Couples and Gender Inequality*

Kristian Urup Olesen Larsen†

[Latest version]

Revised August 2023

This study shows that entering into a cohabiting partnership leads to long lasting earnings losses for women, even after accounting for the impact of child penalties. While ⅔ of the total effect of cohabitation is due to partnered women having higher fertility than unpartnered women, an income penalty of 5% is directly due to cohabitation, and not due to children. Cohabitation also reduces women's propensity to work evenings and weekends, and to hold a second job. The effects are temporary, and disappear if couples dissolve. I investigate household specialization and gender norms as potential mechanisms, and find that while specialization is unable to account for the effect, intergenerational transmission of traditional gender norms predicts the magnitude of the penalty. My analysis relies on a unique identification of cohabiting but otherwise informal partnerships in Danish administrative data.

Keywords: Cohabitation, Gender Inequality, Family Formation
JEL Codes: D63, D13, J12, J13, J16.

*I thank Claus Thustrup Kreiner and Søren Leth-Petersen for their supervision; Natalia Zinovyeva, Cormac O'Dea, members of CEBI, and especially the CEBI PhD students for their many helpful comments. The activities of CEBI are financed by the Danish National Research Foundation grant DNRF134.

†Center for Economic Behavior and Inequality (CEBI), University of Copenhagen. Øster Farimagsgade 5, 1353 Copenhagen K, Denmark. Email: kuol@econ.ku.dk.

One of the defining characteristics of the last century has been the convergence in earnings between men and women in developed economies (Goldin, 2014). A wide range of mechanisms have been pointed out as drivers of the historical development (see Blau and Kahn (2017) for a recent survey), of which a large part revolve around the changing options and choices of families.[1] Still, the direct effect of living as joint households[2] on gender inequality in earnings is unknown. Recent research have documented large effects of children — predominantly a family related life event — on women's earnings both in the short and long term (Angelov, Johansson and Lindahl, 2016; Lundborg, Plug and Rasmussen, 2017; Kleven, Landais and Søgaard, 2019), but not all couples get children, and living in couples might have effects on gender inequality that are separate from those of children.

In this paper I study partnership formation as a distinct source of gender inequality in earnings. My study is made possible by a unique identification of cohabiting couples in the Danish registry data, which is made by Statistics Denmark for use in official statistics on households, families and children.[3] This measure includes couples who are not married or registered, as long as they cohabit. In the analysis I include individuals who form their first couple between 1985 and 2016, totaling 744,000 couple formations, each involving two individuals. Combined with the rich Danish administrative data, I construct a panel of cohabiting individuals, covering most major life events including marriage, having children, and labor market activity, up to a 38-year period for each person.

My findings can be categorized into three broad points. First, I document cohabitation as a novel source of gender inequality. I show that when couples move in together they drive a wedge in their earnings, which lasts the duration of the partnership. Cohabitation alone costs women a *cohabitation penalty* of 5% of the annual earnings they would have earned if they had remained single. Because fertility is higher in

[1]For instance improvements in maternal health care (Albanesi and Olivetti, 2016) and access to oral contraceptives (Goldin and Katz, 2002; Bailey, 2006) changed the costs and risks related to having children. Reduced gender wage gaps (Attanasio, Low and Sánchez-Marcos, 2008) made work more attractive for women and technological improvements in household appliances freed time previously spent on household work (Greenwood, Seshadri and Yorukoglu, 2005) Other topics that have been explored in the literature include differences in human capital and occupations, and labor market discrimination.

[2]As opposed to living alone and acting purely based on individual preferences.

[3]The identification of couples relies on detailed information on individuals addresses and familial relations, which makes it possible to minimize the conflation with roommate-like living arrangements, as well as any cases of siblings, cousins and other family members living together.

couples, women who enter a partnership can also expect to lose another 9% of their annual earnings to the child penalty.[4] When combined, the direct cohabitation penalty and the effect of increased fertility generate a 14% average loss in earnings for women who enter a cohabiting partnership. Compared to the child penalty, the cohabitation penalty is approximately 1/3 the size. The cohabitation penalty also extends to labor supply. Using men as the baseline, cohabitation decreases women's propensity to work weekend- or night-shifts and to work overtime, and it decreases their likelihood of having a second job.

Second, I show the cohabitation penalty is reversible. In couples that separate, women's earnings converge to those of men in the years just preceding separation. Because men are generally unaffected by cohabitation, this is also the earnings level women would have expected to earn, had they remained single instead of entering a couple. This non-permanency of the cohabitation penalty makes it distinct from other well documented events such as unemployment shocks or having children, which have long-lasting effects beyond the initial shock.

Third, I investigate the relevance of two main theoretical mechanisms, household specialization (Becker, 1991) and gender norms (Akerlof and Kranton, 2000; Fernández, Fogli and Olivetti, 2004; Bertrand, Kamenica and Pan, 2015). At its core, the specialization hypothesis is the observation that when partners have different wages, frictionless transfers within the household allows for both partners to become better of by allocating the majority of market labor to the partner with a comparative advantage therein. To test its relevance in the setting of cohabitation gender gaps, I use that specialization is, by construction, genderless. Thus couples in which the woman is primary earner should be inclined to allocate household work to the man, and vice versa. This approach to testing for specialization is similar to ones used in other contexts (Lassen, 2021; Siminski and Yetsenga, 2022; Artmann, Oosterbeek and van der Klaauw, 2022). I find no evidence of specialization, with female primary earners suffering a cohabitation penalty that is quantitatively similar to the aggregate penalty.

I do find evidence suggesting that adherence to traditional gender norms are a driver of the cohabitation penalty. Concretely I show that the magnitude of the

[4] These 9% are the average effect across women who get children and those who do not. To convert to a figure comparable to the child penalty estimated by e.g. Kleven, Landais and Søgaard (2019) or Kleven (2022) it should be scaled by the fertility rate.

cohabitation penalty is strongly correlated with individuals' mothers' share of labor earnings in their childhood home. That is, individuals who grew up in traditional homemaker/breadwinner homes experience cohabitation penalties that are much larger than those who grew up in homes with inverted roles (i.e. where the mother brought home the majority of income). To strengthen this argument, I show that this correlation only exists for individuals whose parents have actually lived together, but disappears if the parents divorced early in the life of their child.

When interpreting the results, it is relevant to know if they are driven by unobservable expectations of having children in the future, or if the cohabitation penalty is driven by factors unrelated those expectations. To this end, I develop a double event design that jointly estimates event-style coefficients along two axes (time to cohabitation and time to getting kids). I prove that assuming individuals form their fertility beliefs rationally, this double event design controls fully for anticipation effects. I discuss the validity of this assumption, and show empirically that fertility anticipation is unlikely to be driving the effects.[5]

My paper contributes to the large and overlapping literatures on gender inequality in earnings (see Bertrand (2011); Olivetti and Petrongolo (2016) and Blau and Kahn (2017) for reviews) and families economic behavior (Mincer and Polachek, 1974; Albanesi, Olivetti and Petrongolo, 2022).[6] Methodologically I borrow from and contribute to the recent literature on the earnings losses associated with children (Angelov, Johansson and Lindahl, 2016; Lundborg, Plug and Rasmussen, 2017; Kleven, Landais and Søgaard, 2019; Kleven et al., 2019; Cortés and Pan, 2020) and the underlying causes for those earnings losses (Kleven et al., 2020; Kleven, 2022). I contribute conceptually by showing that couple formation is in itself an independent channel driving gender inequality, which, because it almost universally precedes children, has gone unnoticed, hidden in the pre-trends of child-penalties. I further document gender norms as the likely driver in a setting adjacent to that of children.

[5]Sans the special case where fertility beliefs are entirely orthogonal to realized fertility, in which case my empirical tests cannot distinguish between no fertility anticipation and no *rational* fertility anticipation.

[6]Much of the literature on households economic decisions and the influence of households on female labor supply is structural in nature (for instance Basu, 2006; Mazzocco, 2007; Pollak, 2013; Chiappori and Mazzocco, 2017; Jakobsen, Jørgensen and Low, 2022). While I do not provide any direct contributions to this strand of literature, my results may nevertheless be of interest to those using such structural models in their research.

In terms of subject matter, my paper is closely related to the literature on marital wage premiums. While one strand of literature have documented a marriage premium for male wages (Ginther and Zavodny, 2001; Antonovics and Town, 2004; Bardasi and Taylor, 2008) as well as a marriage penalty for women (Juhn and McCue, 2016), others attribute the effects to selection into marriage (see e.g. Jakobsson and Kotsadam, 2016). I update the analysis to a time when non-married cohabitation is increasingly relevant as the de facto timing of family formation (Kuperberg, 2019), and I am, to the best of my knowledge, the first to employ an event-study methodology to address these questions. Doing so permits clear graphical evidence on both selection into partnership formation and the gender-specific effects of partnerships. As described, I find significant negative effects on women's earnings, but little on men. This is at odds with the existence of a male marriage premium, although it should be noted that cohabitation and marriage are different, especially in the degree of long-term commitment they imply (Voena, 2015; Aldén et al., 2015).

I also relate to the literature on the mechanisms of households economic behavior and intra-household gender inequality (Zinovyeva and Tverdostup, 2021; Daly and Groes, 2017), especially the literature on the role of household specialization (Artmann, Oosterbeek and van der Klaauw, 2022; Hersch and Stratton, 2002; Foged, 2016) and the literature on the influence of gender norms on household's division of labor (Fortin, 2005; Baker and Jacobsen, 2007; Bursztyn, González and Yanagizawa-Drott, 2020; Giuliano, 2020; Bertrand et al., 2021). I contribute by evaluating the role of specialization and gender norms for the cohabitation penalty, and provide evidence indicating gender identity norms are important for inequality within households.

The paper proceeds as follows. Section I describes my data, as well as some relevant details of the Danish setting. Section II first describes the econometric strategy I use to estimate the effect of cohabitation and then presents structural conditions under which the estimated effects can be interpreted as direct, and not driven by fertility anticipation. Section III presents the key results that document the cohabitation penalty, and section IV dives into the possible mechanisms driving it. Section V concludes.

I. Data and Institutional Details

The subsequent empirical analysis is based on administrative registers from Denmark covering the full population from 1980 to 2018. The data contains third party reported information on earnings and labor supply as well as detailed information on individuals family background and place of residence. Uniquely, this dataset contains family identifiers which not only cover married and formally partnered couples, but also identify informally cohabiting partnerships based on information on shared residence and (a lack of) familial links. This feature of the data offers a big improvement compared to using marriages as proxies for partnership formation, since the two have become increasingly decoupled in recent decades (Stevenson and Wolfers, 2007).

The partnership identifiers are constructed by the Danish statistical bureau — Statistics Denmark — and are used to construct the official statistics on "households, families and children". Partnerships identify all pairs of individuals living together where those two persons are either married, in a civil union (used before the legalization of same-sex marriage in 2012) or have children together. Additionally, a pair of cohabiting individuals are considered partners if they are of different sex, within 15 years of age, share no familial links and live together without any additional residents at their address. This definition is likely to cover the vast majority of couples, but does suffer from two issues. First, it excludes informal same-sex couples (to avoid conflation with cohabiting roommates), why I focus on heterosexual couples in my analysis. Second, the partnership identifier erroneously classifies roommate arrangements that specifically consist of one man and one woman living together as partnerships. Assuming the effect of cohabiting on these mixed gender roommate arrangements is zero (or just smaller than the effect in true partnerships), including them in my analysis will tend to bias the estimated effect sizes towards 0, working against finding any effects of cohabitation.

A. Sample Selection

Through life people might have more than one cohabiting partnership, and the circumstances of individuals second or third partnerships might differ substantially from the circumstances during their first partnership. Children are more likely to be present and individuals have more experience with, and are potentially more skilled at, living with a partner. In this analysis, I focus on individuals first ever cohabiting partnership with a duration of at least 3 years. I omit cohabitation spells with a very short duration because these are more likely to capture roommate-like arrangements and other types of cohabitation where there is no actual relationship. To ensure that the cohabitation spells are indeed the first in peoples lives, I limit the sample to people who are observed in the data when they are 18 years old. I also require that individuals are observed for at least five year before the beginning of their partnership and one year after (event times -5 to $+1$). Couples who break up are additionally required to remain in the data from one year before breaking up until five years after. Couples that still exist by the end of my data window are assigned an arbitrary partnership duration of 99 years and kept in the data.[7] These restrictions exclude individuals who arrive in Denmark and quickly enter a partnership as well as those who either leave Denmark after breaking up or die.[8] In both cases I do so because these types of individuals do not tend to have the same association with the labor market as the general population.

While information on earnings, labor supply and government transfers are 3rd party reported, addresses are self reported and may therefore be manipulated. Generally there are no incentives to misreport ones address, and failing to update it when moving can trigger a fine. However, some high school applicants and students might benefit from manipulating their registered address because of high school admission requirements and an increase in the size of student grants given to high school students who do not live with their parents. While most of this manipulation happens by moving address to family members who live closer to a desired school, I cannot rule out that such manipulation will occasionally appear as cohabiting partnerships in my data. Therefore, I only consider partnerships that last three years or longer, and where both partners were 18 or older at the start of the partnership. Some social transfers might also be adjusted based on household income, incentivizing address manipulation. In these cases the incentive generally encourages individuals to report as if they live alone, even if they live with a partner. These couples will be missed in the analysis, but unless these specific couples have effects that are very different from

[7] The choice of 99 as partnership duration for couples who do not break up within the data period is inconsequential since I handle couple durations non-parametrically in the following analysis.

[8] Given the requirement to observe individuals at age 18 and the span of the data, these will be individuals who pass away at age 56 or younger, making them a small group of individuals.

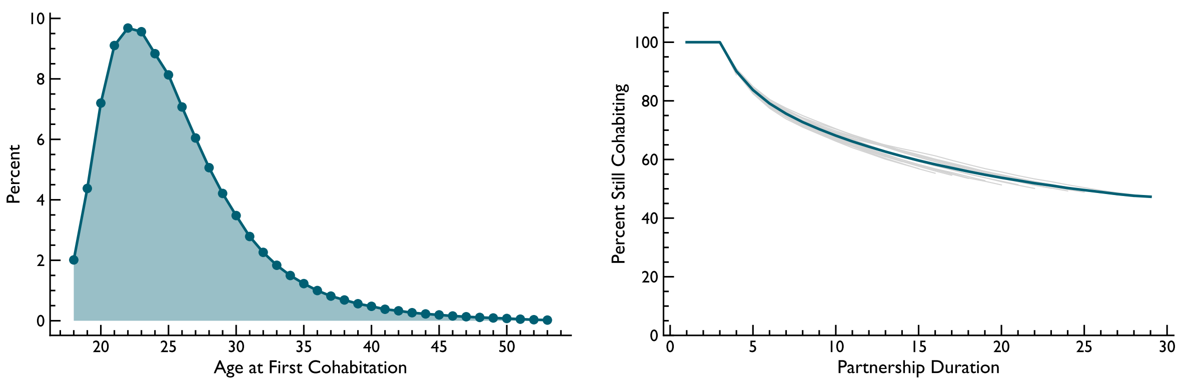
the remaining population, excluding them will not influence my results.

B. Descriptive Statistics

Figure 1 characterizes the partnerships recorded in the data by plotting the age distribution of individuals when they begin their first partnership (panel (a)) and the share of partnerships that remain together over time, up to a duration of 29 years (panel (b)), which is the maximum duration observable due to the sampling criteria. Of all the cohabiting individuals in the sample, 80% have formed their partnership before age 30, with the remaining 20% finding their partner later. Partnerships are on average long-lasting. It takes 12 years for more than 1/3 of partnerships to have dissolved and at the maximum horizon of 29 years observable in the data, 47% still live together.

Figure 1: *Partnership Characteristics*

(a) *Age Distribution* **(b)** *Partnership Durations*



Note: Descriptive figures. Panel (a) show the empirical distribution of ages of partners in the year they begin their first cohabiting partnership. Panel (b) plots the percent of partnerships still existing at varying durations. The light gray lines plot the partnership survival probability separately for different years of partnerships starting, and the blue line averages across these lines.

Table 1 reports sample averages split by gender, all of which are measured the year that individuals begin their cohabitation spell. The dataset contains approximately 600,000 individuals of each gender. Women are on average 24 years old when they begin their cohabitation spell. Men on the other hand are typically two years older at 26. Consequentially women are more likely to be enrolled in education than men at 27% for women compared to 15% for men. Because the partners are not required to both simultaneously satisfy the sample inclusion criteria, there are slight differences in the characteristics of the partnerships between men and women. Women's first partnership

Table 1: *Sample Averages*

	Women	Men
Demographics		
Individuals	612,046	588,674
Age	24.642	26.485
Enrolled in Education (%)	27.093	15.376
Partnerships		
Age Difference	2.449	1.383
Partnership Duration (years)	7.585	7.405
Gets Married (%)	62.059	61.518
Gets Children Together (%)	78.112	80.371
Gets Children (%)	85.288	83.172
Children From Earlier (%)	14.768	13.493
Years to Birth of First Child	1.948	2.204
Income & Labor		
Yearly Labor Income	135,255	211,321
Partners Yearly Labor Income	212,659	134,977
Unemployed (%)	14.727	8.874

Note: The table shows descriptive statistics separately for men and women. In all cases where the characteristics are time-varying, they are measured in the year that the individual begins cohabiting with their first partner.

tends to have slightly larger age differences, but the partnerships are otherwise almost identical in terms of couple attributes, marriage rates and fertility, with men apparently delaying children slightly longer in their first relationship than women do. The fact that women are more likely to be enrolled in education or transition from education to the labor market is also reflected in their income and unemployment rates.

II. Measuring Responses to Cohabitation

A. Econometric Approach

Under idealized conditions cohabitation could be randomized over the life cycle to allow for studying its effect on earnings. Because such randomization does not exist naturally and is not feasible to implement experimentally, existing studies of the effect of household formation try to account for selection into cohabitation (in practice, into marriage) using econometric methods such as panel fixed effects and switching models (Ribar, 2004). One exception to this trend is Ginther and Zavodny (2001), who use shotgun weddings as a natural experiment to show that men who marry conventionally, and men married by shotgun wedding have similar marriage premiums, suggesting selection only play a minor role in the male marriage premium. Still there is a lack of convincing evidence of the effects of cohabitation for a number of reasons. First, the decision to cohabit with a partner is likely endogenous to hard-to-observe variables that relate to (time evolving) personality traits and individuals exposure to "marriage markets" through e.g. attending education.

Second, most of the existing literature considers the effect of marriage, not cohabitation. Because cohabitation generally precedes marriage by several years, estimates using marriages as markers of couple formation measure something fundamentally different from the direct effect of living with a partner.[9] This discrepancy has increased in importance over time as extramarital cohabitation has become more common.

I focus on the gender gap in earnings that is caused by cohabitation. To estimate this, I follow the event study methodology of Kleven, Landais and Søgaard (2019). Letting $-5 \leq t \leq 10$ denote for each individual the time in years that has elapsed since they began their first cohabiting partnership, with negative values indicating that this relationship has yet to begin, I estimate separate regression equations of the form

$$y_{it}^{g} = \sum_{j \neq -2} \tau_j^g \mathbf{1}_{(t=j)} + \mathbf{C}_{it}^{g} + \mathbf{D}_{it}^{g} + X_{it}^{g}\beta^{g} + \nu_{it}^{g} \tag{1}$$

for each gender $g \in \{m, f\}$. Here τ_j^g's measure the effect of interest, i.e. the effect of cohabitation across event time. X_{it}^g is a set of controls, typically including variables controlling for children and $\mathbf{C}_{it}^g$ is a full set of cohort specific age profile fixed effects, meaning I rely on differences in the timing of cohabitation to identify the main effect. $\mathbf{D}_{it}^g$ is a set of event coefficients measuring time until the couple breaks up. Without breakup event coefficients, breakups will mechanically confound my results because cohabitation is not always permanent. As shown in appendix B including $\mathbf{D}_{it}^g$ in the regression ensures that the τ_j^g coefficients measure the effect of cohabitation conditional on individuals living together. Without $\mathbf{D}_{it}^g$, the τ_j^g's would be biased towards 0 as more and more people separate from their partner.

In addition to baseline event regressions, I consider a modification of the same model which summarizes the effect over the first five years of cohabitation. Defining a

[9]What exactly is being measured by the marriage literature is debatable. In countries where marriage offers additional legal protections in case of divorce, or where marriage is for cultural or legal reasons binding for life, the decrease in uncertainty about the future likely plays a role as it permits more planning. Depending on the exact research design, the effect of marriage might also include the effects of cohabitation by itself, as well as effects of increased fertility.

post cohabitation dummy $T_{it} = \mathbf{1}_{(t \geq 0)}$ and an indicator for females F_i, and restricting the sample to cover only five years before and after cohabitation, these models are of the form

$$y_{it}^g = \delta^g F_i + \alpha^g T_{it} + \tau^g T_{it} F_i + \mathbf{C}_{it}^g + \mathbf{D}_{it}^g + X_{it}^g \beta^g + \nu_{it}^g \tag{2}$$

with the parameter of interest being τ^g. In the interest of simplicity, I estimate most results separately for men and women without accounting for intra-household aspects. In the situations where specifically the within-couple gender gap is the object of interest, I adjust the above method by replacing the gender-specific variable y_{it}^g with a within couple gender gap $y_t^f - y_t^m$ as dependent variable in the regressions, which is then defined over a sample of couples instead of over a sample of individuals. Compared to the separate method, this allows me to study within couple gender gaps, but looses the opportunity to attribute changes to either the male or female, since this method does not differentiate between either gender contributing to the total gender gap.

In the context of child-penalties the above regressions must typically be estimated in levels to preserve individuals with zero income due to maternity leave, and percentage effects can instead be recovered by computing

$$P_t = \frac{\hat{\tau}_t^f - \hat{\tau}_t^m}{E[\tilde{y}_{it}^f | t]}, \tag{3}$$

that is, the gender gap scaled by average predictions $\tilde{y}_{it}^f$ from a model of women's earnings like the one described in equation (1), but which omits the event time coefficients from the estimation. Like in the case of children, I avoid using log transformed variables, because many individuals have children soon after starting cohabitation. Instead, I pursue two approaches to quantify relative effects. First I apply the described scaling to estimate effects relative to women's counterfactual earnings. Second, whenever I estimate within household effects, I have the option of scaling not by individuals own counterfactual earnings, but by the households total outcome. Doing so, the within household gender gap in percent of household income becomes

$(y_t^f - y_t^m)/I_t^{HH}$ which I occasionally use as outcome in couple-level regressions.[10]

Another concern relating to children is the potential for reverse causality such that having children together drives people to also live together. If such cases are common it becomes less clear if fertility should be thought of as a mediator of the cohabitation penalty or the other way around. Empirically figure C.4 reveals that nearly all the response in fertility occurs in the years after cohabitation begins, meaning reverse causality from fertility to cohabitation decisions is unlikely to be an issue.

The advantages of the event study approach are at least threefold. First, focusing on the gender gap in earnings loosens the requirements for identification of a causal effect compared to the methods commonly applied in the literature on marriage premiums. Whereas previous studies on the earnings effect of cohabitation must assume to have sufficiently controlled for unobservables that drive both entry into partnerships and trends in outcomes, I rely on an assumption that such unobservables evolve similarly between men and women along the event time axis. I can informally test the degree to which this is true by graphically inspecting the pre-trends in my results, as differences in the selection mechanism between men and women would show up here. Second, comparing event studies between subgroups (e.g. men and women) has the same interpretation as difference-in-difference estimates with group specific fixed effects, meaning the results can easily be interpreted as gender gaps. Because men, as it turns out, are hardly affected by cohabitation, the difference between coefficients for men and women can be interpreted as the effect of cohabitation on women. Third the event studies trace out the full dynamic path of outcomes around event time, which means they can simultaneously show the effect of cohabitation (post event time zero), and the dynamic selection into cohabitation (pre event time zero), which is permitted as long as it is identical for men and women.

B. Anticipated Fertility

A first order effect of cohabitation is that it increases fertility significantly over the baseline fertility of singles (see figure C.4). This means women in couples are more likely to be affected by a child penalty, which can cost them a substantial fraction

[10]For the majority of households, $I_t^{HH} \approx y_t^f + y_t^m$, but total income also encompasses transfers and other types of non-labor income, making it a more stable measure of the resources available to the household.

of pre-child earnings (Kleven, Landais and Søgaard, 2019). Anticipating this, the increased fertility among couples might also affect women (and men) before they have children. For example, many high-paying jobs feature some degree of dynamic returns to effort (Kleven et al., 2023). Since having children might prevent women from reaping these rewards, the increase in fertility will tend to drive women out of those high-paying jobs when they commence cohabitation.

When interpreting the cohabitation penalty estimates, it is important to know if the effect is predominantly running through this anticipated fertility channel, or if other direct effects of cohabitation also contribute to the total effect. In many cases the main estimate of interest is one that removes the influence from fertility, since the fertility related effects are better interpreted as child penalties. Controlling for the realized fertility pathway is straight forward, because children are observable and can simply be included as controls. But because fertility beliefs are unobservable, there is no easy way to control for the fertility anticipation pathway.

To make progress, I invoke an assumption of *rational fertility anticipation* which puts structure on how fertility beliefs relate to realized fertility. The following proposition defines this assumption mathematically (in (4)) and states an ensuing regression model which can control for fertility beliefs given the rationality assumption.

Proposition 1 (Controlling For Rational Anticipation)**.** *Let $e_{it}(x)$ be an individual's fertility beliefs at time t about fertility x periods in the future. Assume $e_{it}(x)$ i) affects y_{it} linearly and ii) is formed rationally, meaning beliefs obey*

$$\mathbb{E}\Big(e_{it}(x) \mid c_{it}, k_{it}, \{k_{it+x} \mid x \in 1, 2, ...\}\Big) = k_{it+x}, \tag{4}$$

where c_{it}, k_{it} and k_{it+x} are indicators for respectively cohabitation, contemporaneous children and future children at time $t + x$. Then a regression model featuring dual event axes, one measuring cohabitation event time t and another measuring child event time $t - E_i^k$,

$$y_{it}^g = \Gamma^g + \sum_{r \neq 0} \tau_{D,r}^g \mathbf{1}_{(t=r)} + \sum_{p \neq 0} \phi_p^g \mathbf{1}_{(t-E_i^k=-p)} + u_{it}^g, \tag{5}$$

where Γ^g, $\tau_{D,r}^g$ and ϕ_p^g are parameters, provides estimates $\widehat{\tau}_{D,r}^g$ of the direct effect of cohabitation over event-time, that are not biased by realized or anticipated fertility.

Proof. See appendix A.[11] ■

Concretely, proposition 1 suggests adding a secondary event axis to the cohabitation event studies — measuring event time to having children — in order to control for fertility anticipation. The intuition underlying proposition 1 derives from the interpretation of pre-trends in standard event studies with one event axis. When treatment is known in advance, and other mechanisms of selection can be ruled out, researchers commonly interpret non-zero coefficients in the pre-treatment periods as capturing anticipation effects of the treatment. Similarly, when child event coefficients are added as controls to my cohabitation event studies, the pre-trends along the child event axis absorbs the influence of unobservable fertility beliefs, because the beliefs are (by assumption (4)) driven exclusively by the underlying fertility.[12]

The rational anticipation assumption means fertility beliefs are, at the time they are formed, unbiased estimates of future realized fertility, conditional on the fertility outcomes that are observed ex-post. Intuitively, this means that in a sample of individuals who all have identical realized fertility observed in the data, the average beliefs they held at any time should be equal to their future fertility outcomes. As a consequence, rational anticipation implies time-consistency of beliefs, meaning that beliefs held at different times, but all relating to fertility in a fixed period, are identical

[11] Let me give an abstract explanation of what I do in the simplest terms possible, noting that a fuller exposition is available in appendix A. Consider a randomly assigned treatment D, which *i)* affects an outcome y directly with effect size τ_D, and *ii)* affects a variable x, without revealing the exact value of x before after y has been determined. Uncertain of x, individuals act based on their beliefs when setting y. One might thus write down the reduced-form expression for y,

$$y = c + \tau_D D + \tau_e D + u,$$

where τ_e are effects of x-anticipation, and seemingly inseparable from τ_D. Now, to add some structure, assume that because individuals cannot respond directly to x, they instead form beliefs $B(D)$ about x, such that $\tau_e = \delta\left(B(1) - B(0)\right)$. To maximize simplicity assume that beliefs affect outcomes linearly such that c equals $\delta B(0)$ up to a constant a. Assume individuals beliefs are rational, such that upon learning x, $B(D)$ is an unbiased estimator of x. Specifically,

$$\mathbb{E}(B(D) \mid D, x) = x.$$

Let $\bar{y} = \mathbb{E}(y \mid D, x)$, then by application of the definition of τ_e and the rational anticipation assumption,

$$\bar{y} = a + \tau_D D + \delta x.$$

Since both D and x are observed by the researcher, this conditional expectation function can be estimated with OLS, and provides an estimate of τ_D. In a nutshell, this shows how controlling for an anticipated variable can solve the identification problem of separating direct changes from anticipation effects.

[12] A more flexible version of the result in proposition 1 interacts the cohabitation- and child-event axes, loosening an underlying linearity assumption. I cover this briefly in appendix A.

in expectation. This part of the rational anticipation assumption can be relaxed by including additional observable variables in the conditioning set in the expectation in (4) — equivalent to adding those variables as an additional regression controls in the regression specification in (5).

To give an example of how the assumption of strictly rational beliefs might break down, imagine a case where singles systematically overestimate their own fertility, and only adjust those beliefs after unsuccessfully trying to have a child, e.g. after infertility treatment reveals that their true fertility rate is low. In this case, individuals have based their behavior in the early years of the partnership on fertility beliefs which were irrational, since they did not match their true fertility rate. Note that the assumption does not rule out that some individuals follow this path of gaining negative information over time, but requires that the ex ante expectation is that information that is revealed later is neutral.

Empirically Kuziemko et al. (2018) shows that first time mothers underestimate the employment costs of motherhood, which although distinct in that it relates to the costs of fertility and not the fertility rate itself, is a type of irrational beliefs. However, because Kuziemko et al. finds that women underestimate the costs of children, their results independently suggest that fertility anticipations should not have large effects. Indeed, they show that before motherhood women make decisions related to human capital that account less for future fertility than they would have if they had more accurate information on the costs of motherhood, consistent with anticipation-effects being smaller than optimal.

While also not speaking directly to fertility beliefs, the literature on gaps between intended and realized family sizes also provide some information on the empirical relevance of rational anticipations. Generally this literature find that realized fertility is somewhat lower than fertility intentions (Beaujouan and Berghammer, 2019), suggesting intended fertility is not fully rational. But, since it is relatively easy for families to stop having children once they reach their ideal size, while it can be difficult to reach the ideal size in the first place, it is not surprising that average realized fertility is below intended fertility. If individuals internalize this asymmetry, it also means that their expected fertility should be lower than intended fertility, and thus closer aligned with realized fertility, in line with the rationality assumption.

The purpose of this section is not to argue strongly about the validity of the assumption in (4), since it is fundamentally unverifiable without access to data on fertility beliefs. Instead, it is to provide a natural starting point for thinking about cohabitation penalty estimates. If one accepts that rationality is a good approximation of real fertility beliefs, the above section provides a direct way to control for those fertility beliefs. Even in case one is only willing to accept that fertility beliefs are partially rational, comparing models that control for rational anticipation to various degrees can be informative about the likely magnitude of anticipation effects. For example, if the inclusion of rational anticipation controls does not alter the estimated cohabitation penalty, it suggests that the anticipation motive is small.[13] This is by itself more informative than what could be learned without viewing the estimates through this structural lens.

III. Couple Formation and the Gender Gap in Earnings

A. The Impact of Cohabitation on Earnings

Having laid out my estimation approach, I now turn towards documenting the effect of cohabitation on the earnings of each gender. In the following section I describe and quantify the gender gap that arises when cohabiting partnerships are formed. I also show that the decline in earnings is associated with a move towards less demanding job characteristics for women, and investigate how the cohabitation penalty evolves over the entire duration of partnerships.

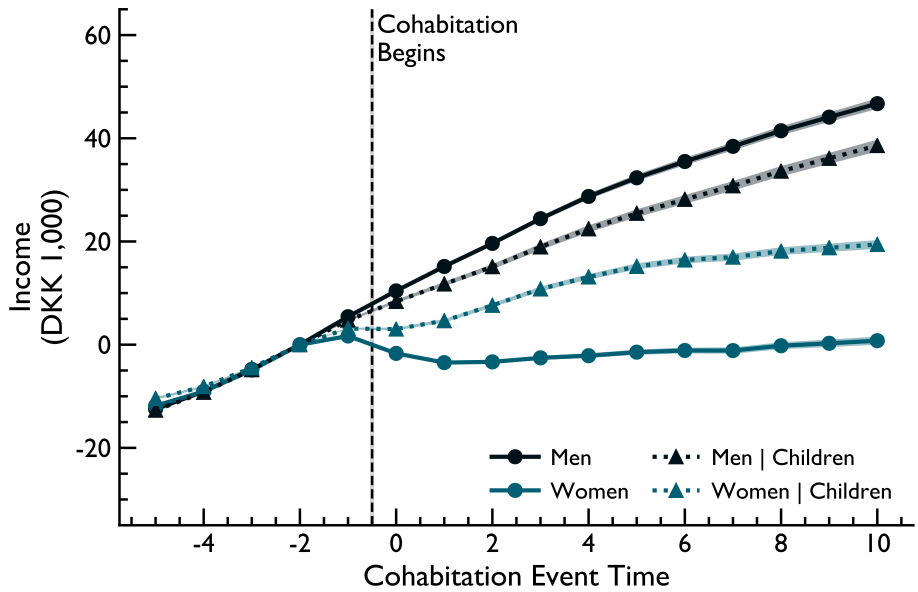
Figure 2 plots the evolution of labor earnings over event-time to first cohabitation for men and women.[14] The effects are measured relative to the income two years before cohabitation begins, and are all conditional on non-parametric age by cohort trends and controls for couples breaking up, as described in equation (1). Alongside the point estimates, the figure includes 95% confidence bands, which are so narrow they are barely needed.

The fully drawn lines with circular markers show estimates unconditional on children. Before cohabitation men and women are on similar upwards trends, but

[13] Or fertility beliefs are entirely orthogonal to the rationality hypothesis, such that realized and believed fertility are uncorrelated.

[14] I report the effect in levels because fertility is high in the years following cohabitation, causing many women having years with zero earnings, which would be lost by log-transforming the outcome variable.

Figure 2: *Labor Income Over Cohabitation Event Time*



Note: The figure plots regression coefficients on cohabitation event time from models like the ones described in section II. Fully drawn lines with circular markers are results from regression equation containing cohabitation event time coefficients, breakup event-time coefficients and age-by-cohort fixed effects. In the dashed lines with triangular markers an indicator variable has been added, which is 1 after individuals first child has been born. The regressions have been run separately for men and women. 95% confidence bands are indicated by shaded areas.

starting one year before moving in with a partner, women diverge from this trend. This likely captures a slight delay between relationships starting date, and the time of cohabitation. After cohabitation this divergence in earnings paths continues to grow in absolute terms. Without conditioning on children, the total effect of cohabitation starts out at approximately -13,000 DKK/year at event time 0, and grows steadily to -50,000 DKK/year after ten years, reflecting the gradual increase in realized fertility over time.

Because fertility is much higher in couples than amongst singles (fertility responses to cohabitation are depicted in figure C.4 in the appendix), more women will be subjected to a child penalty at positive event times. To account for this, I add controls for children to the regressions (shown in dashed lines). Doing so does not change the qualitative findings, but the effect size is reduced to around 35% what I estimate without controlling for children. Conditional on children, the cohabitation penalty grows from -5,000 DKK/year at event time 0 to -19,000 DKK at event time 10. At event times before cohabitation the fertility controls have very little influence, reflecting the low fertility rates before partnerships form. The main specification controls for the arrival of the individual's first child. In figure C.6 I show that further controlling for potential second and third children does not change the results, and in figure C.7 I include controls for the full event time axis before and after the birth of individuals first child, again not altering results. As discussed in section II controlling for the full event time axis of children also absorbs fertility anticipation effects, if individuals are rational in their anticipatory behavior. This is because rationality links together the fertility rate in the future and the fertility beliefs individuals hold contemporaneously.

Table 2 explores how the method of controlling for fertility affects the estimated cohabitation penalty. Each column reports only the coefficients on the terms interacted by a female dummy, as these are the coefficients of interest, but all models follow the standard procedure of including both baseline and interaction terms. The models estimate the change in the gender gap in earnings over the first five years of cohabitation. Column (1) reports the total cohabitation penalty without any controls for fertility. This estimate consist in part of the direct cohabitation penalty and in part the child penalty times the fertility rate after cohabitation. Column (2) adds gender-specific dummies for having children which reduce the cohabitation penalty by slightly more than half, as should be expected from figure 2. To see how these estimates relate to one another, notice that with a back of the envelope calculation, one can compute an implicit fertility rate over the first five years of cohabitation from columns (1) and (2) which comes out to 41.3%, matching the fertility estimates in figure C.4.[15] Columns (3) and (4) both account for anticipated fertility to the extent those anticipations are rational, following the method laid out in section II.B. Column (3) includes gender-specific child event time coefficients additively, which eliminates anticipation under the conditions laid out in proposition 1, while column (4) report estimates from a generalized version which interacts the gender-specific child event time indicators with the post-cohabitation dummy. Each of these estimates deviate marginally from than the one in column (2) which only account for realized fertility, but the differences are not of economic significance, suggesting that (rational) fertility anticipation is not driving the cohabitation penalty. The child event coefficients in column (3) do reveal some anticipation captured in the pre-trends, but the effects are small (from -5,500 DKK at child event time -4 to +1,650 DKK at event time -1) compared to the size of the child penalty. The

[15]The calculation uses that the cohabitation penalty estimated in column (1) is a pooled estimate of the true cohabitation penalty plus the fertility rate times the child penalty. Column (2) provides estimates of the cohabitation penalty and child penalty separately, meaning the implicit fertility rate can be recovered with a bit of algebra as being $(12,067 - 31,618)/(-47,317) = 0.413$.

anticipation is also relatively constant before child event time -1, which explains why the simpler model in column (2) yields a similar estimate of the cohabitation penalty. Figures C.8a to C.8c show event study plots corresponding to each of the approaches to controlling for children used in columns (2-4), reaffirming that the results are robust to the exact method chosen to control for (anticipated) fertility.

In addition to influencing results directly, the fact that fertility is high in the years following cohabitation means it is not possible to estimate relative effect sizes by log transforming the outcome variable. If I did so, especially women would drop out of the sample because spending time on parental leave leaves them with zero earnings for extended periods. The alternative proposed by Kleven, Landais and Søgaard (2019) and most commonly used in the literature is described in equation (3). It involves estimating the effects in levels, as in figure 2, and rescale them by predicted female earnings. This approach is in principle fine to use in the context of cohabitation, but it is very sensitive because the denominator, predicted female earnings, vary by orders of magnitude from event time -5 (where individuals are on average 19 years old) to event time $+10$ (where individuals are 34 years old).

For that reason figure 3 reports a different measure of the relative size of the cohabitation penalty, which measure the gender gap $y^f - y^m$ in percent of household income. It is based on those couples for which both partners fulfill the sample criteria, and is computed by directly calculating the within couple gender gap in percent of the couples household income (regardless of whether the couple is actually living together or not), and using this value as the outcome in an event-style regression similar to the ones described in (1) and used in figure 2. Because this analysis relies on information from both the male and female partner of a couple, the regression includes fixed effects which cover all combinations of male and female birth year and male and female age. I only control for children on the female side because the majority of the fertility will be collinear between the man and woman. There are some instances where this is not the case, but since children generally have small effects on men's labor market outcomes, including them does not affect the results. Breakups are perfectly collinear between the male and female side of a partnership, so I include ungendered controls for those.

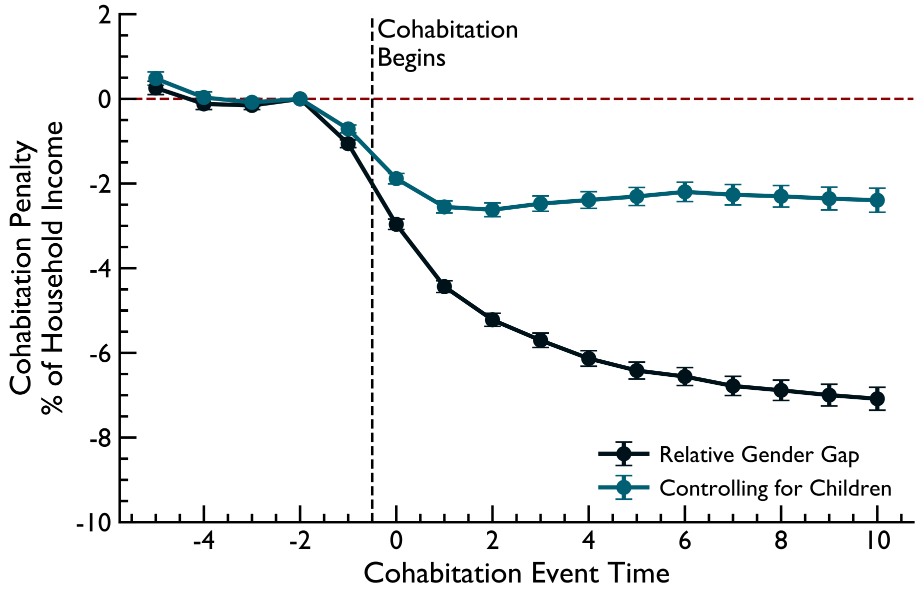
The total effect, including the fertility pathway, drives a persistent gender gap of up to 8% of the household income, which is growing even though household earnings

Table 2: *Cohabitation Penalties With Varying Fertility Controls*

	Labor Income (DKK)			
	(1)	(2)	(3)	(4)
Cohabitation Penalty				
Female×Post	$-31{,}618.510^{***}$	$-12{,}067.640^{***}$	$-13{,}866.450^{***}$	$-10{,}739.760^{***}$
	(179.513)	(226.201)	(224.623)	(505.312)
Selected Control Variables				
Female×Has Children		$-47{,}317.620^{***}$		
		(363.512)		
Female×$\mathbf{1}$[Event Time Kid $= -4$]			$-5{,}563.098^{***}$	
			(226.319)	
Female×$\mathbf{1}$[Event Time Kid $= -3$]			$-3{,}929.114^{***}$	
			(167.269)	
Female×$\mathbf{1}$[Event Time Kid $= -2$]			-	
			-	
Female×$\mathbf{1}$[Event Time Kid $= -1$]			$1{,}651.623^{***}$	
			(174.634)	
Female×$\mathbf{1}$[Event Time Kid $= 0$]			$-38{,}078.910^{***}$	
			(251.490)	
Female×$\mathbf{1}$[Event Time Kid $= 1$]			$-58{,}154.520^{***}$	
			(297.488)	
Female×$\mathbf{1}$[Event Time Kid $= 2$]			$-37{,}448.540^{***}$	
			(349.749)	
Female×$\mathbf{1}$[Event Time Kid $= 3$]			$-48{,}227.460^{***}$	
			(402.290)	
Female×$\mathbf{1}$[Event Time Kid $= 4$]			$-47{,}728.970^{***}$	
			(470.669)	
Female×Event Time Kid×Post	No	No	No	Yes
FE	Age x Cohort	Age x Cohort	Age x Cohort	Age x Cohort
Observations	12,136,214	12,136,214	12,136,214	12,136,214
R^2	0.343	0.352	0.356	0.357
Adjusted R^2	0.343	0.352	0.356	0.357

Note: The table shows estimated cohabitation penalties in levels with varying approaches to controlling for fertility. Column (1) serves as a baseline and includes no controls. Column (2) includes gender-specific dummies for having children (only the female-specific one is reported in the table) and column (3) includes a full set of gender by event time coefficients. Again only the female specific coefficients are included in the table. The controls contain dummies covering anywhere between -5 and +10 on the child event axis with two additional pooled estimates for anyone more than 5 years before, or 10 years after having children. Only the range from -4 to +4 are shown due to space constraints, but estimates at longer horizons are stable and similar to those at event times -4 to -1 and 0 to 4 respectively. Column (4) controls for interacted child event time and cohabitation indicators, as I show is required to control for fertility anticipation in section II. Control parameters are not shown to save space. Child event time -2 is omitted from the estimation in column (3). *p<0.1; **p<0.05; ***p<0.01.

Figure 3: *Cohabitation Penalty in Percent of Household Income*



Note: The figure plots the cohabitation penalty, when measured as a percentage of the couples total income, $(y^f_{it} - y^m_{it})/I^{HH}_t$. Without accounting for children, the gender gap starts out close to zero before cohabitation, but declines to -8% of household income over the first 6-10 years. Accounting for children the decline is smaller, at -3% to -2%. 95% confidence bands are indicated by error bars.

increase over event time. Controlling for children, the cohabitation penalty quickly settles at around -2 to -3% of household income.

Compared to the way of calculating relative penalties from absolute values introduced by Kleven, Landais and Søgaard (2019) (and described in equation (3)), the denominator is approximately twice as large using this method. This means the effect sizes here should be multiplied by 2 to bring them into alignment with what I obtain using equation (3), and the broader child penalty literature. This rescaling assumes men and women earn the same, which is the conservative approach to take. In the case men earn significantly more than women, the denominator becomes larger than $2y^f$, meaning the estimates should be adjusted with a factor of more than 2 to make them comparable with estimates obtained using the traditional method.

Table C.1 in the appendix report cohabitation penalties estimated using the approach of Kleven, Landais and Søgaard (2019). Using their method, I estimate a cohabitation penalty of -5.3% conditional on children and a total effect of cohabitation of -14% - both of which are consistent with the results I obtain when scaling by household income.

Returning to results measured in levels, table 3 reports summarized cohabitation penalties estimated over the first five years of partnership to document the robustness of the results shown so far. Columns 1 and 2 replicate the baseline results from table 2. In all the cases the regressions include age-by-cohort fixed effects and control for breakup timing, meaning the main estimates can be interpreted as conditional on the couples remaining together. Column 1 in table 3 reports the aggregate cohabitation penalty which includes both the direct cohabitation effect and the indirect effect mediated through increased fertility. Column 2 adds gender-specific controls for children which absorb the fertility mediated effect. The coefficient on Female×Post falls from DKK $-31,500$ to DKK $-12,000$ as a consequence, implying the fertility channel accounts for a little more than half of the total effect. Column 3 additionally controls for either partner being enrolled in education, which could temporarily suppress their income, and column 4 adds a control for being married, which might imply a larger willingness to specialize in the household, than if individuals do not marry. In both cases the added controls only have negligible effects on the coefficient of interest, which I interpret as providing robustness to the cohabitation penalty.

Column 5 estimates the regression specification with the full set of controls in a subset consisting only of individuals who do not get children with their first partner (but might get children with subsequent partners, or have children from a previous relationship which does not satisfy the sample criteria). The intent is to provide an alternative and more intuitive approach to controlling for fertility, and indeed the coefficient estimate of DKK $-14,000$ is not different from the estimate in columns 2-4, in an economically significant way. Column 6 restricts the sample to couples which last for 7 years or less before dissolving. If couples are entirely unable to predict the longevity of their relationship one should expect the estimate in this sample to be identical to the one found for the full sample. On the other hand, if some couples are predictably shorter lasting than others, couples might react to that when first moving in together.

In appendix table C.1 I report the cohabitation penalty across additional subsamples and find that effects are consistent across couples that end up married, end up with children and are non-danes. The effect is slightly larger in some immigrant populations and smaller if either partner has children already before beginning the partnership, but are otherwise of similar size as the effects reported in table 3.

Table 3: *Cohabitation Penalties With Controls and in Subsamples*

	Dependent Variable: Labor Income (DKK)					
	Full Sample				No Kids From Relation	Short Duration
	(1)	(2)	(3)	(4)	(5)	(6)
Cohabitation Penalty						
Female×Post	−31,618.510***	−12,067.640***	−11,612.640***	−11,681.420***	−13,715.420***	−10,553.130***
	(179.513)	(226.201)	(230.172)	(230.331)	(438.801)	(411.999)
Female	−37,741.010***	−31,938.720***	−31,989.420***	−21,826.090***	−19,978.410***	−22,259.430***
	(163.954)	(159.813)	(159.841)	(350.240)	(900.019)	(678.259)
Post	28,549.420***	25,198.950***	24,045.650***	24,029.720***	13,024.870***	19,920.830***
	(170.979)	(197.774)	(199.783)	(199.938)	(485.227)	(419.391)
Control Variables						
Female×Has Children		−47,317.620***	−46,362.750***	−47,228.420***	−46,325.650***	−40,876.100***
		(363.512)	(383.606)	(384.856)	(922.457)	(793.481)
Female×Married			−3,559.452***	−4,229.135***	−7,640.555***	−1,232.998
			(428.654)	(428.920)	(1,191.864)	(1,029.042)
Female×In Education				−11,738.850***	−10,266.800***	−7,588.402***
				(385.884)	(1,017.388)	(754.513)
Has Children		420.936	−1,368.779***	−1,370.940***	−33,269.010***	−13,564.120***
		(298.550)	(312.841)	(313.613)	(821.651)	(656.053)
Married			7,553.067***	7,730.501***	14,533.070***	2,151.426**
			(349.491)	(349.601)	(1,015.540)	(854.710)
In Education				−3,840.117***	7,775.176***	−3,827.821***
				(396.724)	(1,016.905)	(748.643)
Breakup Controls	Yes	Yes	Yes	Yes	Yes	Yes
FE	Age×Cohort	Age×Cohort	Age×Cohort	Age×Cohort	Age×Cohort	Age×Cohort
Observations	12,136,214	12,136,214	12,136,214	12,136,214	2,141,434	2,681,211
R^2	0.343	0.352	0.352	0.353	0.326	0.292
Adjusted R^2	0.343	0.352	0.352	0.353	0.325	0.292

Note: The first four columns estimate the cohabitation penalty in DKK using various controls that might affect women's income in the years after forming a relationship. The Fifth column shows regression results for the full specification on a sample restricted to individuals who do not have children with their partner (although they might have children with other partners at other times). The sixth column similarly restricts the sample only to individuals whose partnership lasts 7 years or less before dissolving. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

In summary, the results are consistent with a cohabitation penalty of $\approx -12,000$ DKK annually independently of a range of control variables and in a number of sub samples. The stability of the estimates across regressions suggest my research design, which uses men as counterfactual outcomes for women, does a good job of eliminating confounders because they tend to affect the genders equally over event time.

B. Impacts on Labor Supply

In addition to the typical register data, I also have access to responses from the Danish Labor Force Survey (LFS) between 2000 and 2018, which can be linked back to the administrative data through the Danish personal identification number. The survey only cover a random subset of the population, but because cohabitation is close to universal, the overlap between the cohabitation sample and survey respondents is sufficiently large to carry out pooled analysis.[16] Using the same 5 year pooled difference-in-difference regression as before (described in equation (2)), table 4 estimate the cohabitation penalty in a range of self reported measures of labor supply.[17] All the results reported are conditional on children.

In the first column, I report results on *actual hours*, which is collected by asking people about the number of hours of work they performed in a particular week, two to four weeks in the past, at the time of surveying. I find a cohabitation penalty in actual hours of -0.605 hours/week, but it is imprecisely measured. Yet, the point estimate is consistent with women falling behind men on labor supply by roughly 35 minutes per week in the first five years of cohabitation. This happens even when women's labor supply is ex-ante lower (21 hours/week) than that of men (28 hours/week).

Because the LFS question asks about a specific week of work, instead of e.g. contractual weekly hours, it picks up small and/or infrequent changes in labor supply, that other labor supply measures are unable to account for. For instance, cohabitation

[16] The exact number of yearly respondents varies throughout time, and only a subset are given the questions on labor supply. Concretely, this leaves 2-6,000 respondent per year between 2000 and 2018, who overlap with my cohabitation sample and participate in the LFS within ± 5 years of their cohabitation spell beginning.

[17] Figure C.9 in the appendix show full event-style regressions for each of the variables in the table. Because the LFS data are collected in a rotating panel, one might be concerned my estimates in this section are vulnerable to selection in- and out of the Labor Force Survey. To test if this is the case, figure C.11 estimate the income difference between LFS participants and non-participants over event time. I find zero difference between the two groups, suggesting selection in and out of the LFS as people transition from singles to partnerships is not an issue.

Table 4: *Cohabitation Penalties in Variables Recorded in the Danish Labor Force Survey*

	Actual Hours (Hours/week)	Odd Hours	Overtime (%)	Second Job
DD Estimate	-0.605*	-0.050***	-0.020***	-0.024***
	(0.288)	(0.008)	(0.004)	(0.005)
Ex-Ante Level	21.166	0.385	0.039	0.120
Breakup Controls	Yes	Yes	Yes	Yes
FE	Age×Cohort	Age×Cohort	Age×Cohort	Age×Cohort
Observations	67895	84648	84648	84648

Note: All estimates are conditional on realized fertility. The variable Odd Hours indicates respondents either sometimes or commonly working evenings, nights, Saturdays, or Sundays. The Overtime variable indicates a respondent having non-zero paid or unpaid overtime. The Second Job variable indicates a respondent having non-zero hours in a job that is not their primary one. *$p<0.1$; **$p<0.05$; ***$p<0.01$.

might drive changes in peoples behavior around taking time off to fulfill household obligations (for example by taking occasional single days off, or leaving their job early). Such changes are likely not large enough to warrant a change of contractual hours, but can add up to significant effects on earnings over time, making the LFS variable ideal for measuring labor supply effects of cohabitation.[18]

The LFS also contains information on a number of categorical questions relating to the nature of labor being supplied. I report cohabitation penalties for each of these three variables in columns 2-4. These effects are all estimated more precisely than the effect on hours, and uniformly suggest women move towards less demanding employment when beginning cohabitation. From column 2, cohabitation affects the gender gap in propensity to work odd hours — defined as working either weekends or evenings — by -5 percentage points. Compared to 38% of women working odd hours before cohabitation, this is a drop in women's probability of working odd hours of 13% compared to men. There is also a cohabitation penalty in the propensity to work overtime of -2 percentage points (column 2) and in the propensity to work a second job of -2.4 percentage points.

In summary, the results presented in table 4 show cohabitation drives women out

[18] In addition to actual hours worked, the Labor Force Survey also contains a question on individuals wished hours of work. Using this variable I estimate a positive cohabitation penalty of 1.291***(0.282) hours/week, suggesting cohabitation increases the mismatch between actual and preferred hours for women. Unfortunately, the wished-hours question is poorly phrased, making comparison between actual and wished hours difficult, so this result should be interpreted with caution.

of work that happens outside of normal work hours. While the coefficient on hours is imprecisely estimated, this seems to be associated with a decline in total weekly hours of work of 35 minutes. These results are all consistent with women allocating more time to household obligations, which limit the time they have available to supply labor.

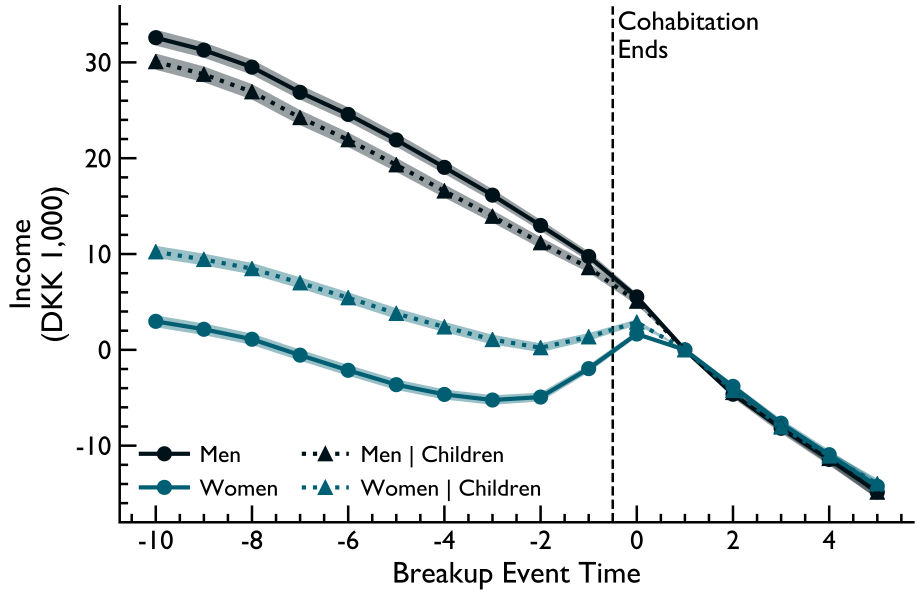
C. Couple Dissolution

I have so far documented what the cohabitation penalty looks like when couples are formed. Those estimates have all been conditional on the couples continued existence, partialling out the effect of some couples disbanding over time. In this subsection I turn my attention towards studying how the cohabitation penalty evolves as couples dissolve, and more generally how it evolves throughout the couples' lifecycle. Understanding how the cohabitation penalty changes as couples duration increase, and especially what happens when couples dissolve can shed light on whether cohabitation cause scarring effects as those seen in the unemployment literature (See e.g. Jacobson, LaLonde and Sullivan, 1993), or if individuals (especially women) are able to return to their pre-partnership situation again after couples break up. While distinguishing between the two cases does not immediately inform about which mechanisms drive the cohabitation penalty, it does provide some useful information in thinking about why and how the cohabitation penalty operates. If cohabitation is costly for women even after breaking up the relationship, human-capital driven explanations are likely to play a role, and the nature of the immediate cohabitation penalty is likely such that it decreases women's human capital accumulation. On the other hand, if no permanent scarring occurs, the effect is likely along margins where human capital does not change much.

Figure 4 plots the breakup event time coefficients which are estimated as part of producing the main cohabitation penalty event graph which is plotted in figure 2.[19] On the x-axis is now breakup event time, spanning from -10 years before breakup to +5 years after, with 0 indicating the first year after the couple has broken up. Because the estimation of the breakup coefficients is done jointly with the estimation of the cohabitation effects, these breakup effects can be thought of as measuring the breakup effect conditional on couples already being cohabiting. Without the joint estimation,

[19] Recall the estimation includes both cohabitation event dummies and breakup event dummies.

Figure 4: *Labor Income Over Breakup Event Time*



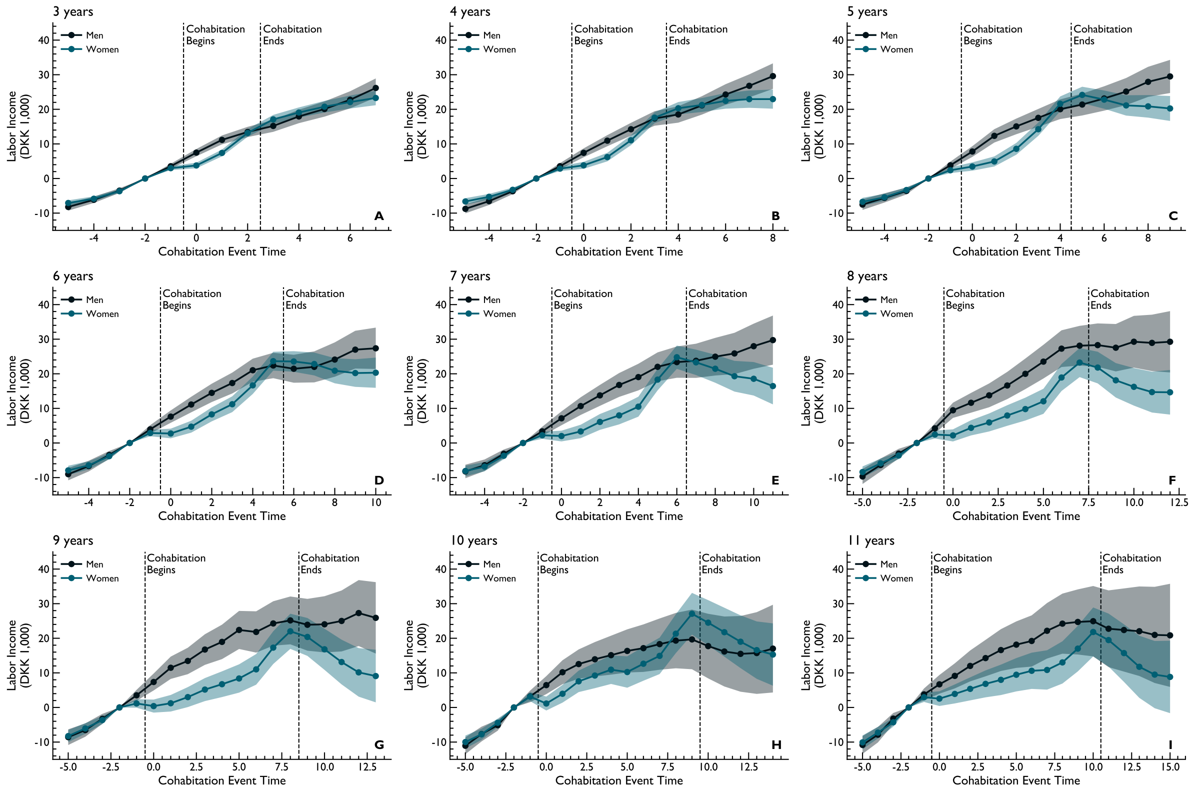
Note: The figure plots regression coefficients on breakup event time from models like the ones described in section II. Fully drawn lines with circular markers are results from regression equation containing cohabitation event time coefficients, breakup event-time coefficients and age-by-cohort fixed effects. In the dashed lines with triangular markers an indicator variable has been added, which is 1 after individuals first child has been born. The regressions have been run separately for men and women. 95% confidence bands are indicated by shaded areas.

breakup effects relating to event times long before breakup (i.e. those plotted towards the left side of the figure) would be biased by couples who had not even started cohabiting at that point in time, and thus experienced no effects of cohabitation.

From figure 4 it can be seen that women's earnings tend to catch up to those of men in the years just before breaking up. This is by itself not surprising for two reasons. First, separation can in many cases be anticipated by both partners, giving women time to increase their labor supply in anticipation of the breakup. Second, the increase in women's earnings before separation might be what is driving breakups if gender-traditional norms discourage couples where a woman earn more than (or the same as) the man (Folke and Rickne, 2020). It is worth noting that the size of the gap that women close as they approach breakup is approximately equal to the size of the cohabitation penalty estimated in figure 2. The earnings of both men and women are on declining curves as they approach breakup, implying there is overall a tendency for those who divorce to fall behind the aggregate cohort specific age profile in earnings.

In figure 5 I divide the sample by the duration of individuals cohabitation spell and estimate event studies for each partnership duration between 3 and 11 years

Figure 5: *Cohabitation Penalties Split by Duration of Partnership*



Note: The figure plots regression coefficients on cohabitation event time from models like the ones described in section II. All models include an indicator variable for having children in the set of controls, along with age-by-cohort fixed effects. The models have been estimated separately on couples that stay together between 3 and 10 years, showing the effect of cohabitation over the entire duration. The regressions have been run separately for men and women. 95% confidence bands are indicated by shaded areas.

separately. Doing so, breakup event time is collinear with cohabitation event time for a given duration, meaning event coefficients measure the combined effect of both events over time. Each panel in figure 5 reports event study estimates for one such sub-sample of individuals with a given fixed partnership duration. All the regressions control for children and each include a five-year pre-event window before cohabitation, as well as a five-year post-event window after cohabitation. The five years prior to cohabitation serve as control periods, in which parallel trends should hold in order for the research design to be valid, while the post-cohabitation periods need not be on parallel trends. If for example cohabitation has permanent gender-specific effects, or if dissolution affects men and women differently, the post period coefficient will differ between men and women.

One particular way in which men and women might be affected differently after breaking up is through the possibility of rematching with another partner. Assuming second time cohabitation affects earnings similarly to the first spell, rematching will tend to drive down women's post-breakup coefficients, even if rematching rates are identical between the genders. This is exactly what I observe throughout all the durations plotted in figure 5, and is especially pronounced at longer durations. This effect occurs gradually following breakups, since rematching is not instantaneous. For this reason an entirely transient effect of cohabitation (e.g. one without any permanent scarring effects) will only result in convergence between men and women in the years immediately succeeding breaking up.

While noisy, especially at longer durations, the overall picture is that couples of all durations show a cohabitation penalty when formed, which fully disappears at the time of dissolution. The penalty tends to be largest towards the middle of the relationship, and women begin their rebound in earnings years before breakups occur.[20]

IV. Testing the Mechanisms: Specialization or Gender Norms?

A. Beckerian Specialization

In the simplest conceptual terms, households produce two goods. One is made by working in the labor market (money), and the other produced at home through housework (cooking, cleaning, childcare and so on). Because partners differ in their productivity in these two types of work, they will benefit from specializing in either one as long as transaction costs between them are not too large (Becker, 1991; Browning, Chiappori and Weiss, 2014). In theoretically simplified cases, it is often optimal for the couple to fully specialize, such that one individual does all the housework, and the other does all the work in the labor market. The result however, carries over to less stylized settings, where partial specialization is a common result.

This simple theoretical motif is a classical explanation for why couples should divide their time spend working at their jobs and at home differently from how singles divide their time. Given some initial inequality that favors women over men in housework, it can also explain the overall tendency for women to work more

[20]The discrepancy between the monotone increase in effect size in figure 2 and the closing gaps in figure 5 can seem confusing. To understand why the two approached look different, recall that in figure 2 I include controls for breakup event time, meaning the estimated cohabitation penalty is conditional on not having broken up.

hours at home, and for men to earn more money in the labor market. While the term specialization is sometimes used to refer more broadly to couples that divide their responsibilities unequally, it is this Beckerian specialization that I discuss here. Whether specialization is a driver of the cohabitation penalty, is therefore a question of determining if the size of comparative advantages that exist within couples can predict the degree of specialization these couples show.

Should high degrees of specialization be expected from newly cohabiting couples? As Lundberg, Pollak and Stearns (2016) point out, a high degree of specialization requires a high degree of commitment to the relationship. If the breadwinner cannot be made to commit to the couple long-term, specializing in the homemaking role is a risky choice, because spending years working in the house degrades one's value in the labor market. Given the non-committed nature of most cohabiting couples, one should thus not expect specialization to be the driver of the cohabitation penalty. On the other hand, the human capital gains of marginal labor supply is possibly small on the intensive margin where the cohabitation penalty plays out, and the amount of housework demanded in the early stages of a partnership low enough that specializing is feasible.

A.I. Approach

To test the role of Beckerian specialization in shaping the cohabitation penalty I construct a test similar to the one used by Lassen (2021), who studies specialization in the context of a parental leave reform in Denmark, as well as by Siminski and Yetsenga (2022) and Artmann, Oosterbeek and van der Klaauw (2022) on respectively Australian and Dutch data. My test relies on the fact that, unlike some other explanations of gender inequality in couples, specialization is genderless by definition. Because the core of the theory is an economic incentive to exploit comparative advantages, it is symmetric across couples where either the man or woman holds a comparative advantage in market work. This does not mean that specialization implies the aggregate effect of cohabitation should be close to zero, since comparative advantages might be unequally distributed between men and women ex ante. But if specialization is the primary driver of the cohabitation penalty, equalizing comparative advantages would move the cohabitation penalty closer to zero.

Thus, a natural test of the specialization hypothesis is to investigate if couples facing the same imbalance in comparative advantages, but distributed oppositely by gender, also have opposite signed (and sized) cohabitation penalties.

To implement this test in practice, two issues needs to be discussed. First, I do not observe housework productivity in the dataset, so I rely on measures of earnings capacity to proxy for partners comparative advantage in market work. The validity of this approach relies on the differences in productivity in household production being smaller than the differences in labor market productivity, such that labor market productivity differences themselves predict who have the comparative advantage therein. Letting π^g denote the earnings capacity of either the man ($g = m$) or woman ($g = f$) in the relationship, I define the woman's relative advantage as

$$S_1 = \frac{\pi^f}{\pi^m + \pi^f}. \tag{6}$$

This expression is bounded between 0 and 1, and measure the woman's share of the households earnings capacity. Couples where either the man or woman has a clear advantage in the labor market will be characterized by this number S_1 being far from 1/2.

Second, individuals are forward-looking in reality, so couples should not base their labor division purely on the contemporaneous relative earnings capacities, but also how they expect them to evolve in the future. This is especially true if there are frictions in the role allocation within the household (such that the man and woman cannot freely switch between being the breadwinner and homemaker) or if human capital accumulates with experience. How much weight couples place on the present and future earnings capacity is uncertain, so I pragmatically use two measures that span from high to low discounting.

1. **Backward-Looking Measure:** The first measure I use is ex-ante earnings, measured two years before cohabitation starts. This measure is entirely backwards looking. It is intended to approximate individuals earnings capacity exactly at the beginning of cohabitation, while being measured before cohabitation to ensure exogeneity from the cohabitation event itself.

2. **Forward-Looking Measure:** The second measure is an attempt to approximate

the lifecycle earnings of individuals by using their educational attainment two years before cohabitation.[21] Using an auxiliary prediction model estimated on the full population I predict the average earnings at ages 35-40 for every education (by year of graduation) and use these figures as a measure of long term earnings capacity of individuals in the main sample of first time cohabiting partners.

A.II. Results

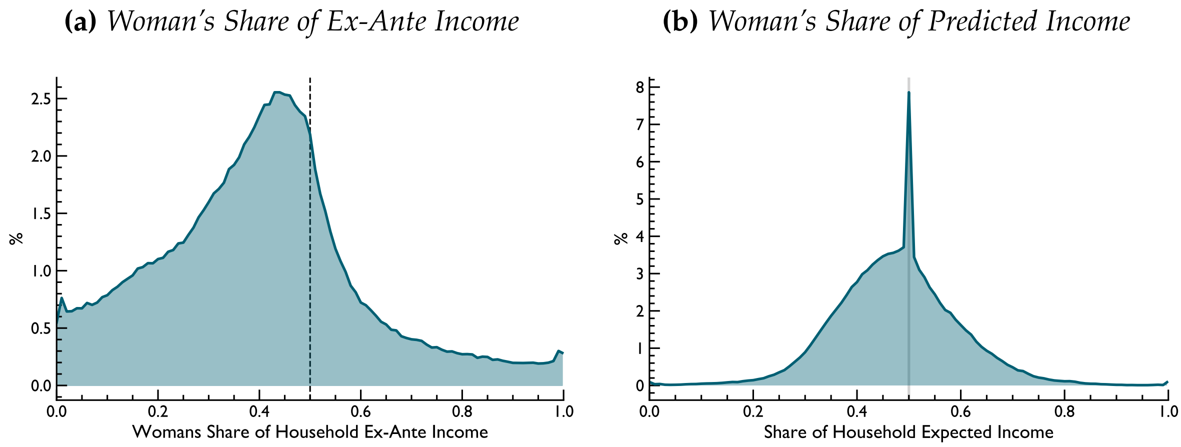
Figure 6 shows the distribution of female relative earnings capacity, S_1, across women in the dataset. Panel (a) shows S_1 when computed using ex-ante earnings as a proxy for earnings capacity (the backwards looking measure) and panel (b) show S_1 using the forward-looking measure based on educational attainment. Regardless of measurement method, in most couples the man has more than half of the earnings capacity. The spike at 1/2 in panel (b) is due to couples who share the same education and have graduated together in the same year, as both partners are predicted to earn the exact same amount in these couples.

While figure 6 show that there is heterogeneity in the relative earnings capacity of women across households, figure 7 test how well each of the two earnings capacity measures predict realized earnings at ages 30-50. The income bins on the x-axis rank individuals based on one of the two measures of earnings capacity, while the y-axis measure average realized income of the cohabitation panel sample. Finding a correlation between the earnings capacity, which is measured at cohabitation event time -2, and realized earnings in midlife shows that the earnings capacity measures contain information that couples can feasibly use to predict their long term earnings at the time cohabitation begins.

Figure 7 also shows the correlation between the education based measure and realized earnings across the whole earnings capacity distribution (the blue line), suggesting this is a good proxy for long term earnings capacity. This is to be expected, because the education based measure is designed to predict earnings at around the same ages as what I plot on the y-axis here. The backwards looking measure on the other hand is not monotonically correlated with midlife earnings. In the bottom 25%

[21]Because individuals are potentially quite young when this measurement is made, I also consider ongoing non-completed enrollments when determining their educational attainment. This is equivalent to assuming people finish the degrees they enroll in. In those cases where individuals educational attainment is unobserved at the time of measurement, I remove them from the data.

Figure 6: *Distribution of Women's Share of Household Labor Productivity Across Couples*



Note: The figure shows the distribution of each of the measures of relative earnings potential. Panel (a) uses the measure based on ex-ante earnings. 74% of females would be secondary earners in their couple, if their income was held steady at its level the year before cohabitation began. Panel (b) shows the distribution of the relative earnings measure based on individual's highest level of education the year before cohabitation. The spike at 0.5 is due to individuals sharing the same education.

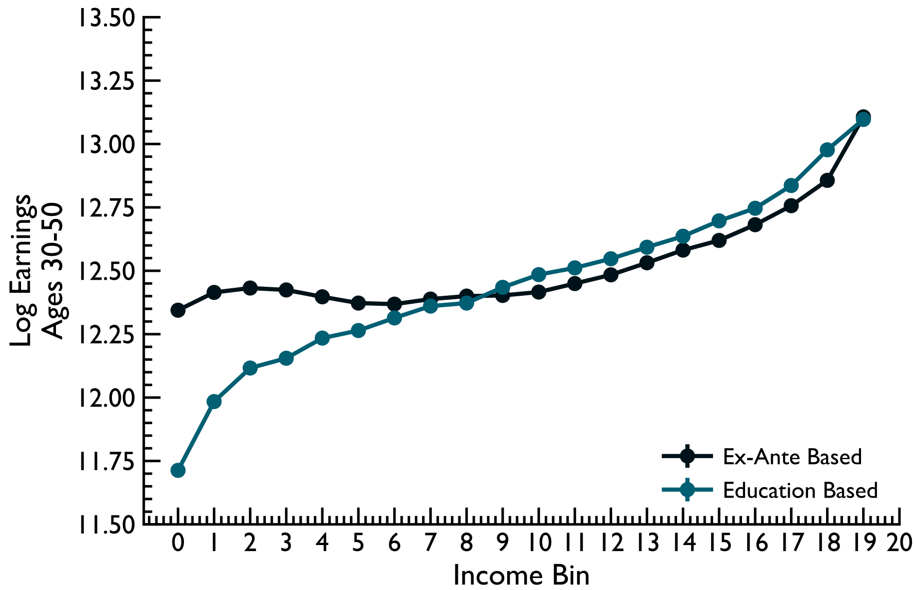
of the ex-ante earnings distribution the predictive power is low,[22] reflecting that many first time cohabitors have not yet completed education, and are employed in various non-permanent jobs while they study. Above the bottom 25% however, the ex ante measure reflects quite closely the one based on educational attainment.

So is the cohabitation penalty correlated with the incentive to specialize? I use $S_1 > 0.6$ as a cutoff for identifying couples with a female primary earner and $S_1 < 0.4$ to identify couples with male primary earners. Defining primary earners symmetrically in this way (instead of e.g. comparing the p% of couples where female earners bring home most of the household income with the p% of couples where men bring home most) ensures that the economic incentive for specialization is the same in both groups. In the extreme case of perfect specialization, one should expect men and women to react identically given the same incentive, meaning the cohabitation penalty when measured between females who are primary earners and males who are primary earners, should be zero. With less than perfect specialization, the gender gap might not be zero, but will be smaller than in the full population.

Figure 8 plots the cohabitation penalty, measured as the difference between the earnings of females who are primary earners in their relationship and males, who

[22]Strictly, the correlation is close to 0 all the way to the median income bin on the x-axis, but from the 25th to 50th percentile, the correlation appears to be picking up.

Figure 7: *Earnings Capacity Measures Plotted Against Realized Adult Earnings*



Note: The figure plots results of a regression of log earnings measured between ages 30 and 50 against dummies indicating 20 bins of each earnings-potential measure. 95% confidence bands are indicated with vertical bars, but are too narrow to be visible.

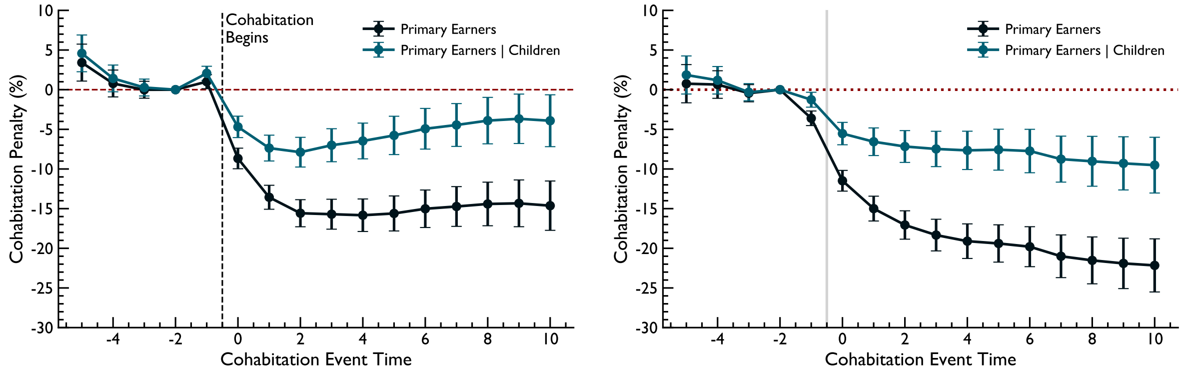
likewise are primary earners in their relationship. I make the comparisons in this way — between men and women who are primary earners in their respective couples — because in particular the backwards looking measure is sensitive to mean reversion in individuals earnings. Due to transitory income shocks, both men and women who are primary earners according to the backwards looking measure are likely to have temporary high incomes, and thus drop back down to their permanent income in the years just after being classified as primary earners. This trend is shared between men and women as long as their transitory income shocks are identical, meaning the difference-in-difference estimate absorbs the effect. It is however not shared e.g. within the couple, meaning the pre-trends in such comparisons would suffer heavily from differences in mean reversion.

Panel (a) uses the ex-ante based income measure to select the sample of primary earners while panel (b) uses the education based measure. In both cases the null hypothesis of specialization predicts zero effects, while the alternative of no specialization would imply that cohabitation penalties in this setting are of similar magnitude as in the whole population.

In both cases I observe cohabitation penalties of −15% to −20% unconditional

Figure 8: *Cohabitation Penalties for Female Primary Earner Couples*

(a) *Backward-Looking: Ex-Ante Based* **(b)** *Forward-Looking: Education Based*



Note: Each figure shows cohabitation penalties for women who are characterized as primary earners, using men who are primary earners in their relationship as baseline. In panel (a) primary earners are defined as earning at least 60% of household income ex ante, while in panel (b) they are defined by their education predicting that they will earn at least 60% of household income. The cohabitation penalties are estimated using diff-in-diff models between men and women using between-couple variation. 95% confidence bands are indicated by error bars.

on children and −10% to −5% once controlling for fertility. All the post cohabitation estimates are significantly different from zero, meaning full specialization is unlikely to be explaining the cohabitation penalty.

Appendix figure D.12 mirrors the results in figure 8 but using secondary earners. In panel (a) using the ex-ante measure, which suffers heavily from mean reversion in earnings and in panel (b) using the forward-looking measure, which replicates the patterns in figure 8.

B. Gender Norms

An alternative explanation to the purely economical one, is that gender norms dictate how couples must divide labor between them, resulting in an unequal division for cultural reasons. If this is the case, it is not the economic circumstances of the couple that drive the cohabitation penalty, but their opinions. Such norm based behavior can conceptually be described by assuming individuals utility also depend on adherence to social categories (Akerlof and Kranton, 2000, 2002, 2005). Concretely, men might perceive that they are supposed to be breadwinners, and women that they should be homemakers, and therefore act as such to not suffer the disutility from diverging from a cultural norm. They might also enforce such views on each other as a type of externality to living together, in which case they might individually be dissatisfied with their situation, but collectively choose to divide labor unevenly anyway.

B.I. Approach

Neither the administrative data I have access to, nor the Labor Force Survey, contains information that can directly inform about peoples true cultural convictions or beliefs. So, instead of directly addressing the role of gender norms for the cohabitation penalty, I take advantage of the availability of almost 40 years of administrative data to construct measures of gender norm compliance that I can track across generations. Concretely, I rely on being able to merge parents to their children over a time horizon that is long enough to observe correlations in parents gender norm compliance when their children are still young, and the cohabitation penalty those children are impacted by when they grow up. The rationale is that the gender norms that prevail in one's childhood home are likely to affect ones own preferences over gender identity and couple norms when older. Such intergenerational norm transmission has been demonstrated in other norm-driven domains such as energy consumption (Hansen and Jacobsen, 2020) and patience (Brenøe and Epper, 2022), and directly in the domain of gender norms using US data by Farré and Vella (2013).

More specifically, I consider the subset of individuals in my cohabitation sample (the main individuals) for whom I observe their parents for at least three years while the main individual is still younger than 18. Amongst the parents I compute a measure similar to the relative earnings capacity measure in equation (6), that calculate the share of the parents total household income over those years, which was earned by the mother,

$$S_2 = \frac{\sum_{\text{Main age}<18} y_t^{\text{mom}}}{\sum_{\text{Main age}<18} y_t^{\text{mom}} + y_t^{\text{dad}}} \tag{7}$$

This number gives an indication of the overall parental labor division under which an individual in my primary sample have grown up. If S_2 is equal to zero, the main person lived their whole childhood with a stay-at-home mom, and inversely if it is equal to 1, they lived with a stay-at-home dad until they turned 18. I divide S_2 into three equal sized bins denoting childhood homes where mothers played a small (Low), medium (Mid) or large (High) role in the labor market output of the family.

From the perspective of the couple, there are two values of S_2, one relating to the woman's parents and one to the man's parents. The analysis is symmetric across either individuals parents, but results can in principle differ if men and women carry forward gender norms in different manners.

B.II. Results

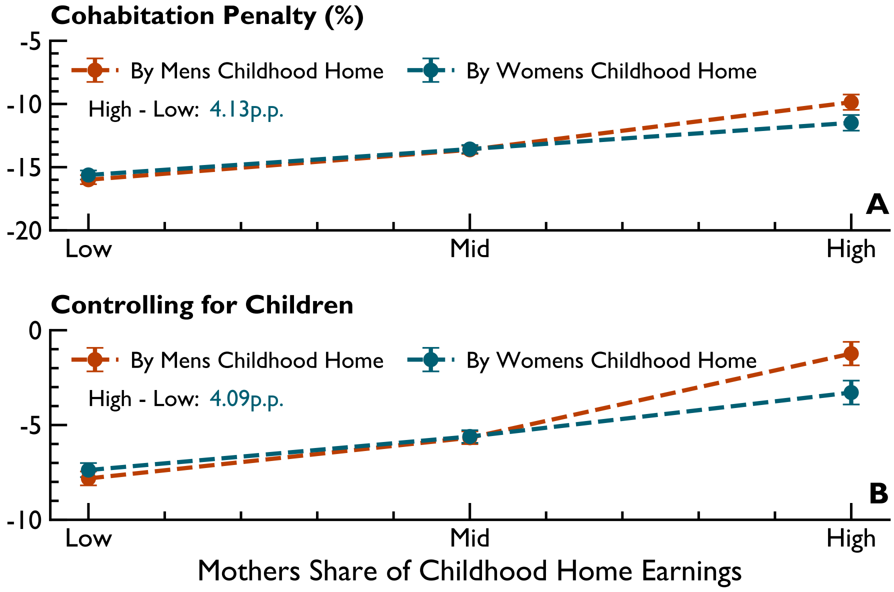
Figure 9 plots the cohabitation penalty across three equal sized bins of S_2, the cohabiting partners' mothers relative earnings in their respective childhood homes. Since both partners in the couple have parents, I plot the cohabitation penalties both over the males mothers income share (red line) and over women's mothers income share (blue line). The top panel reports the unconditional cohabitation penalty, including the effect from children, while the bottom panel reports results controlling for children. Consequentially the magnitude of effects is smaller in the bottom panel, consistent with the total effect of cohabitation being partially mediated by children.

Independently of whether I control for children or not, I find a monotone relationship between how "gender traditionalist" individuals' parents labor market earnings are, and how large the cohabitation penalty is for newly formed couples, hinting that gender norms can possibly explain why the cohabitation penalty exists.

The correlation between compliance with traditional gender roles between one's parents, and the magnitude of the gender gap that arises from cohabiting with a partner is, while suggestive, insufficient to prove that gender norms are driving the cohabitation penalty. While intergenerationally persistent gender norms is one mechanism which can explain the observation, other mechanisms (e.g. intergenerational correlation in employment patterns) would also cause the observed correlation. Ideally, it would be possible to observe or induce random variation in the amount of gender norm compliance in the parents' generation, giving (quasi-)experimental variation in the exposure to traditional gender roles that people grow up under.

Lacking truly exogenous variation in gender norms, I use the fact that divorces amongst parents, while potentially endogenous to gender norms, affect individuals exposure to the type of household their parents lead. If parents divorce early in their child's life, their relative earnings carry little information about the type of household the individual is actually exposed to during their childhood. Inversely, if the parents stay together for most or all of the childhood, their relative earnings are a good proxy for their child's exposure to traditional gender norms. Following this line of

Figure 9: *Cohabitation Penalty by Mothers Share of Childhood Home Earnings*

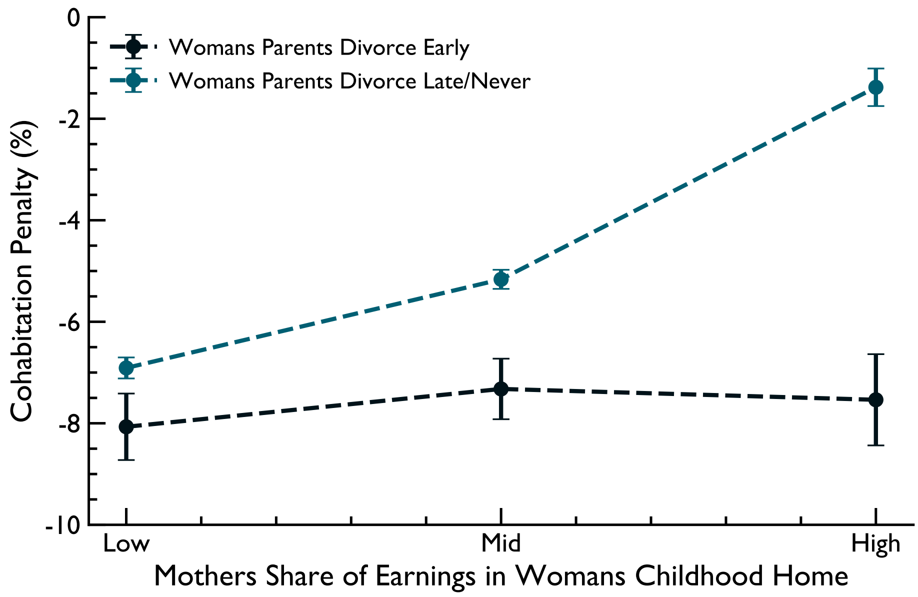


Note: This figure shows that the size of the cohabitation penalty is correlated with a measure of how well individuals parents conformed to traditional gender roles when they were growing up. The top panel reports the total cohabitation penalty while the bottom panel adds controls for children to the regressions. 95% confidence bands are shown as error bars, standard errors are clustered at the individual level.

thinking, if gender norms are driving the cohabitation penalty, individuals whose parents divorced early should experience cohabitation penalties that are independent of their mothers' income share, while individuals with parents who stay together should show a correlation.

Figure 10 carries out this test, using women's family background as the basis (results using men's family background can be found in figure D.13). It shows, as expected, that women whose parents have divorced late (after their child turn 18) have cohabitation penalties that are highly correlated with their parents' compliance with traditional gender norms (blue line). Women whose parents divorced early (before their child turned 3) on the other hand show no such correlation (black line). The figure also show a difference in levels between the early- and late-divorce groups. This is likely because those who divorce when they have young children are, across other characteristics, different from those who do not. These other characteristics might by themselves influence the magnitude of cohabitation penalty that is passed on to their children, without being correlated with the specific component of gender norm compliance for which I proxy by measuring mothers share of household earnings.

Figure 10: *Cohabitation Penalty by Women's Childhood Home, Split By Exposure*



Note: This figure shows that the correlation between cohabitation penalty magnitude and childhood homes compliance to traditional gender roles is driven by women whose parents live together for a significant portion of their childhood, suggesting a causal mechanism. The cohabitation penalty estimates are conditional on children. 95% confidence bands are shown as error bars, standard errors are clustered at the individual level.

Interpretation of figure 10 should be done with care, as parents propensity to divorce is unlikely to be independent of their compliance with gender norms. How and if divorced parents choose to remarry is also potentially endogenous to their gender norms. Yet, assuming these types of endogeneity are second order effects compared to the direct effect of differences in exposure to traditional gender norms, figure 10 show exactly what one should expect if parents' compliance with traditional gender norms causally affect their children's cohabitation penalty when they grow up.

V. Conclusion

In this paper I have used unique partner identifiers in administrative data from Denmark to document that the transition from single to cohabiting partnership drives gender inequality, both indirectly via fertility and directly via a *cohabitation penalty* in earnings. Beyond earnings, I show cohabitation decreases women's labor supply as well as their propensity to work evenings or weekends, to work overtime and to hold secondary jobs.

To document the effect, I have shown event studies of earnings as men and women move into cohabitation, revealing a divergence in earnings trajectories that cannot fully be explained by children. Estimating the effect with event studies is demanding of the data, but allows me to trace the full dynamic evolution of earnings as people transition in- and out of cohabitation. This eliminates problems with distinguishing selection from effect that has plagued the literature on marriage premiums.

I find that cohabitation independently of children cost women 2-3% of their households total earnings, or 5% of their own counterfactual earnings. I have also shown that the cohabitation penalty is reversed when couples break up, reinforcing the notion that the cohabitation penalty is in fact caused by cohabitation, and not confounders that change when cohabitation begins.

In terms of mechanisms, I have first shown that the cohabitation penalty does not vary with the earnings capacity of partners as one would expect if Beckerian specialization explained the effect. Rather, women with high earnings capacity relative to their partners are affected by a cohabitation penalty of similar magnitude as the full population, suggesting there is no correlation between the economic incentive for specialization, and the amount and direction of specialization couples undertake.

When investigating the role of gender norms, I find that the cohabitation penalty is larger for people who were brought up in gender traditional homes. Reinforcing the notion that this correlation is exactly driven by exposure to the gender traditional division of labor in the childhood home, I also find that people whose parents divorce early in their life, do not show such a correlation. While the evidence is not sufficient to prove that gender norms are driving the cohabitation penalty, it suggests that this might indeed be the case.

In summary, what this paper shows is that forming cohabiting partnerships directly causes gender inequality in earnings, and that this effect appears to be driven by gender norms rather than any direct economic incentives.

References

Akerlof, George A., and Rachel E. Kranton. 2000. "Economics and Identity." *The Quarterly Journal of Economics*, 115(3): 715–753. Publisher: Oxford University Press.

Akerlof, George A., and Rachel E. Kranton. 2002. "Identity and Schooling: Some Lessons for the Economics of Education." *Journal of Economic Literature*, 40(4): 1167–1201.

Akerlof, George A., and Rachel E. Kranton. 2005. "Identity and the Economics of Organizations." *Journal of Economic Perspectives*, 19(1): 9–32.

Albanesi, Stefania, and Claudia Olivetti. 2016. "Gender Roles and Medical Progress." *Journal of Political Economy*, 124(3): 650–695. Publisher: The University of Chicago Press.

Albanesi, Stefania, Claudia Olivetti, and Barbara Petrongolo. 2022. "Families, Labor Markets, and Policy." *NBER Working Papers*.

Aldén, Lina, Lena Edlund, Mats Hammarstedt, and Michael Mueller-Smith. 2015. "Effect of Registered Partnership on Labor Earnings and Fertility for Same-Sex Couples: Evidence From Swedish Register Data." *Demography*, 52(4): 1243–1268. Publisher: Duke University Press.

Angelov, Nikolay, Per Johansson, and Erica Lindahl. 2016. "Parenthood and the gender gap in pay." *Journal of Labor Economics*, 34(3): 545–579. Publisher: University of Chicago Press.

Antonovics, Kate, and Robert Town. 2004. "Are All the Good Men Married? Uncovering the Sources of the Marital Wage Premium." *American Economic Review*, 94(2): 317–321. Publisher: Jeffrey Gray.

Artmann, Elisabeth, Hessel Oosterbeek, and Bas van der Klaauw. 2022. "Household specialization and the child penalty in the Netherlands." *Labour Economics*, 78: 102221.

Attanasio, Orazio, Hamish Low, and Virginia Sánchez-Marcos. 2008. "Explaining Changes in Female Labor Supply in a Life-Cycle Model." *American Economic Review*, 98(4): 1517–1552.

Bailey, Martha J. 2006. "More Power to the Pill: The Impact of Contraceptive Freedom on Women's Life Cycle Labor Supply." *The Quarterly Journal of Economics*, 121(1): 289–320. Publisher: Oxford Academic.

Baker, Matthew J., and Joyce P. Jacobsen. 2007. "Marriage, specialization, and the gender division of labor." *Journal of Labor Economics*, 25(4): 763–793. Publisher: The University of Chicago Press.

Bardasi, Elena, and Mark Taylor. 2008. "Marriage and Wages: A Test of the Specialization Hypothesis." *Economica*, 75(299): 569–591. Publisher: [London School of Economics, Wiley, London School of Economics and Political Science, Suntory and Toyota International Centres for Economics and Related Disciplines].

Basu, Kaushik. 2006. "Gender and Say: a Model of Household Behaviour with Endogenously Determined Balance of Power*." *The Economic Journal*, 116(511): 558–580.

Beaujouan, Eva, and Caroline Berghammer. 2019. "The Gap Between Lifetime Fertility Intentions and Completed Fertility in Europe and the United States: A Cohort Approach." *Population Research and Policy Review*, 38(4): 507–535.

Becker, Gary S. 1991. *A Treatise on The Family: Enlarged edition.* Harvard university press.

Bertrand, Marianne. 2011. "New Perspectives on Gender." *Handbook of Labor Economics*, 4(PART B): 1543–1590. Publisher: Elsevier.

Bertrand, Marianne, Emir Kamenica, and Jessica Pan. 2015. "Gender identity and relative income within households." *Quarterly Journal of Economics*, 130(2): 571–614. Publisher: Oxford University Press.

Bertrand, Marianne, Patricia Cortes, Claudia Olivetti, and Jessica Pan. 2021. "Social Norms, Labour Market Opportunities, and the Marriage Gap Between Skilled and Unskilled Women." *The Review of Economic Studies*, 88(4): 1936–1978.

Blau, Francine D., and Lawrence M. Kahn. 2017. "The Gender Wage Gap: Extent, Trends, and Explanations." *Journal of Economic Literature*, 55(3): 789–865. Publisher: American Economic Association.

Brenøe, Anne Ardila, and Thomas Epper. 2022. "Parenting values and the intergenerational transmission of time preferences." *European Economic Review*, 148: 104208.

Browning, Martin, Pierre-André Chiappori, and Yoram Weiss. 2014. "Economics of the Family." *Economics of the Family*. Publisher: Cambridge University Press ISBN: 9780521795395.

Bursztyn, Leonardo, Alessandra L. González, and David Yanagizawa-Drott. 2020. "Misperceived Social Norms: Women Working Outside the Home in Saudi Arabia." *American Economic Review*, 110(10): 2997–3029.

Chiappori, Pierre-Andre, and Maurizio Mazzocco. 2017. "Static and Intertemporal Household Decisions." *Journal of Economic Literature*, 55(3): 985–1045.

Cortés, Patricia, and Jessica Pan. 2020. "Children and the Remaining Gender Gaps in the Labor Market." *NBER Working Papers*.

Daly, Moira, and Fane Groes. 2017. "Who takes the child to the doctor? Mom, pretty much all of the time." *Applied Economics Letters*, 24(17): 1267–1276. Publisher: Routledge _eprint: https://doi.org/10.1080/13504851.2016.1270410.

Farré, Lídia, and Francis Vella. 2013. "The Intergenerational Transmission of Gender Role Attitudes and its Implications for Female Labour Force Participation." *Economica*, 80(318): 219–247.

Fernández, Raquel, Alessandra Fogli, and Claudia Olivetti. 2004. "Mothers and Sons: Preference Formation and Female Labor Force Dynamics*." *The Quarterly Journal of Economics*, 119(4): 1249–1299.

Foged, Mette. 2016. "Family migration and relative earnings potentials." *Labour Economics*, 42: 87–100. Publisher: North-Holland.

Folke, Olle, and Johanna Rickne. 2020. "All the Single Ladies: Job Promotions and the Durability of Marriage." *American Economic Journal: Applied Economics*, 12(1): 260–87. Publisher: American Economic Association.

Fortin, Nicole M. 2005. "Gender Role Attitudes and the Labour-market Outcomes of Women across OECD Countries." *Oxford Review of Economic Policy*, 21(3): 416–438.

Ginther, Donna K., and Madeline Zavodny. 2001. "Is the Male Marriage Premium Due to Selection? The Effect of Shotgun Weddings on the Return to Marriage." *Journal of Population Economics*, 14(2): 313–328. Publisher: Springer.

Giuliano, Paola. 2020. "Gender and culture." *Oxford Review of Economic Policy*, 36(4): 944–961.

Goldin, Claudia. 2014. "A Grand Gender Convergence: Its Last Chapter." *American Economic Review*, 104(4): 1091–1119. Publisher: American Economic Association.

Goldin, Claudia, and Lawrence F Katz. 2002. "The Power of the Pill: Oral Contraceptives and Women's Career and Marriage Decisions." *Journal of Political Economy*, 110(4).

Greenwood, Jeremy, Ananth Seshadri, and Mehmet Yorukoglu. 2005. "Engines of Liberation." *The Review of Economic Studies*, 72(1): 109–133. Publisher: [Oxford University Press, The Review of Economic Studies, Ltd.].

Hansen, Anders Rhiger, and Mette Hove Jacobsen. 2020. "Like parent, like child: Intergenerational transmission of energy consumption practices in Denmark." *Energy Research & Social Science*, 61: 101341.

Hersch, Joni, and Leslie S. Stratton. 2002. "Housework and Wages." *The Journal of Human Resources*, 37(1): 217–229. Publisher: [University of Wisconsin Press, Board of Regents of the University of Wisconsin System].

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993. "Earnings Losses of Displaced Workers." *The American Economic Review*, 83(4): 685–709. Publisher: American Economic Association.

Jakobsen, Katrine Marie, Thomas Jørgensen, and Hamish Low. 2022. "Fertility and Family Labor Supply."

Jakobsson, Niklas, and Andreas Kotsadam. 2016. "Does marriage affect men's labor market outcomes? A European perspective." *Review of Economics of the Household*, 14(2): 373–389.

Juhn, Chinhui, and Kristin McCue. 2016. "Evolution of the Marriage Earnings Gap for Women." *American Economic Review*, 106(5): 252–56. Publisher: American Economic Association.

Kleven, Henrik. 2022. "The Geography of Child Penalties and Gender Norms: Evidence from the United States."

Kleven, Henrik, Camille Landais, and Jakob Egholt Søgaard. 2019. "Children and gender inequality: Evidence from Denmark." *American Economic Journal: Applied Economics*, 11(4): 181–209. Publisher: American Economic Association.

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. 2019. "Child Penalties across Countries: Evidence and Explanations." *AEA Papers and Proceedings*, 109.

Kleven, Henrik, Camille Landais, Johanna Posch, Andreas Steinhauer, and Josef Zweimüller. 2020. "Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation."

Kleven, Henrik, Claus Thustrup Kreiner, Kristian Larsen, and Jakob Egholt Søgaard. 2023. "Micro vs Macro Labor Supply Elasticities: The Role of Dynamic Returns to Effort."

Kuperberg, Arielle. 2019. "Premarital Cohabitation and Direct Marriage in the United States: 1956–2015." *Marriage & Family Review*, 55(5): 447–475. Publisher: Routledge.

Kuziemko, Ilyana, Jessica Pan, Jenny Shen, and Ebonya Washington. 2018. "The Mommy Effect: Do Women Anticipate the Employment Effects of Motherhood?" *NBER Working Papers*. Publisher: National Bureau of Economic Research.

Lassen, Anne Sophie. 2021. "Gender Norms and Specialization in Household Production: Evidence from a Danish Parental Leave Reform." *Working Paper / Department of Economics. Copenhagen Business School*. Publisher: Copenhagen Business School [wp] Number: 04-2021.

Lundberg, Shelly, Robert A. Pollak, and Jenna Stearns. 2016. "Family Inequality: Diverging Patterns in Marriage, Cohabitation, and Childbearing." *Journal of Economic Perspectives*, 30(2): 79–102. Publisher: American Economic Association.

Lundborg, Petter, Erik Plug, and Astrid Würtz Rasmussen. 2017. "Can Women Have Children and a Career? IV Evidence from IVF Treatments." *American Economic Review*, 107(6): 1611–37. Publisher: American Economic Association.

Mazzocco, Maurizio. 2007. "Household Inter Temporal Behaviour: A Collective Characterization and a Test of Commitment." *The Review of Economic Studies*, 74(3): 857–895. Publisher: Oxford University Press, Review of Economic Studies, Ltd.

Mincer, Jacob, and Solomon Polachek. 1974. "Family Investments in Human Capital: Earnings of Women." *Journal of Political Economy*, 82(2): S76–S108. Publisher: University of Chicago Press.

Olivetti, Claudia, and Barbara Petrongolo. 2016. "The Evolution of Gender Gaps in Industrialized Countries." *Annual Review of Economics*, 8: 405–434. Publisher: Annual Reviews.

Pollak, Robert A. 2013. "Allocating Household Time: When Does Efficiency Imply Specialization?"

Ribar, David C. 2004. "What Do Social Scientists Know About the Benefits of Marriage? A Review of Quantitative Methodologies." *IZA Discussion paper*.

Siminski, Peter, and Rhiannon Yetsenga. 2022. "Specialization, Comparative Advantage, and the Sexual Division of Labor." *Journal of Labor Economics*, 40(4): 851–887. Publisher: The University of Chicago Press.

Stevenson, Betsey, and Justin Wolfers. 2007. "Marriage and Divorce: Changes and Their Driving Forces." *The Journal of Economic Perspectives*, 21(2): 27–52. Publisher: American Economic Association.

Voena, Alessandra. 2015. "Yours, Mine, and Ours: Do Divorce Laws Affect the Intertemporal Behavior of Married Couples?" *American Economic Review*, 105(8): 2295–2332. Publisher: American Economic Association.

Zinovyeva, Natalia, and Maryna Tverdostup. 2021. "Gender Identity, Coworking Spouses, and Relative Income within Households." *American Economic Journal: Applied Economics*, 13(4): 258–84. Publisher: American Economic Association.

Online Appendix

A. Econometrics

A natural question given the close link between cohabitation and fertility, is whether the effect measured by cohabitation event dummies is truely an effect of cohabitation itself (such as by altering indviduals productivity, time constraint, etc.), or whether the effect is driven by changing beliefs about ones future fertility. For example, if the labor market features a large degree of dynamic compensation, and having children removes women entirely from the labor market, it might not be worthwhile supplying a high amount of effort when childless if the probability of becoming a mother is high. Since the fertility rate changes discontinuously at the beginning of cohabitation, these types of fertility considerations are also likely to change, and consequentially estimates of the cohabitation penalty will pool together direct effects and effects of fertility anticipation.

The purpose of this section is to clarify how fertility anticipations influence the cohabitation estimates, and to develop a method of controlling for fertility anticipation even when they are unobserved to the researcher. The method relies on a structural assumption of *rational fertility beliefs*, which ties together fertility anticipation (which is unobservable) and realized future fertility (which is observable).

Consider a situation where income at event time t, y_{it}, is a function of a binary indicator for cohabitation, c_{it}, a binary indicator the presence of children k_{it}, the individuals expectations about the arrival of children in the future, e_{it}, and noise η_{it}.

$$y_{it} = \Gamma(c_{it}, k_{it}, e_{it}, \eta_{it}). \tag{8}$$

In this model, there are four potential outcomes spanning cohabitation and children. The two treatment variables can change independently when discussing potential outcomes, but are empirically correlated. Letting $k_{it}^{(c)}$ denote the potential value of the child indicator when $c_{it} = c$ and likewise $e_{it}^{(c,k)}$ the fertility beliefs when $c_{it} = c$ and $k_{it} = k$, the total effect of cohabitation at time t can be written

$$ATE(t) = \mathbb{E}\left[\Gamma\left(1, k_{it}^{(1)}, e_{it}^{(1,k_{it}^{(1)})}, \eta_{it}^{(1,k_{it}^{(1)})}\right) - \Gamma\left(0, k_{it}^{(0)}, e_{it}^{(0,k_{it}^{(0)})}, \eta_{it}^{(0,k_{it}^{(0)})}\right)\right]. \tag{9}$$

This quantity can be identified under standard assumptions — specifically assuming that $\eta_{it}^{(1,k)}, \eta_{it}^{(0,k)} \perp c_{it}$, meaning unobservables are independent of treatment (typically denoted the unconfoundedness or parallel trends assumption). It measures the effect of cohabitation, allowing all other variables which are causally influenced by cohabitation to change, and therefore summarize the direct effect of cohabitation, the direct effect of changing fertility and the effect of changing fertility anticipations. Partialling out the direct effect of children is straight forward, because children are observable, and can be included as control variables in a regression. Partialling out fertility anticipations on the other hand requires some work, because fertility beliefs are unobserved.

A. Potential Outcomes

To make progress it is first necessary to express y_{it} in terms of its potential outcomes. First, at any given time an individual can be either single or living with a partner, but both potential outcomes are never observed simultaneously. The quantity that is observed in the data can, by expressing in terms of potential outcomes of cohabitation (e.g. individuals are either single or in a partnership), be written

$$y_{it} = \Gamma_{(0,k_{it}^{(0)})} + c_{it} \left(\Gamma_{(1,k_{it}^{(1)})} - \Gamma_{(0,k_{it}^{(0)})} \right), \tag{10}$$

where $\Gamma_{(c,k)} \equiv \Gamma\left(c, k, e_{it}^{(c,k)}, \eta_{it}^{(c,k)}\right)$ to keep notation light. This expression is similar to the standard potential outcomes decomposition, but does not fully decompose y_{it} into its potential outcomes in this specific setting, because k_{it} is also a treatment variable, along which one can further decompose. Conditional on being in a partnership, the observed outcome is

$$\Gamma_{(1,k_{it}^{(1)})} = \Gamma_{(1,0)} + k_{it}^{(1)} \left(\Gamma_{(1,1)} - \Gamma_{(1,0)} \right) \tag{11}$$

and likewise, conditional on being single the observed outcome can be written

$$\Gamma_{(0,k_{it}^{(0)})} = \Gamma_{(0,0)} + k_{it}^{(0)} \left(\Gamma_{(0,1)} - \Gamma_{(0,0)} \right). \tag{12}$$

Finally the presence of children itself ($k_{it}^{(c)}$) also depends on the outcome of the cohabitation variable. The realized value k_{it} can, just like the realized outcome in y_{it}, be written in terms of potential outcomes reflecting that fertility behavior might differ depending on whether individuals are cohabiting or single,

$$k_{it} = c_{it}k_{it}^{(1)} + (1 - c_{it})k_{it}^{(0)}. \tag{13}$$

Using the above, y_{it} can be written fully in terms of its potential outcomes, but before doing so, it helps keep notation light to implement a simplifying assumption.

Assumption 1 (Normalization)**.** *Assume* $k_{it}^{(0)} = 0$.

This assumption has two implications. First, it implies that fertility amongst singles is zero. Second, it implies that cohabitation is an absorbing state once a child is born, since the alternative would cause some people to have children while single after a breakup. The assumption is conceptually inconsequential, as all the following derivations carry through without it, albeit modified to include additional normalizing terms to keep track of the enlarged space of possible states. Returning to (10), after substituting in (11) and (12) and applying assumption 1, y_{it} can be written[23]

$$y_{it} = \Gamma_{(0,0)} + c_{it}\Delta_c\Gamma_{(c,0)} + k_{it}\Delta_k\Gamma_{(1,k)}, \tag{14}$$

where Δ_x is the difference operator such that e.g. $\Delta_c\Gamma_{(c,0)} = \Gamma_{(1,0)} - \Gamma_{(0,0)}$.

B. Identification

Presently, $\Delta_c\Gamma_{(c,0)}$ is a difference over both the cohabitation indicator variable itself, as well as the fertility beliefs that are associated with each value of the cohabitation indicator. To separate these two effects define

$$\gamma_{(x,y,z,w)} = \Gamma(x, y, e_{it}^{(z,w)}, \eta_{it}^{(x,y)}) \tag{15}$$

to distinguish the values at which the direct effects are evalueated from those at which the anticipations effects are evaluated (notice $\Gamma_{(x,y)} = \gamma_{(x,y,x,y)}$). Adding and

[23]Before applying assumption 1, substitution yields

$$\begin{aligned} y_{it} = \Gamma_{(0,0)} &+ k_{it}^{(0)}\left(\Gamma_{(0,1)} - \Gamma_{(0,0)}\right) + c_{it}\left(\Gamma_{(1,0)} - \Gamma_{(0,0)}\right) \\ &+ c_{it}\left(k_{it}^{(1)}\left(\Gamma_{(1,1)} - \Gamma_{(1,0)}\right) - k_{it}^{(0)}\left(\Gamma_{(0,1)} - \Gamma_{(0,0)}\right)\right). \end{aligned}$$

Then set $k_{it}^{(0)} = 0$ and use (13) to get $k_{it} = c_{it}k_{it}^{(1)}$ and let $\Delta_x\Gamma_{(x,y)} = \Gamma_{(1,y)} - \Gamma_{(0,y)}$.

subtracting $c_{it}\gamma_{(0,0,1,0)}$ to $c_{it}\Delta_c\Gamma_{(c,0)}$ produces

$$c_{it}\Delta_c\Gamma_{(c,0)} = c_{it}\left(\Gamma_{(1,0)} - \gamma_{(0,0,1,0)}\right) + c_{it}\left(\gamma_{(0,0,1,0)} - \Gamma_{(0,0)}\right). \tag{16}$$

The first term in (16) measures the direct effect of cohabitation conditional on fertility anticipations being as those of a cohabiting individual, so let $\tau_D = \Gamma_{(1,0)} - \gamma_{(0,0,1,0)}$ denote the direct effect of cohabitation. Likewise the second term measures the anticipation effect of cohabitation, $\tau_e = \gamma_{(0,0,1,0)} - \Gamma_{(0,0)}$. These anticipation effects are specifically those of individuals in couples who do not have children yet. The anticipations change again when such a couple have children, and this effect is still included in the child effect $\tau_k = \Delta_k\Gamma_{(1,k)}$. Returning to the expression for y_{it}, it can then be written

$$y_{it} = \Gamma_{(0,0)} + c_{it}\left(\tau_D + \tau_e\right) + k_{it}\tau_k, \tag{17}$$

highlighting the crux of the identification issue. The direct effect of cohabitation (τ_D) is inseparable from the anticipation effects of cohabitation (τ_e). The only approach that can potentially recover τ_D, is one that expresses τ_e as a function of other variables that can be controlled for. One natural assumption to make, is that individuals beliefs are rational, meaning they are functions only of the true probability of having children in the future.

First, to make the problem tractable, I introduce an assumption of linearity. So far, anticipations have not been defined in a way where they relate to specific periods in the future (or past). This has served to keep notation as simple as possible in the preceding text, but now, as I begin to evolve the setup towards a practical solution of the identification problem, it is helpful to implement a notion of relative timing. Thus, let $e_{it}(x)$ be individual i's beliefs at time t about their fertility at time $t + x$, and $e_{it}^{(c,k)}(x)$ be those beliefs, evaluated in the potential outcome where $c_{it} = c$ and $k_{it} = k$. The full set of beliefs e_{it}, held by the individual, is then given by

$$e_{it} = \{e_{it}(x) \mid x \in \mathcal{X}\} \tag{18}$$

where $\mathcal{X} = \{1, 2, 3, ...\}$ is the range of relative time periods over which the individual holds fertility beliefs. Having expanded on the definition of e_{it}, I turn to an assumption.

Assumption 2 (Linearity). *Recall that $\Gamma(c_{it}, k_{it}, e_{it}, \eta_{it})$ is the structural outcome model for y_{it} defined in (8). Assume the structural model is linear in anticipations with coefficient δ_x, that is*

$$\frac{\partial \Gamma}{\partial e_{it}(x)} = \delta_x. \tag{19}$$

Also assume the effects of fertility beliefs are additively separable across x, meaning

$$\Delta \Gamma = \sum_{x>0} \frac{\partial \Gamma}{\partial e_{it}(x)} \Delta e_{it}(x). \tag{20}$$

where $\Delta\Gamma$ is the change in Γ that occurs upon changing each $e_{it}(x) \in e_{it}$ by amount $\Delta e_{it}(x)$.

To make assumption 2 concrete, note that it can be implemented by assuming Γ takes the functional form

$$\Gamma(c_{it}, k_{it}, e_{it}, \eta_{it}) = \Psi(c_{it}, k_{it}) + \psi(e_{it}) + \eta_{it} \tag{21}$$

where Ψ and ψ are functions, and $\psi(e_{it}) = \sum_{x>0} \delta_x e_{it}(x)$ ensures fertility beliefs are additively separable across time periods.

Assumption 2 implies the anticipation-mediated effect of cohabitation $\tau_e = \gamma(0,0,1,0) - \Gamma(0,0)$, can be written as $\tau_e = \sum_{x>0} \tau_e(x)$ where

$$\tau_e(x) = \delta_x \left(e_{it}^{(1,0)}(x) - e_{it}^{(0,0)}(x) \right). \tag{22}$$

Inserting this expression of τ_e in (17) yields

$$y_{it} = \Gamma_{(0,0)} + c_{it}\tau_D + c_{it} \sum_{x>0} \delta_x \left(e_{it}^{(1,0)}(x) - e_{it}^{(0,0)}(x) \right) + k_{it}\tau_k. \tag{23}$$

Here it is important to remember that $\Gamma_{(0,0)}$ and τ_k are both structurally defined to depend on fertility beliefs e_{it}, so they are not constants. Before continuing with y_{it}, let me briefly turn to expressing realized beliefs $e_{it}(x)$ in terms of potential outcomes of c_{it} and k_{it}. Using that $c_{it}k_{it} = k_{it}$ by the simplification of assumption 1, total anticipations x periods ahead can be written in terms of potential outcomes as

$$e_{it}(x) = e_{it}^{(0,0)}(x) + c_{it} \left(e_{it}^{(1,0)}(x) - e_{it}^{(0,0)}(x) \right) + k_{it} \left(e_{it}^{(1,1)}(x) - e_{it}^{(1,0)}(x) \right). \tag{24}$$

Noticing that $(1/\delta_x)c_{it}\tau_e(x)$ appears in this expression, it can be isolated, and the result inserted in the expression for y_{it} in (23) to provide

$$y_{it} = \Gamma_{(0,0)} + c_{it}\tau_D + \sum_{x>0} \delta_x \left(e_{it}(x) - e_{it}^{(0,0)}(x) - k_{it} \left(e_{it}^{(1,1)}(x) - e_{it}^{(1,0)}(x) \right) \right) + k_{it}\tau_k \quad (25)$$

which, by collecting terms relating to the same potential outcomes, can be rewritten

$$y_{it} = \tilde{\Gamma}_{(0,0)} + c_{it}\tau_D + \sum_{x>0} \delta_x e_{it}(x) + k_{it}\tilde{\tau}_k \quad (26)$$

where

$$\tilde{\Gamma}_{(0,0)} = \Gamma_{(0,0)} - \sum_{x>0} \delta_x e_{it}^{(0,0)}(x) \quad (27)$$

and

$$\tilde{\tau}_k = \tau_k - \sum_{x>0} \delta_x \left(e_{it}^{(1,1)}(x) - e_{it}^{(1,0)}(x) \right). \quad (28)$$

Here $\tilde{\Gamma}_{(0,0)}$ and $\tilde{\tau}_k$ are respectively the baseline outcome and the effect of children, both with the effect of fertility anticipations subtracted out linearly. These are both independent of fertility anticipations by assumption 2.[24] Now, I introduce the key assumption that transforms the problem from one expressed in terms of unobservable $e_{it}(x)$'s, to observable future realized fertility k_{it+x}.

Assumption 3 (Rational Anticipation)**.** *Assume individuals fertility beliefs $e_{it}(x)$ are rational, such that they are unbiased estimators of k_{it+x}, conditional on the ex-post observed realization of this variable,*

$$\mathbb{E}\Big(e_{it}(x) \mid c_{it}, k_{it}, \{k_{it+x} \mid x \in 1, 2, ...\} \Big) = k_{it+x}. \quad (29)$$

This implies the expected effect of fertility beliefs $\mathbb{E}\left(\tau_e(x) \mid c_{it}, k_{it}, \{k_{it+x} \mid x \in 1, 2, ...\}\right)$ is proportional to the change in fertility caused by cohabitation. Intuitively, conditional on observing the sequence $\{k_{it+x} \mid x \in 1, 2, ...\}$ ex-post, this assumption implies individuals fertility beliefs $e_{it}(x)$, which were formed ex-ante, were on average correct.

Letting $\bar{z} = \mathbb{E}(z \mid c_{it}, k_{it}, \{k_{it+x} \mid x \in 1, 2, ...\})$ and implementing assumption 3 in

[24] This follows immediately when taking derivatives of $\tilde{\Gamma}_{(0,0)}$ and $\tilde{\tau}_k$. For example $d\tilde{\Gamma}_{(0,0)}/de_{it}^{(0,0)}(x) = \delta_x - \delta_x = 0$ for all x.

(26),

$$\bar{y}_{it} = \bar{\bar{\Gamma}}_{(0,0)} + c_{it}\tau_D + \sum_{x>0} \delta_x k_{it+x} + k_{it}\bar{\bar{\tau}}_k. \tag{30}$$

Owning to assumption 2 both $\bar{\bar{\Gamma}}_{(0,0)}$ and $\bar{\bar{\tau}}_k$ are constants[25], and since c_{it} and k_{it+x} are observable by the researcher, this equation can be estimated with OLS to provide an unbiased estimate $\hat{\tau}_D$ of τ_D. When estimated as an event study, as I do empirically, the quantity of interet is $\bar{y}_{ir} - \bar{y}_{i0}$, where 0 is a reference period with $c_{i0} = 0$ (and $k_{i0} = 0$ by assumption 1) and period r is one of possibly multiple periods around the reference period. Adding $\bar{y}_{i0}$ on both sides, this difference has the conditional expectation function

$$\bar{y}_{ir} = \bar{\bar{\Gamma}}_{(0,0)} + c_{ir}\tau_D + \sum_{x>0} \delta_x k_{ir+x} + k_{ir}\tau_k \tag{33}$$

Define $\tilde{\delta}_x = \tau_k$ for $x \leq 0$ and δ_x otherwise in order to collect the direct effect of kids with the anticipation coefficients,

$$\bar{y}_{ir} = \bar{\bar{\Gamma}}_{(0,0)} + c_{ir}\tau_D + \sum_{x\geq 0} \tilde{\delta}_x k_{ir+x}. \tag{34}$$

This expression is still structural in nature since it is derived directly from the potential outcomes model, but is now almost identical to a regression model for y_{it}. To get the expressions entirely compatible with a standard event study setup, notice that one can exchange the lagged/leaded child dummies k_{it+x} with a child event axis. This can be done because the set of child dummy-indicators k_{it+x} (equal to $\mathbf{1}(t + x - E_i^k \geq 0)$ with E_i^k being the year individual i has their child-event) can be rewritten to a child event-time axis without loosing information. Specifically the difference between two adjacent child-dummies is zero everywhere except for the specific child event time $-x$,

$$k_{it+x} - k_{it+x-1} = \mathbf{1}(t - t_0^k = -x), \tag{35}$$

[25]Specifically they are

$$\bar{\bar{\Gamma}}_{(0,0)} = \Psi(0,0) \tag{31}$$
$$\bar{\bar{\tau}}_k = \Psi(1,1) - \Psi(1,0). \tag{32}$$

when using the specific linear functional form for Γ given in (21).

which is a child event time indicator. Thus, equivalently to controlling directly for $\sum_x k_{it+x}$ one might control for the event coefficients $\sum_x \mathbf{1}(t - t_0^k = -x)$. This is validated in figure B.3 using simulated data.

Returning to (34), let $\tau_{D,r}$ be event-time dependent versions of the coefficient on c_{ir} in order to generalize the model to accommodate multiple periods. The conditional expectation function then implies the regression model

$$y_{it} = \Gamma + \sum_{r \neq 0} \tau_{D,r} \mathbf{1}_{(t=r)} + \sum_{p \neq 0} \phi_p \mathbf{1}_{(t - E_i^k = -p)} + u_{it} \tag{36}$$

with Γ picking up the level of $\bar{\tilde{\Gamma}}_{(0,0)}$, and $\tau_{D,r}, \phi_p$ being parameters. Because the sum over child-event time now both needs to capture anticipation and effects of having children, it runs over both positive and negative values, with one arbitrarily chosen period omitted. This expression proves proposition 1 in the main text. Note that the regression above have been written with superscript g in the main text to signify that it is to be estimated separately for each gender. However, there is nothing in the proof as such that requires this sample split.

Genderalizing to the nonlinear case

The linearity assumption that makes $\sum_{x>0} \delta_x e_{it}(x)$ cancel out in $\tilde{\Gamma}_{(0,0)}$ and $\tilde{\tau}_k$ simplifies computation, but is not strictly required. I use it to get a simple expression of $\tau_e(x)$, and consequently it ensures that expectation cancel out in the other parameters of the model. However, it is a knife's-edge case that relies on the δ_x's reappearing across the different potential outcomes due to linearity.

In general, both the baseline outcome $\tilde{\Gamma}_{(0,0)}$ and the effect of children $\tilde{\tau}_k$ might depend on fertility beliefs. Still assuming rational fertility anticipation, these effects of anticipation can be controlled for using ex-post realized fertility, but the regression specification must be flexible enough to absorb both the effect of anticipations at baseline and the anticipation effects of cohabitation. To handle this added freedom in the model, let $\phi_{x,r}$ be anticipation parameters that are now allowed to vary over cohabitation event time r, and consider the regression model

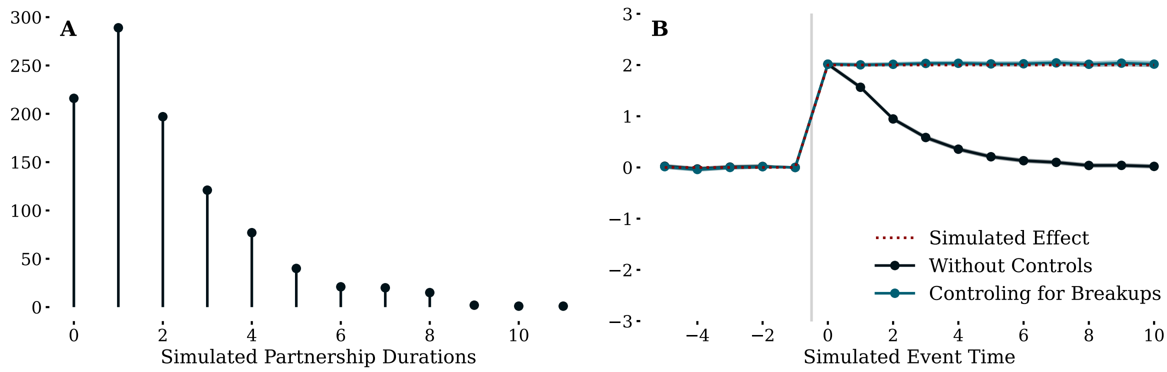
$$y_{it} = \Gamma + \sum_{r \neq 0} \tau_{D,r} \mathbf{1}_{(t=r)} + \sum_{r \neq 0} \sum_{p \neq 0} \phi_{p,r} \mathbf{1}_{(t=r)} \mathbf{1}_{(t - E_i^k = -p)} + u_{it} \tag{37}$$

which estimate a separate path of child-event-time coefficients $\phi_{p,r}$ for each event time on the cohabitation event axis. This model has the required flexibility to correctly attribute effects of anticipation to the $\phi_{p,r}$ parameters, and given assumption 3 still holds, the solution to the OLS problem will still provide unbiased estimates of $\tau_{D,r}$.

B. Simulation Results

Simulating Breakup Controls

Figure B.1: *Simulated results with and without controlling for breakups.*



Note: This figure shows simulation results that verify the validity of controlling for breakup event time in cohabitation event regressions. Panel A shows the duration-distribution of the simulated partnerships. Panel B shows coefficient estimates from regression models estimated on simulated data, respectively with (blue line) and without (black line) breakup event time controls.

Figure B.1 plots data from a simple simulation exercise that demonstrates the need to control for breakup timings to recover the true effect of cohabitation. The data are constructed by simulating 1000 individuals over event time, who all enter cohabitation at time 0 and leaving cohabitation randomly after x_i years, where x_i is drawn from a fixed exponential distribution and rounded to an integer. The distribution of x_i's is plotted in figure B.1 panel A.

Individuals outcomes are governed by

$$y_{it} = \zeta \cdot \mathbf{1}_{(\text{Cohabiting})} + \varepsilon_{it} \tag{38}$$

which states that y_{it} is given by the cohabitation effect ζ (as long as it is relevant) plus normally distributed noise ε_{it}. Figure B.1 panel B plots event coefficients from two regressions, along with the true theoretical effect of cohabitation. The first regression (without controls) regresses individuals outcomes on event time dummies, with event time -1 as reference. The second model (controling for breakups) adds a second event axis to the regression, measuring time to separation. The exact specification of this model is

$$y_{it} = \sum_{k \neq -1} \delta_k \mathbf{1}_{(\text{cohabitation event time } k)} + \sum_{l \neq -1} \phi_l \mathbf{1}_{(\text{breakup event time } l)} + \varepsilon_{it} \tag{39}$$

The results of the simulation exercise confirms that it is necessary to control for breakups to recover the true cohabitation penalty, assuming the penalty is local to the duration of the partnership, as it allows the flexibility required to account for the cohabitation effect disappearing once couples dissolve.

Simulating Breakup and Child Controls

I also run a set of simulations which adds effects of children to the simulated data in a more complex environment than what I described above. In this simulation, individuals begin cohabitation at time $t = 0$, which has time specific effects

$$\beta_d(t) = (\gamma_d t + \Delta_d)\mathbb{1}_{[t \geq 0]}\mathbb{1}_{[t \leq d_i]} \tag{40}$$

where d_i is the partnership duration of individual i, for which it holds that $d_i \mid D_i \sim \text{Poisson}(\lambda_d)$ and $\gamma_d, \Delta_d, \lambda_d$ are parameters. The variable D_i indicates if the individual experiences a breakup, or remains cohabiting forever, and follows $D_i \sim \text{Bernoulli}(p_d)$.

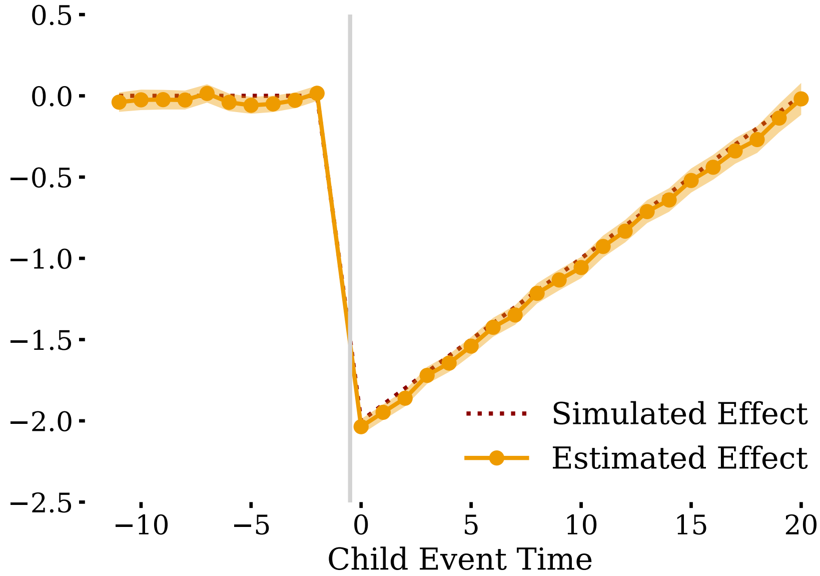
Kids appear randomly from 3 years before cohabitation onwards, and like for breakups they follow $k_i \mid K_i \sim \text{Poisson}(\lambda_k)$, with K_i indicating if the individual ever has children, and $K_i \sim \text{Bernoulli}(p_k)$. The effect of children is given by

$$\beta_k(t) = \delta(t - k_i) + (\gamma_k(t - k_i) + \Delta_k)\,\mathbf{1}_{[t - k_i \geq 0]} \tag{41}$$

where $\delta, \gamma_k, \Delta_k$ are parameters and $t - k_i$ measures time along the child event axis.

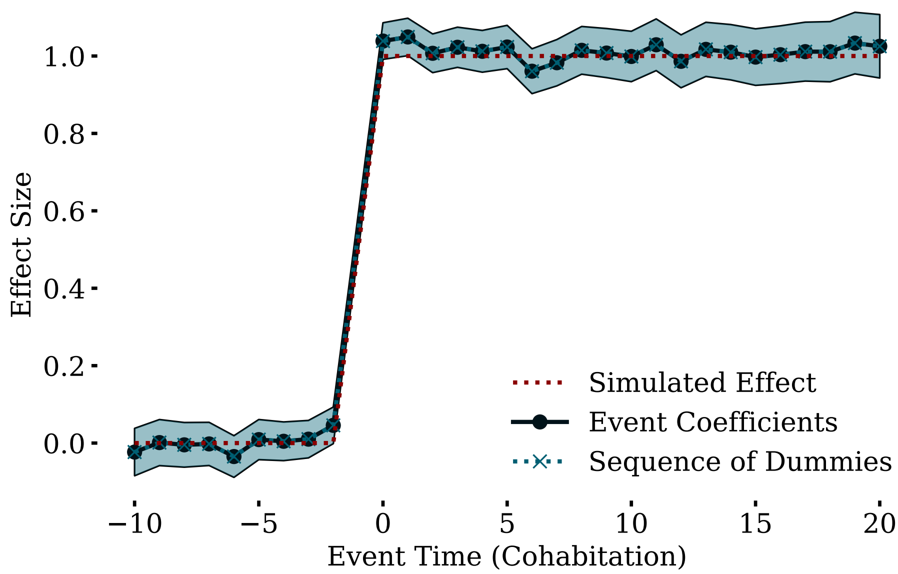
I replicate the results from above in this setup, and as shown in figure B.2 I can also produce unbiased estimates of the child effect in this setting.

Figure B.2: *Simulated Regression Results for Specification with Controls Along Correlated Event Axes. Estimated Effect of Children.*



Note: This figure shows the child-event-time coefficients estimated in simulated data, that contains both cohabitation, breakups and children as described in the main text.

Figure B.3: *Simulated Regression Results Using Two Specifications of the Child Control Axis*



Note: This figure shows the child-event-time coefficients estimated in simulated data, that contains both cohabitation, breakups and children as described in the main text. The black dots control for children via event time coefficients $\mathbf{1}(t - t_0^k = x)$, while the blue crosses uses a sequence of dummies equal to $\mathbf{1}(t + x - t_0^k \geq 0)$.

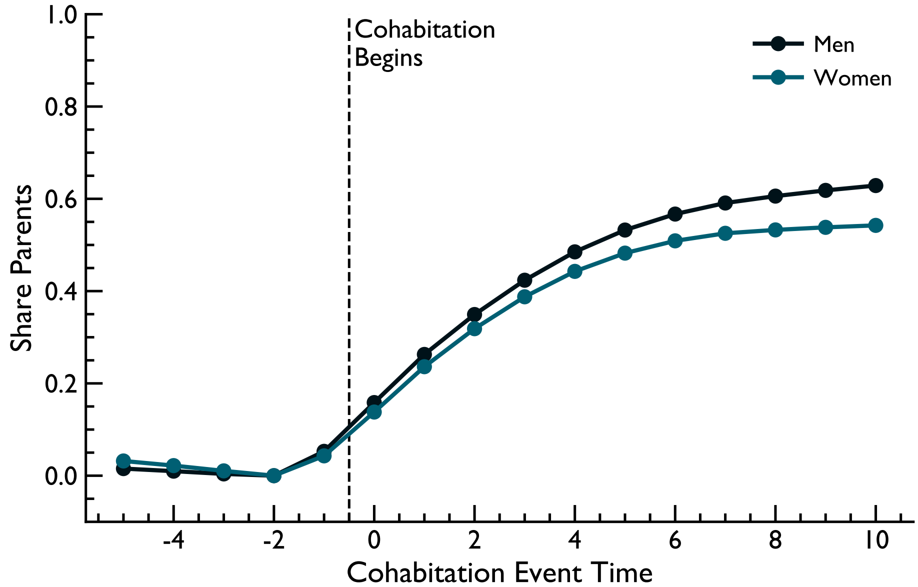
C. The Cohabitation Penalty

Table C.1: *Cohabitation Penalties in Subsamples*

index	Estimate	Predicted Earnings	Pct. Effect	Observations
Total Effect of Cohabitation	-31618.51*** (179.513)	225095	-14.05%	12.1m
Unmediated by Children	-12067.639*** (226.201)	225095	-5.36%	12.1m
Event Controls for Children	-13866.45*** (224.623)	225095	-6.16%	12.1m
Gets Married With Partner	-11990.732*** (293.803)	232257	-5.16%	7.5m
Gets Kids With Partner	-11180.933*** (280.57)	231322	-4.83%	7.3m
Have Kids From Earlier Relationship	-9822.089*** (611.981)	200604	-4.9%	1.8m
Non-Danish Individuals	-13321.84*** (1559.635)	167237	-7.97%	398.1k
Western Origin	-13066.54*** (3540.757)	169957	-7.69%	58k
Middle Eastern or North African Origin	-19523.033*** (2454.775)	162643	-12.0%	171.4k

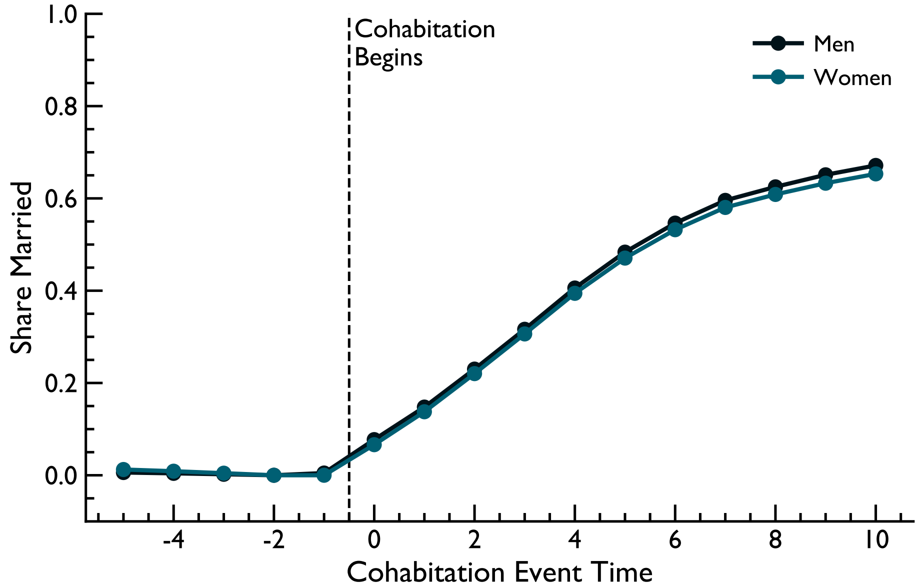
Note: The table shows estimates cohabitation penalties across a range of subsamples. The first column shows the estimates in DKK. The second column show the counterfactual earnings predicted using a regression omitting the treatment variable, also in DKK. The percentage effect is then computed as the estimated effect divided by the counterfactual following equation (3). All estimates except (1) are conditional on realized fertility.

Figure C.4: *Fertility over Cohabitation Event Time*



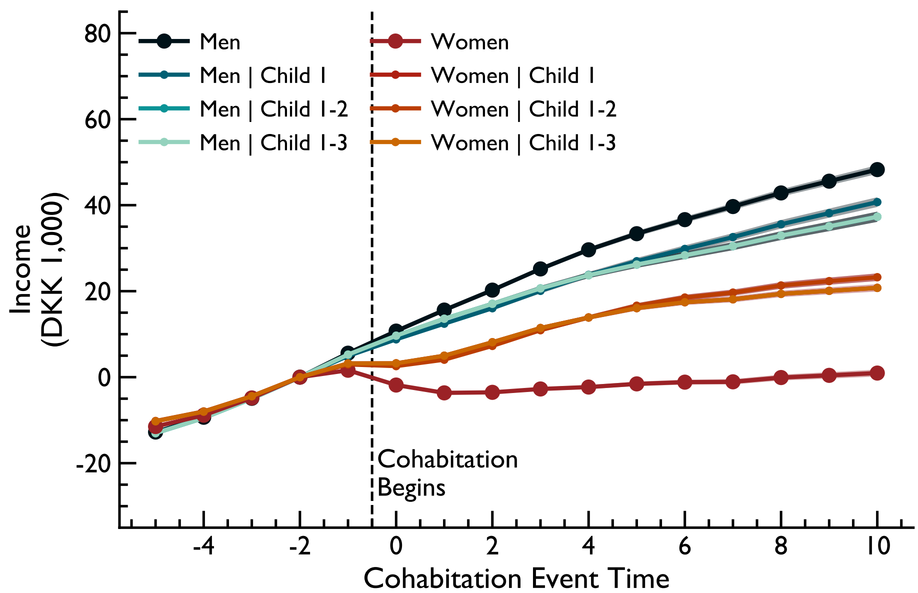
Note: This figure shows the effect of cohabitation on fertility. It is made by estimating models identical to the ones used to estimate the effect of cohabitation on earnings in the main text, but replacing the dependent variable with an indicator for having children. 95% confidence bands are indicated by shaded areas.

Figure C.5: *Marriage over Cohabitation Event Time*



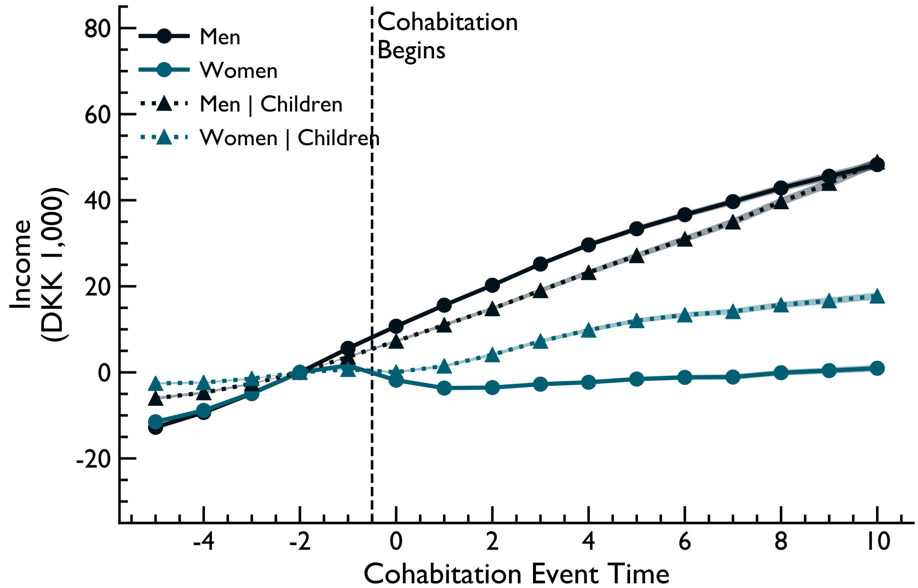
Note: This figure shows the effect of cohabitation on marriage. It is made by estimating models identical to the ones used to estimate the effect of cohabitation on earnings in the main text, but replacing the dependent variable with an indicator for being married. 95% confidence bands are indicated by shaded areas.

Figure C.6: *Cohabitation Penalties with Controls for up to Three Children*



Note: This figure replicates the results shown in figure 2 but adds additional regression results which include controls not only for the firstborn child, but children and 1-2, 1-3 as well. Each additional child is included via separate binary indicators. 95% confidence bands are indicated by shaded areas.

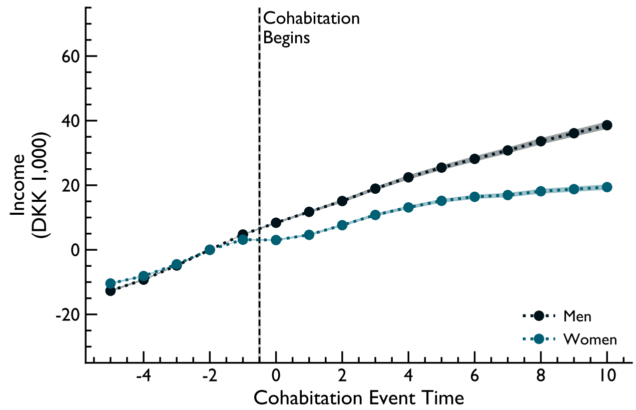
Figure C.7: *Cohabitation Penalties with Event-style Controls for Children*



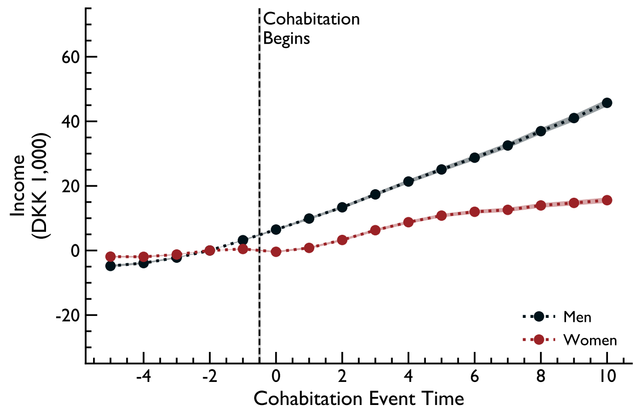
Note: This figure replicates the results shown in figure 2, but includes a full set of child event time coefficients in the set of control variables when accounting for children. 95% confidence bands are indicated by shaded areas.

Figure C.8: *Cohabitation Penalties Using Child Controls of Increasing Complexity*

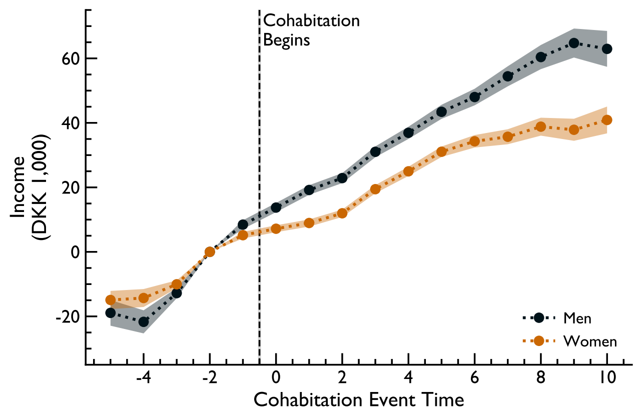
(a) *Controls for Child Dummy*



(b) *Controls for Child Event Axis*



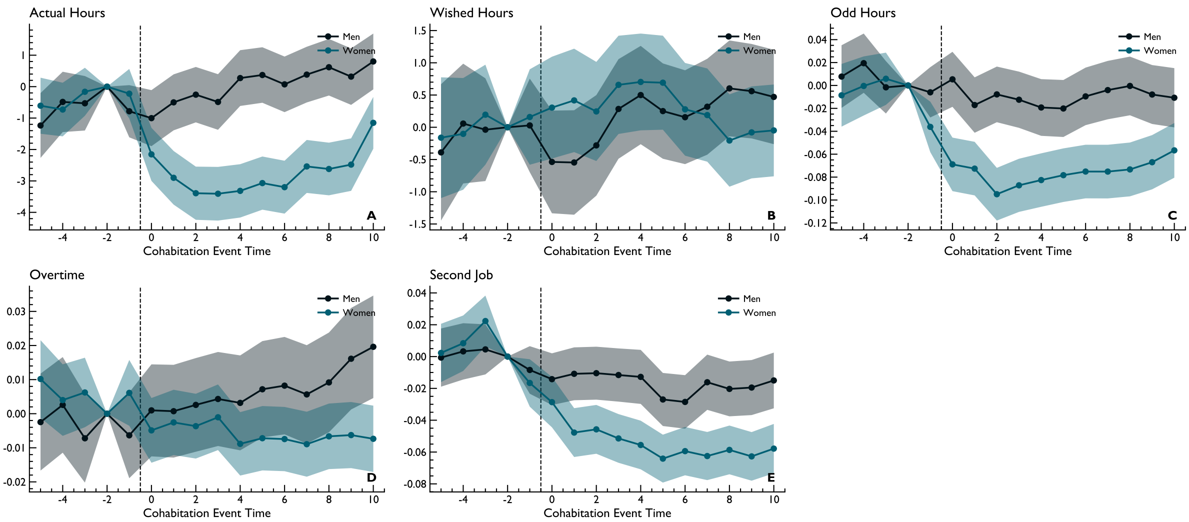
(c) *Controls for Child×Cohabitation Event Interactions*



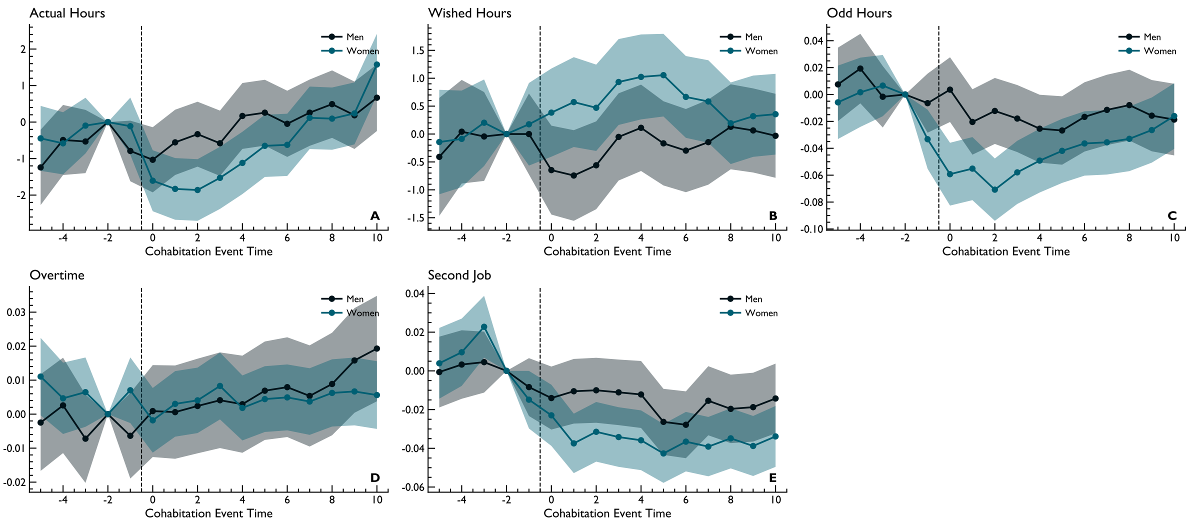
Note: This figure replicates the results shown in figure 2, but only reports the results conditional on children. Each panel reports results using a different control strategy. Panel (a) controls for fertility using a dummy for having children. Panel (b) uses a full child-event-time axis to estimate two sets of event coefficients simultaneously. Panel (c) interacts the cohabitation and child event axes, as suggested in section II.B. 95% confidence bands are indicated by shaded areas.

Figure C.9: *Cohabitation Penalties in Variables Recorded in the Danish Labor Force Survey*

(a) *Without Child Controls*

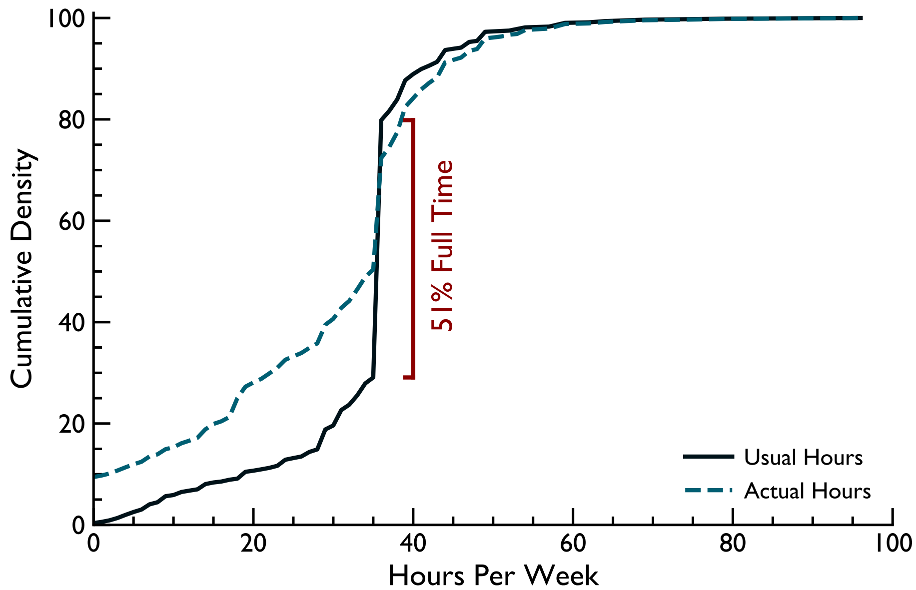


(b) *With Child Controls*



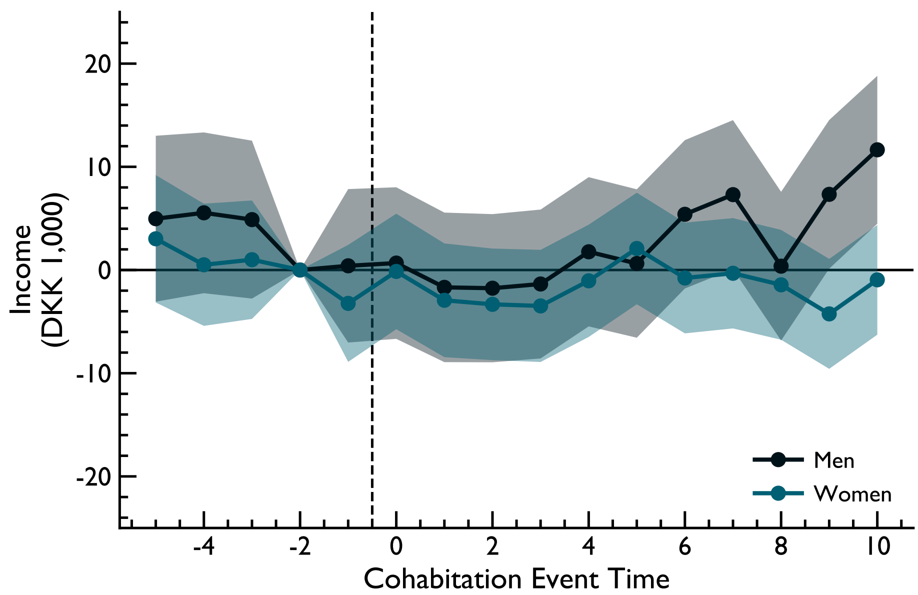
Note: This figure shows event-style results for the LFS variables that I report summary estimates for in the main text in table 4. 95% confidence bands are indicated by shaded areas.

Figure C.10: *Cumulative Distribution of Self Reported Usual Hours in the LFS*



Note: This figure shows the cumulative distribution of the "usual hours" reported by respondents of the Labor Force Survey. Respondents are asked to provide their hours of work in a typical work week, with options spanning whole numbers between 0 and 98 hours per week.

Figure C.11: *LFS Participants Income Difference From Non-participants Over Event Time*

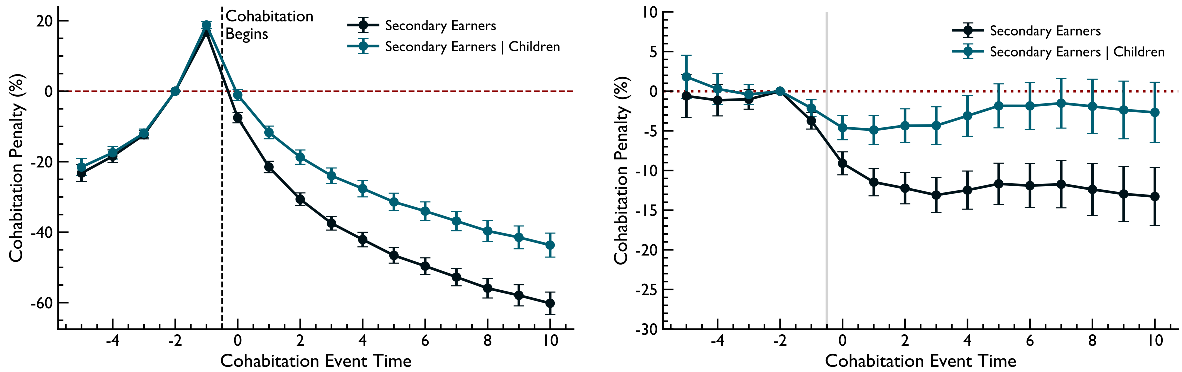


Note: This figure investigates if selection into the LFS survey is changing over cohabitation event time, by estimating the difference in income for participants and non-participants at every event time, relative to event time -2. The coefficients are based on a regression of the form $y^g_{it} = \sum_{j\neq-2} \mathbf{1}_{(t=j)}\beta^g_j + \sum_{j\neq-2} \mathbf{1}_{(i \text{ LFS participant at } t)}\mathbf{1}_{(t=j)}\gamma^g_j + \mathbf{C}^g_{it} + u^g_{it}$ with the parameter of interest being γ^g_j. Absent differential selection over event time, I expect to observe $\gamma^g_j = 0$ across event times and gender.

D. Mechanisms

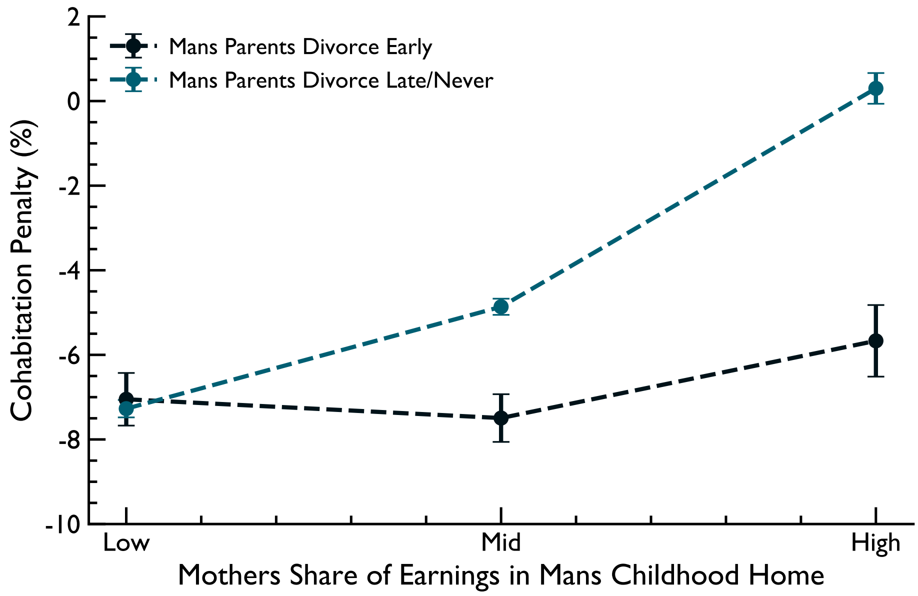
Figure D.12: *Cohabitation Penalties for Female Secondary Earner Couples*

(a) *Individuals Ex-Ante Income* **(b)** *Education Predicted Income*



Note: Each figure shows cohabitation penalties for women who are characterized as secondary earners, using men who are secondary earners in their relationship as baseline. In panel (a) secondary earners are defined as earning at most 40% of household income ex ante, while in panel (b) they are defined by their education predicting that they will earn at most 40% of household income. The cohabitation penalties are estimated using diff-in-diff models between men and women using between-couple variation. 95% confidence bands are indicated by error bars.

Figure D.13: *Cohabitation Penalty by Mens Childhood Home, Split By Exposure to Household*



Note: This figure shows that the correlation between cohabitation penalty magnitude and childhood homes compliance to traditional gender roles is driven by men whose parents live together for a significant portion of their childhood, suggesting a causal mechanism. The cohabitation penalty estimates are conditional on children. 95% confidence bands are shown as errorbars, standard errors are clustered at the individual level.