. Unfortunately, the EPH Puntual does not include questions about sharing housework. Hence, with the placebo runs, we cannot draw any conclusion about the effects of aging on the bargaining power outcomes. A possible concern with this placebo test is that standard errors are large. For this reason we propose an additional test in Section 6.2, using a methodology that provides stronger evidence that aging effects cannot explain the bargaining power results either.

[34] 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.

6.2 Merging the Placebo and the DD: a "triple" difference approach

In this Section, we re-estimate the effects of the reform with a methodology that avoids contamination of the reform effect with other effects coming from aging. Results from Section 6.1. suggest that contamination is unlikely. Nonetheless in this Section, we address this problem again but this time using a different approach, which has certain advantages relative to the previous section.

The methodology used here is similar to a triple difference (DDD). The idea is to have both a placebo treatment (similar to the one in Section 6.1), that produces the potential aging effect, and a difference-in-difference (similar to our main specification in (1)), that produces the effect of the reform plus the potential aging effect. By subtracting the former from the latter (the third difference), we obtain the effect of the reform alone.

The ideal placebo would be given by two contemporaneous groups of the same age as the treatment and control groups, respectively, that would not be affected by the reform. Since these contemporaneous groups do not exist, we create two non-contemporaneous groups, which we denote by "placebo sample", from observations from older cohorts observed before the reform. Due to the change in methodology of the EPH, however, these observations cannot go very far back in time. Hence, in order to construct the placebo sample with the relevant age-profiles from 2003 onwards, we had to reduce the number of cohorts and years that we use from the benchmark sample of Section 5.

The DDD results are valid as long as the aging effects, just like in the placebo runs of Section 6.1., are constant for different cohorts for the same age-profiles. Moreover, due to the loss of some observations from our benchmark sample of Section 5, results may differ even in the absence of aging effects.

Table 9 illustrates the difference between the benchmark sample (Section 5)—those observations enclosed in a frame—and the sample used in this exercise—those observations in the shadow area. Amongst the observations used in this exercise, we would like to differentiate between those used as a "placebo", which purpose is to capture the effect due to aging, and those used as the regular DD, which capture both the effect of

the reform and the aging effect. The former are the ones in the left-hand side shaded rectangles, i.e. cohorts 1939-1941 and 1947-1949, all observed before treatment, while the latter are the ones in the right-hand side shaded rectangle and a subset of our benchmark sample i.e. cohorts 1942-1944 and 1950-1952. Crucially, observations on both shaded rectangles have exactly the same age-profile.

A diff-in-diff amongst the left-hand side shaded rectangles, using a fictitious treatment year, 2004, would identify the aging effect, just like our placebo treatment from Section 6.1. A diff-in-diff amongst right-hand side shaded rectangles, using the real treatment year 2007, would identify the sum of the effect of the reform and any aging effect, just like our benchmark analysis in Section 5. To set up the triple difference regression, denote by: a) "BS" a dummy variable that takes value 1 for the benchmark sample (i.e. the right-hand side rectangles, which are a subset of the sample used in Section 5) and value zero for the "placebo sample"; b) "Old" a dummy variable that takes value 1 for those observations from cohorts 1939-1944 and value zero for observations from cohorts 1948-1953; c) "Late" a dummy variable that takes value 1 for observations from the "late" period or "post-treatment" period. Thus, for observations from the placebo sample, Late=1 for the periods 2004, 2005, 2006, while for observations from the benchmark sample, Late=1 for the periods 2007, 2008, 2009, otherwise it takes value 0. The reason for having different periods for Late is because our placebo group is not contemporaneous as in a traditional DDD. With these definitions, the DDD would look like the following, where the coefficient β would be the effect of the reform net of any effects due to aging:

$$\begin{aligned} y_{it} = \alpha + \delta_1 Old_{it} + \delta_2 BS_{it} + \delta_3 Late_{it} + \delta_4 Late_{it} \times BS_{it} + \delta_5 Old_{it} \times BS_{it} + \\ \lambda Late_{it} \times Old_{it} + \beta BS_{it} \times Late_{it} \times Old_{it} + X_i'\gamma + \varepsilon_{i,t} \end{aligned} \tag{2}$$

where X_i is a vector of individual characteristics and ε_{it} is the residual. The interaction of $Late_{it} \times Old_{it}$ would identify the effects of aging alone. Now introduce a complete set of cohort, δ_i^C, and time (year-quarter) dummies, δ_t, which replace the terms "Old" alone, "BS" alone, "Late" alone, and the terms $Old_{it} \times BS_{it}$ and $Late_{it} \times BS_{it}$. The resulting estimating equation is:

$$y_{it} = \alpha + \lambda Late_{it} \times Old_{it} + \beta BS_{it} \times Late_{it} \times Old_{it} + \delta_i^C + \delta_t + X_i'\gamma + \varepsilon_{i,t} \tag{3}$$

In Table 10 we show the estimated β coefficients using specification (3). The estimated effects of the reform on income are slightly stronger than those obtained in Section 5, and the effects on divorce are also strengthened, i.e., the estimated impact on the probability of divorce is now 4.8 pp (versus 2.6 pp) although the new estimate is not statistically different from the original one, and the result on divorce is still driven only by the high 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

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. Results show that, although the reform reduced the labor force participation of husbands, it did also reduce that of their wives (see Panel A of Table 12)—none of them statistically significantly according to our DDD methodology (see Panel B of Table 12).[35] More importantly, however, the effects on husbands and wives are not statistically different from each other, with either methodology. Hence, we cannot conclude that it is men's free time that justifies their larger participation on household chores.

We then look at couples where the husband is still active in the labor market. We acknowledge that being active in the labor market is an endogenous decision but it is still informative to look at this subsample. The idea is that, if we find effects on household chores in this subsample, we are sure they are not due to effects on time availability. We find (not reported due to brevity) that the effects on income are similar to those obtained in the rest of the paper and the effects on household chores are, if anything, slightly reinforced.

[35] Our results on female labor force participation are in line with those of Bosch and Guajardo 2012.

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.

Whether these results can be extrapolated to other age groups or regions is an important question. Regarding the former, our results are certainly extrapolated to an age group as young as the control group i.e. ages 51-59, that is the assumption of the differences-in-differences methodology. It is likely, however, that even younger groups would suffer from larger effects judging from their much higher divorce/separation rates. Regarding the geographical extrapolation, we argue that the low numbers of di-

vorced/separated women among the group of 60-65 years old—comparable only to those of Bolivia, Chile, and Peru— indicate that social and monetary costs of divorce/separation amongst seniors in Argentina are likely amongst the highest in the region in which case, at least our results on divorce/separation, would be regarded as a lower bound to other societies.

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Tables

Table 1: 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$)	480.59	958.43	332.85	783.39	424.07	893.45
Personal income (2009 PPP USD)	321.27	580.07	222.39	474.11	283.44	540.74
Education (high=1, low=0)	0.46	0.47	0.34	0.33	0.41	0.42
Born abroad	0.07	0.09	0.1	0.11	0.08	0.09
Divorced or separated	0.19	0.18	0.14	0.16	0.17	0.18
Legally married	0.70	0.71	0.76	0.73	0.72	0.72
In union (not legally married)	0.11	0.11	0.1	0.11	0.11	0.11
Married or in union, living only with spouse	0.13	0.2	0.36	0.43	0.22	0.29
Active (in the labor market)	0.58	0.55	0.34	0.22	0.49	0.43
Observations	11,051	10,433	6,460	6,092	17,511	16,525
Panel B: Women with a lower level of education						
Pension recipient	0.03	0.06	0.11	0.74	0.07	0.35
No personal income	0.47	0.45	0.59	0.17	0.52	0.33
Personal income (Argentine Pesos: AR$)	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	6,189	5,609	4,382	4,071	10,571	9,680
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	4,862	4,824	2,078	2,021	6,940	6,845

Table 2: 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$)	190.53	406.01	141.91	510.42	168.95	451.53
Personal income (2009 PPP USD)	127.33	245.94	94.73	308.98	112.86	273.42
Wife's share of income within couple	0.23	0.23	0.19	0.33	0.21	0.27
Born abroad	0.08	0.11	0.12	0.11	0.1	0.11
Legally married	0.83	0.84	0.87	0.86	0.85	0.85
Married or in union, living only with spouse	0.17	0.25	0.4	0.46	0.27	0.34
Couple's age difference	2.54	2.23	2.37	2.43	2.47	2.32
Couple's education difference	-0.31	-0.33	-0.44	-0.43	-0.36	-0.37
Wife is uniquely responsible for housework	0.4	0.44	0.46	0.43	0.43	0.43
Husband does housework	0.22	0.26	0.28	0.35	0.25	0.3
Have domestic service or external help	0.01	0.01	0.01	0.02	0.01	0.01
Active (in the labor market)	0.46	0.43	0.26	0.15	0.37	0.31
Observations	5,160	4,613	3,808	3,527	8,968	8,140
Panel B: Women with a lower level of education living only with husband/partner						
Pension recipient	0.02	0.05	0.14	0.8	0.1	0.49
No personal income	0.54	0.49	0.64	0.14	0.6	0.28
Personal income (Argentine Pesos: AR$)	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
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	1,081	1,469	1,564	2,312	2,645
Panel C: Women with a higher level of education living only with husband/partner						
Pension recipient	0.062	0.095	0.29	0.72	0.2	0.41
No personal income	0.34	0.29	0.4	0.13	0.37	0.21
Personal income (Argentine Pesos: AR$)	804.4	1,670	682.3	1,184	730.7	1,427
Personal income (2009 PPP USD)	535.8	1,008	454.3	713.8	486.6	860.7
Wife's share of income within couple	0.35	0.35	0.31	0.35	0.33	0.35
Born abroad	0.08	0.07	0.06	0.07	0.07	0.07
Legally married	0.74	0.81	0.9	0.85	0.84	0.83
Couple's age difference	4.08	4.43	2.99	2.48	3.42	3.45
Couple's education difference	0.66	0.52	0.29	0.41	0.44	0.47
Wife is uniquely responsible for housework	0.36	0.4	0.44	0.4	0.41	0.4
Husband does housework	0.44	0.37	0.40	0.37	0.42	0.38
Have domestic service or external help	0.08	0.11	0.09	0.07	0.09	0.09
Active (in the labor market)	0.66	0.64	0.38	0.22	0.49	0.43
Observations	585	929	762	863	1,347	1,792

Table 3: 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 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 (US$ 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	12,552	12,552	12,552
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 (US$ 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	8,453	8,453	8,453
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 (US$ 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	4,099	4,099	4,099
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	12,552	8,453	4,099
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 (US$ 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	3,033	3,033	3,033	2,984	3,033	3,033	3,033
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 (US$ 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 5 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 (US$ PPP)	(4) Divorced or Separated
$BS_{it} \times Late_{it} \times Old_{it}$	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	11,881	11,881	11,881	11,881
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 (US$ PPP)	(4) Divorced or Separated
$BS_{it} \times Late_{it} \times Old_{it}$	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	8,145	8,145	8,145	8,145
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 (US$ PPP)	(4) Divorced or Separated
$BS_{it} \times Late_{it} \times Old_{it}$	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	3,736	3,736	3,736	3,736
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 $BS_{it} \times Late_{it} \times Old_{it}$ is the estimated parameter β of equation 3. "BS" takes value 1 for the benchmark sample and value zero for the "placebo sample"; "Old" takes value 1 for observations from cohorts 1939-1944 and value zero for observations from cohorts 1948-1953; "Late" takes value 1 for observations from the "late" period or "post-treatment" period. See Section 6.2 for more details. 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 $\lambda Late_{it} \times Old_{it}$. 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 (US$ 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
$BS_{it} \times Late_{it} \times Old_{it}$	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	3,070	3,070	3,070	3,016	3,070	3,070	3,070
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 $BS_{it} \times Late_{it} \times Old_{it}$ is the estimated parameter β of equation 3. "BS" takes value 1 for the benchmark sample and value zero for the "placebo sample"; "Old" takes value 1 for observations from cohorts 1939-1944 and value zero for observations from cohorts 1948-1953; "Late" takes value 1 for observations from the "late" period or "post-treatment" period. See Section 6.2 for more details. 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, level of education attained and the interaction term $\lambda Late_{it} \times Old_{it}$. 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.

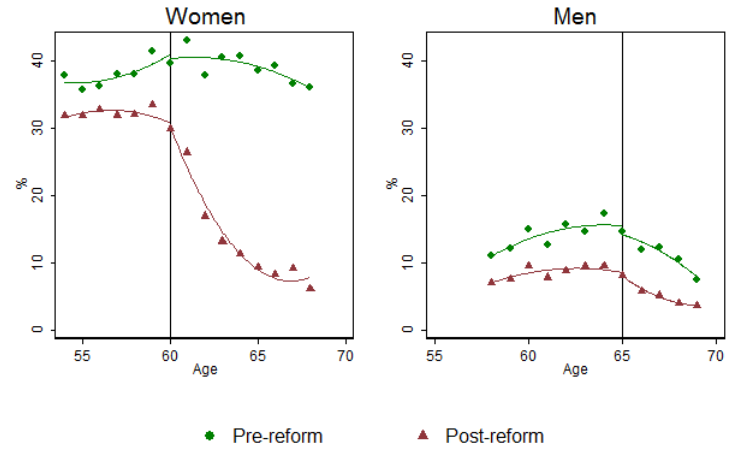
Table 12: The effect of the reform on the labor status
Sample of women with lower level of education living only with husband/partner

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	3,033	3,033
Mean dependent variable	0.248	0.516
Panel B: Alternative specification of Section 6.2. (equation 3)		
	(1) Wife is in the labor market	(2) Husband is in the labor market
$BS_{it} \times Late_{it} \times Old_{it}$	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	3,069	3,069
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 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. In the second panel, the coefficient on $BS_{it} \times Late_{it} \times Old_{it}$ is the estimated parameter β of equation 3. "BS" takes value 1 for the benchmark sample and value zero for the "placebo sample"; "Old" takes value 1 for observations from cohorts 1939-1944 and value zero for observations from cohorts 1948-1953; "Late" takes value 1 for observations from the "late" period or "post-treatment" period. See Section 6.2 for more details. 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, level of education attained and the interaction term $\lambda Late_{it} \times Old_{it}$. 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.

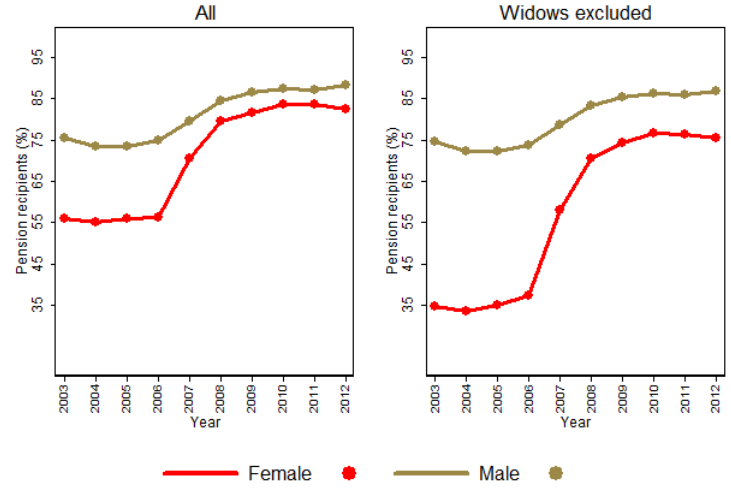
Figures

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

Figure 3:
Pension recipients

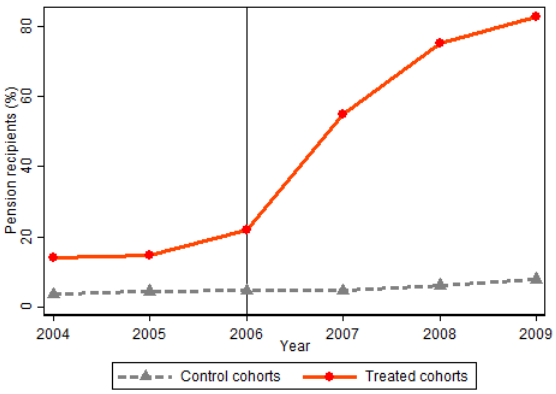


Figure 4:
Women without personal income

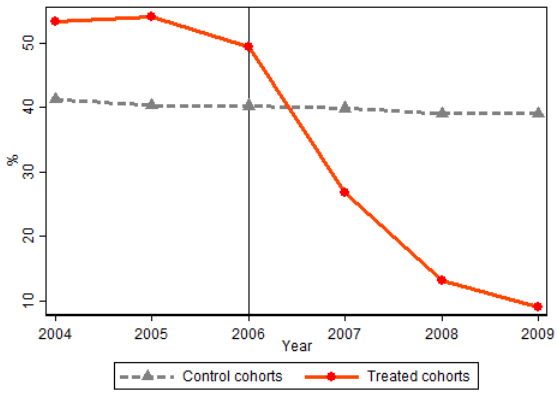


Figure 5:
Evolution of monthly personal income

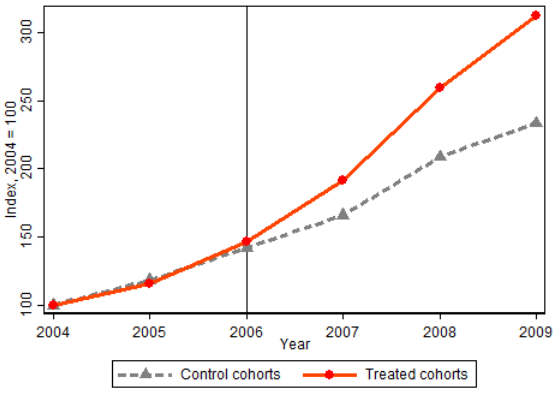
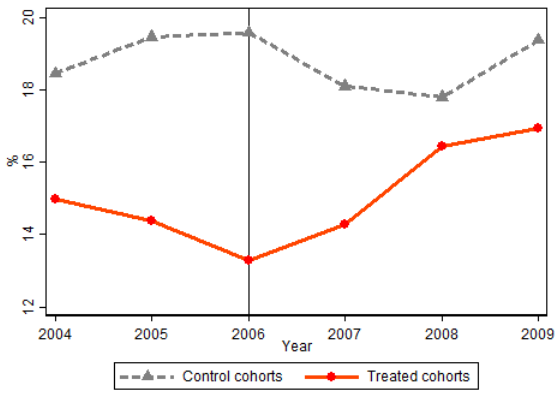


Figure 6:
Woman is divorced/separated



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

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

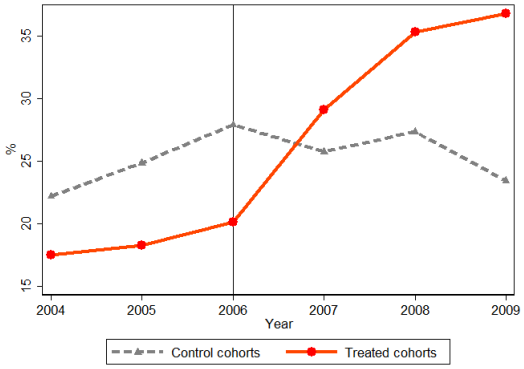


Figure 8: Woman is uniquely responsible for housework

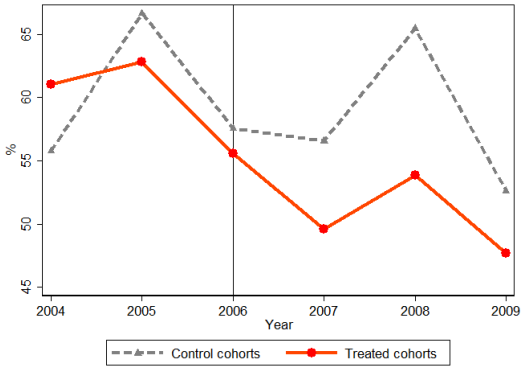
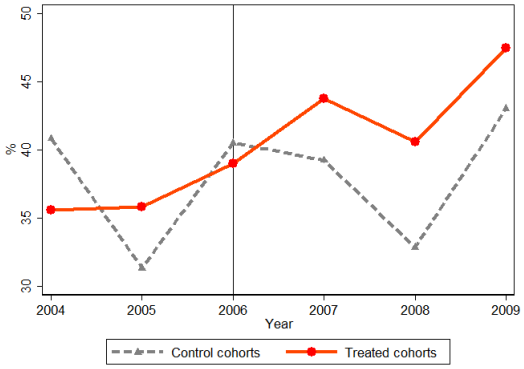


Figure 9: Husband does housework



Appendix 1

Figure A.1: News related to the moratorium

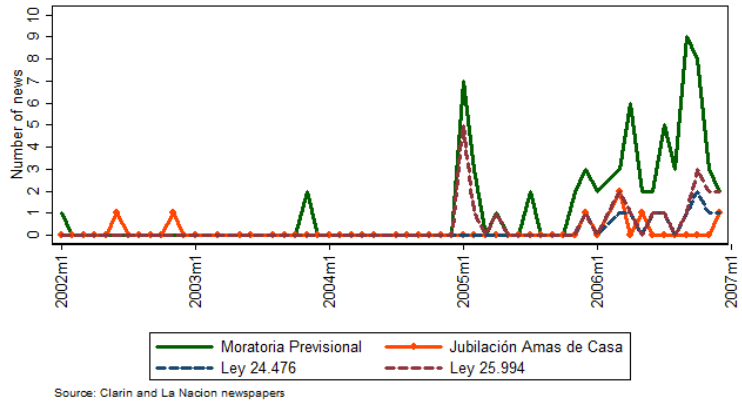


Figure A.2: Google searches related to the moratorium

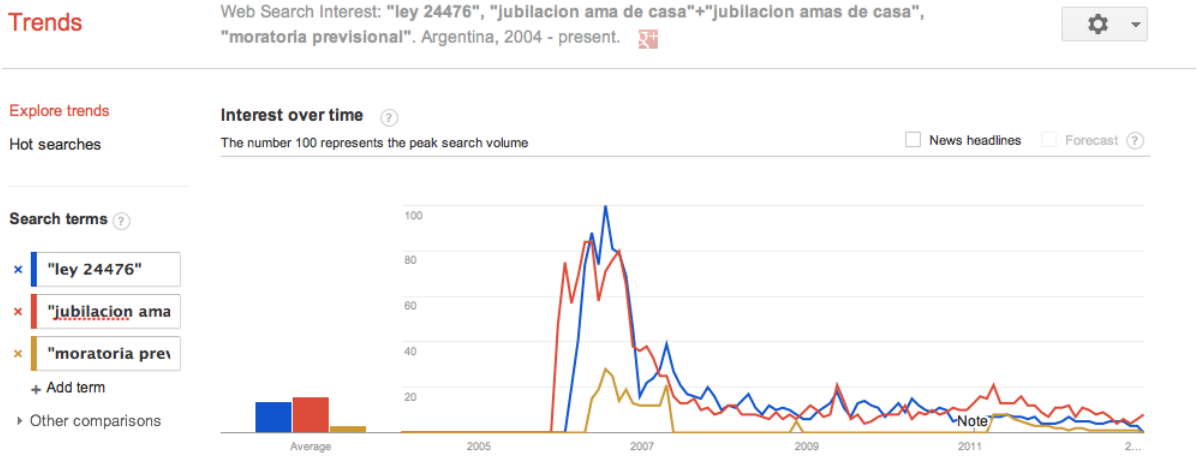


Table A.1: Effects of the reform on widowhood and singlehood

	(1) Widow	(2) Single
Post*Treated	0.001	0.006
SE	(0.009)	(0.011)
Observations	46,044	46,044
Obs. in treatment group	19,215	19,215
Mean dependent variable	0.233	0.0786

Note: The dependent variables are: a dummy that equals one when the woman is a widow (column 1); a dummy that equals one when she is single (column 2). The coefficient on Post*Treated is the estimated parameter β of equation 1. Period 2004-2006 is the pre-treatment period and period 2007-2009 is the post-treatment period. The sample includes all women (married/cohabiting, single, widow and divorced/separated). 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), and dummy variable that equals one if the woman was born abroad. 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. Cluster robust sandwich standard errors are in parentheses (SE clustered at the level of urban area). *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: IV Regressions. Sample of women with a lower level of education living only with husband/partner

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

	(1) Wife is uniquely responsible for housework	(2) Husband does housework	(3) Domestic service or external help
Wife's share of couple's income	-0.460***	0.369***	0.00116
SE	(0.167)	(0.111)	(0.0425)
First-stage F-statistic	43.3	43.3	43.3
Observations	4,866	4,866	4,866

Note: The dependent variables are: a dummy that equals one if the wife is the only household member responsible for housework (column 1), a dummy variable that equals one if the husband does housework (column 2), and a dummy variable that equals one if the household has domestic service or external help for housework (column 3). The sample includes all low-educated married/cohabiting women living only with their partners and born in the periods 1941-1944 and 1950-1953. Data covers the period 2004-2009. 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, differences between spouses' age and level of education attained, and 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. We instrument the variable "Wife's share of couple's income" by using the reform (Post*Treated) as an instrumental variable. First stage results are shown in column 4 of Table 6. 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. Cluster robust sandwich standard errors are in parentheses (SE clustered at the level of urban area). *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: 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

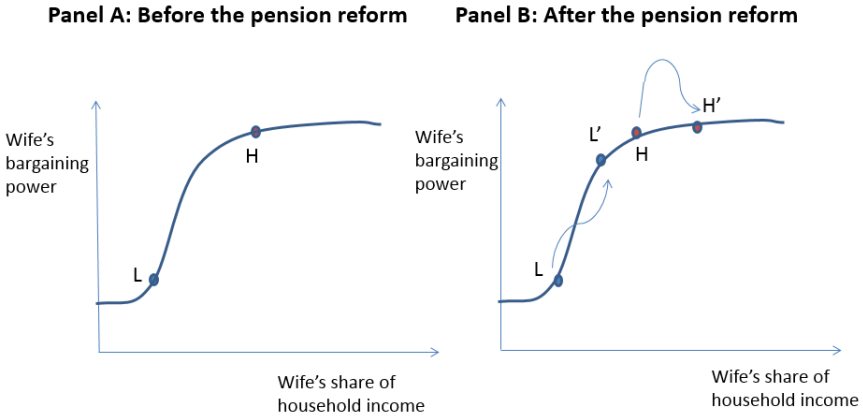
	(1) Receive a pension	(2) Without personal income	(3) Wife's income (U$S PPP)	(4) Wife's share of couple's income	(5) Wife is uniquely responsible for housework	(6) Husband does housework	(7) Domestic service or external help
Post*Treated	0.609	-0.443	98.950	0.149	-0.057	0.025	0.008
SE	(0.023)***	(0.027)***	(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	7,335	7,335	7,335	6,868	7,335	7,144	7,335
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.

Appendix 2

In this Appendix, we provide a simple model of bargaining inside the household that would lead to empirical bias due to selection caused by divorce. Along the lines of the theoretical literature on bargaining and decision making in the household, we envision a model whereby the wife's bargaining power in the household is a non-decreasing function of her share of income, as depicted in panel A of the following figure:

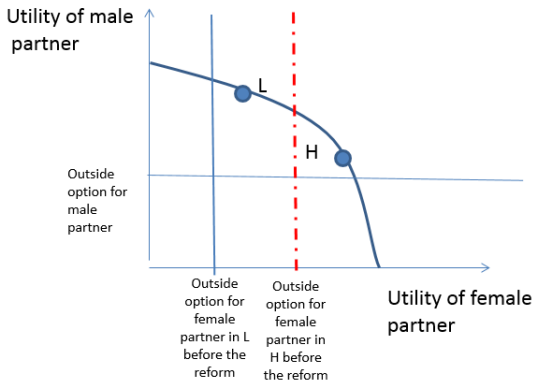
Figure A.3: Female bargaining power in the household as a function of her share of income



Imagine two different women, L and H, with the same total household income but different shares and different bargaining power in their households. The reform would increase their share of income as well as their households' total income—for simplicity assume it does not affect the function in panel A of Figure A.3. After the reform, both women L and H would increase their bargaining power but L would increase it much more than H, as panel B in Figure A.3 shows.

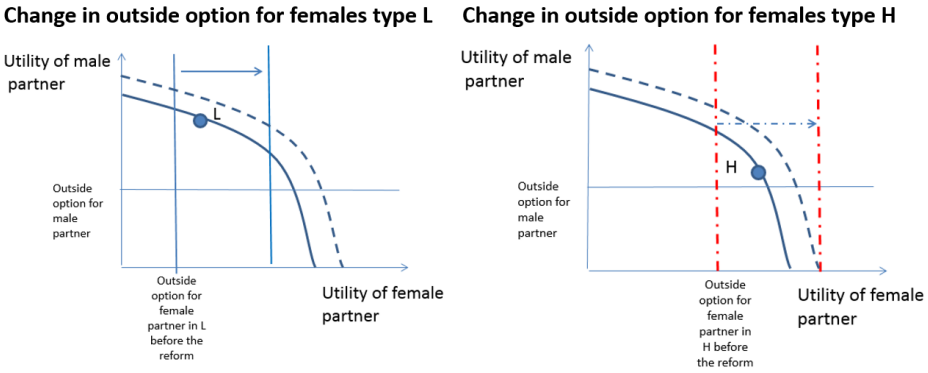
Individuals L and H have different initial outside options in the case of divorce (for example related to their different initial income levels or education) but for simplicity assume their husbands have the same outside option. Due to their different outside options and bargaining power, the two women will enjoy different initial levels of utility within the marriage, with L enjoying less than H as shown in Figure A.4.

Figure A.4: Household utility frontier before the reform



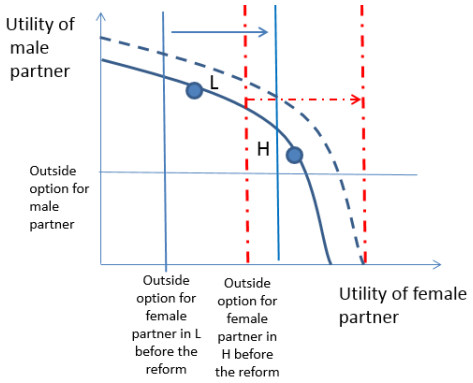
The increase in total household income due to the reform shifts the utility frontier to the right as depicted by the dashed utility frontier in Figure A.5 as well as both women's outside options, i.e. their utilities in case of divorce, as depicted by the parallel movement of the vertical lines in Figure A.5.

Figure A.5: Effects of the reform on the utility frontier and outside options



Even if these women's outside options were to shift by the same amount, the effects on marriage stability may be very different, with woman L remaining married and gaining bargaining power while woman H divorces/separates —because no point in the new utility frontier satisfies her binding outside option—as depicted in Figure A.6.

Figure A.6: The effect of the reform on both women



In this simple model, those with lower gains in bargaining power within the marriage, such as women of type H, divorce after the reform while those with the highest gains, such as women of type L, remain married.