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Gender inequality and intra-household bargaining power: empirical evidence from developed and developing countries.

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Coordinatore del Dottorato
ch. prof. Pietro Dindo

Supervisore
ch. prof. Francesca Zantomio

Secondo Supervisore
ch. prof. Agar Brugiavini

Dottorando
Annarita Macchioni
Giaquinto
Matricola 956375

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I am sure that there is a future version of me who is proud I was strong enough.

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Preface

Although women represent half of the worldwide population, there is no country in the world where women and men are equal.

The World Economic Forum estimated that, at the current trajectory globally, it would take 135.6 years to close the gender gap and achieve parity between males and females (World Economic Forum, 2021). In developing and developed countries, women experience systematic inequality in all domains, including health, education, political representation, and the labour market. Gender gaps are different depending on whether the country is low, middle, or high income. In fact, in high-income countries, women experience inequalities mainly in political representation and in the labour market domain. In contrast, if we consider low- and middle-income countries, they face discrimination also concerning health and education.

Besides being a human right, gender equality is crucial for economic efficiency. Gender parity has implications for households' outcomes and the whole economy. On one side, within collective modelling, changes in women's relative bargaining position lead to changes in household allocations towards that individual's preferences, empowering women and allowing "*those who have been denied the ability to make choices [to] acquire such an ability*" (Kabeer, 1999, p.24). On the other side, not using the full potential in terms of labour, decisions, and competencies of women because of gender-based discriminations and poor bargaining and decision-making power is a waste of resources, which further harms economic growth and productivity. Gender parity and women's empowerment might be a remedy against high population growth rate, environmental degradation and indeed the low status of women (Batliwala,1994) likewise to eradicate poverty, hunger, food insecurity and malnutrition (Klugman 2002; FAO, 2019).

This thesis shed new light on the link between gender inequality and intrahousehold bargaining power with respect to health-related outcomes, human capital accumulation, labour market and fertility.

Using UK longitudinal data, the first chapter of this thesis investigates whether relative gender specialisation and intrahousehold bargaining power in income production, as opposed to home production, might be responsible for asymmetric response to partner's health shock in terms of the labour market and informal care. Empirical results show a lack of response in labour market outcome but a significant response to partner's health shock in informal care provision, irrespectively from gender. No evidence emerges for behavioural responses driven by gender specialisation in labour income production versus home production, which would have resulted in asymmetric responses by gender, with women increasing time devoted to paid work, and men increasing time dedicated to informal care in the event of partners' health shock.

The second chapter focuses on birth control rights which is fundamental for gender equality and women's empowerment. Historically, oral contraceptives, most notably the pill, transferred from men to women the control on contraception, shifting out the frontier of women's available choices in terms of educational and career planning. Using a quasi-experimental design exploiting the staggered and uncoordinated introduction of the contraceptive pill on-demand to young, adult, unmarried women in 14 European countries between the 60s and 80s, this work explores economically relevant consequences and gender inequality implications induced by a change in female bargaining power stemming from women's increased control over contraception. Using Survey of Health, Ageing and Retirement in Europe (SHARE) data, results show that the pill induced a significant and sizable increase in women's educational attainments and labour market outcomes.

Chapter three focuses on developing countries, where gender inequalities are more severe, a large share of the population is concentrated in rural areas, and livelihoods are mainly agricultural-based. In this context, assets are crucial to income-generating activities and well-being. Yet, a systematic gender asset gap hinters women's empowerment, limiting their voice and agency in the society and within marriage. Given power relations within marriage, women result also disempowered regarding fertility decisions. Collective modelling of households predicts that a change in bargaining- power resulting from a change in the distribution and control of resources, such as assets, might affect different outcomes, including childbearing. To address endogeneity, the empirical strategy exploits a pro-woman legal innovation implemented in Nepal in 2002 that provided married women equal rights to their husbands' property immediately after marriage. Using Nepal's cross-sectional data from Demographic Health Survey (DHS), regression discontinuity in time and before-after comparison on matched women, this chapter shows that female asset ownership induced a sizable and statistically significant reduction in fertility and has demographic implications for developing countries.

Chapter 1

Labour supply and informal care responses to health shocks within couples: evidence from the UKHLS

Annarita Macchioni Giaquinto[1], Andrew M. Jones[2], Nigel Rice[3], Francesca Zantomio[4]

Abstract: Shocks to health have been shown to reduce labour supply for the individual affected. Less is known about household self-insurance through a partner's response to a health shock. Previous studies have presented inconclusive empirical evidence on the existence of a health-related `added worker effect'. We use UK longitudinal data to investigate within households both the labour supply and informal care responses of an individual to the event of an acute health shock to their partner. Relying on the unanticipated timing of shocks, we combine coarsened exact matching and entropy balancing algorithms with parametric analysis and exploit lagged outcomes to remove bias from observed confounders and time-invariant unobservables. We find no evidence of a health-related 'added worker effect'. A significant and sizeable increase in spousal informal care, irrespective of spousal labour market position or household financial status and ability to purchase formal care provision, suggests a substitution to informal care provision, at the expense of time devoted to leisure activities.

Keywords: health shocks, added worker effect, labour supply, informal care, matching methods, panel data

JEL codes: C14, I10, I13, J14, J22

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[1] Ca' Foscari University of Venice, FAO; annarita.macchioni@unive.it
[2] University of York, Monash University
[3] University of York, Monash University
[4] Ca' Foscari University of Venice

1.1 Introduction

The ageing population in Western Countries is becoming one of the most significant social transformations of this Century. According to the European Commission, the old-age dependency ratio, namely the proportion of people aged 65 or older relative to those aged 15-64 years old, is projected to increase from 29.6 % in 2016 to 51.2% in 2070 (European Commission, 2018). Most notably, population ageing has significant implications for social protection systems having to incur higher spending on social insurance programmes and healthcare; and for labour markets, in terms of longer working lives, as witnessed by the rising trend in statutory retirement ages across European countries.

As working life increases, so does the risk of experiencing a health shock while engaged in labour market activity, age being one of the most relevant predictors of health deterioration. Health shocks represent a major source of economic risk. An established literature shows how shocks reduce labour supply for an individual, entailing a significant reduction in earnings (see, for example, recent works by Flores et al. (2019), Lenhart (2019) and Jones et al., (2020) and literature cited therein). Depending on healthcare financing arrangements, the economic consequences of health shocks might extend to an increase in health-related expenditures, leading to the risk of catastrophic payments, reduced access to credit and consumer borrowing, as recently shown for the US by Dobkin et al. (2018). Wealth deteriorations and negative spillover effects might then extend to other family members (Zwysen, 2015), thus involving the whole household.

Other household members, and partners in particular[5], might provide an important source of informal insurance against this economic risk that operates in conjunction with the formal social and healthcare insurance available. Interest in family and partners' responses is growing (e.g. see Dobkin et al., 2018; Fadlon and Nielsen, 2021; and also Gathmann et al., 2020 on the reverse issue of job loss and health spillovers in couples). Enhancing understanding of a partner's response within a household is key to assessing how couples' financial and non-financial wellbeing is affected by shocks, the role of informal insurance mechanisms in complementing social insurance provision, and population welfare.

From a theoretical point of view, under a collective labour supply framework (Apps and Rees, 1997; Chiappori, 1997), the effect of a health shock on spousal labour supply is ambiguous. The income effect arising from the loss of earnings by the person whose health deteriorates (only partially compensated by disability benefits or pension entitlements, given prevailing replacement rates) might increase spousal labour supply, in the spirit of what has been called the Added Worker

[5] We are concerned with within household responses between partners. We use partners and spouses interchangeably, although we do not restrict analysis to married couples.

Effect (AWE) (Mincer, 1962, Lundberg, 1985). While the income effect is diminished if a health shock affects individuals living on pension or non-labour income, additional consumption needs arising from disability might occur, for example in terms of transport, heating, formal care or other extra-costs of disability. In addition to an income effect, in countries such as the USA, where employment-contingent health insurance plays a major role, the importance of extending healthcare coverage to the individual experiencing the health shock creates an additional incentive for a partner to seek suitable employment (Bradley et al., 2013).

In contrast to an AWE, the event of a health shock might also be expected to lead to a reduction in the labour supply of a partner. A shock-induced disability might limit home production necessitating additional spousal involvement at the expense of time devoted to work. Home production in the form of informal care provision would appear particularly relevant. Complementarity of partners' leisure, enhanced by newly acquired health information possibly indicating a shortening lifespan might also contribute to reducing, rather than increasing, spousal labour supply. Indeed, complementarity in the non-market time of older husbands and wives is documented by Kneisner (1976), and confirmed by Hamermesh (2002) and Hallberg (2003) who find that partners prefer consuming leisure at the same time of the day and adjust work duties and schedules accordingly. Complementarity in leisure has also been identified as one of the main drivers of joint retirement decisions (Gustman and Steinmeier, 2000; Stancanelli and Van Soest, 2016).

Previous studies have provided inconclusive empirical evidence on the existence of a health-related AWE[6]. Some studies (van Houtven and Coe, 2012; Garcia Gomez et al., 2013, for the US and Netherlands respectively) have found no empirical support for an AWE, or even a reduction in men's labour supply following a health deterioration for a female partner (but no response in women's labour supply when their male partner's health deteriorates). In these studies, home production needs and the complementarity of leisure appear to dominate the income effect, especially for men who are less exposed to major income losses should their partner's health deteriorate. Lack of an economically significant AWE for non-fatal heart attacks or strokes has recently been confirmed by Fadlon and Nielsen (2021), who use Danish administrative records and

[6] Even for the unemployment related AWE, evidence on whether increases in labour supply happen or not, is mixed. According to Ashenfelter (1980), spousal labour supply acts as an insurance against partner's unemployment . Lundberg, (1985), Juhn and Potter (2007), Ayhan (2015) and Giannakopoulos (2015) find a positive AWE, but only at the extensive margins. Analysing different European countries, Bredtmann et al. (2014) relate the AWE variation registered along the extensive and intensive margins to welfare regimes and business cycles. However, Heckman and MaCurdy (1980) find no evidence of AWE and explain the result with lifecycle dynamics; Cullen and Gruber (2000) attribute the lack of AWE to the role of unemployment benefit programs. A further explanation is that women's low labour force attachment under a traditional division of labour could explain the lack of an AWE (Prieto-Rodriguez and Rodriguez-Gutierrez, 2000; Başlevent and Onaran, 2003; Bentolila and Ichino, 2008). Relatedly, previous studies suggest that intra-household specialization plays a role in shaping spousal labour market adjustments.

construct counterfactuals by exploiting households where both partners experience a health shock but at different times. They attribute the absence of an AWE to the lack of need for self-insurance, given the generous social insurance coverage available in Denmark which almost fully compensates the earnings loss: while the decline in earnings for the individual affected is estimated to be 19%, the corresponding reduction in household post-transfer income amounts to only 3%. In a strikingly different institutional context, a recent contribution by Dobkin et al. (2018) documents the lack of an AWE following hospitalizations in the US. Despite comparable (to Denmark) drops in earnings suffered by hospitalised individuals (about 20% of previous earnings on average), only about 10% of this reduction is compensated by social insurance; yet no AWE is detected.

Contrasting results have emerged for both men and women's responses to a partner's health shock. A reduction in men's labour supply is found in Berger (1983), Blau and Riphahn (1999), Charles (1999) and Nahum (2005). However, a small increase in men's labour supply is found by Coile (2004) and confirmed by Johnson and Favreault (2001), the latter in terms of a reduction in the probability of retirement. For women, Charles (1999) found an increase in labour supply in response to a shock to their male partner's health using US data, but a decrease in a male partner's labour supply in response to a female partner's health shock. This is interpreted as consistent with the idea of a relative gender specialization in income production (men) and home production (women) and a partner's response aimed at compensating for the reduction in time use of the partner who experiences a health shock. Several studies report heterogeneity in the responses of women, reflecting baseline labour market attachment (Berger 1983; Blau and Riphahn, 1999; Jimenez Martin et al., 1999), and in response to disability insurance eligibility and generosity (Berger and Fleisher, 1984; Chen, 2012).

Previous studies have acknowledged the importance of distortions to home production following a health shock, but most have discussed these indirectly while focusing on labour supply. For example, by considering how labour supply adjustments vary by income, and noticing that the reduction in labour market participation is larger for higher income couples, Garcia Gomez et al. (2013) hint at a preference for leisure as an explanatory mechanism, as higher income individuals can afford to purchase home production and informal services in the market. However, it might well be that partners prefer informal home production and care provision, despite market alternatives.

Our study extends the literature by considering both the labour supply and the informal care responses of household partners of individuals who experience an acute health shock. We do this by exploiting household panel data drawn from the Understand Society survey, conducted in the UK since 2009. Information on both labour and home production is collected for every adult household member across a number of waves. This provides direct evidence on informal care (covering both informal care provided to the shocked partner, and to other household members) as well as labour supply responses. The identification strategy follows previous contributions in the field that exploit acute health shocks, such as heart attack, stroke and cancer, as a source of unanticipated variation in the timing of health deteriorations (e.g. Smith, 1999 and 2005; Datta Gupta et al., 2015; Trevisan et al., 2016; Jones et al., 2020). Conditioning on a wide range of observable individual characteristics for both partners, as well as household- and couple-level characteristics, we assume that the chance that a partner experiences an acute health shock at any particular point in time is conditionally random, and match household couples where one partner experiences the health shock, with observationally identical household couples where neither partner experiences a health shock.

Following Jones et al. (2020), matching is performed through a combination of Coarsened Exact Matching and Entropy Balancing. This approach is suited to a setting that offers a much larger number of controls than treated units. ATTs are then obtained through parametric modelling on the matched sample. We do this separately for each of the outcomes: employment, hours worked, informal care provision and hours of care for the non-shocked partner. Our results for labour supply show no evidence of a health-related AWE. However we find a sizeable increase in informal care provision. Together with lack of a significant change in spousal working hours, the increase in spousal time devoted to informal care - which is detected irrespective of affordability of formal care, as proxied by household income - suggests a substitution to personal involvement in caring, at the expense of time devoted to other non-work activities.

1.2 Data

We use nine waves of Understanding Society, the UK Household Longitudinal Study (UKHLS) that, starting in 2009, builds on the previous British Household Panel Study (BHPS), but offers a larger sample size of about 40,000 households and 100,000 individuals (at wave 1). While the BHPS has been widely used to study health and labour dynamics, the larger UKHLS sample is important as it allows analysis of sub-populations previously regarded as too small for research (Buck *et al.*, 2012): such as couples experiencing one of the three types of health shocks that we select (heart attack, stroke or cancer).

While the fieldwork of each UKHLS wave lasts about two years, all individuals aged 16 or above living in a target household are interviewed yearly, allowing us to use up to nine interviews undertaken by the same person between 2009 and 2019. During the first interview, individuals are asked about their past life history and their health history in terms of diagnoses and events, including the onset of heart attacks, strokes or cancers[7]. This allows us to observe whether an individual had already experienced an acute health shock of the type we select. During subsequent interviews individuals report any new diagnosis or onset of health problems that occurred since the previous interview, so that an annual life history of health shocks can be constructed and updated. In addition, the survey collects a wider set of characteristics that are informative of underlying health risks: for example diagnoses of coronary heart disease, angina, diabetes and high blood pressure, all related to cardiovascular risk (Braunwald, et al., 2015); the presence of a long-standing illness or disability, limitations in activities of daily living (ADLs); information about past and current smoking and intensity; and parents' longevity (whether each parent was alive when the respondent was aged 14), indicative of relevant genetic characteristics.

Demographic information covers age, gender, race, marital status, number of children, and household size. Detailed information collected on individual labour market activity includes employment status (both employment and self-employment), hours worked and earnings. Available socioeconomic indicators cover education (the highest qualification achieved), various income sources (labour, pension, investment and transfers including different types of benefit income e.g. disability-related, means-tested), and home ownership. Individual level and source-specific income information provides indicators of household income composition (e.g. income sources which would not be exposed to health risk, such as pension income and investment income) and each partner's contribution to overall household income. These serve the purpose of assessing the level of household economic exposure to the monetary impact of a partner's health shock.

Finally, individuals are interviewed yearly on care provided to other household members, and their identity, as well as on the intensity of care provided to each, measured by the number of hours provided (in bands: 0-4; 5-9; 10-19; 20-34; 35-49; 50-99; 100 or more). Care received by other informal caregivers living outside the household is also traced[8]. Wider types of home production, such as a variety of household chores, are covered only in specific waves, and for this reason cannot be exploited in our analysis. Descriptive statistics on the full list of variables employed in our study, on the sample selected for analysis, are reported in Table 1, and discussed in Section 3.2.

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[7] The full list covers: asthma; arthritis; congestive heart failure; coronary heart disease; angina; heart attack or myocardial infarction; stroke; emphysema; hyperthyroidism or an over-active thyroid; hypothyroidism or an under-active thyroid; chronic bronchitis; any kind of liver condition; cancer or malignancy; diabetes; epilepsy; high blood pressure; clinical depression.

[8] Information on formal care received by (paid) providers is collected only in two waves.

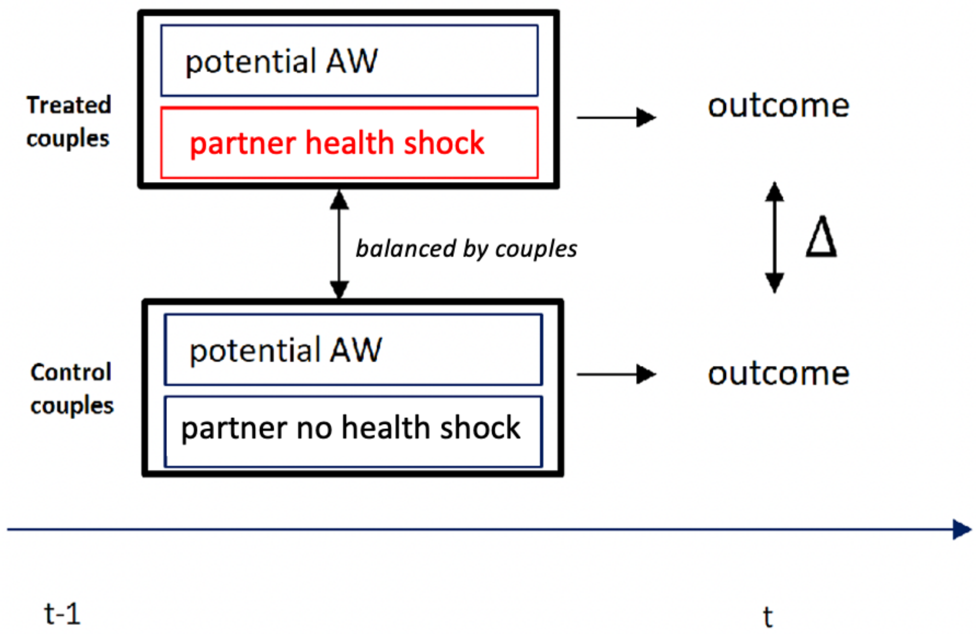
1.3 Empirical Methods

1.3.1 Research design

The main challenge for identifying the causal effect of a health shock stems from potential selection bias with respect to labour market outcomes of a partner (see e.g. Siegel, 2006). Empirically documented mechanisms such as assortative mating (Greenwald et al., 2014) and its reflection in terms of partners' health-relevant behaviours such as smoking, diet and exercise (e.g. see Clark et al., 2006) and labour supply patterns; comorbidity in couples (Guner et al., 2018); joint determination of partners' labour supply and home production decisions all contribute to concerns about unobserved heterogeneity and reverse causality. A way to address such concerns is to exploit some source of unanticipated variation in health. Previous authors have, for example, exploited road injuries and commuting car accidents (Dano, 2005; Halla *et al.*, 2013), unplanned hospitalizations (Garcia-Gomez et al., 2013; Belloni et al., 2019) or the onset of acute health shocks (e.g. Smith, 1999 and 2005; Datta Gupta et al., 2015; Trevisan et al., 2016; Jones et al., 2020). We follow this last approach and use the onset of a heart attack, stroke or cancer experienced by one partner in a household to study the spousal (i.e. the unaffected partner and potential added worker) behavioural response. The first two types of health shock are cardiovascular events chosen because they occur suddenly at an identifiable, yet unpredictable, point in time (Braunwald, 2015); the third type, cancer, although a progressive condition, is often asymptomatic and typically becomes known upon diagnosis. While individuals might reasonably be expected to anticipate their own health risk, in the light of known risk factors, the timing of an acute health shock is likely to be unanticipated. Moreover, the focus on major health conditions minimises the scope for misreporting and recall bias that might be present in analyses based on milder or other progressive conditions.

The research design is illustrated in Figure 1. We study the behaviour of individuals (potential added workers (AW)) whose partner experiences an acute health shock between time $t-1$ and time t: these couples represent our treatment group. The treated couples are compared to a control group of couples, selected so that both partners are individually observationally equivalent (up to the time of the shock) to those in the treatment group, except that neither experiences an acute health shock. The potential AW's responses are observed from time t onwards. Pre-shock (i.e. as of $t-1$ in Figure 1) observational equivalence between partners in treated and control couples is based on a broad set of individual and household variables that accounts for demographic and socioeconomic characteristics, labour market activity, health risks, past acute shocks, and lagged outcomes (both labour supply and informal care).

Figure 1: Research Design



Identification assumes conditional independence. That is, conditioning on these variables is enough to regard the time-specific acute health shock as random, so that no remaining unobserved characteristic would jointly affect the chance of a partner experiencing a time-specific health shock and the spousal labour market and informal care response observable from time t onwards. Conditional independence, while untestable, rests on the rich set of observed variables and the fact that the longitudinal data controls for time-invariant unobservables through lagged outcomes.

To achieve observational equivalence between treatment and control couples, we adopt the preprocessing approach discussed by Ho *et al.* (2007). This uses matching methods to balance the distribution of confounders between treated and control units, to reduce model dependence, before using parametric modelling on the matched data to tackle any remaining imbalance. In this respect the preprocessing approach is doubly robust to either misspecification in the matching or parametric modelling steps.

For preprocessing we follow Jones et al. (2020), who model individual responses to health shocks based on the same UKHLS data and show how a combination of Coarsened Exact Matching (Iacus, King and Porro, 2012) and Entropy Balancing (Hainmueller, 2012) allows attaining a tight balancing of confounding covariates. Combining the two matching methods retains the advantages of each. Coarsened Exact Matching aims at achieving exact matching through stratification followed by exclusion of strata where either only treated or only control units are found. CEM corresponds to exact matching for binary variables but coarsens continuous variables into intervals and is less data hungry than exact matching for these variables. CEM has the monotonic imbalance bounding property of improving the balance on each covariate without worsening that of others, although at the cost of reducing the sample size available for estimating causal effects as the set of included confounders increases (Iacus et al., 2011). A further implication is that CEM balances the joint distribution of confounding covariates, including interactions and nonlinearities. For this reason, it is used here to attain common support and tight balancing for a limited set of key covariates. Once extreme units are discarded from the common support through CEM, Entropy Balancing (EB) balances the full set of confounders. EB operates by minimizing an entropy distance metric subject to balance constraints (for example equality of means between treated and matched controls) and normalizing constraints, generating weights to be applied in the following regression analysis. While EB operates on univariate distributions for each confounder separately, it is possible to extend the algorithm so that balancing extends to interactions and co-moments. After preprocessing by CEM and EB, average treatment effects on the treated- ATTs- (i.e. potential added workers' response to a partner's acute health shock), are obtained through parametric regression models[9] on the preprocessed data. The estimated equation for producing ATTs on preprocessed data is:

$$y_i = \alpha + \beta treated_i + \gamma X_i + \varepsilon_i$$

where y_i is the outcome of interest of the potential added worker, $treated_i$ is a dummy variable equal to one if partner experienced an acute health shock between time $t-1$ and time t. Zero otherwise. β is the average treatment effect on the treated (ATT). X_i are additional control variables.

1.3.2 Implementation

The sample for analysis is restricted to couples where both partners are observed, and are cohabiting, for at least two points in time, *t* and $t-1$, which could correspond to any two interviews across the nine waves available. In the vast majority of cases (91.18%) these are two

[9] Depending on the distribution of the outcome to obtain the average treatment effects on the treated (ATT), we implement probit or OLS.

consecutive waves[10]. Also, we select couples where at least one partner (the potential added worker) is aged below the gender-specific state pension age, regardless of whether employed or not. After discarding couples with missing information on relevant variables, the number of couples in our sample is 49,207.

Treatment assignment operates dynamically, and at the level of the couple, accounting for each partner's history of health shocks. In more detail, all couples begin as untreated in the first wave they are interviewed. At any later wave, a couple is assigned to the treatment group if at least one acute health shock is observed for one partner (the shocked partner), and the other partner is under the state pension age (the potential added worker). The wave that the shock occurs is considered as time t , where outcome measurement begins.

For treated couples where multiple health shocks are observed (possibly to both partners), we consider only the first shock recorded in the UHKLS observational window, and recode their treatment status to missing in any following wave. Couples where a health shock is observed, but the partner is older than the state pension age as of time t, are also discarded. We further drop couples where both partners experience a contemporaneous health shock (3 cases) and couples where the two respective health shocks happen in immediately consecutive years (8 cases). In total, we observe 484 unique couples assigned to the treatment group.

The potential control group includes all couples where no shock is ever observed during the UHKLS observational window, as long as one partner is aged below the gender-specific state pension age. Treated couples never serve as controls. After dropping couples with missing information on relevant variables, there are 48,723 potential control couples, that is, approximately 100 couples on average for each treated couple. It is important to stress that while a treated couple is used only as such, and only once (in t, the year of reported shock for one partner in the couple), a potential control couple could be used to form the counterfactual for multiple treated couples.

Table 1 reports descriptive statistics for the treated and potential control sub-samples, showing that characteristics are highly unbalanced. In terms of potentially shocked partner characteristics (top panel of Table 1), partners that actually experience an acute health shock are on average older, less educated, more likely to be (past or present) heavy smokers, less healthy according to a variety of general health and disability indicators, exhibit a higher prevalence of specific CVD risk factors, and have fathers with lower longevity. Considering the potential added worker characteristics (mid panel of Table 1), individuals whose partner experiences an acute health shock are on average older, less educated, less likely to be active in the labour market and more

[10] In a further 6.3% of cases, two waves elapse since the previous interview. So, overall, in 97.5% of cases, either one or two waves elapse since the previous interview.

likely to be providing informal care to their partner. For household level characteristics, significant differences are apparent in household size, equivalent income, probability of social renting and wave of interview (bottom panel of Table 1).

To control for selection bias arising from observables, we first implement CEM to achieve common support and exact matching on AW-gender, labour market activity and informal care provision as of $t-1$; as well as on (potentially shocked) partners' gender and diagnosis of a CVD risk factor[11]. On top of these binary variables, CEM includes (potentially shocked) partner's age, as a key predictor of risk of health shock, coarsened into five bands (with cut-offs at age 28, 43, 58 and 73 years). These variables were selected based on known risk factors; or because they are key predictors of the AW time allocation decision. Importantly, exact matching on AW's lagged outcomes (in terms of extensive margins) contributes to removing bias from time-invariant unobservables.

CEM stratifies treated and potential control couples into 142 strata, and retains only the couples found in a subset of 77 strata where at least one treated and one potential control couple are found. This corresponds to discarding from further analysis 2 treated couples, and 1,239 control couples, as shown in Table 2. In each matched stratum, the number of treatment couples is systematically lower than the number of potential control couples. CEM weights account for this while maintaining exact matching on the relevant binary variables, and on the coarsened age groups.

Table 1: Descriptive statistics

	Treatment couples (n=484)		*Potential controls couples* (n=48,723)		*Pval (diff)*
Shocked/non-shocked partner	*Mean*	*s.d.*	*Mean*	*s.d.*	
partner's age	50.28	9.51	42.11	11.54	0.000
partner' gender: male	0.48	0.50	0.47	0.50	0.431
partner's race: white	0.87	0.34	0.81	0.39	0.001
partner's education	3.57	1.79	4.25	1.63	0.000
partner's LM participation (t-1)	0.57	0.50	0.79	0.40	0.000
partner's father dead when aged14	0.06	0.25	0.03	0.17	0.000
partner's mother dead when aged14	0.01	0.11	0.01	0.11	0.779
partner's natural children (t-1)	2.02	1.60	1.66	1.39	0.000
partner's current smoker	0.26	0.44	0.20	0.40	0.001
partner's regular smoker past	0.26	0.44	0.21	0.40	0.003
partner's heavy_smoker (current/past)	0.14	0.35	0.07	0.26	0.000
partner's number of limitations (t-1)	0.46	1.13	0.20	0.70	0.000

[11] Any previous diagnoses of high blood pressure, diabetes, congestive heart failure, coronary heart disease or angina.

partner's long standing illness/disability (t-1)	0.40	0.49	0.23	0.42	0.000
partner's shock (t-1)	0.22	0.41	0.03	0.17	0.000
partner's risk (t-1)	0.44	0.50	0.20	0.40	0.000
Potential added worker					
AW age	53.22	8.19	46.65	9.57	0.000
AW male	0.49	0.50	0.52	0.50	0.162
AW education	3.65	1.75	4.26	1.62	0.000
AW labour market participation (t-1)	0.65	0.48	0.81	0.39	0.000
AW hours of work (t-1)	23.38	20.19	30.29	19.26	0.000
AW hours of work (t-1), conditional	36.05	13.08	37.45	13.80	0.073
AW provides informal care to partner (t-1)	0.15	0.35	0.03	0.16	0.000
AW hours of care (t-1)	6.15	18.68	2.05	11.75	0.000
AW hours of care (t-1), conditional	32	31.55	34.19	34.75	0.549
Couple level characteristics					
household size (t-1)	3.13	1.36	3.51	1.29	0.000
household equivalent income (t-1)	2106	1406	2359	1461	0.000
home Tenure: social renter	0.16	0.37	0.09	0.30	0.000
home Tenure: homeowner	0.78	0.41	0.82	0.39	0.054
elapsed months between t and (t-1)	12.68	0.14	12.33	0.01	0.001
wave (t)	4.73	2.32	5.06	2.27	0.002

Source: UKHLS, waves 1-9. Note: Variables in bold if t-test of equality of means between treated and controls rejected at the conventional 5% level.

Table 2: Outcomes of Coarsened Exact Matching

	#treated	#controls	*by stratum:*	#treated	#controls
All	484	48,723	mean	6.26	616.68
Matched	482	47,484	median	3	153
Unmatched	2	1,239	min	1	4
			10th perc.	1	13
			25th	1	32
			75th	8	388
			90th	14	1,456
			max	52	6,875

Source: UKHLS, waves 1-9.

EB aims at balancing (in terms of means) the univariate distribution of all remaining potential confounders, as listed in Table 1, along with the (potentially shocked) partner's exact age, rather than relying solely on CEM. We further include in the EB minimization function the first order interactions between each variable and each of the binary variables included in CEM to balance co-moments. For continuous variables, we include in the EB specification quadratic and cubic terms, so that even if the EB distance minimization targets only the first moments of included variables, in practice balancing extends to the second and third moments (Hainmueller and Xu 2013). Table 3 reports the mean differences between matched couples, and the standardized difference in means or percentage bias, which are systematically lower than 1.5%.

Table 3: Balancing of observables

	Mean difference		*Bias*	
Shocked/non shocked partner	*Unbalanced*	*Balanced*	*Unbalanced*	*Balanced*
partner's age	8.622	0.000	88.9	0
partner' gender: male	0.039	-0.002	7.8	-0.3
partner's race: white	0.057	0.000	15.7	0.1
partner's education	-0.6791	-0.002	-39.7	-0.1
partner's labour market participation (t-1)	-0.223	0.000	-49.3	-0.3
partner's father dead when aged14	0.021	0.001	10.4	0.9
partner's mother dead when aged14	-0.087	-0.008	2.2	-0.0
partner's natural children (t-1)	0.362	-0.005	24.1	-0.6
partner's current smoker	0.064	0.002	17.7	0.4
partner's regular smoker past	0.079	0.000	18.2	0.2
partner's heavy_smoker (current/past)	0.092	0.001	27.2	0.4
partner's number of limitations (t-1)	0.754	0.016	48.4	1.0
partner's long standing illness/disability (t-1)	0.262	0.001	55.1	0.4
partner's shock (t-1)	0.186	0.003	58.8	1.0
partner's risk (t-1)	0.247	0.001	54.8	0.3
Potential Added Worker				
AW age	6.563	-0.061	73.7	-0.7
AW male	-0.032	0.006	-6.4	1.1
AW education	-0.607	-0.010	-35.8	-0.6
AW labour market partecipation (t-1)	-0.160	-0.004	-36.5	-1.0
AW hours of work (t-1)	-6.898	-0.025	-35.0	-0.1
AW provides informal care to partner (t-1)	0.119	0.004	43.2	1.3
AW hours of care (t-1)	4.102	0.037	26.3	0.2
Couple level characteristics				
household size (t-1)	-0.435	-0.006	-33.5	-0.5
household equivalent income (t-1)	-252.8	1.1	-17.6	0.1
home tenure: social renter	0.069	0.003	20.8	1.0
home tenure: homeowner	-0.034	-0.003	-8.5	-0.8
elapsed months (t)	0.359	-0.003	13.0	-0.1
wave (t)	-0.328	-0.003	-14.3	-0.1

Source: UKHLS, waves 1-9.

1.4 Results

Table 4 reports, for each outcome, the estimated ATT and relative size effect (RSE[12]) at time *t* (top panel) and at *t+1* (bottom panel). In both cases, no significant adjustment in the (potential AW) partner's labour supply emerges, neither along the extensive nor intensive margins. However partners significantly increase their involvement in informal care provided to the shocked spouses[13]. The ATT amounts to a 14 percentage point increase in the probability of providing informal care in the year of the shock, which is double the counterfactual probability. This effect persists in the following year, although halved in size (7.5 percentage point increase) to a 57% increase in the counterfactual probability. The expected number of hours of informal care also increases, particularly in the year of the shock, by about 3.5 hours a week, which is a 50% increase on the counterfactual average. However, conditional on providing informal care, no significant increase in hours is registered, suggesting that the effect on the unconditional number of hours reflects an adjustment on the extensive margin.

Table 5 shows estimated ATTs and RSE on the same outcomes, but measured at later points in time, i.e. *t+2*, *t+3* and *t+4*. Expanding the post-shock time horizon offers an indication of the dynamic pattern of response, which is displayed in Figure 2. However, these estimates, obtained on progressively reduced samples suffer from a lack of precision. They are also possibly biased by non-random attrition as treated couples are more likely to leave the panel, leading to a downward bias in estimated ATTs over time. Bearing this limitation in mind, estimates reported in Table 5 suggest that the results obtained in the very short term, in terms of lack of a labour supply response and increase in informal care, do show some persistence.

[12] The ATT expressed as a percentage of the contemporary average counterfactual outcome measured in the matched control sample.

[13] A similar increase occurs when including other household members, together with the shocked partner, suggesting that the bulk of additional informal care is devoted to the partner.

Table 4: ATT in the short run, full sample

	n (treated)	n (controls)	ATT	Std. Err.	P val	RSE
Potential AW outcome, as of t						
Labour market participation	481	47,449	-0.002	0.016	0.898	-0.003
Hours, unconditional on LMP	478	47,035	0.091	0.680	0.893	0.004
Hours, conditional on LMP	300	37,802	0.121	0.727	0.868	0.003
Informal care provision to partner	481	47,460	**0.137**	0.030	0.000	1.015
Hours of care, unconditional on providing care	478	47,427	**3.443**	1.152	0.003	0.525
Hours of care, conditional on providing care	132	2,764	6.483	4.902	0.188	0.172
Potential AW outcome, as of t+1						
Labour market participation	408	39,492	-0.027	0.022	0.228	-0.044
Hours, unconditional on LMP	404	39,080	-0.996	0.897	0.267	-0.046
Hours, conditional on LMP	259	31,551	-1.169	1.214	0.336	-0.035
Informal care provision to partner	399	39,338	**0.075**	0.026	0.005	0.573
Hours of care, unconditional on providing care	399	39,304	2.225	1.277	0.082	0.362
Hours of care, conditional on providing care	112	2,229	0.099	5.341	0.985	0.003

Source: UKHLS, waves 1-9.

Notes: ATT estimate in bold if significant at the conventional 5% level.

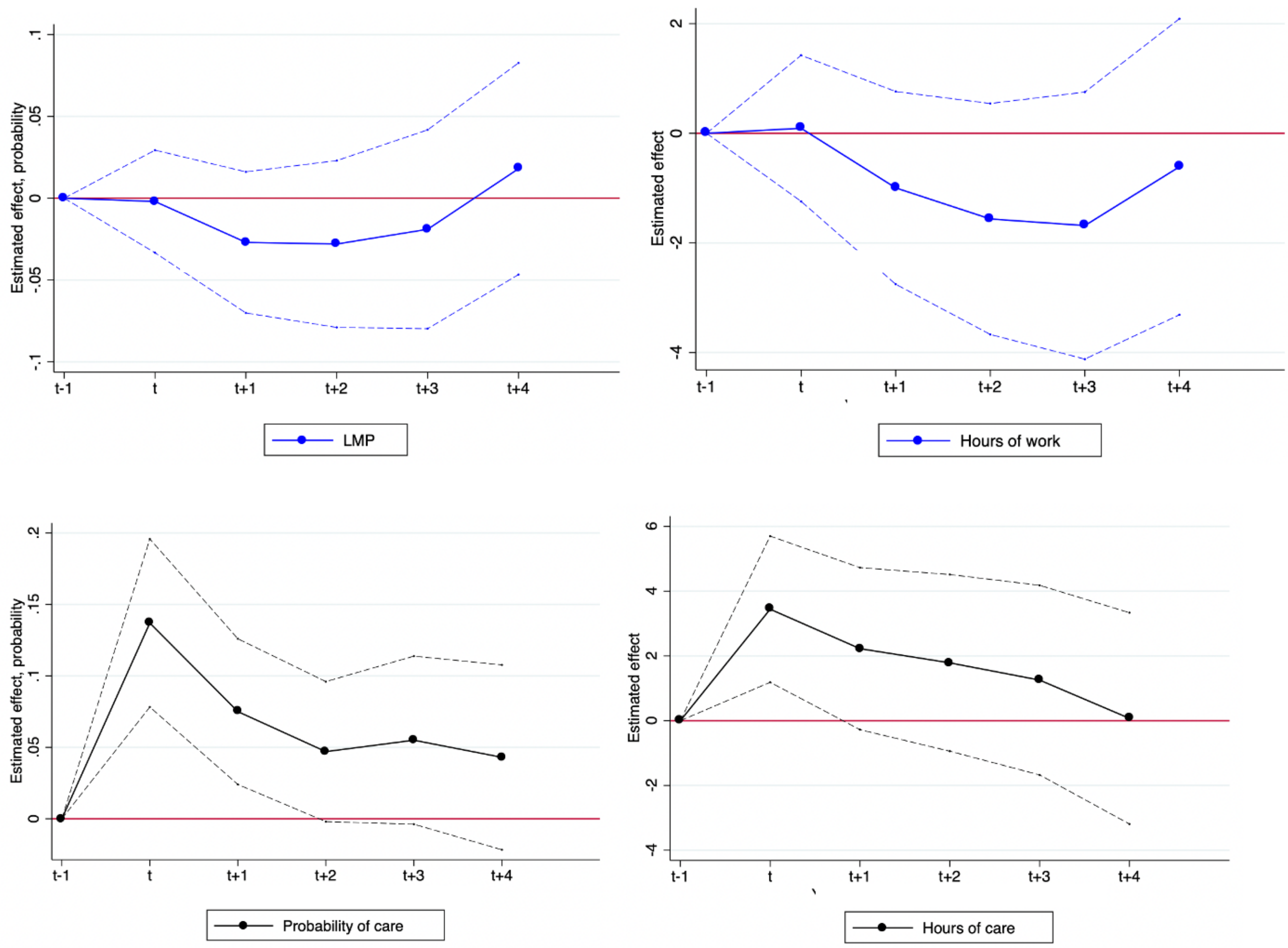
Table 5: ATT in later years, full sample

	n (treated)	n (controls)	ATT	Std. Err.	P val	RSE
Potential AW outcome, as of t+2						
Labour market participation	336	32,237	-0.028	0.026	0.287	-0.047
Hours, unconditional on LMP	333	31,869	-1.563	1.075	0.147	-0.076
Hours, conditional on LMP	213	25,787	-1.791	1.490	0.230	-0.057
Informal care provision to partner	321	31,977	0.047	0.025	0.057	0.370
Hours of care, unconditional on providing care	318	31,943	1.786	1.392	0.200	0.312
Hours of care, conditional on providing care	86	1,757	2.685	6.638	0.687	0.092
Potential AW outcome, as of t+3						
Labour market participation	271	25,549	-0.019	0.031	0.543	-0.033
Hours, unconditional on LMP	268	25,192	-1.684	1.243	0.176	-0.086
Hours, conditional on LMP	174	20,401	-1.906	1.754	0.278	-0.065
Informal care provision to partner	254	25,266	0.055	0.030	0.071	0.455
Hours of care, unconditional on providing care	252	25,241	1.494	1.277	0.402	0.228
Hours of care, conditional on providing care	64	1,377	7.369	5.341	0.930	0.023
Potential AW outcome, as of t+4						
Labour market participation	215	19,121	0.018	0.033	0.589	0.033
Hours, unconditional on LMP	211	18,814	-0.611	1.378	0.658	-0.033
Hours, conditional on LMP	136	15,232	-0.348	1.964	0.860	-0.013
Informal care provision to partner	200	18,849	0.043	0.033	0.202	0.355
Hours of care, unconditional on providing care	199	18,823	0.072	1.665	0.966	0.012
Hours of care, conditional on providing care	52	988	-2.786	8.759	0.752	-0.090

Source: UKHLS, waves 1-9.

Notes: ATT estimate in bold if significant at the conventional 5% level.

Figure 2: Behavioural response (ATT) to a partners' health shock



1.4.1 Health shocks while active in the labour market

The lack of a positive health-related AWE might be attributable to the income loss following a health shock being of limited relevance. For example, if the shocked partner had already retired from the labour market or was relying on non-labour income sources. To investigate this possibility, we consider a restricted subset of couples where the shocked partner was active in the labour market in the year prior to the shock (i.e. in $t-1$). Descriptive statistics for basic demographics and lagged outcomes in this subsample are reported in Appendix Table A.1. These reveal how these potential AWs are, on average, slightly younger, and more likely to be women. Table 6 reports ATTs for this subsample. While health shocks induce a significant increase in labour market exits for shocked individuals, together with a consequent income loss[14], even in this sub-sample no AWE is detected. In fact, the point estimates on labour supply outcomes becomes negative in the year following the shock. Evidence suggesting that the loss of household labour income following a health shock does not result in a positive AWE also emerges when we further restrict the sample to couples where, in the year prior to the shock, the shocked partner's labour income contributed more than 50% of household income (results reported in Appendix, Table A.2).

As in the full sample, we find a striking behavioural response in informal care provision in the year of shock: the ATT on the likelihood of providing informal care is 7.4 times the counterfactual value (reduced to 2.8 in the following year). The significant ATT on the (unconditional) number of hours of care provided amounts to more than a doubling of the counterfactual value in the year of shock, but loses statistical significance in the following year. Again, this behavioural response relates to the extensive margin rather than the conditional number of hours of care provided.

[14] The ATTs obtained for the labour market participation of the shocked partner, not reported here, are in line (3 to 4 per cent reduction in LMP in the first year past shock occurrence) with evidence from Jones at al. (2020) who, using the same data and methodological approach, report a 7 per cent reduction in the shocked individual's earnings.

Table 6: ATT in the short run, if shocked partner was labour market active as of (t-1)

	n (treated)	n (controls)	ATT	Std. Err.	P val	RSE
Potential AW's outcome, as of t						
Labour market participation	280	38,660	0.006	0.019	0.759	0.007
Hours, unconditional on LMP	277	37,484	0.417	0.951	0.661	0.014
Hours, conditional on LMP	224	31,931	0.244	0.839	0.772	0.007
Informal care provision to partner	280	37,836	**0.215**	0.054	0.000	7.414
Hours of care, unconditional on providing care	280	37,824	**2.263**	0.930	0.015	1.358
Hours of care, conditional on providing care	47	1,373	10.841	10.466	0.309	0.414
Potential AW's outcome, as of t+1						
Labour market participation	236	31,606	-0.036	0.032	0.261	-0.046
Hours, unconditional on LMP	232	31,255	-1.332	1.245	0.285	-0.048
Hours, conditional on LMP	191	26,743	-1.340	1.341	0.318	-0.040
Informal care provision to partner	234	31,489	**0.083**	2.140	0.032	2.862
Hours of care, unconditional on providing care	234	31,471	2.049	1.167	0.080	1.298
Hours of care, conditional on providing care	39	1,139	7.921	14.974	0.603	0.417

Source: UKHLS, waves 1-9.
Notes: ATT estimate in bold if significant at the conventional 5% level.

1.4.2 Gender effects and shock-induced disability.

Table 7 and Figure 3, report results separately for men and women whose partner experienced an acute health shock[15]. Neither men nor women adjust their labour supply in the year of shock or the following year. While the ATTs are never statistically significant, the point estimate for women, who may be vulnerable to larger income losses when the male partner experiences a health shock, is systematically negative, suggesting that any income effect, which would induce an increase in labour supply, is outweighed by other factors. Indeed, both women and men significantly increase their informal care provision when their partner experiences a health shock. In the year of the shock this amounts to a 60% increase in the probability of caring for women and more than doubles (150%) for men who have lower baseline probabilities of caring than women (13.5% for men and 15.7% for women). In the following year (i.e. t+1), the increase in informal care provision

[15] Descriptive statistics for gender-specific lagged outcomes are reported in the Appendix, Table A.3.

persists for women in both statistical significance and magnitude, but loses statistical significance for men.

Table 8 and Figure 4, report results separately for individuals whose shocked partner does experience an increase in functional limitations (ADLs) when the health deterioration occurs, and for individuals whose partner does not. The remarkable gradient visible in the informal care adjustment, by shocked partner's increase in disability (number of functional limitations) documents the central role partners play as informal care providers, when that need arises. A lack of labour supply adjustment is common across the two subgroups of couples. Such evidence suggests that beyond informal care needs other mechanisms act as counterweights to the income effect that would otherwise increase labour supply.

Table 7: ATT in the short run, by potential AW's gender

	Male						Female					
Potential AW's outcome as of t	n (treat)	n (contr)	ATT	Std. Err.	P val	RSE	n (treat)	n (contr)	ATT	Std. Err.	P val	RSE
Labour market participation	233	24,533	0.0003	0.022	0.990	0.000	248	22,916	-0.009	0.022	0.650	-0.015
Hours, unconditional on LMP	232	24,325	0.206	1.135	0.856	0.008	246	22,710	-0.301	0.807	0.710	-0.016
Hours, conditional on LMP	155	20,750	0.308	1.106	0.781	0.008	145	17,052	-0.179	0.992	0.857	-0.006
Informal care provision to partner	232	24,540	**0.183**	0.049	0.000	1.578	248	22,920	**0.094**	0.035	0.008	0.610
Hours of care, unconditional on providing care	231	24,522	2.926	1.503	0.052	0.511	247	22,905	**3.516**	1.781	0.049	0.480
Hours of care, conditional on providing care	62	1,281	8.383	7.360	0.259	0.221	70	1,483	5.656	7.613	0.460	0.151
Potential AW's outcome as of t+1												
Labour market participation	201	20,269	-0.019	0.031	0.543	-0.030	207	19,223	-0.030	0.031	0.324	-0.051
Hours, unconditional on LMP	197	20,075	-1.132	1.483	0.446	-0.045	207	19,005	-1.135	1.092	0.299	-0.067
Hours, conditional on LMP	138	17,167	-1.294	1.881	0.492	-0.035	121	14,384	-1.019	1.654	0.538	-0.036
Informal care provision to partner	197	20,178	0.042	0.034	0.220	0.356	200	19,160	**0.094**	0.039	0.015	0.657
Hours of care, unconditional on providing care	198	20,160	0.261	1.596	0.870	0.047	201	19,144	3.695	2.029	0.069	0.554
Hours of care, conditional on providing care	54	1,011	0.380	8.282	0.964	0.012	58	1,218	1.985	8.346	0.813	0.065

Source: UKHLS, waves 1-9.

Notes: ATT estimate in bold if significant at the conventional 5% level

Figure 3: Behavioural response (ATT) to a partners' health shock, by potential AW's gender

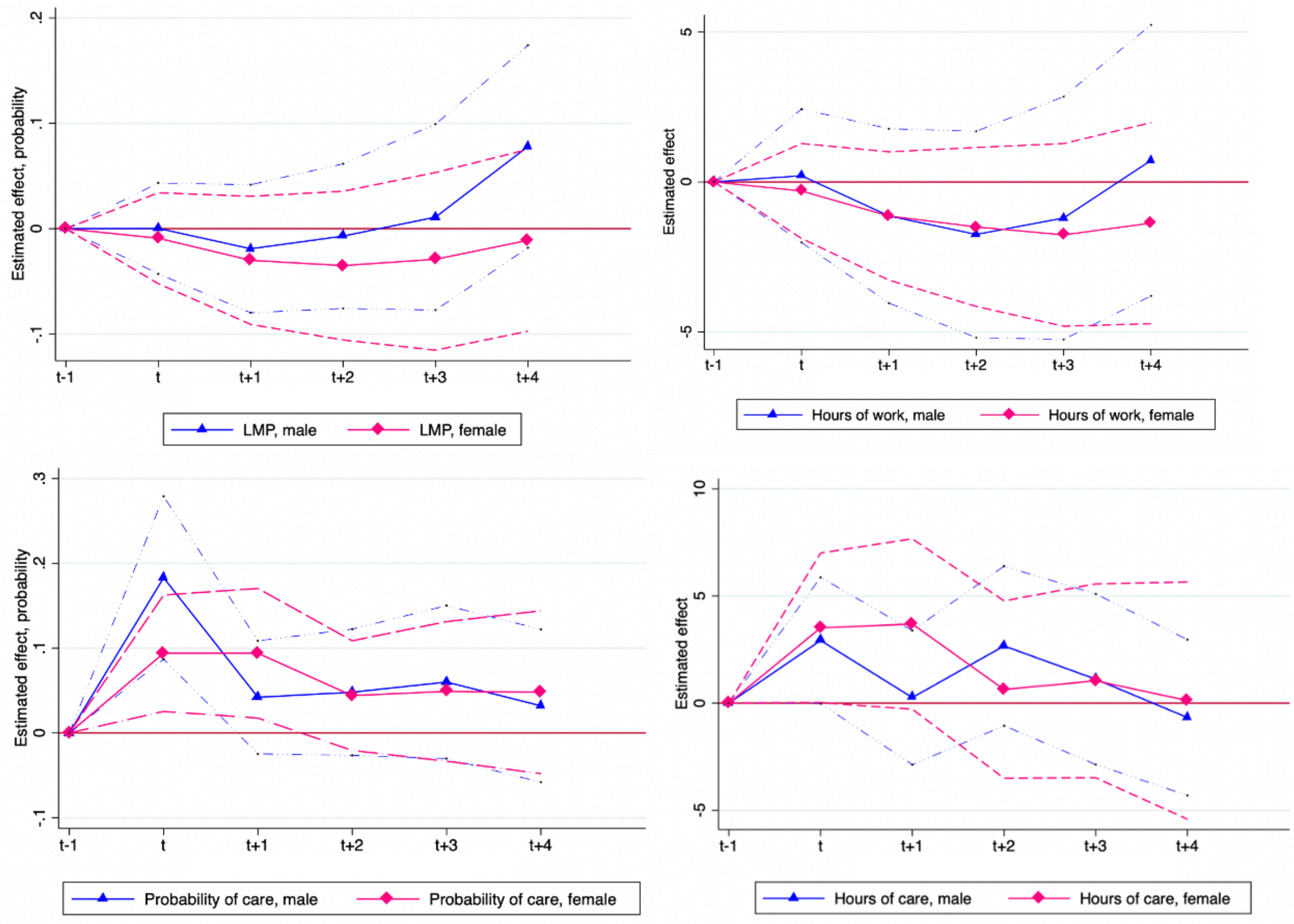


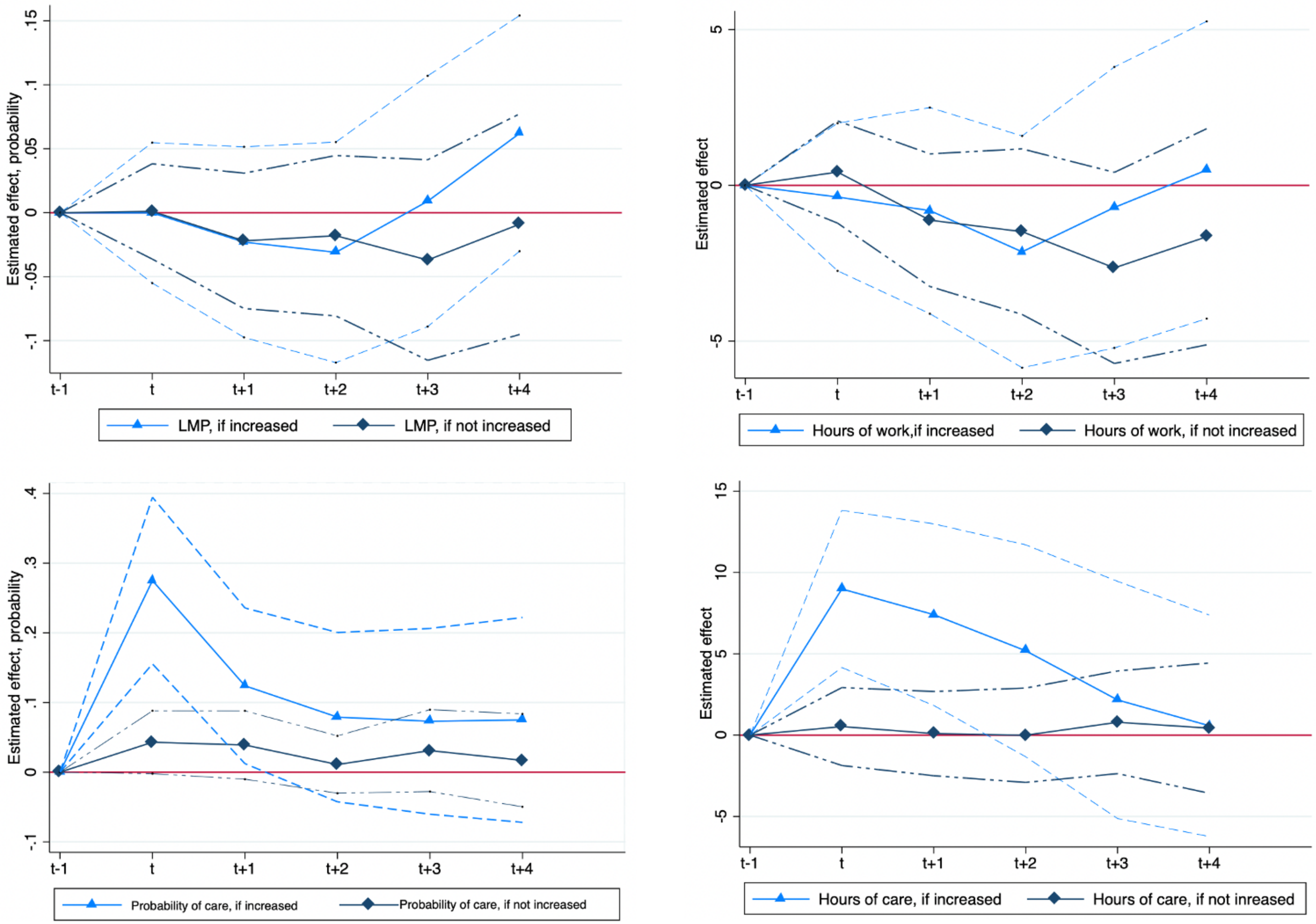
Table 8: ATT in the short run, by increase in shocked partner's number of limitations

	If no increase in reported ADLs						*If increase in reported ADLs*					
After one year (t)	n (treated)	n (controls)	ATT	Std. Err.	P val	RSE	n (treated)	n (controls)	ATT	Std. Err.	P val	RSE
Labour market participation	321	42,862	0.001	0.019	0.956	0.002	159	3,915	-0.000	0.028	0.992	0
Hours, unconditional on LMP	321	42,487	0.427	0.837	0.610	0.018	157	3,879	-0.374	1.209	0.757	-0.017
Hours, conditional on LMP	210	34,656	0.604	0.856	0.481	0.017	89	2,943	-1.009	1.506	0.504	-0.032
Informal care provision to partner	321	42,873	0.043	0.023	0.069	0.344	159	3,916	**0.275**	0.061	0.000	2.254
Hours of care, unconditional on providing care	319	42,850	0.527	1.222	0.666	0.078	158	3,908	**8.987**	2.465	0.000	1.537
Hours of care, conditional on providing care	62	1,986	3.125	7.028	0.658	0.075	69	538	12.240	7.852	0.124	0.372
After two years (t+1)												
Labour market participation	276	35,822	-0.022	0.027	0.407	-0.034	131	3,136	-0.023	0.038	0.550	-0.042
Hours, unconditional on LMP	273	35,440	-1.118	1.086	0.304	-0.050	130	3,107	-0.810	1.688	0.632	-0.042
Hours, conditional on LMP	180	29,015	-0.809	1.414	0.568	-0.024	78	2,378	-2.237	2.659	0.402	-0.070
Informal care provision to partner	273	35,690	0.039	0.025	0.118	0.325	125	3,116	**0.124**	0.057	0.029	0.743
Hours of care, unconditional on providing care	273	35,672	0.092	1.321	0.945	0.015	125	3,103	**7.407**	2.849	0.010	1.311
Hours of care, conditional on providing care	54	1,717	-3.306	7.617	0.666	-0.082	37	414	16.482	11.202	0.153	0.588

Source: UKHLS, waves 1-9.

Notes: ATT estimate in bold if significant at the conventional 5% level.

Figure 4: Behavioural response (ATT) to a partners' health shock, by increase in partner's number of limitations



1.4.3 Placebo checks

Balancing observed confounders does not guarantee against bias arising from additional unobserved confounders, such as changes in risk and time preferences, potentially affecting both health and time use. In order to assess whether our strategy has successfully removed potential sources of bias, we estimate treatment effects for placebo outcomes, i.e. outcomes for which the treatment is expected, *a priori*, to have no effect. This is, for example, the case for lagged outcomes observed at $t-2$, two years before the health shock is reported, as the matching adjustment exploits only $t-1$ outcomes as lagged outcomes. Significant ATTs estimated on outcomes at $t-2$, would signal pre-existing differences in unobservables between treated couples and matched controls. However, results from this placebo test, reported in Table 9, reveal that, following preprocessing, no statistically significant difference in $t-2$ outcomes is detected.

Table 9: Placebo checks: ATT on outcomes measured in $t-2$

	n (treated)	n (controls)	ATT	Std. Err.	P val
Outcomes as of t-2					
Labour market participation	356	38,514	-0.012	0.018	0.499
Hours worked	352	38,210	-0.034	0.832	0.967
Informal care provision to partner	356	38,491	0.004	0.015	0.781
Hours of care provided	355	38,467	1.170	1.112	0.293

Source: UKHLS, waves 1-9.
Notes: ATT estimate in bold if significant at the conventional 5% level.

1.5 Conclusion

Informal insurance within households may protect against the economic consequences of health shocks. However, the literature is ambiguous on the existence of a health-related "Added Worker effect". We contribute to this literature by providing evidence on the within household informal care responses to a health shock of a partner as a mechanism that may counteract income effects that would otherwise increase a partner's labour supply.

We do this by exploiting nine waves of panel data drawn from Understanding Society. Major health events such as heart attacks, strokes and cancers, offer a source of unanticipated variation in the timing of health shocks. We assume the chance that one partner experiences a major health shock at any particular point in time is conditionally random, and match couples where one partner experiences a health shock with observationally identical (in terms of labour, demographic, health, socioeconomic characteristics and lagged outcomes) controls. The matching algorithm combines coarsened exact matching and entropy balancing in a setting that offers a much larger number of control than treated units. ATTs are obtained through parametric modelling on the matched samples. Placebo tests on pre-shock outcomes fail to detect systematic differences between treated and matched control couples - which would have suggested a role for selection bias on unobservables.

Results indicate that, in the case of UK couples where one partner experiences an acute health shock, there is no evidence that, on average, the labour supply of their partner increases. This is in line with the recent findings of Fadlon and Nielsen (2021) in Denmark and Dobkin et al. (2018) in the US. Instead, and although lacking in precision, our point estimates suggest a possible reduction in labour supply, at least in the short run, for both men and women. The loss of labour income, which has been estimated to be around 7% of counterfactual individual earnings for shocked individuals (see Jones et al., 2020), does not result in a corresponding increase in their partners effort to earn labour income, at least in the short run. A plausible explanation for this is the presence of a national healthcare system in the UK, as opposed to an employment-contingent health insurance system, together with the availability of social security coverage in terms of disability-related benefits. Indeed, in related work Jones et al. (2020) detect a spike in disability benefit receipt after major income shocks, with an estimated ATT amounting to twice the baseline counterfactual value of disability benefit coverage.

Potential added workers of both genders display a significant response to their partner's health shock in informal care provision, suggesting that any negative income effect of a health shock is fully offset by care giving. No evidence emerges for behavioural responses driven by gender specialization in labour income production versus home production which would have resulted in asymmetric responses by gender, with women increasing time devoted to paid work, and men increasing time devoted to informal care in the event of partners' health shock. Our results hold whether or not the individual experiencing a health shock is active in the labour market prior to the shock.

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Appendix

Table A.1: Descriptive statistics for AW's age and lagged outcomes,

if shocked partner was labour market active as of (t-1)

	Mean	Std. Dev.
AW's age	51.81	7.82
Hours of work (t-1)	29.93	18.09
Hours of care (t-1)	2.05	10.62
Informal care provision (t-1)	0.05	0.23
Labour market participation (t-1)	0.83	0.38
Partner's age	52.97	7.85

Source: UKHLS, waves 1-9.

Table A.2. ATT, if shocked partner's labour income (t-1) >50% of total household's income

	n (treated)	n (controls)	ATT	Std. Err.	P val	Relative effect
After one year (t)						
Labour market participation	250	35,487	0.005	0.019	0.791	0.006
Hours, unconditional on LMP	247	35,161	0.326	1.012	0.747	0.011
Informal care provision to partner	250	35,492	**0.201**	0.056	0.000	8.739
Hours of care	250	35,481	**2.847**	0.957	0.003	2.421
After two years (t+1)						
Labour market participation	213	29,665	-0.039	0.034	0.254	-0.048
Hours, unconditional on LMP	209	29,344	-1.466	1.334	0.272	-0.050
Informal care provision to partner	211	29,549	0.072	0.039	0.062	3.130
Hours of care	211	29,536	**2.463**	1.176	0.037	2.116

Source: UKHLS, waves 1-9.

Notes: ATT estimate in bold if significant at the conventional 5% level.

Table A.3 Descriptive statistics on potential AW's characteristics, by potential AW's gender

	Male		*Female*	
	Mean	Std. Dev.	Mean	Std. Dev.
AW's age	54.66	8.53	51.98	7.54
Hours of work (t-1)	27.56	21.16	19.49	18.44
Hours of care (t-1)	5.78	18.95	6.43	18.50
Informal care provision (t-1)	0.13	0.33	0.16	0.36
Labour market participation (t-1)	0.69	0.46	0.61	0.49
Partner's age	53.67	9.13	57.32	9.22

Source: UKHLS, waves 1-9

Chapter 2

The power of the (red) *pill* in Europe: pharmaceutical innovation and female empowerment

Annarita Macchioni Giaquinto[16]

Abstract: Birth control is fundamental for gender equality and women's empowerment. Historically, oral contraceptives, most notably the *pill*, transferred from men to women the control on contraception, shifting out the frontier of women's available choices in terms of educational and career planning. This paper uses a quasi-experimental design exploiting the staggered and uncoordinated introduction of the contraceptive *pill* on-demand to young, adult, unmarried women in 14 European countries between the 60s and 80s to explore the causal link between the pharmaceutical innovation of oral contraceptives and further female achievements. Using SHARE data, results show that the *pill* induced a significant and sizable increase in women's educational attainments and labour market outcomes due to higher human capital investments.

Keywords: pharmaceutical innovation, contraception, education, labour market, women's empowerment, gender equality

JEL codes: J13, J16, J18

2.1 Introduction

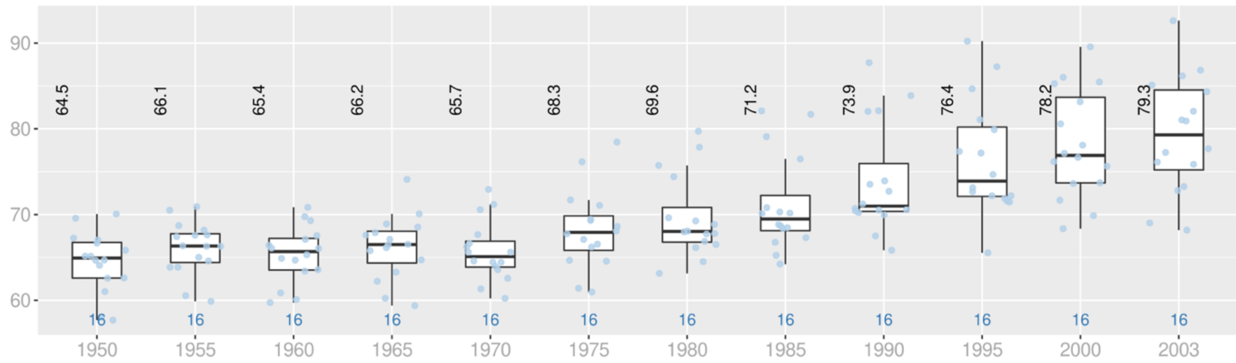
This work explores the causal link between the pharmaceutical innovation aimed at increasing female birth controls rights and female educational and professional outcomes exploiting the

[16] Università Ca' Foscari of Venice, FAO, annarita.macchioni@unive.it

staggered and uncoordinated introduction of the contraceptive *pill* in 14 European countries[17] between 1960 and 1980.

The twentieth-century saw outstanding achievements for gender equality. Interestingly, if we look at the Gender Equality Index[18] From a historical perspective (historical GEI provided in Figure 1), the index increased sharply between the late 1960s and late 2000s, translating into more equality favouring women.

Figure 1: Europe, Historical Gender-Equality Index



Source: Dilli et al., 2019

So far, Western Europe has progressed the most, reaching a gender parity of 77.6% [19]North America is the second-most gender equal with 76.4%, followed by Latin America and the Caribbean (71.2%) and Eastern Europe and Central Asia (71.1%). East Asia and the Pacific region (68.9%) are followed by South Asia (62.3%). Sub-Saharan Africa (67.2%) and the Middle East and North Africa remain countries that are still falling behind, reaching a gender parity of 60.9% only (World Economic Forum, 2020).

Despite this progress in advancing gender equality and the increasing importance posed on the international level, in developing and developed countries women still experience systematic inequality in all domains, including health, education, political representation, and the labour market (World Economic Forum, 2020).

[17] Austria, Germany, Sweden, Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, Poland, Hungary, Portugal

[18] Historical Gender Inequality Index: the composite index aims at evaluating progress in closing the gender gap in health, socioeconomic resources, and politics since 1950. It ranges from 0%, perfect inequality, to 100%, perfect equality.

[19] These percentages are based on the relative gaps closed between women and men across four key areas: health, education, economy, and politics, with 100% describing perfect equality, while 0% describing perfect inequality.

One of the main factors explaining systematic gender gaps is fertility and unequal parenthood impacts on men and women (among others Kleven and Landais, 2017; Waldfogel 1998; Paull 2008; Bertrand et al. 2010; Goldin 2014; Angelov et al. 2016; Kleven et al. 2019). Poor control over the timing and number of childbearing disrupts female achievements. Birth control rights are fundamental for gender equality and women's empowerment.[20], as they can shift out the frontier of available choices for women, providing them with the opportunity to better plan education choices, lowering the costs of college education and professional career.

During the twentieth century, the introduction of reliable contraception, such as the *pill*, radically changed women's ability to control their reproductive lives.

Reliable contraception empowers women as it provides them with a convenient and reliable long-term method, thanks to which they could enjoy additional freedom over crucial life choices independently from their partner.

An increase in female empowerment can also be obtained via the so-called "*expectation effect*" (IWPR, 2019): women and girls tend to invest more in human capital accumulation due to their revised expectations stemming from the possibility of controlling fertility irrespectively from the use of the *pill*.

From the sociological point of view, the *pill* was a real revolution challenging gendered cultural and social norms as entitled women to have sexual intercourses with no reproductive aims. Women could separate their sexual life from their reproductive role and oral contraception allowed couples to have sex without commitment, relying on an effective way to control unplanned childbearing (Goldin and Katz, 2002).

The relative scarcity of empirical evidence on the impact of oral contraception acknowledged birth control as a fundamental mechanism in empowering women but focused exclusively on the United State's context. Looking at educational attainments, Hock (2007) exploits the law that lowered the major age threshold. The latter decreased from 21 to 18 years old hence American women between 18 and 21 years old started having access to the contraceptive *pill* because they were considered legally entitled. Empirical findings show the causal effect of early access to contraceptives on women's college enrolment, which increased by nearly 12 per cent.

This result is confirmed by Bailey et al. (2012) and Ananat and Hungerman (2012) that find evidence of increased human capital investments due to early access to contraception, measured

[20] Empowerment refers to "the processes by which those who have been denied the ability to make choices acquire such an ability" while increasing their agency (Kabeer, 1999, p.24). Women's agency is their ability to define their own goals and act on them (Kabeer, 1999). According to Sen's capability approach, agency is defined as the freedom to achieve whatever he or she decides he or she should achieve (Sen, 1985). Achievements are interrelated with agency because they represent how agency is exercised and its consequences

respectively as college enrolment and levels of education. Also, Edlund and Machado (2015) investigate the causal effect of increased access to contraception for minors on female educational outcomes. The authors exploit a change in marriage laws that allowed minors to access the *pill* and find a sizable and significant positive effect on the female probability of ever attending college, which increased by four percentage points, or 10 per cent.

Further educational attainments translate into labour market comparative advantages, professional and earnings achievements, and better career choices.
Goldin and Katz (2002), focusing on US college-graduate women, find that birth control increases the share of women in professional careers. Bailey (2006) investigates labour market outcomes and finds that early access to contraception in the US caused an increase in labour force participation and hours of work for women. Bailey et al. (2012) find evidence of an increase in wages in the long term. Finally, Browne and LaLumia (2014) find a sizable and significant effect on the probability that a woman experiences poverty.

All these studies focused on the US country, and further investigation on the effect of birth control on female outcomes in the European context is required. Compared to Europe, the US context has significant historical and cultural background differences, as well as profound differences in institutional settings, first and foremost in terms of access to health services and educational opportunities. Assessing the effect of reliable and available birth control contraception on female outcomes is still a crucial topic that requires further attention from policymakers and researchers. Further investigation is needed in both developing and developed countries, particularly among the more disadvantaged sub-groups. New evidence could inform policies and programs on sustaining female empowerment and gender equality through better contraceptive policies. These include increasing access to contraceptives when cultural or information barriers are present, subsidising contraception if required, and a more in-depth understanding and evaluation of its short or long-term benefits.

To the best of my knowledge, the only study existing in the EU context shows that birth control rights, measured as access to both abortion rights and the *pill*, is strongly linked to an increase in life satisfaction of women of childbearing age (Pezzini, 2005). Nevertheless, the effect of birth control on females' labour market and educational achievements lacks extensive analysis.

This paper provides new evidence on the causal effect of the birth control *pill* on EU women's empowerment and achievements. In fact, to the best of my knowledge, none of the previous studies looked at extensive educational outcomes and labour market outcomes in the European context providing a convincing causal relation on the effect of the *pill*.

By exploiting the variation given by the staggered and uncoordinated introduction of the *pill* on-demand to young, adult, unmarried women in 14 European countries between the 1960s and 1980s, in what follows, I identify the link between contraceptive innovation on female achievements for European women. Using the Survey of Health, Ageing and Retirement in Europe (SHARE) for the cross-sectional difference in difference estimation strategy, I find that oral contraception induces a sizable and statistically significant effect. The *pill* increases female outcomes, measured as years of education and college graduation probability, which translates into further labour market comparative advantage, measured as the probability of a professional career.

The remainder of the paper is organised as follows: Section 2 describes the institutional background, while Section 3 presents the data and sample selection. Section 4 describes the estimation strategy, and Section 5 presents the main results. Section 6 provides evidence on placebo analysis, and Section 7 concludes.

2.2 Institutional background

Besides abstinence, historically, men and women have used different contraceptive methods to control childbearing.

The first contraception method was the *coitus interruptus* (Santow, 1995), which has an estimated failure rate between 29 and 62 % (Santow, 1993) and relies on men's discretional choice and ability to withdraw in time. Other contraceptive strategies involve fertility-awareness-based methods, which imply a deep understanding and monitoring of females' fertile days and the menstrual cycle. This method has a failure rate between 4 and 25% during the first year of use (Trussel et al., 2009)

After the Second World War, condoms were introduced in Europe. This tool effectively prevents pregnancy, with a failure rate estimated between 0.4 and 1.6 % (Santow, 1993), even though take-up relies exclusively on the men's side, which has lower incentives compared to women due to potential discomfort during intercourse.

The first oral contraceptive *pill* was invented in the 50s, introduced in 1960 in the US with the name of *Envoid,* but commercialised as long-term contraception starting from 1961. This pharmaceutical innovation revolutionised contraception with a birth control tool based on hormonal methods. The oral contraceptive was welcomed with enthusiasm among women. Since its introduction, over 200 million women have used oral contraception worldwide (Kleinman, 1990). In the US, by 1965, 16 per cent of married women of reproductive age were using the pill. By the late 80s, users increased to four in five women (Dawson, 1990).

The *pill* reached Europe in the next few years (Wharton & Blackburn, 1988). Some European countries enforced legal limitations on pill use during its early introduction. Physicians were allowed to prescribe the *pill* conditional on women's marital status[21] or as a pharmaceutical remedy against amenorrhea or irregular periods. Despite legal limitations, a small proportion of single women might have obtained oral contraception before the law allowed them, for example, by convincing their doctor to have irregular menarche. However, this number is negligible (Goldin and Katz, 2002) as prescriptions needed to be repeated monthly, and doctors needed to be convinced every month. Moreover, to the best of my knowledge, no record exists of a doctor's successful prosecution on providing contraceptive services without legal requirements in Europe for those years.

In the period between the 60s and the early 80s, also European single, adult women started having access to the *pill* on demand: they started getting access to the *pill* irrespectively to their marital status and without any restriction on the reason (Wharton & Blackburn, 1988). In Europe, the *pill* reached the same US consensus: the use rate among women aged 15–44 was up to 40% in Austria, Belgium, Germany, Hungary, and the Netherlands. Instead, lower use rates were found in Mediterranean countries, such as Spain with 16% or Italy with 14% (IARC, 1999).

The Economist defined oral contraception as "the liberator" and the most significant advance in science and technology in the 20th century. The oral contraceptive *pill* was markedly different concerning existing contraception methods. First, the *pill* has an estimated efficacy between 99% and 99.9% (MClure, 1981), depending on whether it is combined estrogen-progestin or progestogen-only. Second, the choice and take-up rely on the female counterpart only and eliminate any discomfort during sexual intercourse. For these reasons, access on-demand to contraceptive *pill* radically changes birth control rights, transferring from men to women the control on contraception (Bailey, 2006) while empowering them.

2.3 Data and sample selection

This paper uses the Survey of Health, Ageing and Retirement in Europe (SHARE), a cross-national longitudinal household panel survey, providing a rich set of information on socioeconomic variables and health status.

Since 2004, the SHARE questionnaire has been administered biannually to individuals aged 50 or older in the year of interview and to their partner (irrespectively form age), who have regular domicile in the respective European SHARE country. SHARE data implement a multi-stage stratified sampling design to ensure representativity of the target population. Each country is partitioned into strata to allow geographical coverage and enhance estimation efficiency, then primary stage units (PSU) are usually selected within these strata with a probability proportional to their size (PPS). Age-eligible individuals and their all-age partners belonging to selected households are interviewed. All respondents in any previous wave are also interviewed in the following ones. Refreshment samples are provided for each wave, allowing younger age-cohorts that were not age-eligible in previous waves to enter the sample and compensate for potential attrition.

Ideally, we would like to rely a comparable longitudinal data that collect information for all women and men in all cohorts before and after the pill on demand in all the 14 European countries, with information on their life cycle oral contraceptive use and access, their marital status, their educational attainments, and life cycle labour career. In the absence of such ideal data, analysis exploit cross-sectional information with a retrospective approach for 14 European countries. I used seven waves available in the SHARE survey to benefit from the longitudinal dimension and obtain the maximum amount of information on time-invariant variables of men and women such as educational attainments, ever worked, the maximum level of career and year of birth.

Since the target population in the SHARE dataset are individuals aged 50 or above in 2004 and onward, this allows selecting young individuals belonging to the age-cohorts of interest in the year of introduction of the pill on demand between 1961 and 1980, depending on the country.

The year of introduction of the pill on demand is defined throughout this paper as year *x*. In the 14 European countries, year *x* is reported in Table 1. For example, in Austria, the oldest cohort of individuals available in SHARE data was born 59.23 years before year *x*, where year *x* is 1962. The youngest cohort was born 54.45 years after year *x*.
Table 9 in Appendix provides the distribution of ages in SHARE data to year *x* for every country.

Table 1: Introduction of the pill to young unmarried women on demand - year x

Country	year *x*
Austria	1962

Germany	1961
Sweden	1964
Netherlands	1964
Spain	1978
Italy	1976
France	1967
Denmark	1966
Greece	1980
Switzerland	1961
Belgium	1968
Poland	1966
Hungary	1968
Portugal	1962

Source: Author's elaboration from different sources.

Depending on the country *j* and on year *x*, I define the two groups of individuals: (i) cohorts of women and men aged 14-18 at year *x* defines **fully exposed** individuals (ii) cohorts of women and men aged 23-27 at year *x* defines **not exposed** individuals.

Table 2 shows sample size after sample selection. I can rely on 11170 not exposed individuals composed of 5830 females and 5340 males, and on 13177 fully exposed, which includes 7073 females and 6104 males.

Further details on sample selection by country are provided in Table 11 in Appendix.

Table 2: Sample selection

	Not exposed	Fully exposed
Females	5830	7073
Males	5340	6104
Observations	11170	13177

Source: SHARE, waves 1-7.

Table 3 provides descriptive statistics for the two sub-groups. On average, exposed individuals are aged 16 at the introduction of the *pill* on-demand in their country, slightly less likely to be ever married and ever have a child, more educated with a higher probability of college graduation, and more likely to have worked and to have a professional career. [22]

Descriptive by gender are provided in Appendix (Table 10).

Table 3: Descriptive statistics

	(1) Not exposed	(2) Fully Exposed	(3) Diff (1)-(2)
Age at year *x*	24.92	16.05	8.87***
Migrant before 23	0.04	0.04	0.04
Migrant before 19	0.03	0.03	0.03
Ever married	0.95	0.93	0.02***
Never child	0.10	0.11	-0.01**
Ever worked	0.93	0.96	-0.03***
Professional	0.09	0.12	-0.03***
Years education	10.35	11.24	-0.89***
College graduate	0.20	0.25	-0.05***
Female	0.52	0.54	-0.01*
Observations	11170	13177	24347

Source: SHARE, waves 1-7. Note: Variables in bold if difference in mean statistically significant at 95% CI
** $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

2.4 Empirical strategy

Disentangling causal effects from other socioeconomic factors that may drive both contraceptive access or use and female outcomes is challenging. The same predictors of better female achievements could predict higher contraception access or use. Endogeneity could seriously challenge our research question, for example, due to selection on unobservables or in terms of reverse causality.

To address potential endogeneity concerns, I use a credible external source of variation of access to the pill on demand to identify the causal effect of birth control on female outcomes. In Europe, the contraceptive pill became available on-demand to unmarried adult women at different times to different cohorts of women, with uncoordinated laws varying by country and date of implementation. Variation in timing and by country allows comparing birth cohorts of women

[22] A professional job is defined as the maximum level of career achieved. It is 1 if the maximum level achieved is Legislator, senior official or manager or Professional, 0 otherwise.

versus men across and within country. This quasi-experimental design allows isolating the causal effects of birth control through oral contraception on women's educational outcomes and how this translates into labour market comparative advantages.

In Europe, by age 23, most individuals should have been out of college education. Individuals aged 23 or older in year x, where year *x* is the year of introduction of the pill on-demand in country *j*, are defined as **not exposed**. For them, the effect of the opening of the pill should be close to zero in terms of further educational attainments, irrespectively of gender. In particular, they should not have benefited from this birth control innovation since they should have left college before the introduction [23].

For younger cohorts, the effect should be an increasing function of their year of birth. Younger cohorts are defined as **fully exposed**: individuals aged 18 or younger in year x, where year *x* is the year of introduction of the *pill* on-demand in country *j*.

Country *j* [24], year of birth *k* and gender jointly determine an individual's access to the birth control *pill* on-demand and define whether individual *i* is **not exposed** to the *pill* or **fully exposed**. In this way, she or he cannot self-select.

The estimation strategy relies on comparing the differences in outcomes between females and males in two age groups. Males are directly comparable because, at the same age when the *pill* is introduced, they have not benefitted directly from its potential use[25]. Using males as controls might raise issues concerning the parallel trend assumption required for causal identification, as women's educational attainment and labour market outcomes might show differences in trends already in the pre-*pill* period. To support the identification strategy exploited in this work, i.e. in the absence of the treatment, outcomes of interest trends would have been the same in both males and females, I provide graphical evidence. Figure 2 shows parallel pre-trends between males and females before the *pill* introduction in the outcome of interest. [26]

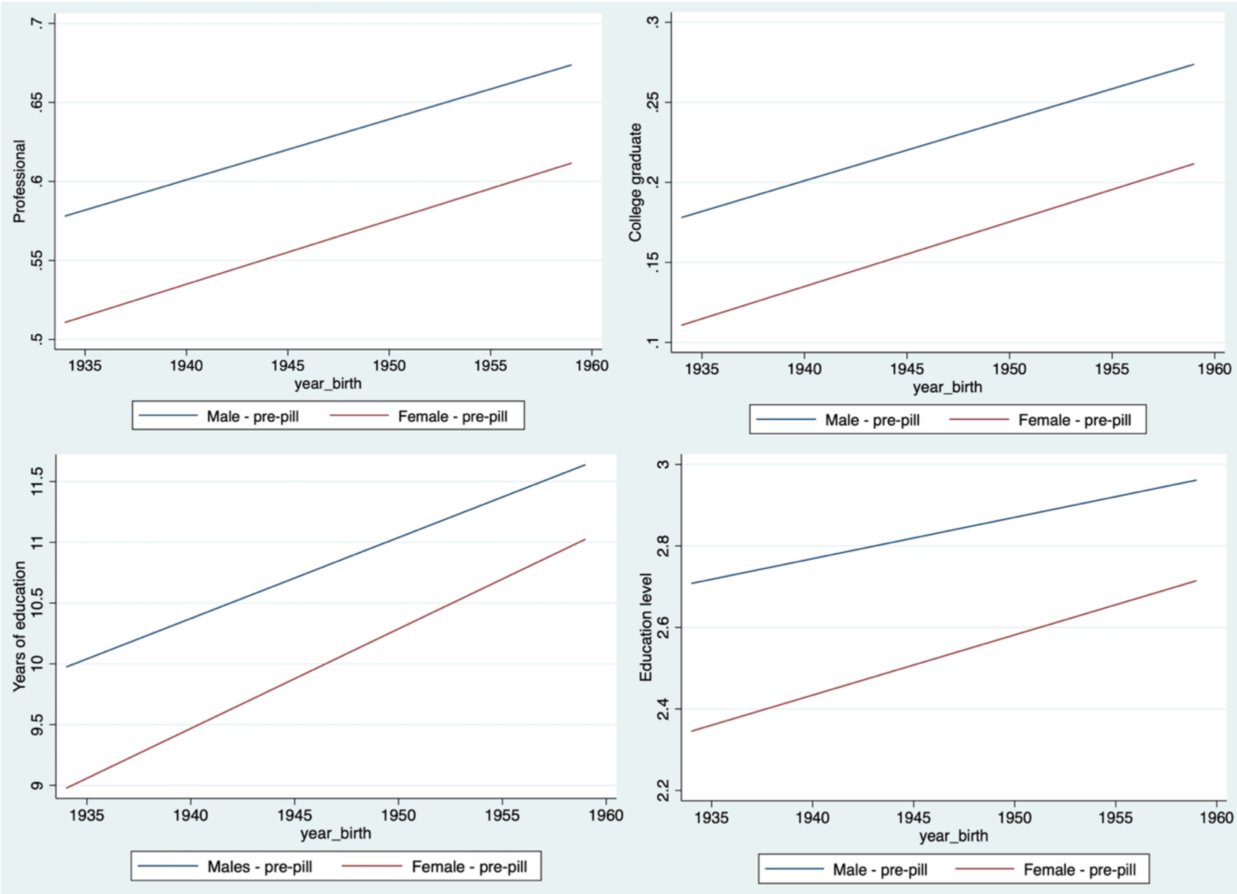
[23] Later graduation and delayed college start could lead a few individuals **not exposed** to potentially benefit from access to the pill in their last years of college. However, this represents a negligible share of individuals in all countries (with percentage varying between 5 and 10% of the overall sample). This choice represents a conservative approach, as in this case the estimate effect of the pill on the demand is downward bias.

[24] Country of residence *j* is highly correlated with country of birth; in the sample, only 2.72% of individuals migrate before 23. For this reason, I can rely on the fact that the country of residence is not endogenous with respect to the pill use and access, which would induce bias in the pill's effect (Duflo 2001; Rosenzweig and Wolpin, 1988).

[25] Even though using male as control might result in downward bias our results due to potential spillover effects.

[26] One caveat in showing pre-parallel trends in my setting is that given that we have multiple countries and multiple cohorts belonging to the pre-pill period, the visual inspection of pre-parallel trends between treated and control might be difficult to inspect (Pischke, 2005).

Figure 2: Male and female trends in the outcome of interest before the pill introduction



Source: SHARE, waves 1-7.

The two age-groups objects of the comparisons are defined as: **fully exposed**, composed by individuals aged 14-18 in year *x* -where year *x* is the year in which the *pill* was introduced to young, adult unmarried women on demand- and **not exposed**, composed by individuals aged 23-27 in year *x*.

Figure 3 clarify the basic idea of the research design: since year *x* varies across countries, **fully exposed** individuals belong to the years of birth included in the interval defined by (*x*- 14) and (*x*-18) cohorts, while **not exposed** individuals belong to the interval between (*x*- 23) and (*x*-27) cohorts. Cohorts aged 19-22 in year *x* are excluded from the analysis to allow a clear assignment to not exposed or fully exposed, as their college career was ongoing during those years.

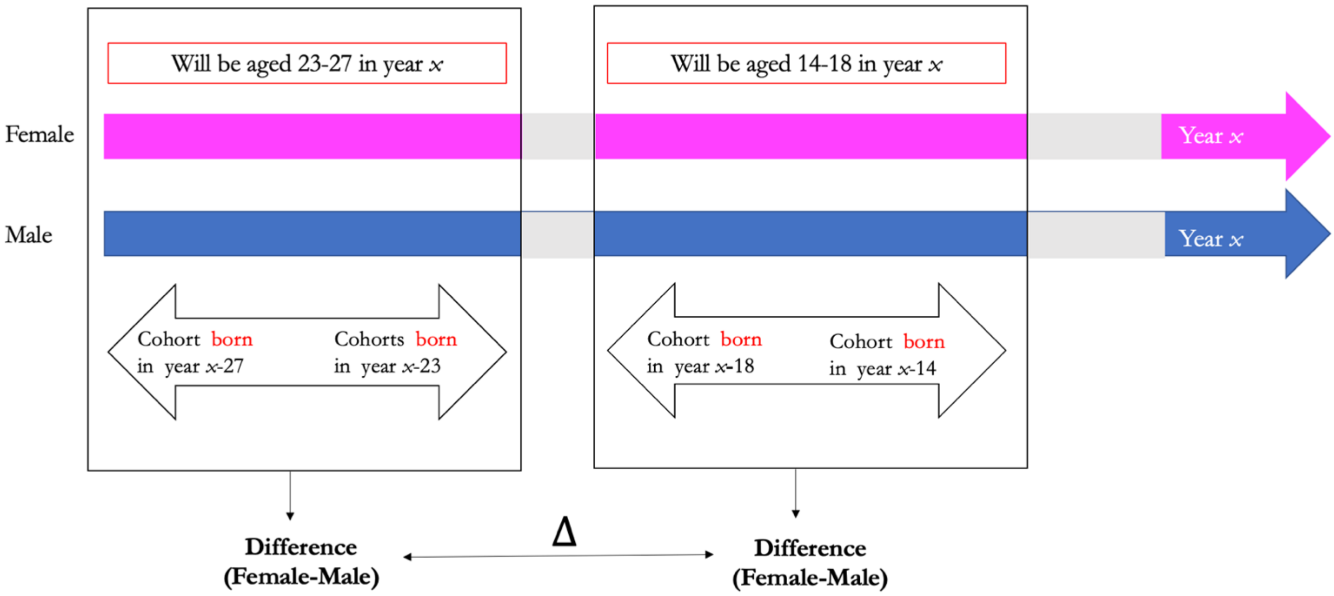
Estimating equation is defined as:

$$Y_{ijk} = c + a_j + b_k + dP_{jk}T_i + eT_i + fP_{jk} + \varepsilon_{ijk}$$

Y_{ijk} is the outcome of interest of individual *i* resident in country *j* born in year *k*, T_i is a dummy for being female, $P_{jk}$is a dummy and denotes whether, in country *j*, year of birth *k* belongs to the younger cohort (fully exposed), c is a constant, a_j is country fixed effect, b_k is year of birth fixed effect.

Our coefficient of interest is d, which captures the difference in the differences in the outcomes of interest and could be interpreted as the causal effect of the birth control innovation, under the assumption that in the absence of the *pill*, the increase in the outcome of interest would not have been systematically different between females and males. represents the intention to treat (ITT) as it is the estimate of the effect of offering the pill, regardless of their take-up.

Figure 3: Research design



2.5 Main results

2.5.1 Educational attainments

In what follows, I investigate whether having access to the *pill* on demand (being fully exposed) is translated into educational achievements, measured as years of education, probability of college degree and educational level [27]. Depending on the distribution of outcome of interest, I implement an OLS, probit or ordered probit model. All specifications include year fixed effects and year of birth fixed effects to account for time-invariant heterogeneity across countries and years of birth.

Table 4 reports the estimates of the *pill*'s effect on demand on years of education. Column 1 shows that being female reduces years of education by 0.744 years, statistically significant at 99% Confidence Interval (CI). However, being a female with access to the *pill* on demand produces a sizable and significant reduction of this gap by 0.275 years.
This result relies on the identification assumption that no-omitted time-varying heterogeneity is correlated to the policy introduction, such as abortion law or other social trends. The identification assumption might hold after controlling for these factors.
Thus, to account for time-varying unobservables potentially correlated with the *pill* introduction, Columns 2 and 4 include country-year of birth linear trends. In this case, being female reduces years of education by 0.776; however, being female and fully exposed to the *pill* reduces this gap by 0.283 years, confirming a sizable, positive and statistically significant effect on women's years of schooling.
Results are robust, controlling abortion rights as shown in Columns 3 and 4. Abortion right is a treatment indicator equal to one if individual *i* was at least 18 in the year of introduction of the right of abortion in his/her country *j,* zero otherwise.

[27] The dependent variable educational level has seven ordered categories following the International Standard Classification of Education (ISCED):

0	None
1	ISCED-97 code 1 - Primary education
2	ISCED-97 code 2 - Lower secondary education
3	ISCED-97 code 3 - Upper secondary education
4	ISCED-97 code 4 - Post-secondary education, non-tertiary education
5	ISCED-97 code 5 - First stage of tertiary education
6	ISCED-97 code 6 - Second stage of tertiary education

Controlling for country-year of birth linear trends [28] and abortion rights make the estimates higher or unchanged, suggesting that results are not biased upward by potential time-varying heterogeneity, omitted programs or abortion rights effect.

Table 5 provides estimates of the effect of the *pill* on-demand on the probability of college graduation from a probit model.[29] Compared to their male peers, women have a sizable and significant lower probability of being college graduates, namely -7.5 percentage points. Even though, being a female with access to the *pill* on demand produces a sizable and significant reduction of this gap by 3.6 percentage points. Results are robust to different specifications. Columns 2 and 4 include year of birth-country linear trends. In this case, being female reduces the probability of achieving a college degree by 7.5 percentage points, but for females fully exposed, this gap is significantly reduced by 3.7 percentage points. Columns 3 and 4 control for abortion rights. As shown, results are robust to different specifications, and estimation does not present evidence of potential time-varying heterogeneity or omitted programs effects.

Table 6 shows the estimates from an ordered probit when educational attainment is used as dependent variable. The dependent variable educational level has seven ordered categories following the International Standard Classification of Education (ISCED) 1997, ranging from 0 - no education to 6 - second stage of tertiary education. Further details are provided in Appendix. All specifications include country and year of birth fixed effects. Columns 2 and 4 control for time-varying heterogeneity, while columns 3 and 4 control for abortion right on demand. Estimation results are robust to different specifications. Educational level is better (from 0 to 6) for males or for being a female fully exposed to the *pill*. Marginal effects are provided in Table 7. Being a female is associated with lower educational attainments measured as educational level. However, being female but having access to the *pill* is associated with being 0,8 % less likely to be in Educational level 0; 2,6% less likely to have primary education (Educational level 1); 1,3 % less likely to have upper secondary education (Educational level 2). Women fully exposed to the *pill* have 0,7% more probability of an upper secondary education (Educational level 3), 0,3% more probability of post-secondary education (Educational level 4), 3,3% more likely to achieve the first stage of tertiary education (Educational level 5) and 0,3 % more likely to achieve the second stage of tertiary education (Educational level 6).

[28] Results are robust also with country-year of birth squared trends.
[29] Results are robust also with linear probability model.

On average, females experience fewer years of school, a lower probability of college graduation and lower educational level than their male counterparts. However, females belonging to the fully exposed cohorts experience a less deprived situation and observe a reduction of this gender-based gap in human capital investments. Taken together, these findings suggest that the *pill* induced a positive and significant effect on women's educational achievements.

Table 4: Years of education

	(1) Years of education	(2) Years of education	(3) Years of education	(4) Years of education
fully exposed	-0.216	0.0480	-0.203	0.0460
	(0.200)	(0.210)	(0.200)	(0.22)
female	-0.774^{***}	-0.776^{***}	-0.773^{***}	-0.776^{***}
	(0.070)	(0.070)	(0.070)	(0.070)
female_fullyexp	0.275^{**}	0.283^{**}	0.277^{**}	0.283^{**}
	(0.947)	(0.947)	(0.947)	(0.947)
abortion_law			-0.727^{*}	-0.424
			(0.338)	(0.510)
N	24347	24347	24347	24347
Country fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes
Abortion right	No	No	Yes	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: Probability of college degree

	(1) College probability	(2) College probability	(3) College probability	(4) College probability
fully exposed	-0.031	-0.017	-0.030	-0.017
	(0.022)	(0.024)	(0.022)	(0.023)
female	-0.075^{***}	$- 0.075^{***}$	-0.075^{***}	-0.075^{***}
	(0.008)	(0.008)	(0.008)	(0.008)
female_fullyexp	0.036^{***}	0.037^{***}	0.037^{***}	0.037^{***}
	(0.011)	(0.011)	(0.011)	(0.011)
abortion_law			-0.035	- 0.056
			(0.033)	(0.055)
N	24347	24347	24347	24347
Country fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes
Abortion right	No	No	Yes	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* *$p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

Table 6: Educational level

	(1) Education Level	(2) Education Level	(3) Education Level	(4) Education Level
fully exposed	-0.085	-0.021	-0.088	-0.020
	(0.056)	(0.060)	(0.058)	(0.061)
female	-0.247***	-0.248***	-0.247***	-0.248***
	(0.021)	(0.021)	(0.020)	(0.020)
female_fullyexp	0.128***	0.128***	0.127***	0.128***
	(0.027)	(0.027)	(0.027)	(0.027)
abortion_law			0.141	0.080
			(0.081)	(0.129)
N	24347	24347	24347	24347
Country fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes
Abortion right	No	No	Yes	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
** $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

Table 7: Educational level - marginal effects

	(1)	(2)	(3)	(4)
Educational level 0	-0.008*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)	-0.008*** (0.002)
Educational level 1	-0.026*** (0.005)	-0.026*** (0.005)	-0.026*** (0.005)	-0.026*** (0.005)
Educational level 2	-0.013*** (0.003)	-0.013*** (0.003)	-0.013*** (0.003)	-0.013*** (0.003)
Educational level 3	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)	0.007*** (0.002)
Educational level 4	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)
Educational level 5	0.033*** (0.007)	0.033*** (0.007)	0.033*** (0.007)	0.033*** (0.007)
Educational level 6	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.003*** (0.001)

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.5.2 Labour market comparative advantages

In the following section, I address whether further human capital investments translate into labour market comparative advantages, measured as the probability of a professional career.

Evidence shows a positive and significant effect of the *pill* on getting a professional job[30], conditional on having ever worked (Table 8)[31]. Unconditional estimates for being professional are provided in Appendix: evidence is in line with the conditional estimates (Table 14). Compared to their male counterpart, women have a sizable and significantly lower probability of having a professional career by 6.5 percentage points. However, being a fully exposed female produces a sizable and significant reduction of this gap by 1.9 percentage points. Columns 2 and 4 include year of birth-country linear trends. Columns 3 and 4 also control for abortion rights. As shown, results are robust to different specifications. Estimates are higher or unchanged after controlling for both country-year of birth linear trends and legal abortion on demand, suggesting no upward bias due to potential time-varying heterogeneity or abortion right effect.

On average, females experience a lower probability of ever working and a lower probability of a professional career than their male peers. However, there is a reduction of the gender-based gap for females belonging to the fully exposed cohorts. Taken together, these findings suggest that the *pill* resulted in comparative advantages on the labour market, stemming from higher human capital investments.

[30] A professional job is defined as the maximum level of career achieved. It is 1 if the maximum level achieved is Legislator, senior official or manager or Professional, 0 otherwise.
[31] In what follows, I use a probit model. However, also estimation from a linear probability model yield significant and sizable results

Table 8: Probability of professional career, conditional of ever worked

	(1) Professional	(2) Professional	(3) Professional	(4) Professional
fully exposed	-0.008	-0.000	-0.007	-0.000
	(0.017)	(0.018)	(0.017)	(0.017)
female	-0.065***	-0.065***	-0.065***	-0.065***
	(0.006)	(0.007)	(0.007)	(0.006)
female_fullyexp	0.019**	0.019**	0.019**	0.019**
	(0.027)	(0.009)	(0.009)	(0.009)
abortion_law			-0.043	-0.014
			(0.031)	(0.048)
N	21973	21973	21973	21973
Country fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes
Abortion right	No	No	Yes	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.6 Placebo results

The identification strategy relies on the assumption that without the birth control *pill* introduced on-demand in years *x*, the increase in females' outcomes would not have been systematically sizable and statistically significant when compared to males' ones. However, the patterns of males' and females' educational and labour market achievements could be systematically different across countries, challenging the identifying assumption. The identification strategy is also violated if other factors are correlated to the *pill* introduction on-demand to young unmarried women in year *x* in country *j*. For example, increased support for women's rights and feminism during the 1960s and 1970s could cofound the effect of contraception. For this reason, I provided graphical evidence on the pre-*pill* parallel trend between males and females in the outcome of interest in Section 2.4. To address the potential failure of the parallel trend assumption, compelling evidence relies on a trend-augmented version of the differences-in-differences model (among others Friedberg 1998; Autor 2003; Besley and Burgess 2004). Similarly, I used country-year of birth linear trends in some specifications [32] to capture these and other potential social changes. In general, this strategy might not be efficient, as it absorbs most of the treatment effect (Vandenberghe, 2019). Even though, including time trend in my analysis does not downward bias my results.

Access to abortion could also potentially confounds the estimated effects of oral contraception. However, I check that contraceptives and abortion access on-demand at the country level are not collinear. Pearson's correlation is statistically significant at the conventional 95% Confidence Interval (CI), but very low (0.220). Moreover, estimates are robust after controlling for abortion access on demand.

Maternity leave policies could also trigger results as, in principle, they might act as a substitute for abortion (Goldin and Katz, 2002; Pezzini, 2005). Maternity leave policies are particularly relevant for labour market outcomes (Brugiavini et al., 2011), but those may also influence females' educational attainments due to the *expectation effect* (IWPR, 2019). At the country level, Pearson's correlation between the year of introduction of the *pill* on-demand and maternity leave policy is 0.249 and not statistically significant. Further details on the year of introduction of abortion on demand and maternity leave policies for the 14 European countries are provided in Appendix (Table 12 and 13).

[32] I also repeat the analysis using country-year of birth squared trends without significant changes in final results

To test the plausibility of estimates, I provide placebo analysis implementing control experiments (Duflo, 2001) and comparing individuals older than 27 in year *x,* which are not exposed at all to the benefits of the *pill* during their potential college years. To check the existence of systematic differences between males and females which are not driven from the *pill* introduction, I consider random cohorts up to 45 years old in year *x*, where year *x* is the year in which the *pill* was introduced to young unmarried women on demand in country *j*. I compare the differences in the differences in outcomes between females and males in placebo age-groups of individuals, within and across countries, not exposed to the *pill* during their college years (Heckman and Hotz, 1989, Rosenbaum, 1987).

Being female systematically reduces the probability of educational achievements as measured by years of education, probability of college degree, educational levels. However, the increase in female educational achievements between cohorts in these two age groups is not statistically different from zero.

Being female also reduces the probability of a professional career. Also when looking at labour market outcomes, the increase between cohorts in the two age groups is not statistically different from zero. Tables 15-19 in Appendix reports the results from placebo exercises.

As an additional placebo exercise, I performed several random reshuffles of the dates of the pill introduction across countries to compare cohorts of males and females aged 14-18 and 23-27 at year *x random*, obtaining systematically insignificant results.

Taken together, these results are reassuring with respect to the effectiveness of the identification strategy and provide evidence that the differences in the differences between females and males who are fully exposed and not exposed are not driven by inappropriate identifying assumptions.

2.7 Conclusion

This paper evaluates female achievements derived from birth control pharmaceutical innovation**.** I analyse 24,347 individuals from 14 European countries comparing males and females aged 14-18 or 23-27 in the year of introduction of the *pill*, (year *x*) in country *j*.

The main finding is that the introduction of *pill* on-demand to young, adult, unmarried women has a sizable and statistically significant positive effect on female educational outcomes measured as years of education, probability of college graduation and educational level. This higher human capital investment translates into labour market comparative advantages, measured as the probability of a professional career. Also in this case, the introduction of the *pill* has a sizable and statistically significant positive effect on labour market outcomes for females belonging to fully exposed cohorts.

Concerns of endogeneity are discussed. Results are robust to different specifications. As robustness checks, I implement placebo analysis on different cohorts of men and women that have not been exposed to the *pill* on demand during their college years; hence they should not have benefitted from the *pill* introduction. In this case, estimates are not statistically different from zero for all our outcomes of interest. I also randomly reshuffle the dates of the pill introduction across countries, obtaining systematically insignificant coefficients. Placebo exercise provides evidence that inappropriate identifying assumptions do not drive the *pill*'s effects on fully exposed females.

Hence, analysis strongly confirm that birth control innovation in the form of the contraception *pill* available on-demand to young unmarried women increases women's empowerment. These findings align with previous literature that focused on the US context.
Despite historical relevance, mt results are relevant to inform effective family planning policies on the importance of allowing women to better plan childbearing in the process towards gender equality and female empowerment. Although institutional differences need to be considered, results show the importance of promoting access to reliable contraception in countries where birth control policies are not legal, most notably developing countries. Access to birth control is relevant also for marginalised sub-groups in developed countries, as policies to increase access are required.

Women with more say regarding contraception and higher empowerment might have future generation implications by reducing the cost of avoiding unintended pregnancies, as planned children might have better health outcomes and improved outcomes later in life. Given the relative scarcity of empirical investigation between oral contraception and future generation implications, this topic represents an exciting avenue for promising future research.

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Appendix

Table 9: Distribution of age in SHARE data with respect to year x

Country	year *x*	Age youngest cohort at year *x*	Age oldest cohort at year *x*
Austria	1962	-54.45	59.23
Germany	1961	-54.52	51.38
Sweden	1964	-51.57	61.52
Netherlands	1964	-46.49	57.52
Spain	1978	-38.54	75.63
Italy	1976	-39.90	71.55
France	1967	-49.02	62.51
Denmark	1966	-49.49	58.98
Greece	1980	-38.15	73.38
Switzerland	1961	-53.49	53
Belgium	1968	-49	63.49
Poland	1966	-51	61.58
Hungary	1968	-49	63.42
Portugal	1962	-52.51	47.51

Source: SHARE, waves 1-7

Table 10: Descriptive statistics by gender

	(1) Not exposed Male	(2) Not Exposed Female	(3) Fully Exposed Male	(4) Fully Exposed Female
Age at x	24.94	24.91	16.08	16.03
Migrant before 23	0.04	0.04	0.04	0.04
Migrant before 19	0.03	0.03	0.03	0.03
Ever married	0.94	0.95	0.92	0.95
Never child	0.11	0.09	0.13	0.10
Child before17	0.00	0.01	0.01	0.01
Child before18	0.01	0.02	0.01	0.03
Child before19	0.01	0.05	0.02	0.06
Child before20	0.02	0.09	0.03	0.10
Child before21	0.03	0.15	0.05	0.17
Child before22	0.05	0.22	0.08	0.25
Child before23	0.09	0.31	0.13	0.33
Child before25	0.21	0.48	0.27	0.47
Marriage before19	0.01	0.05	0.01	0.05

Marriage before21	0.04	0.14	0.05	0.14
Marriage before23	0.13	0.23	0.15	0.23
Professional	0.12	0.06	0.15	0.09
Years education	10.78	9.94	11.55	10.98
College graduate	0.24	0.17	0.27	0.23
Observations	5340	5830	6104	7073

Source: SHARE, waves 1-7.

Table 11: Sample selection - cohorts not exposed and fully exposed defined by year x, by country

Country	year *x*	Not exposed - aged 23-27 at year *x*	Fully exposed- aged 14-18 at year *x*
Austria	1962	775	964
Germany	1961	1,036	1,137
Sweden	1964	896	1,152
Netherlands	1964	731	1,266
Spain	1978	1,247	492
Italy	1976	1,163	952
France	1967	849	1,307
Denmark	1966	635	848
Greece	1980	1,132	680
Switzerland	1961	455	714
Belgium	1968	1,030	1,534
Poland	1966	561	1,066
Hungary	1968	460	673
Portugal	1962	400	392
Observations		11170	13177

Source: SHARE, waves 1-7.

Table 12: Legal abortion on demand - year of introduction

Country	Abortion on demand
Austria	1975
Germany	1972
Sweden	1974
Netherlands	1984
Spain	2010
Italy	1978
France	1974
Denmark	1973
Greece	1986
Switzerland	2002
Belgium	1990
Poland	NO
Hungary	1953
Portugal	2007

Source: Author's elaboration from different sources

Table 13: Maternity leave policy introduction - year of introduction

Country	Maternity leave
Austria	1950
Germany	1970
Sweden	1970
Netherlands	1970
Spain	1970
Italy	1970
France	1970
Denmark	1970
Greece	1970
Switzerland	1970
Belgium	1970
Poland	1974

Source: Brugiavini et al., 2011

Table 14: Probability of professional career, unconditional

	(1) Professional	(2) Professional	(3) Professional	(4) Professional
after	-0.008	-0.000	-0.007	-0.000
	(0.017)	(0.017)	(0.017)	(0.017)
female	-0.071***	-0.071***	-0.071***	-0.071***
	(0.006)	(0.006)	(0.006)	(0.006)
female_fullyexp	0.023**	0.022**	0.023**	0.022**
	(0.008)	(0.008)	(0.008)	(0.008)
abortion_law			-0.039	-0.013
			(0.029)	(0.045)
N	23192	23192	23192	23192
Country fixed effects	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes
Abortion right	No	No	Yes	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15: Placebo 28-32 vs 33-38

	(1) years_edu	(2) years_edu	(3) college_grad	(4) college_grad	(5) edu_level	(6) edu_level	(7) professional	(8) professional
placebo1	0.060	0.048	0.019	0.015	-0.034	-0.045	0.042	0.052
	(0.147)	(0.148)	(0.014)	(0.014)	(0.042)	(0.043)	(0.075)	(0.076)
female	-1.096***	-1.098***	-0.081***	-0.081***	-0.350***	-0.352***	-0.477***	-0.479***
	(0.086)	(0.086)	(0.008)	(0.008)	(0.025)	(0.025)	(0.049)	(0.049)
placebo1_fem	0.098	0.094	0.002	0.002	0.051	0.052	0.030	0.029
	(0.115)	(0.115)	(0.011)	(0.011)	(0.033)	(0.033)	(0.064)	(0.064)
N	16598	16598	16598	16598	16598	16598	16598	16598
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes	No	Yes	No	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
** $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

Table 16: Placebo 29-34 and 35-39

	(1) years_edu	(2) years_edu	(3) college_grad	(4) college_grad	(5) edu_level	(6) edu_level	(7) professional	(8) professional
placebo2	0.041	0.008	0.006	0.002	-0.023	-0.033	-0.145	-0.159
	(0.150)	(0.151)	(0.014)	(0.014)	(0.043)	(0.044)	(0.078)	(0.079)
female	-1.155***	-1.151***	-0.091***	-0.091***	-0.382***	-0.382***	-0.493***	-0.494***
	(0.087)	(0.087)	(0.008)	(0.008)	(0.025)	(0.025)	(0.051)	(0.051)
placebo2_fem	0.051	0.049	0.008	0.008	0.054	0.054	0.040	0.039
	(0.117)	(0.117)	(0.011)	(0.011)	(0.034)	(0.034)	(0.067)	(0.067)
N	15882	15882	15882	15882	15882	15882	15882	15882
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes	No	Yes	No	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17: Placebo 30-35 and 36-41

	(1) years_edu	(2) years_edu	(3) college_grad	(4) college_grad	(5) edu_level	(6) edu_level	(7) professional	(8) professional
placebo3	0.058 (0.154)	0.039 (0.155)	0.017 (0.014)	0.013 (0.014)	0.044 (0.045)	0.037 (0.045)	-0.165 (0.081)	-0.188 (0.082)
female	-1.139*** (0.090)	-1.133*** (0.090)	-0.086*** (0.008)	-0.086*** (0.008)	-0.358*** (0.026)	-0.358*** (0.026)	-0.506*** (0.054)	-0.506*** (0.054)
placebo3_fem	0.025 (0.119)	0.019 (0.119)	0.000 (0.011)	0.001 (0.011)	0.017 (0.035)	0.017 (0.035)	0.055 (0.070)	0.053 (0.070)
N	15100	15100	15100	15100	15100	15100	15100	15100
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes	No	Yes	No	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18: Placebo 33-37 and 38-43

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	years_edu	years_edu	college_grad	college_grad	edu_level	edu_level	professional	professional
placebo4	-0.028	-0.007	-0.004	-0.003	-0.012	-0.006	0.187	0.091
	(0.164)	(0.165)	(0.014)	(0.015)	(0.048)	(0.049)	(0.094)	(0.097)
female	-1.207***	-1.214***	-0.090***	-0.091***	-0.377***	-0.378***	-0.537***	0.547***
	(0.098)	(0.098)	(0.009)	(0.009)	(0.029)	(0.029)	(0.068)	(0.069)
placebo4_fem	0.108	0.119	0.009	0.010	0.034	0.035	0.056	0.068
	(0.129)	(0.129)	(0.011)	(0.011)	(0.038)	(0.038)	(0.084)	(0.085)
N	12796	12796	12796	12796	12796	12796	12796	12796
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes	No	Yes	No	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
** $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

Table 19: Placebo 35-39 and 40-44

	(1) years_edu	(2) years_edu	(3) college_grad	(4) college_grad	(5) edu_level	(6) edu_level	(7) professional	(8) professional
placebo5	0.019	0.017	0.003	0.006	-0.025	-0.019	-0.053	-0.001
	(0.174)	(0.175)	(0.015)	(0.015)	(0.052)	(0.053)	(0.107)	(0.110)
female	-1.096***	-1.101***	-0.075***	-0.076***	-0.360***	-0.361***	-0.514***	-0.518***
	(0.103)	(0.103)	(0.009)	(0.009)	(0.032)	(0.032)	(0.080)	(0.081)
placebo5_fem	-0.041	-0.036	-0.011	-0.010	0.010	0.012	0.003	0.010
	(0.135)	(0.135)	(0.011)	(0.012)	(0.041)	(0.041)	(0.097)	(0.097)
N	11328	11328	11328	11328	11328	11328	11328	11328
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-year linear trends	No	Yes	No	Yes	No	Yes	No	Yes

Source: SHARE, waves 1-7. Note: Bold figures report statistically significant coefficient. Robust standard error in parentheses.
** $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$*

Chapter 3

Fertility implications to assets ownership in the rural context: evidence from a policy reform in Nepal

Agar Brugiavini[33], Annarita Macchioni Giaquinto[34], Erdgin Mane[35], Francesca Zantomio[36]

Abstract: This work explores the causal link between asset ownership and fertility. In developing countries, assets are crucial to income-generating activities and well-being. Yet, a systematic *gender-asset gap* hinters women's empowerment, limiting their voice and agency in society and within marriage. Collective modelling of household decisions predicts that a change in bargaining- power resulting from a change in the distribution and control of resources, such as assets, might affect fertility outcomes. To address endogeneity, we exploit a pro-woman legal innovation implemented in Nepal in 2002 that provided married women equal rights to their husbands' property immediately after marriage. Using Nepal's cross-sectional data from DHS, we apply regression discontinuity in time and before-after comparison on matched women. Results show that female asset ownership induced a sizable and statistically significant reduction in fertility and demographic implications for developing countries.

Keywords: fertility, asset ownership, women's empowerment, gender equality, cross-sectional data

JEL codes: J12, J16, J18

[33] Università Ca' Foscari of Venice

[34] Università Ca' Foscari of Venice, FAO, annarita.macchioni@unive.it

[35] FAO

[36] Università Ca' Foscari of Venice, CRIEP, HEDG (York)

3.1 Introduction

This work explores the causal link between female asset[37] ownership and fertility in the rural context of developing countries, exploiting an exogenous Nepali nationwide legal reform to reduce gender-based discrimination in female property rights and asset ownership.

Women's empowerment and gender equality are still relevant issues in the development arena: the World Economic Forum estimated that, at the current trajectory globally, it would take 135.6 years to close the gender gap and achieve parity between males and females (World Economic Forum, 2021). Gender parity and women's empowerment might be a remedy against the high population growth rate, environmental degradation, and the low status of women (Batliwala,1994). Gender parity and empowerment have implications within the household and for productivity. On one side, those affect women's well-being and have future-generation implications in terms of better children's outcomes (Duflo, 2012); on the other side, low social and political power within the community might hinder the productivity of female income-generating activities. For example, Goldstein & Udry (2008) find that women are less likely to leave their agricultural plots fallow in Ghana due to a higher risk of expropriation, affecting soil fertility and agricultural output.

In developing countries, particularly in the rural context, gender inequality and dis-empowerment are severe, and asset ownership and control remain major constraints for most women. Women have typically limited access and control over assets (Doss *et al.*, 2020; Doss *et al.*, 2015, Kieran *et al.,* 2015, Ambler *et al.,*2017, Deere and Leon, 2003, Agarwal, 1994), both tangible and intangible.[38]. The systematic gap between males and females in asset ownership has been theorised as *gender asset gap* (Doss *et al.,* 2011). Women's assets are also less valued or lower in terms of quality than men's ones (Deere and Doss, 2006, Deere *et al.*, 2013, Quisumbing and Maluccio, 2003). However, assets -both as resources and as a means of storing wealth - are crucial to individuals' and households' well-being and income-generating activities. Female asset ownership and control have the potential to affect different household outcomes and decisions (Agarwal, 1994, Haddad et al., 1997, Schultz, 2001, Quisumbing and Maluccio, 2003, Doss, 2006, Meinzen-Dick et al., 2019), including fertility.

Fertility decisions can be a dimension of women's empowerment and gender equality. Empowerment has been defined and conceptualised in different ways; however, one of the most accepted

[37] Assets are defined as a store of value representing a benefit or a series of benefits accruing to the economic owner by holding or using the entity over a period of time (United Nations et al., 2008; UN Methodological Guidelines for the Production of Statistics on Asset Ownership from a Gender Perspective- EDGE, 2019).

[38] Tangible assets include agricultural land, real estate and other property, non-land agricultural assets (such as large and small agricultural equipment or livestock). Intangible assets include human capital assets, financial assets (formal and informal savings, insurance), social assets (group and community membership) (Meinzen-Dick *et al.,* 2011).

and popular definitions is the one provided by Kabeer (1999), which defines Women's Empowerment as "the processes by which those who have been denied the ability to make choices acquire such an ability" (Kabeer, 1999, p.24). In Kabeer's definition, empowerment has three inter-related dimensions: resources (i.e., access, but also future claims, to both material and human and social resources), agency (i.e., processes of decision making) and achievements (i.e. outcomes). Achievements depend on agency and resources, but the level of resources influences agency itself. Expanding resources and, therefore, agency has the potential to empower women and foster gender equality, allowing women to control their bodies better and exercising reproductive preferences (among others, Sen et al., 1994).

In the context of patriarchal power relations and sharp gender inequalities, women have also poor control over their bodies and sexual domain, exposing them to a poor agency in crucial life-choice such as childbearing.

Economists recognise fertility as part of household decisions interplay with saving, consumption, and labour supply. Collective models of households' behaviour offer the theoretical framework to analyse it. Collective modelling has provided a better description of how households make decisions and allocate resources, stressing the relevance of intra-household dynamics and bargaining processes (Doss, 1996, Thomas, 1990, Gray, 1998, Chiappori 1988, 1992, 1997). Therefore, the number and timing of intended children can be modelled as the outcome of non-unitary household decision process, where the outcomes depend on the distribution and control of resources within households' members and their preferences. Within collective modelling, changes in intra-household bargaining power or changes in household income can potentially change the costs and benefits associated with offspring. An increase in assets owned by an individual increases that person's relative bargaining position and changes household's allocations towards that individual's preferences (McElroy and Horney, 1981; Manser and Brown, 1979, Chiappori 1988, 1992, 1997). When households members share different preferences over the number of children (Rasul 2001, Mason and Taj, 1987, and Pritchett 1994) and since the health costs of pregnancy and the time spent caring for children make childbearing more costly for women (Eswaran, 2002), women might have a preference toward smaller families (Agrawal, 2012; Upadhyay et al., 2014). Non-unitary models of household behaviour suggest that an increase in women's bargaining power influences fertility, and it is an essential factor affecting family size (Schultz, 1990, Rao and Greene, 1993; Rasul, 2001, Upadhyay et al., 2014 for a review). For example, Gudbrandsen (2013), using a natural experiment in Nepal, has found that women with a higher level of autonomy have fewer children.

Indeed, existing empirical evidence confirms that female property rights are associated with higher intra-household bargaining power. Mishra and Sam (2016) have found that increased female land rights empowered Nepalese women. Owner-women have greater autonomy in household decision-making, control over income and female empowerment (Santos et al., 2014; Allendorf, 2007; Datta, 2006;

Menon, et al., 2013; Field, 2005; Fafchamps and Quisumbing, 2005; Swaminathan, Lahoti and Suchita, 2012; Kumar and Quisumbing, 2012, Mishra and Sam 2016).

Several studies have also shown how changes in marriage market conditions, with reference to the introduction of divorce law or sex ratio, affect post-marital behaviours (Chiappori et al. 2002; Angrist 2002). In the same way, changes in marriage market condition induced by increased property right upon marriage is expected to increase female post-marriage bargaining power.

Asset ownership and control might also be a relevant source of income for women. Asset ownership and control promote female economic activities that allow them to be economically independent while reducing their risk of poverty and social exclusion. Given the relevance of asset ownership and control in the rural context, property rights over assets mean that owner-women can access formal credit and extension groups. The latter has the potential to release credit constraints, boost investments and eventually increase productivity. Property rights could also allow selling, renting, or producing and controlling income from exploiting the asset. Income generated from assets and controlled by women might be used in different ways compared to income controlled by solely men (among others: Duflo 2003, Doss 2006), including to afford contraceptive methods (Chakrabarti, 2017), which provides women with a safe and reliable tool to control their reproductive lives. On one side, a positive income effect might increase fertility, assuming that children are a normal good. At the same time, more income reduces the incentive to send children to early work and reduce the opportunity cost of schooling for them, encouraging households to have better "quality" children at the expense of "quantity" (Becker and Lewis, 1973). On the other side, better job market opportunities derived from asset ownership and control might increase female opportunity costs of childbearing (Rosenzweig and Evenson, 1977, Willis, 1973), which tend to reduce the number of children.

Only a few studies have tried to prove that increased female asset ownership and control might be responsible for declining fertility. Chakrabarti (2017) in Nepal have found an association, albeit no causation, between female land ownership and reduced births. Field (2005) have found that a land titling program in Peru decreases fertility. Besides the relative scarcity of empirical evidence on the impact of increased property rights on fertility, previous literature acknowledges asset ownership as fundamental in empowering women in the rural context. However, none of the existing works offers rigorous causal identification on the effect on childbearing.

This paper contributes to existing studies offering causal evidence on the link between female asset ownership and fertility decisions through an exogenous source of variation in female asset ownership: a pro-woman legal reform regarding property rights upon marriage, passed in Nepal in 2002. Identification relies on two approaches, Regression Discontinuity in Time (RDiT) and a Before-After comparison on observationally identical women. These two estimation strategies allow exploring changes in women's fertility careers attributable to the pro-woman legal innovation. Using the most recent available data from the Demographic and Health Survey (DHS) for Nepal, i.e., the 2011 and 2016 cross-sections, we find that increased female access to property rights induced a sizable and statistically significant reduction in fertility.

The remainder of the paper is organised as follows: Section 2 describes women's socioeconomic and institutional context in the Nepalese society, while Section 3 presents the legal property rights framework. Section 4 describes the data used, while Section 5 presents two different estimation strategies. Section 6 presents the estimates of the causal effect of increased property right on fertility. Finally, Section 7 concludes.

3.2 Women in Nepal

The setting for our study is Nepal. Nepal is a South-Asian country, classified as a lower-middle-income country with a Gross National Income per capita 2020 of 1,090 US $ (2019 as of 1, July 2020) (World Bank, 2020). Nepal's economy is mainly agriculture-based, with a large share of the working-age population engaged in subsistence farming and living in rural areas (CBS, 2000).

Equality between females and males has shown significant improvements in the last twenty years. The Global Gender Gap index[39] (World Economic Forum, 2021) in Nepal increased from 0.55 in 2006 to 0.68 in 2020, growing at an average annual rate of 1.62%. Women's life expectancy increased by nine years in the last 19 years (World Bank, 2019), and it is two years higher than men's one. The literacy rate for adult women increased from 34.88 % in 2011 to 59.72% in 2018 (World Bank, 2018). Moreover, female secondary or higher educational attainments increased markedly. Despite recent progress, gender inequality continues to plague Nepalese women. Women still face limited earning opportunities, mainly concentrated in non-waged employment or unpaid family work. Women comprise 65 per cent of the workforce in agriculture, although statistics do not include much of the unpaid family labour in subsistence agriculture (FAO, 2010). Nevertheless, a large wage and earnings gap exists between men and women due to occupational segregation and social norms (Ruppert Bulmer et al., 2020).

Women's duties within society and within the household are defined by traditional division of roles, social hierarchy, and gender-based norms. Care responsibilities limit the time available to engage in full-time jobs or seek better labour market opportunities. Women also experience mobility constraints, as well as security concerns and restrictive legislation, preventing them from migrating to seek better or temporary job opportunities (Ruppert Bulmer et al. 2020)

At the national level, the fertility rate, namely total births for women, has decreased from 4.73 in 1995 to 1.87 in 2019 (World Bank, 2019). Nevertheless, it shows heterogeneous birth differences for women between urban-rural residence, ecological zones, development regions, education, and wealth quintile (Chakrabarti, 2017). For example, comparing women in rural and urban areas, there is a difference of 1 birth for women (New ERA, and ICF International Inc., 2011). In Nepal, fertility is defined by marital status. Cohabiting is very limited, and sexual intercourse outside marriage is socially unacceptable (Chakrabarti, 2017; Ghimire & Axinn, 2013; Maitra, 2004). Therefore, the age of marriage is the onset of women's exposure to pregnancy. The female median age of marriage, which predicts exposure to fertility, is 17.5 years old (UNICEF, 2011), and 75% of women get married before 19 (ICF International Inc. 2011). The median difference in age with husband is 4 years, but 35% of women are between 6- and 18-younger (ICF International Inc. 2011; ICF International Inc. 2016). Early marriage

[39] The Global Gender Gap index scores from 0 (perfect inequality) to 1 (perfect equality).

and notable differences in age with husband undermine power within marriage (Chandra-Mouli et al., 2013; Santhya, K.G. 2011), particularly regarding sexual domain (Pande et al., 2011), exposing women to a higher risk of pregnancy with little voice and agency over their preferences.

The median age of the first childbearing is 19, and the median time between marriage and first childbearing is 21 months (New ERA, and ICF International Inc., 2011). Yet, the adolescent birth rate is still 63.67 for every 1000 women aged 15-19 (as of 2018) (World Bank, 2019). After two years of marriage, 75% of women have at least one child (NEW ERA and ICF International Inc. 2011). By 5 years from marriage, we can observe more variability in the number of childbearing roughly 40% of women have 1 child, 40% have two children, and only 10 % have more. Nepalese women complete their fertility career early in life, with childbearing concentrated within the first years of marriage. The last childbearing happens at a median age of 25, and 90% of women complete their fertility career by 31 (New ERA, and ICF International Inc., 2011). Overall, inequality between females and males in the attribution of power marks Nepalese society. Women's rights remain limited by male authority passing from the father to the husband.

3.3 Asset's ownership and control: the legal framework and the 2002 reform

Assets can generally be purchased or acquired through inheritance and marriage as gifts, transfers, and government transfers (Doss *et al.*, 2011; Oduro *et al.*, 2011). There are different factors liming women's tenure and security rights over assets, such as disadvantaged market-based forms of property acquisition due to lack of earnings and financial means, legal framework, marital regimes[40] and gender-based norms and stereotypes (Gaddis et al., 2019, Doss *et al.*, 2012, Fafchamps and Quisumbing, 2007).

Analysing the legal framework regarding women's property rights, Nepalese society is typically classified as a patriarchal and patrilineal country (Chakabrati, 2017, Brunson, 2010; Cameron, 1995; Furuta & Salway, 2006; Halim, Bohara, & Ruan, 2011; Koolwal, 2007) as it has always been discriminatory towards women. The 1853's National Code (*Muluki Ain*) restricted women's property rights to only gifts (*Daijo and Pewa*) and bequests and prescript that a daughter could not inherit paternal property as long as at least one male relative or her mother were alive.

[40] Marital regimes can be identified as: i) separation of property: all types of assets acquired before or during marriage remain separate property; ii) partial community of property: assets acquired during marriage become common property, but assets acquired before marriage and through inheritance remain separate property; and iii) absolute community of property: all assets acquired before or during marriage become common property (Deere and Diaz, 2011; Doss and Deere, 2008; Deere and Doss, 2006).

In 1963, the sixth amendment of the National Code entitled sons and daughters to have equal rights to inherit. However, only unmarried daughters of 35 years or older were entitled to an equal share of the parental property as her brothers. In case of marriage, she had to return the property unless her mother, father, brothers, and brothers' sons were dead. Marital regime in Nepal recognised a partial community of property upon marriage only with some binding conditions on woman's age and marriage duration. Assets acquired through marriage become common property only after the wife turns 35 and is married for at least 15 years.

Nevertheless, assets acquired before marriage and through inheritance remain separate property. Starting from 1977, female ownership and control over assets were fostered by several policies to achieve a better gender balance (Pandey et al., 2003). Systematic and progressive reforms to the Nepalese constitution allowed very limited property rights for women to evolve into more equal rights in the last decades.

A constitutional amendment in 2002 reformed marital regimes and expanded women's rights. The reform prescript that since 2002 married women were entitled to equal rights to their husband's property immediately after marriage, irrespectively of wife's age and marriage duration. Within the introduction of this new property regime – which will be exploited as a source of exogenous variation in female access to property rights - both spousal consents were mandatory for any transaction, such as renting or selling. Joint ownership expanded the female *bundle of rights*. [41](Johnson and Quisumbing, 2009) associated with the ownership of assets. Under this new marital regime, immediately after marriage, the wife had fewer rights with respect to solely ownership, but more rights than complete exclusion from the property, including having voice over alienation, access, extraction or commercial exploitation and management. Studies have confirmed that promoting joint registration of both spouses improved female asset ownership and control (O'Sullivan 2017; Ali et al. 2014; Goldstein et al. 2018) and that the right to claim marital property increases the female share of asset ownership (Doss *et al.* 2012). Moreover, compelling evidence has established that countries with more egalitarian legal regimes towards women have a higher share of female asset ownership. Pro-woman legal reforms could be an important mechanism to foster women's property rights (Gaddis et al., 2019; Deininger et al., 2013 and 2018).

Overall, as expected, female asset ownership in Nepal have marked significant progress: in 2011, Nepalese female ownership over land and other property covered 19.7 per cent of overall households

[41] Following Johnson and Quisumbing (2009), ownership and related rights can be further classified as:

- Access: right to be on a piece of land and use it
- Extraction and commercial exploitation: the right to claim outputs and income obtained by the asset
- Management: the right to decide about its use
- Exclusion: the right to exclude others from the use of the asset
- Alienation: the right to sell, lease, gift the asset

(CARE, 2015; IOM, 2016), while it was only 10.8 per cent in 2001 (IOM, 2016). Unfortunately, we are not able to measure gender equality in asset ownership immediately before and after the reform due to the lack of sex-disaggregated data in the relevant years, a common issue that hinters gender analyses in the development and rural context (Doss *et al.*, 2018; EDGE, 2019; Doss 2014).

3.4 The Demographic and Health Survey

The analysis is based on the two most recent cross-sections of DHS data from Nepal, namely DHS 2011 and 2016 (Ministry of Health and Population (MOHP) (Nepal), New ERA, & ICF International Inc, 2012 and 2017).

DHS Nepal is a nationally representative cross-sectional survey of households implementing a two-stage probability sampling to identify a nationally representative population sample. First, primary stage units (PSUs) are stratified by geographic region or urban/rural areas, then primary stage units (PSUs) are selected from each strata through probability proportional to size (PPS). Households are selected within PSUs with equal probability selection. The data collection was conducted by New ERA, a local research firm, under the Ministry of Health (MOH) of the Government of Nepal, ICF and the financial support of The United States Agency for International Development (USAID).

The DHS survey is composed of Household Questionnaire, Woman's Questionnaire and Man's Questionnaire, with information collected through face-to-face interviews. The Household Questionnaire covers information on household characteristics such as wealth quintile, source of water, type of toilet facilities, materials used for the floor of the dwelling unit, and ownership of various durable goods and migration. The Household Questionnaire lists all household members in sampled households collects basic demographic information on each person listed, such as age, sex, marital status, and education. Age and sex of household are used to identify women and men eligible for individual questionnaires. All women of reproductive age (15-49 years old) belonging to sampled households are eligible for interview and in every second household selected, men aged 15-49 are also eligible to be interviewed. The women's questionnaire collects women's background characteristics (age, year of birth, education), pregnancy history and childhood mortality, marriage, and sexual activity. The Man's Questionnaire collects the same information as the Woman's Questionnaire but without reproductive history and maternal and child health data. However, matching women with their husbands allows observing the male partner's background characteristics.

Ideally, to our research ends, we would like to analyse a representative sample of women observed over time, in a longitudinal dataset collecting information on their fertility career, their life-cycle asset's ownership and control, their education, and their life-cycle labour supply throughout the preceding and successive years of the law, and corresponding information on their partners. In the absence of such data, we seek to exploit available cross-sectional information with a retrospective approach. Although the DHS data were collected in 2011 and 2016, we can reconstruct the timing and number of childbearing marriage history and retrieve time-invariant characteristics such as education, year of birth, year of marriage and ethnicity. Unfortunately, we cannot reconstruct women's labour supply history before, around, and after marriage, as the labour market questions refer to the last 12-months. Also, we cannot reconstruct women's life-cycle pattern of asset ownership and control, as this information was collected in 2011 only. Despite these limitations, we can exploit a large sample of 25,512 women of their reproductive age and compare women's fertility careers immediately before and after the pro-women legal reform that happened in 2002.

In light of the considerations on fertility in Nepal, fertility career of women is evaluated as the total number of children born at 5 years from marriage, which is the outcome of interest in this research. Table 1 provides descriptive statistics of our sample of women married between 1993 and 2007, distinguishing between before and after the reform.

As shown by Table 1, on average, women married after 2002 have statistically fewer kids at 5 and 4 years from marriage than women married before 2002. The difference in total kids at 5 from marriage is 0.17 or 10% in relative terms, while the difference in total kids at 4 years of marriage is 0.08, which is very low in relative terms. Hence choosing to evaluate fertility career in the first 5 years from marriage allows us to select the shorter timespan with enough variability before and after the reform. Further statistically significant differences between women married before and after 2002 are also observed in terms of Dalit [42] prevalence, women's educational level, years of education and partner's educational level.

[42] Dalit is the lowest caste in Nepal. Dalit population experience caste-based discrimination and are most backward in social, economic, educational, political and religious fields, and deprived of human dignity and social justice (NDC, 2008)

Table 1: Descriptive statistics

	(1) Women Married Before 2002	(2) Women Married After 2002	(3) Diff (1)-(2)
Age at marriage	17.16	17.77	-0.62***
Difference in age with a partner	4.23	3.94	0.29**
Age of respondent at 1st birth	19.56	19.77	-0.21**
Total kids at 5 yrs from marriage	1.67	1.50	0.17***
Total kids at 4 yrs from marriage	1.36	1.27	0.08***
Total kids at 3 yrs from marriage	1.02	1.02	0.00
Ethnicity: hill brahmin	0.12	0.12	0.00
Ethnicity: hill chhetri	0.23	0.25	-0.01
Ethnicity: terai brahmin/chhetri	0.02	0.01	0.00
Ethnicity: other terai caste	0.10	0.09	0.01
Ethnicity: hill dalit	0.10	0.12	-0.02**
Ethnicity: terai dalit	0.03	0.03	0.01
Rural	0.54	0.56	-0.02
Highest educational level	0.72	1.19	-0.47***
Education in years	2.75	4.69	-1.93***
Religion: hindu	0.87	0.87	-0.00
Religion: muslim	0.04	0.03	0.01
Religion: kirat	0.01	0.02	-0.00
Religion: christian	0.02	0.02	-0.00
Highest edu level husband	1.47	1.70	-0.23***
Education in years husband	3.14	3.13	0.01
Observations	2699	3940	6639

Note: Variables in bold if difference in mean statistically significant at 95% CI
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.5 Empirical strategy

3.5.1 Regression Discontinuity in Time

The identification challenge, in our setting, concerns the need to disentangle causal effects from the effect of other variables acting as confounders, for example, education, age of marriage etc., that may be driving both asset ownership and fertility. Endogeneity concerns arise both in terms of unobserved heterogeneity (e.g. women's fertility preferences being correlated with their probability of asset ownership and control) and reverse causality, i.e. the possibility that more fertile women might be “rewarded” through the attribution of additional properties. To address the endogeneity of asset ownership with respect to fertility outcomes, we use a plausibly external source of variation introduced by a policy reform, allowing us to isolate the causal effects of increased assets ownership on fertility outcomes.

The policy reform increased female property rights upon marriage. The pro-women legal innovation has been introduced nationwide at the same time for all regions and women of all ages, solving potential self-selection bias. However, given that the only variation in women’s access to the marital property through marriage was overtime for the whole targeted population and that ,in the context of Nepal, the onset of women’s exposure to pregnancy is marriage, we cannot rely on the possibility of using geographic variation or a plausible and clean control group as comparison.

To address this challenge, we exploit a discontinuity in the rule for women’s access to marital property rights after marriage. The year of marriage defines a woman's access to the marital property after marriage and, as a consequence, their treatment status.

Let the receipt of treatment be denoted by the dummy variable $D \in \{0, 1\}$, so that we have $D = 1$ if $X \geq c$ and $D = 0$ if $X < c$, where X is the year of marriage and c is equal to 2002. Women married before 2002 were not treated; hence their access to the marital property was conditional on age and marriage length, i.e. respectively 35 years old and 15 years. Women married after 2002 get access to marital ownership as a direct and immediate consequence of marriage, irrespectively from age and marriage duration; hence they represent treated women.

Let exists for each woman *i,* a pair of “potential” outcomes $Y_i(1)$ if exposed to treatment and $Y_i(0)$ if not exposed to the treatment. The causal effect is represented by the difference $Y_i(1) - Y_i(0)$. However, the fundamental problem of causal inference is that $Y_i(1)$ and $Y_i(0)$ cannot be observed simultaneously. By definition, in RDiT, all women to the right of the cut-off are treated, and those to the left are not treated; therefore, we can only observe $E(Y_i(1) \mid X))$ and $E(Y_i(0) \mid X))$ respectively. The causal effect in this setting is given by

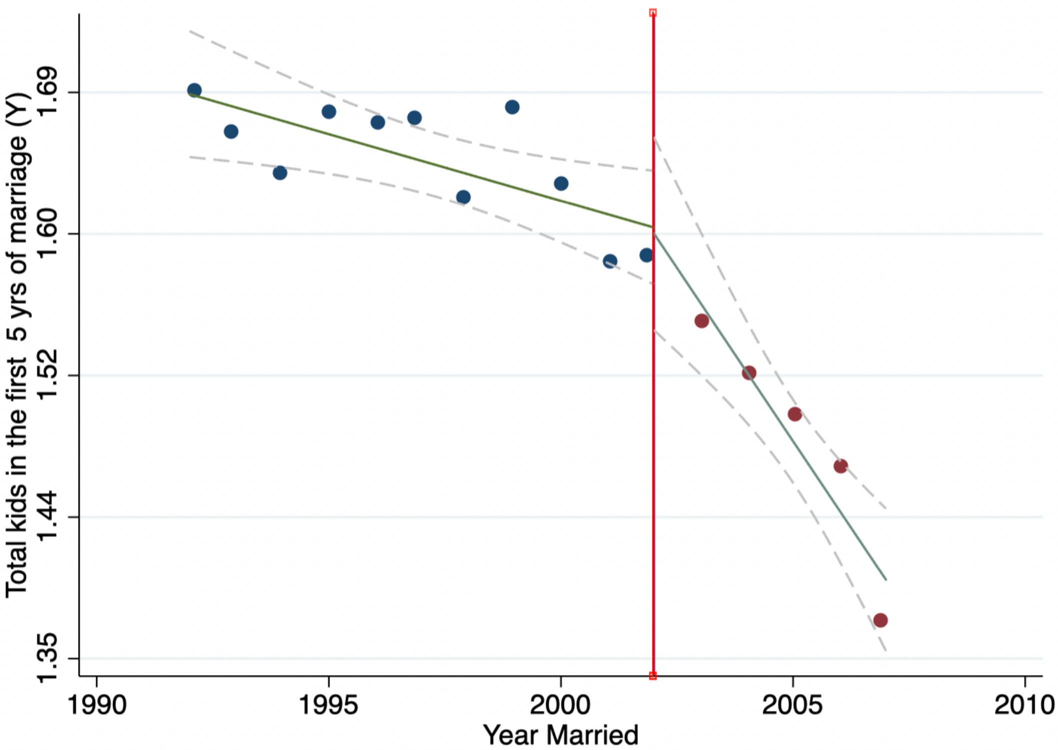
$$[\lim_{\varepsilon \downarrow 0} \mathrm{E}(Y_i \mid X_i) = c + \varepsilon] - [\lim_{\varepsilon \downarrow 0} \mathrm{E}(Y_i \mid X_i) = c + \varepsilon],$$

which is equal to the average treatment effect at the cutoff c, $\mathrm{E}(Y_i(1) \mid Y_i(0) \mid \mathrm{X}) = \mathrm{c}$.

Validity of RDiT evaluation strategy requires continuity of $\mathrm{E}(Y_i(0) \mid \mathrm{X})$ and $\mathrm{E}(Y_i(1) \mid \mathrm{X})$, that is that all potential variables besides the treatment and outcome be continuous at the cut-off, i.e., where the treatment and the outcome discontinuities occur (Imbens and Lumieaux, 2008; Hahn et al. 2001). This continuity condition allows us to rely on the average outcome of women right below the cut-off as a valid counterfactual for those right above the cut-off.

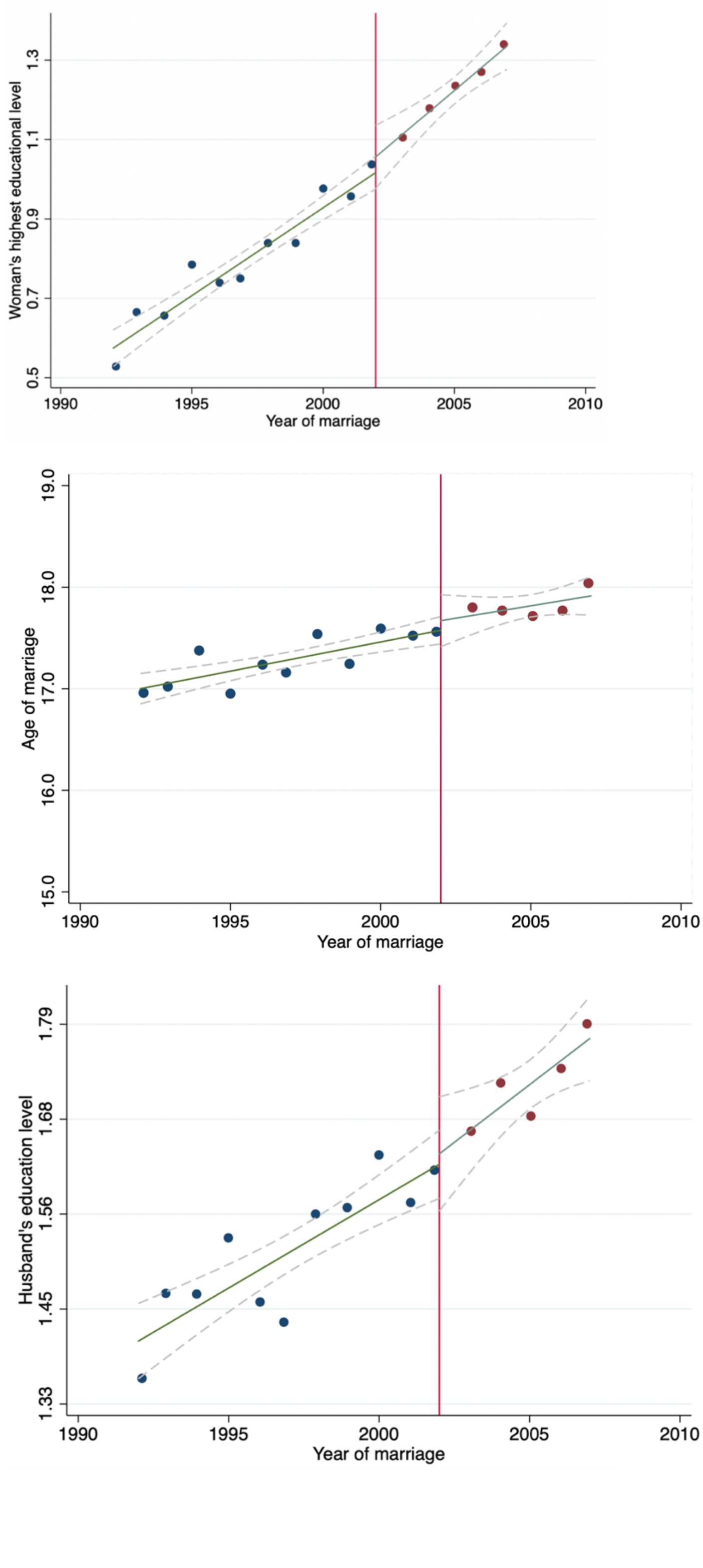
To test the validity of our setting, we provide some supportive descriptive evidence. Figure 1 plots row data on the total kids at 5 years of marriage, relative to the whole distribution of year of marriage (1991-2010). The dots are the average total number of kids in the first 5 years of marriage; the vertical solid red line is when the policy was introduced, the solid black line is the linear fit, while dash grey lines are the confidence interval at conventional 95%. Descriptive evidence suggests a sharp discontinuity in a downward trend in the total number of kids born during the first 5 years of marriage, after the cut-off, i.e., 2002.

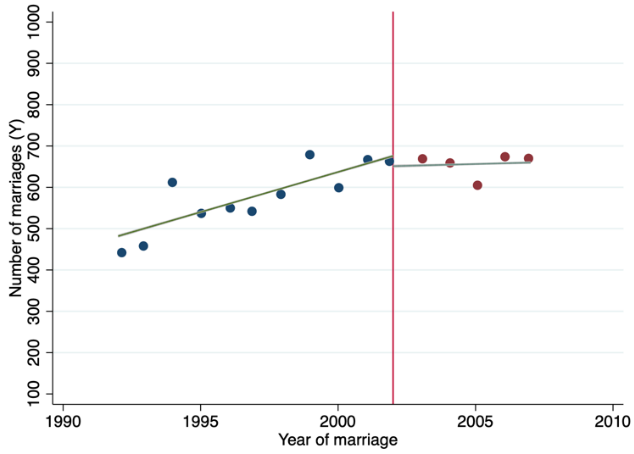
Figure 1 - Relationship between total kids at 5 years from marriage and year of marriage (1991-2007)



Ideally, we might want to observe female asset ownership relative to the whole distribution of year of marriage (1991-2010). Given the reform, timely asset ownership data might provide descriptive evidence on a sharp increase at the cut-off. Unfortunately, we are not able to rely on such data. However, to support the assumption that potential cofounders changes do not drive changes in the outcome at the cut-off or around the cut-off, we show that women's and their partner's relevant characteristics have a continuous distribution. Figure 2 shows that we cannot observe any discrete jump in correspondence of the cut-off for any relevant set of covariates available, such as women's education, husband's education, or age of marriage. Also, we cannot observe any discontinuity around the cut-off of the density of marriage by year. This evidence supports the key identifying assumption that the assignment variable - which in this case is the year of marriage - is random and exogenous and individuals were not unable to manipulate it.

Figure 2: Relationship between different covariates and year of marriage (1991-2007)





To enhance comparability, we further restrict the years of marriage for comparisons to women married between 1993 and 2007. To define a clear assignment to the eligibility status, we apply a "doughnut" that excluded women married in the years closest to the time cut-off of interest[43]. This strategy allows us to compare women whose fertility career was entirely under the old assignment rule for marital property (not treated) with those whose fertility career was entirely under the new one (treated).

We rely on Regression Discontinuity in Time (RDiT), in which the running or forcing variable is time, to estimate the reform's local average treatment effect (LATE) (Ito,2015; Hausman and Rapson 2018; Anderson 2014; Imbens and Lumieux, 2008).

Estimating equation is defined as:

$$y_i = \alpha + \beta treated_i + \vartheta_1 time_t + \vartheta_2 time_t * treated_i + +\gamma X_i + \varepsilon_i \; ; \qquad (1)$$

where y_i is the outcome of interest, namely total kids in the first 5 years of marriage, $treated_i$ is a dummy variable equal to one if in the year that women *i* get married she gets access to marital ownership immediately after marriage, i.e., after 2002. Zero otherwise (i.e., before 2002). $\vartheta_1 time_t$ and $time_t * treated_i$ are flexible functions of time and should capture every factor that changes the outcome smoothly around the cut-off. Time has been normalised to zero in the year of marriage the policy changed. X_i are further controls that shape reproductive behaviour and patterns such as household's wealth quintile, rural area, ethnicity, women's education, religion, the difference in age with husband and husband's education (among others: Aryal, 1991; Choe, *et al.*,2005; Maitra, 2004; Satyavada & Adamchak, 2000); ε_i is the error term.

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[43] Results hold also removing the "doughnut".

Our coefficient of interest is $\boldsymbol{\beta}$, which captures the immediate effect of increased access to property rights. $\boldsymbol{\beta}$ provides unbiased estimates under the assumption that ε_i does not change discontinuously at the cut-off (Anderson, 2014; Pfeiferet al., 2020) and that any potential endogenous relationship between ε_{it} and the year of marriage is eliminated. $\boldsymbol{\vartheta_1}$ provides a general time trend in the outcome for the whole sample, while $\boldsymbol{\vartheta_2}$ accounts for a change in the trend induced by the increase in asset ownership, namely a post-treatment trend.

3.5.2 Before-After design on matched women

As an alternative empirical strategy, we combined non-parametric and parametric techniques (Ho et al., 2007). Matching strategy combines Coarsened Exact Matching (Iacus, King and Porro, 2012) and Entropy Balancing (Hainmueller, 2012) to achieve observational equivalence between treated and not treated women (Jones et al., 2019). Conditional mean independence relies on the *ignorability assumption,* i.e. conditional on a set of explanatory covariates, the only difference in means observed between the two groups is driven by the effect of the treatment. We then estimate the average treatment effect on the treated (ATT) through parametric regression models on the preprocessed data. With respect to mean comparisons between matched treated and not treated, combining parametric techniques with non-parametric techniques has the advantage of allowing control for further covariates and being robust to any misspecification.

For preprocessing, CEM has been used to achieve common support and exact matching on woman's educational level, husband's educational level and residence status in rural areas. Moreover, we included age at marriage coarsened into seven bands (with cut-offs at age 15, 16, 17, 18, 19, 20, 22 and 25 years).

To further balance the distribution of additional potential confounders, we implemented Entropy Balancing on the CEM covariates and ethnicity and religion binary variables. We also included first-order interactions between variables to account for their co-moment distribution.

Women who get married after 2002 (i.e., treated) are on average older at the time of marriage, more educated, have a lower difference in age with husband, and have more educated husbands than women not treated. Nevertheless, descriptive statistics in Table 2 provide evidence on the equivalence achieved between treated and not treated after our combined matching strategy.

In this case, we estimate the Equation:

$$y_i = \alpha + \beta treated_i + \gamma X_i + \varepsilon_i \tag{2}$$

where y_i is the outcome, namely total kids in the first 5 years of marriage, $treated_i$ is a dummy variable equal to one if in the year *t* that women *i* get married there was immediate access to marital ownership upon marriage (namely after 2002). Zero otherwise (namely before 2002). β is the average treatment effect on the treated (ATT) of marital property access right after marriage and the coefficient of interest. X_i are further controls (such as district fixed effects and year of birth fixed effects).

Table 2: Balancing on observables

	(1) Unbalanced not treated mean	(2) Unbalanced treated mean	(3) Diff (1)-(2)	(4) Balanced not treated mean	(5) Balanced treated mean	(6) Diff (4)-(5)
Age at marriage	**17.11**	**17.75**	**-0.64*****	17.75	17.75	-0.00
Difference in age with partner	**4.37**	**4.00**	**0.37****	4.16	4.00	0.16
Ethnicity: hill brahmin	0.12	0.12	0.00	0.12	0.12	0.00
Ethnicity: hill chhetri	0.24	0.24	-0.01	0.24	0.24	-0.00
Ethnicity: terai brahmin/chhetri	0.01	0.01	0.00	0.01	0.01	0.00
Ethnicity: other terai caste	0.10	0.09	0.00	0.09	0.09	-0.00
Ethnicity: hill dalit	**0.10**	**0.12**	**-0.02****	0.12	0.12	-0.00
Ethnicity: terai dalit	0.03	0.03	0.00	0.03	0.03	-0.00
Rural	0.55	0.56	-0.01	0.56	0.56	0.00
Highest educational level	**0.70**	**1.18**	**-0.49*****	1.18	1.18	0.00
Education in years	**2.67**	**4.63**	**-1.97*****	4.53	4.63	-0.11
Religion: hindu	0.87	0.87	-0.01	0.87	0.87	0.01
Religion: muslim	0.04	0.03	0.01	0.03	0.03	-0.01
Religion: kirat	0.01	0.02	-0.00	0.02	0.02	-0.00
Religion: christian	0.02	0.02	-0.00	0.02	0.02	-0.00
Highest edu level husband	**1.43**	**1.68**	**-0.25*****	1.68	1.68	0.00
Education in years husband	3.14	3.13	0.01	3.20	3.13	0.08

Note: Variables in bold if difference in means statistically significant at 95% CI. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.6 Results

3.6.1 Main results

Results from RDiT estimates are provided in Table 3.

Column 1 provides estimates controlling only for rural area and district fixed effects. Column 2 controls for rural area, district fixed effects, and age of marriage, while Column 3 introduces additional covariates such as women's education, husband's education, household's wealth quintile, difference in age between spouses at marriage, ethnic and religious dummies. Further results controlling for year of birth fixed effects to account for potential time-invariant heterogeneity across cohorts are provided in Appendix.

Estimates of the immediate effect of access to marital property rights upon marriage are described by the parameters of interest: $\boldsymbol{\beta}$ and $\boldsymbol{\vartheta_2}$. $\boldsymbol{\beta}$ estimates the immediate effect of the female increase in access to property rights, while $\boldsymbol{\vartheta_2}$ accounts for the potential change in the time-trend throughout the post-reform period caused by the increase in asset ownership.

We find that the effect of access to marital ownership upon marriage and the impact on the time trend throughout the post-reform period shows a significant decrease in total kids born in the first 5 years of marriage, with a jump of about -0.091 ($\boldsymbol{\beta}$) or -5.48% in relative terms, and a significant drop in the post-reform trend of additional -0.041 per year ($\boldsymbol{\vartheta_2}$). These findings suggest that increased property rights for women induced a sizable and statistically significant reduction in fertility outcomes.

Results remain sizable and significant irrespectively of the specification chosen.

Results from before-after comparison on matched women are provided in Table 4. Column 1 provides the simple ATT, while Column 2 controls for time-invariant heterogeneity across cohort and districts through district fixed effects and year of birth fixed effects. Results are statistically significant at 95 % (or above) Confidence Interval. Estimates of the immediate effect of access to marital property rights upon marriage are described by the parameters of interest $\boldsymbol{\beta}$. On average, treated women have 0.115 fewer children in the first 5 years of marriage, a relative size effect of 6.85 %.

Table 3

Tot kids at 5yrs from marriage	(1)	(2)	(3)
ϑ_1	0.003	0.004	0.004
	(0.011)	(0.011)	(0.010)
β	**-0.088****	**-0.091****	**-0.074***
	(0.041)	**(0.040)**	**(0.040)**
ϑ_2	**-0.041*****	**-0.041*****	**-0.038*****
	(0.013)	**(0.013)**	**(0.013)**
Age marriage	No	Yes	Yes
District fixed effect	Yes	Yes	Yes
Rural	Yes	Yes	Yes
Further controls	No	No	Yes
N	6639	6639	6639

Note: Variables in bold if statistically significant. Standard error in parenthesis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.0$

Table 4

Tot kids at 5yrs from marriage	(1)	(2)
β	**-0.177*****	**-0.115****
	(0.018)	**(0.038)**
District fixed effect	No	Yes
Year of birth fixed effect	No	Yes
N	6628	6628

Note: Variables in bold if statistically significant. Standard error in parenthesis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.6.2 Placebo treatment

To assess further potential confounders around the cut-off and the plausibility of RDiT, we provide placebo estimates using "placebo treatment", which is expected, a priori, to have no effects on the outcome. "Placebo treatment" is defined by all cut-off years from 1997 to 2008. As shown in Table 5, β, our parameter of interest, is never statistically significant for treatment defined by cut-off years different from 2002. Placebo analyses are reassuring for our estimation strategy.

Table 5

Tot kids at 5yrs from marriage	1997	1998	1999	2000	2001	**2002**	2003	2004	2005	2006	2007	2008
ϑ_1	0.020	0.015	-0.000	0.006	-0.001	0.004	-0.016**	-0.006	-0.023**	-0.037***	-0.061****	-0.094***
	(0.012)	(0.012)	(0.011)	(0.011)	(0.010)	(0.011)	(0.007)	(0.009)	(0.007)	(0.007)	(0.009)	(0.013)
β	-0.013	-0.040	-0.005	-0.056	-0.057	**-0.091****	-0.004	-0.047	0.023	0.028	0.021	-0.062
	(0.045)	(0.044)	(0.043)	(0.041)	(0.039)	**(0.040)**	(0.037)	(0.038)	(0.040)	(0.040)	(0.048)	(0.049)
ϑ_2	-0.044**	-0.038**	-0.033**	-0.032**	-0.034***	**-0.041*****	-0.044***	-0.078***	-0.096***	-0.125***	-0.132***	-0.114***
	(0.015)	(0.014)	(0.013)	(0.013)	(0.012)	**(0.013)**	(0.012)	(0.012)	(0.014)	(0.014)	(0.021)	(0.024)
Age married	Yes	Yes	Yes	Yes	Yes	**Yes**	Yes	Yes	Yes	Yes	Yes	Yes
District fixed effect	Yes	Yes	Yes	Yes	Yes	**Yes**	Yes	Yes	Yes	Yes	Yes	Yes
Rural	Yes	Yes	Yes	Yes	Yes	**Yes**	Yes	Yes	Yes	Yes	Yes	Yes
N	6051	6272	6404	6503	7379	6639	6860	7249	6606	6454	5595	5050

Note: Variables in bold if statistically significant. Standard error in parenthesis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.6.3 Abortion rights

Other policies affecting fertility outcomes and introduced in the time window considered might challenge our identification strategy. For this reason, we conducted an exhaustive overview of the policy changes introduced around the cut-off.

Increased birth controls rights might be a relevant policy change in our setting. The 11th amendment of the Legal Code of Nepal (*Muluki Ain*) legalised abortion for a broad range of conditions in March 2002 and received Royal Assent in September that year. In December 2003, the Procedural Order for implementing the new legislation was passed, enabling services to begin. The service became effective in 2004 and was implemented for the first time in 2004 in Katmandu only. The service spread across the country some years later but remained a very limited practice. As of April 30, 2006, Comprehensive Abortion Care (CACs) were only 122 in the country: 76 government-run and 46 non-governmental organisations run facilities (MHP and CREHPA 2006).
With a female population of roughly 13 million in 2006 (World Bank, 2006), 1 CAC every almost 105.000 women. Moreover, the cost of abortion in a CAC was estimated between USD 11.33 and USD 28.33 (MHP and CREHPA, 2006; Valente 2014), representing up to almost 50% of monthly mean household income (Central Bureau of Statistics, 2004).

Since its introduction, multiple barriers hinder women's actual right to abortion: low cross-country availability and relatively high cost also limited its potential benefits in terms of control over fertility. For example, in 2007, *Lakshmi Dhikta,* a group of activist lawyers, raised the case of the unaffordability of abortion service to the Nepal Supreme Court, forcing the Court to order the Government enforcement of the abortion right in 2009, 5 years after its approval (Giri, 2002).

These low take-up rates taken with results from placebo estimates are reassuring regarding our estimation strategy: it is unlikely that our results are driven by birth controls rights rather than increased property rights.

3.7 Conclusion

Female asset ownership may be an important source of empowerment for women, also within marriage. Existing literature acknowledges the relevance of asset ownership for women's empowerment. However, the evidence on the dimension of fertility, which can be seen as an exercise of agency and decision-making over sexual domain and control over body, lacks causal investigation. We contribute to this literature by exploiting an exogenous source of variation in the female property right in Nepal for a regression discontinuity in time analysis. We are able to establish the causal link between pro-women legal innovation regarding female property rights and fertility. We implemented a before-after design on matched women as an alternative estimation strategy.

In a nutshell, we show that access to asset ownership right after the marriage has a significant negative effect on the number of offspring in the first five years of marriage, irrespectively of the estimation strategy chosen.

Potential "feminism" movements might have been driven by reduced birth and pro-women legal innovation, even though as Becker argued, "women's movement is primarily a response to other forces that have dramatically changed the role of women rather than a major independent force in changing their role" (Becker,1981, p. 251) and placebo analyses on almost 10 years around the reform are reassuring with respect to the plausibility of our findings.

A potential explanation of our results is that, under the assumption of collective modelling household decision-making, increased female property rights seems to increase female intra-household bargaining power and cause a general increase in women's empowerment. This might be particularly relevant for developing countries, where assets are crucial in the household's livelihood strategy and increasing their property rights are likely to have swift effects. From one side, if women have preferences towards smaller family sizes, increased property rights allow them to better bargain their preferences within marriage while increasing their agency. On the other side, increased property rights might increase the opportunity cost of having a child, so that a woman may want to devote more time to work and economic activity and foster female labour force participation.

Having less childbearing might reduce women's health risks related to pregnancy, allowing better "quality" children at the expense of "quantity". However, more research is needed to assess female asset-ownership implications regarding health and future generation outcomes.

Although institutional differences need to be considered, female asset ownership might have implications for the population dynamics of developing countries. Overall, this analysis suggests the critical connection between female property rights and fertility, favouring women's empowerment within marriage and potentially increasing their welfare.

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Appendix

Table 6

Tot kids 5yrs from marriage	(1)	(2)
ϑ_1	0.003	0.038***
	(0.011)	(0.012)
β	**-0.088****	**-0.101****
	(0.041)	**(0.047)**
ϑ_2	**-0.041*****	**-0.074*****
	(0.013)	**(0.014)**
Year of birth fixed effects	No	Yes
District fixed effects	Yes	Yes
Rural	Yes	Yes
N	6639	6639

Note Variables in bold if statistically significant. Standard error in parenthesis.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Conclusions

The study investigates the relevance of gender equality and intra-household bargaining power in *i)* health-related outcomes induced by partner's health shocks; *ii)* educational and professional achievements derived from increased women's empowerment stemming from complete birth control right; *iii)* fertility implications to asset ownership via increased intra-household bargaining power.

In detail, chapter one investigates whether gender specialisation within the household can account for asymmetric responses in terms of labour market outcome and informal care provision stemming from the partner's acute health shock. Evidence suggests that in the short run, both genders increase home production with no effect on both the intensive and extensive margins of labour market supply. Previous studies report contrasting results in the responses of women as opposed to the one of men (Colie, 2004; Johnson and Favreault 2001; Charles 1999) reflecting baseline labour market attachment (Berger 1983; Blau and Riphahn, 1999; Jimenez Martin et al., 1999), and in response to disability insurance eligibility and generosity (Berger and Fleisher, 1984; Chen, 2012). In my context, a potential explanation of the lack of asymmetric gender-based responses in labour market outcomes might be the presence of an efficient national healthcare system in the UK, together with a relatively generous social security coverage in terms of disability-related benefits. Additionally, a systematic increase in informal care provision seems to suggest an individual's caring preferences and a strengthened complementarity in leisure/home production induced by partner health shock, irrespectively from gender.

In chapter two, my analyses show that birth control innovation in the form of the contraception pill available on-demand to young unmarried women increases women's empowerment in Europe. These findings align with previous literature that focused on the US context (Goldin and Katz, 2002; Hock 2007; Bailey et al., 2012; Ananat and Hungerman, 2012; Edlund and Machado, 2015). In the European context, the only study available shows that birth control rights, measured as access to both abortion rights and the pill, are strongly linked to an increase in life satisfaction of women of childbearing age (Pezzini, 2005). Yet, the effect of birth control on females' labour market and educational achievements lacks extensive analysis in the European context. Nevertheless, Europe and US have large differences in historical and cultural background, profound differences in institutional settings, first and foremost in terms of access to health services and educational opportunities, but also with respect to gender-based discrimination and culture.

Despite historical relevance, my results are relevant to inform effective family planning policies on the importance of allowing women to better plan childbearing in the process towards gender equality and female empowerment. Although institutional differences need to be considered, my results show the importance of promoting access to reliable contraception in countries where birth control policies are not legal, most notably developing countries. Even though access to birth control is relevant also for marginalised sub-groups in developed countries as policies to increase access are required.

More empowered women regarding contraception might have future generations' implications by reducing the cost of avoiding unintended pregnancies. Planned children might have better health outcomes and improved outcomes later in life. Given the relative scarcity of empirical investigation between oral contraception and future generation implications, this topic represents an interesting avenue for promising future research.

In developing countries, women's voice in everyday decisions is minimal, including those on their own reproductive and sexual health, which are often made by their men. Chapter three shows how increased female property rights seems to increase female intra-household bargaining power and cause a general increase in women's empowerment regarding fertility decisions.

Although institutional differences need to be considered, female asset ownership might have implications for the population dynamics of developing countries. Overall, this analysis suggests the important connection between female property rights and fertility, favouring women's empowerment within marriage and potentially increasing their welfare. Having fewer children might reduce women's health risks related to pregnancy, allowing better "quality" children at the expense of "quantity". However, more research is needed to assess female asset-ownership implications in terms of health and future generation outcomes.

Overall, in this thesis, I strengthen and support gender knowledge in developed and developing countries. Climate change and different shocks, such as the economic crisis and COVID-19, negatively affected the global economy and the life of women and men, calling for additional attention on the national and international development arena. The direct and indirect effects of shocks are conditioned by inequalities between and within countries, hence reducing social and economic exclusion of the most vulnerable groups, most notably women, is required. To overcome emerging challenges, reduce inequalities, and promote inclusive, sustainable development, the new evidence can support the development of policies and interventions where the gender dimension is systematically incorporated. Public policies fostering gender equality and women's empowerment represent an urgent priority and a required investment.

Estratto per riassunto della tesi di dottorato

Studente: Annarita Macchioni Giaquinto matricola: 956375

Dottorato: Economia

Ciclo: XXXIII

Titolo della tesi: *Gender inequality and intra-household bargaining power: empirical evidence from developed and developing countries.*

Abstract: This thesis sheds new light on the link between gender inequality and intrahousehold bargaining power with respect to health-related outcomes, human capital, labour market and fertility.
Gender specialisation and intrahousehold bargaining power in income production, as opposed to home production, might be responsible for asymmetric responses to partner's health shock in terms of labour market and informal care. However, results show that no evidence emerges for gender-based behavioral responses driven by gender specialisation. Collective modelling of household decisions predicts that a change in bargaining- power resulting from a change in the distribution and control of resources, such as assets, might affect fertility outcomes. Increased female asset ownership induced a sizable and statistically significant reduction in fertility and has demographic implications for developing countries. Birth control rights and oral contraceptives have the potential to reduce gender inequality and increase women's empowerment. Historically, in Europe, the pill induced a significant and sizable increase in women's educational attainments and in labour market outcomes due to increased control over life choices.

Abstract (Italiano): La seguente tesi fornisce nuova evidenza sulla relazione tra disuguaglianze di genere e *intrahousehold bargaining power* nel contesto della salute e sue conseguenze, accumulazione di capitale umano, mercato del lavoro e decisioni di fertilità. La specializzazione di genere all'interno della famiglia e *intrahousehold bargaining power*, infatti, possono essere responsabili di una risposta asimmetrica tra uomo e donna nel mercato del lavoro e in quello di cura conseguenti a uno shock di salute del partner. Tuttavia, i risultati ottenuti non suggeriscono un comportamento indotto dallo shock basato sul genere. Modelli famigliari collettivi suggeriscono che cambiamenti nel potere contrattuale femminile all'interno del contesto famigliare come quello derivante da acquisizione di *assets* potrebbero influire sulle decisioni di fertilità. Un aumento degli *assets* induce una significativa riduzione di fertilità e potrebbe avere implicazioni demografiche per i paesi in via di sviluppo. Il diritto di controllo sulle nascite e l'accesso alla contraccezione orale hanno il potenziale di ridurre le disuguaglianze di genere e aumentare l'*empowerment* femminile. In Europa, storicamente la pillola contraccettiva ha indotto un aumento significativo nei risultati delle carriere scolastiche e lavorative delle donne come conseguenza di un maggiore controllo sulle proprie decisioni.

Annarita Macchioni G