Empowerment Effects and Intertemporal Commitment of Married Couples: Evidence from Japanese Pension Reform*

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January 23, 2025

Abstract

This study investigates a household's commitment to a resource allocation by utilizing a 2007 Japanese pension reform allowing divorced women to claim a portion of their husband's pension benefits while keeping the household's total benefits unchanged. Although the reform would have had no effect on a couple's decision making under full commitment, we find that it increased wives' leisure activities and decreased their market and domestic work. This suggests that wives were able to increase their welfare by exploiting an improved outside option, and thus their commitment to the resource allocation was less than complete.

Keywords: family; bargaining power; risk sharing; pension; divorce law
JEL codes: D13, D91, H31, J12, K36

*This research is supported by JSPS KAKENHI Grant Number 18J21486. I am deeply grateful to Stacey Chen, Taiyo Fukai, Drew Griffen, Daiji Kawaguchi, Keisuke Kawata, Mizuki Komura, Ayako Kondo, Philip MacLellan, Ryuichi Tanaka, Carlos Carrillo-Tudela, and conference participants at Australian National University, Hitotsubashi University, Osaka University, and the University of Tokyo for their useful comments.

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1 Introduction

Economists have long been interested in the extensive economic gains from marriage, including the joint use of public goods, specialization, and self-insurance of household members. Self-insurance is especially important for the personal security of family members, as the effects of business cycles can be severe when the risks of job displacement are not fully insured (Krebs, 2007). While these risks can be mitigated by conventional unemployment insurance, they can also be hedged by income pooling through marriage, though inter- and intra-family insurance would not completely absorb income shocks (Altonji et al., 1992; Hayashi et al., 1996). In addition, marriage also allows an individual to insure other risks such as longevity risk through the heritage of his/her partner (Kotlikoff and Spivak, 1981).

However, to realize these economic gains, marriage partners must be able to commit to their initial resource allocation plan, and the traditional unitary model of household decision making implicitly requires this by assuming the household is a single decision maker. However, when marriage partners have distinct preferences, this introduces a potential conflict in their respective incentives that can lead to a "hold-up" problem whereby one partner might deviate from the initial allocation plan by exploiting a changed situation to improve his or her welfare. Should this occur, a couple cannot fully enjoy the fruits of marriage. Dufwenberg (2002) finds in theory that a couple's failure to commit to a resource plan leads to a failure to specialize and an under-accumulation of human capital, while Voena (2015) finds that divorce and property division laws can affect both asset allocation and female labor force participation in practice. From these studies, we see that a couple's lack of commitment can make it difficult for a household to achieve the first-best resource allocation, or *ex-ante* efficiency. Economic gains from family formation thus depend on the degree of commitment.

Since the seminal work of Chiappori (1988), research in household decision making has shifted from the unitary model to the collective one in which household members have individual preferences and the resource allocation of the household is obtained through bargaining.[1] However, studies of bargaining in family decision making have typically adopted a static collective framework that is silent about dynamic issues such as divorce or changes in the viability of an outside option for each household member. In adapting the collective model to a dynamic setting, Mazzocco (2007) argues that differing preferences among household members could result in a lack of commitment because one has an incentive to deviate from the initial plan when it is favorable to that member. Since the bargaining position is fixed under full commitment but is allowed to fluctuate under limited commitment so that household members may attempt to

[1]Apps and Rees (1997), Browning and Chiappori (1998), Chiappori (1992), Chiappori (1997), and Blundell et al. (2005) developed the collective model, while others such as Angrist (2002), Attanasio and Lechene (2014), Aura (2005), Cherchye et al. (2012), Duflo (2003), Francis (2011), Ponczek (2011) have tested and rejected the unitary model. Aronsson et al. (2001) is one of the few studies that has not rejected the unitary model.

improve their situation when outside options change, the degree of commitment is testable by examining within-household variation in a bargaining position.

These empirical tests of the degree of commitment are of interest not only regarding the economic benefits of marriage but also for their practical implications in the specification of life-cycle models. While the limited commitment model is more general than the full commitment model, its complexity leads to a computational burden in obtaining a solution (Chiappori and Mazzocco, 2017). Consequently, studies typically attempt to limit this burden through such simplifying assumptions as full commitment (Casanova, 2010), single agent (Adda et al., 2017), or functional form impositions on the Pareto weight (van der Klaauw and Wolpin, 2008).[2] While full commitment may be a good approximation of reality when any distortions to resource allocations associated with hold-up problems are relatively minor, researchers must consider the extent to which the full commitment assumption drives their results. If distortions exist, the model should allow for the possibility of incomplete commitment.

Another reason why limited commitment should be considered is that full commitment has not been supported by some recent empirical studies. For example, in their studies of dynamic models that nest the full commitment case, Mazzocco (2007) and Lise and Yamada (2018) both find evidence inconsistent with full commitment, with Lise and Yamada (2018) finding that household members update their bargaining position but only when the participation condition for marriage is binding. In another study, Blau and Goodstein (2016) test the degree of commitment by examining whether an unexpected inheritance affects the relative bargaining position and household behavior while controlling for household budget and any expected inheritance. Since a couple under full commitment fixes its bargaining position at marriage, it should not be affected by any unexpected shock when preferences and budget constraint are held constant. However, they find mixed results, with full commitment rejected in some specifications but not others.[3] Notwithstanding the mixed results, an advantage of the Blau and Goodstein (2016) approach compared to the former two studies is their identification strategy which does not rely on assumptions about the functional form of preferences or household technology. For example, Mazzocco (2007) assumes the bargaining position to be independent of the level of assets, and Lise and Yamada (2018) assume an interior solution for time allocation. These assumptions are strict, for if unexpected shocks only affect bargaining position, it is not necessary to specify the forms of the household objective function or home production technology.

This study contributes to the literature on household decision making by testing the degree of commitment without relying on any *a priori* assumptions about functional form and by using

[2]Studies that do allow for incomplete commitment include Voena (2015) and Low et al. (2018)

[3]These mixed results are attributed to a lack of statistical power, as inheritance is a rare event. Because inheritance amount has been shown to suffer from serious measurement error (Laitner and Sonnega, 2010), the receipt of an inheritance is used instead, which lowers the statistical power. Additionally, as the study examines several full commitment null hypotheses one-by-one under various specifications, the statistical inference becomes difficult to interpret.

a Japanese dataset that allows us to exploit a major pension reform as a natural experiment using a difference-in-differences (DD) estimation methodology. The Japanese pension reform of 2007 allowed a couple to divide their pension benefits upon divorce. Before the reform, since the bulk of public pension benefits in Japan are proportional to labor earnings, a dependent spouse specializing in home production would have found it difficult to live only on her own pension benefits after divorce. The reform addressed this issue by allowing the spouse with fewer accumulated pension benefits to claim a portion of the partner's pension benefits tracked during the marital period. For our purposes, several features of the reform are beneficial for testing the degree of commitment of a household. Firstly, the reform allows for the sharing of pension benefits while keeping the total amount of benefits unchanged. The unexpected shock of the pension reform thus does not change the household budget, which facilitates our identification of the degree of commitment. Secondly, the new pension division rule applies only to the public pension insurance that covers permanent employees, which leaves a dependent wife of a self-employed spouse unaffected, serving as a control group. Thirdly, the maximum share of pension benefits after division is 50 percent, which means that if both spouses are permanently employed and of a similar age, there is very little room for pension balancing. This allows us to use these households as a control group for the counterfactual inference required for our difference-in-differences (DD) estimation strategy.

Following the Mazzocco (2007) framework, we first constructed a dynamic collective model to investigate the effect of the pension reform under both full and limited commitment, finding that only couples without full commitment were affected. Next, we tested several of the model's predictions, which are, first, that as the reform is expected to mostly affect couples in which the husband's pension benefits are large, and since most young couples have not yet accumulated substantial benefits, the reform is most likely to affect elderly couples.[4] Second, low-net-worth households are more likely to respond to the reform because pension benefits comprise only a small share of the total assets of high-net-worth households that would be divided upon divorce. Third, as noted by Chiappori and Mazzocco (2017), the dynamic collective model implies that household members re-bargain their resource allocation only when one member's participation condition is binding. We tested this prediction using young couples, whose current period participation condition is unlikely to bind because they cannot receive pension benefits for several decades.

We then conducted empirical DD analysis using the *Keio Household Panel Survey* (KHPS), a household panel survey in Japan. The treatment group consists of households in which the husband was a permanent employee and the wife was not, while the control group consists of all other households. The results of our DD estimation do not support full commitment. Consistent

[4]In a study of the reform using relatively young couples of 48 years and younger, Sakamoto (2008) finds no policy impact.

with our model's prediction, we did not find any significant impact on young couples, but elderly wives aged 50–59 increased their leisure time by 5 hours per week (or 5 percent) by decreasing their market and domestic work equally. The elasticities of those outcomes to the lifetime pension benefits received upon divorce are 0.05, 0.20, and 0.05, respectively. A subsample analysis of homeownership as a proxy for individual assets showed that those wives who were most affected by the reform were those who did not own a home, which is also consistent with the model's prediction. Finally, the model passed several tests of the parallel trend assumption of our identification strategy including a placebo test, a specification with a group-specific linear trend, and a triple-differences estimation using young households as an additional comparison group. All in all, our empirical results provide strong evidence that households failed to achieve full commitment.

As such, this study thus highlights the difficulty in making a commitment, as couples fail to achieve full commitment even within a stable marital relationship. This limited commitment implies that *ex-ante* efficient allocation is not necessarily *ex post* efficient and, as a result of this inconsistency, a hold-up problem may occur. The resulting inefficiency could be more serious among young couples because their relatively higher divorce rates indicate that the participation condition is likely to bind. If this is the case, then household behavior, including human capital accumulation and investment in children, may be distorted by a fluctuation in the bargaining position, and this also makes risk-sharing among household members difficult.

A limitation of the study is that the test of full commitment relies on the assumption that couples do not eventually divorce. This limitation is not unique to this study, however, and is typically imposed throughout the literature either explicitly or implicitly (Blau and Goodstein, 2016; Lise and Yamada, 2018; Mazzocco, 2007). When divorce is possible and seems on the horizon, spouses have an incentive to prepare for this future divorce (Mazzocco et al., 2006), which means that the household allocation plan can be contingent on the post-divorce economic situation even under full commitment. This concern would appear to be negligible for elderly couples in Japan, however, due to the low annual and lifetime divorce rates of 0.3 and 3.0 percent, respectively, for Japanese wives aged 55. Therefore, we believe that the assumption that couples do not eventually divorce cannot be the main driver of our findings.

Lastly, this study is related to the large literature on divorce law reforms, the majority of which evaluate the impact of the transition from a mutual-consent regime to a unilateral regime (e.g. Gray 1998; Gruber 2004; Stevenson and Wolfers 2006; Wolfers 2006; Stevenson 2007; Caceres-Delpiano and Giolito 2012; Brassiolo 2016; Garcia-Ramos 2021). However, the assessment of property division law upon divorce is relatively scarce even though this is highly relevant to a homemaker since her welfare upon divorce is deeply reliant on the alimony amount and asset division. To the best of our knowledge, the study of property division law focusing on homemakers is limited to Wong (2014, 2016), and since the Japanese pension reform of 2007

mainly targets homemakers, our study adds new evidence to this scarce literature.

2 Institutional Background

2.1 Japanese Pension System and the Reform of 2007

In order to better understand the context of the study, this section describes the Japanese public pension system and the reform of 2007. The Japanese public pension system consists of three insurance policies: the employee pension (*kosei nenkin*), the mutual aid pension (*kyosai nenkin*), and the national pension (*kokumin nenkin*). The first two policies cover permanent employees (i.e., full-time workers not hired under a time-limited contract) in the private and public sectors, respectively, but are otherwise identical, so we hereafter refer to them collectively as the employee pension.[5] Within the employee pension, participants aged under 70 pay pension premiums as long as they earn labor income, with the amount of the premium proportional to their labor income. The age of eligibility for benefits is around 60, depending on sex and birth cohort, with the age of eligibility higher for men than women and higher for a recent cohort than an earlier cohort (for details, see Table 8).

The pension benefits consist of two parts: a basic part and a proportional part, with the basic part depending only on the number of years the participant paid premiums and the proportional part depending on earnings and the duration of premium payments before retirement. The national pension covers those who are not covered by the employee pension, which includes mainly the self-employed, part-time employees and dependent wives, all of whom pay premiums until they turn 60 and become eligible for benefits at age 65. The national pension is similar to the basic component of the employee pension, except for the age of eligibility and the participants in each respective plan.

Before the pension reform of 2007, the Japanese pension system was thought to be inequitable because spouses were not allowed to claim any fraction of the pension benefits of their partners should they divorce. While a homemaker wife played an important role in enabling her husband to specialize in market work, she previously had no access to his pension benefits. As the average monthly national pension benefit in 2007 was only about 540 U.S. dollars,[6] a homemaker would have difficulty living only on her own pension benefits after divorce.

In order to address this issue, a reform was approved in 2004 and enacted on April 1, 2007 to permit divorcing spouses to divide the proportional part of the household's employee pension

[5]In addition to permanent employees, the pension plans also cover part-time employees who work more than three-quarters of the hours per day and days per month worked by a full-time employee. For example, if a full-time employee works eight hours per day and twenty days per month, then a part-time employee working more than six hours per day and fifteen days per month participates in either the employee or mutual aid pension.

[6]The employee pension paid out about USD 1,610 per month on average. Source: Japanese Ministry of Health, Labour and Welfare: `http://www.mhlw.go.jp/stf/seisakunitsuite/bunya/0000106808_1.html`

records tracked during the marital period.[7] Although the exact proportion of the household pension record claimed by either party is determined by agreement between the spouses, the division must range between 0-50 percent, as the spouse with the smaller pension cannot claim more than half of the total. There is thus no room for division if the records of both spouses are equal.[8] If the spouses fail to agree on a division, a rate is provided by the courts and in 99 percent of cases in 2007, this rate was 50 percent,[9] consistent with the asset division rule which equally divides any assets accumulated during the marital period.[10] Since a divorced homemaker is now assured of at least some income beyond her basic pension, the post-divorce situation is said to have improved.

Several features of the 2007 pension reform help to facilitate our analysis. Firstly, as the reform does not change the total amount of pension benefits received by a household, it does not affect the household budget under marriage, which allows us to isolate changes in allocations due to bargaining. Secondly, as the reform provides a better outside option to dependent wives, we can investigate whether this change affects commitment. Thirdly, as the pension reform leaves some households unaffected, the policy design allows us to create treatment and control households for the counterfactual inferences required for our DD estimation. Since the pension reform applies only to employee pension records, it does not affect the self-employed, who are covered by the national pension. Additionally, when both spouses are permanent employees, they each have their own employee pension record, so the pension splitting opportunities are limited. Hereafter, we refer to this latter type of household as a *dual-permanent* household.[11] As discussed below, households that were either dual-permanent or had one self-employed spouse were used to control for the economic trend in the counterfactual absence of the reform.

[7] As the basic part of the pension is not based on income, it is not divisible.

[8] Each household member can request from the Japan Pension Service an estimate of the pension benefits before or after divorce if he or she is 50 years old or older. Although this system could potentially create an information gap between older and younger households, it is unlikely to play an essential role for two reasons. First, since the rate of the pension contribution is public information, the wife would be able to make a good estimate of the husband's pension benefits from his earnings or, if not available, from the level of consumption. Second, even if the pension benefit amount is privately held by the husband, he has an incentive to reveal either the true value or at least a value close to the true value because pension benefits are the main source of income after retirement, so being untruthful about the pension benefits would result in inefficient intertemporal resource allocation.

[9] Source: Supreme Court of Japan; `http://www.courts.go.jp/vcms_lf/20513001.pdf`.

[10] A second pension reform was implemented in 2008 that allows a dependent wife (or husband) to claim half of the total household employee pension records tracked after May 1, 2008 should they divorce. This reform, which applies to households in which the spouse's annual income is less than about 13,000 U.S. dollars, has a fixed division ratio of 50 percent. However, considering that the first pension reform applies to the entire employee pension records tracked during the marital period, the function of this second reform is, at most, supplementary because it does not apply to pension records before May 1, 2008. As its impact on household behavior is likely to be negligible, our analysis focused only on the impact of the first reform.

[11] Explicitly excluded are those households in which the husband is a permanent employee and the wife works part-time, as part-time employees are typically not covered by the employee pension. However, even if a wife working part-time is covered by the employee pension, the amount of her pension records is likely to be much less than that of a husband who is a permanent employee.

2.2 Divorce Law in Japan

Although Japanese divorce law in principle requires mutual consent before a couple may divorce, because enforcement is not strict, it may in practice operate similarly to a unilateral divorce. Under Article 770 of the Japanese Civil Code, a judicial divorce is permitted if a spouse: (i) has been unchaste; (ii) has been abandoned in bad faith; (iii) is of unknown whereabouts and for at least three years it has not been clear whether s/he is dead or alive; (iv) is suffering from severe mental illness and there is no prospect of recovery; or (v) has any other grave concern making it difficult to continue the marriage. While the fifth condition is ambiguous, the Ministry of Justice has issued a guideline that allows a couple to divorce after five years of separation. As a result, a spouse who wishes to divorce may simply end cohabitation with his/her partner and file a suit for divorce after the required time has elapsed.

As for the distribution of assets upon divorce, the property division rule in Japan assures divorcees equal division of those assets for which both spouses "contributed" to obtaining, but this contribution does not need to be monetary, whereas other assets are divided based on the title. For example, if a wife specializes in home production and has no earnings, her domestic work is regarded as a contribution to purchasing housing. On the other hand, asset division does not apply to assets accumulated before marriage or obtained via inheritance. Since asset division is implemented based on holdings at the time of divorce or the end of cohabitation if a couple separates before divorce, separation can be an effective strategy for divorce without mutual consent, as a spouse can file for divorce several years after the end of cohabitation but asset division is implemented as if the couple had divorced several years earlier at the beginning of the separation.

2.3 Identification of the Policy Impact

We identify the policy impact via the parallel trend assumption and estimate it by the difference-in-differences (DD) method, with the treatment group consisting of households in which the husband was a permanent employee and the wife was not, and the control group consisting of all other households. As noted earlier, since the employee pension insurance covers a permanent employee but not non-permanent employees or self-employed workers, the treatment group is a typical household targeted by the reform. On the other hand, the majority of households in the control group consist of a self-employed husband and a wife without permanent employment, and this type of household is unaffected by the reform. The key identification assumption is that the time trend in the wife's time allocation is the same between the treatment group and the control group.

One concern is that the control group might include some dual-earner households in which both the husband and wife have permanent employment, in which case the time allocation of

the wife working full-time in the labor market is different from that of a wife not employed or working part-time. While this is a reasonable concern, our main empirical finding is robust to excluding those households from the control group. Another concern is that the employment status itself is endogenous and thus affected by the policy. To avoid this issue, we define the treatment status by the employment status in 2003, one year before the approval of the policy change. This prevents our treatment status from being confounded by any change in employment status in response to the policy change.

Finally, since the cumulative pension benefit amount is determined by the contribution history, we recognize that defining treatment status by the employment status at a specific point in time is inadequate. Some husbands in the treatment group may have recently switched from being self-employed to being employed, while some husbands in the control group may have recently done the opposite. Thus it is conceivable that there may be some untreated workers who may be included in the treatment group and vice versa. Since any resulting measurement error would result in a bias toward zero, however, our estimation results should thus be regarded as the lower bound of the policy impact. The magnitude of this error is not likely to be substantial, though, as switching between employed and self-employed status is relatively rare. According to the 2007 Employment Status Survey, 94 percent of men who entered the labor market as an employee or as a self-employed worker maintained that status when they were 45–54 years old. Therefore, we believe that any potential bias from this classification error would be marginal.

3 Theoretical Model of Household Decision Making

In this section, we provide a model to describe how a couple responds to the pension reform under full and limited commitment. In particular, we consider a household in which a husband and wife have distinct preferences and bargain over resource allocation. The resulting household resource allocation is in favor of a member whose outside option; namely, divorce, results in higher welfare. While this is the standard collective model setting (Chiappori, 1988, 1992), a problem arises when dynamic decision making is concerned; that is, commitment to the resource allocation plan (Mazzocco, 2007). Although both members benefit from the household's initial allocation plan given all the information available at time of marriage, a shock to one spouse's outside option could induce him/her to divorce even if it does not affect the household budget under marriage. In such a case, the household needs to modify its initial allocation plan to persuade that member to remain married. Since a lack of commitment makes household resource allocation sensitive to shocks that are unrelated to the household budget or preferences, the household decision becomes time-inconsistent and inefficient. On the other hand, the household may somehow be able to fully commit to the initial allocation plan so that household behavior is contingent on the household budget but not contingent on the realization of uncertainty

to the outside option. For example, as a commitment device, Dufwenberg (2002) incorporates guilt into the model.

If we look at the 2007 pension reform through the lens of this household model, it is regarded as a shock to the outside option of both spouses. Since it does not affect the total amount of pension benefits that the household can receive, a household with full commitment does not change its resource allocation plan. However, in the case of limited commitment, the pension reform increases (decreases) the lower-income (higher-income) spouse's pension benefits upon divorce, leading to the lower-income spouse potentially threatening a divorce unless the consumption plan is modified in their favor. We can thus infer whether or not the household is fully committed by investigating the household response to the pension reform. Furthermore, whether the lower-income spouse's threat to divorce is empty or not depends on the extent to which their outside option is improved by the policy. For example, if the lower-income spouse owned a large asset so that the value of pension benefits was marginal compared to that asset, the value of divorce would be relatively unchanged before and after the reform. Younger households would also not likely be affected by the reform because the higher-income spouse would not yet have accumulated many pension benefits and, additionally, pension eligibility would be decades in the future.[12] In both cases, the pension reform would have at best a marginal impact on the value of divorce, so the threat to divorce would likely be only an empty threat.

3.1 Model

To formally illustrate how the pension reform affects household decision making, consider the following 3-period problem for a 2-member household ($j = 1, 2$) in which the couple marries in period 1 and may or may not choose to divorce in period 2 or 3. The household members supply market and domestic labor in the periods 1 and 2, and retire in period 3. A fraction τ of labor earnings is collected as a pension premium, and the couple receives the pension benefits b_{j3} in period 3. Each spouse derives his or her welfare from private consumption c_{jt} and leisure l_{jt}, where the consumption good is produced by domestic labor h_{jt} and market good g_t. We assume unilateral divorce, so divorce is possible without mutual consent. We denote spouse j's asset and pension benefits upon divorce as a_{jt}^D and b_{jt}^D, respectively.

We now define full and limited commitment. A couple achieves "full commitment" if the *participation condition* that the value of the marriage for each member is greater than or equal to the value of divorce is required only in period 1, whereas the degree of commitment is said to be "limited" if the participation condition must be satisfied each period. Thus, under limited commitment, both members must be satisfied with the marriage each period. Otherwise, an unsatisfied member may threaten to divorce to increase his/her consumption or leisure even if

[12]This may not be true for a young spouse who has high prospects of remarrying somebody with much higher future pension benefits.

that member does not intend to carry through with the divorce.

The household problem under full commitment is therefore:

$$\max_{(c_{jt},l_{jt},b_{jt},g_t,a_{t+1})} \mu E_1\left[\sum_{t=1}^{3}\beta^{t-1}u_1(c_{1t},l_{1t},\theta_{1t})\right]+(1-\mu)E_1\left[\sum_{t=1}^{3}\beta^{t-1}u_2(c_{2t},l_{2t},\theta_{2t})\right], \quad (1)$$

$$\text{s.t. } c_{1t}+c_{2t}=F(h_{1t},h_{2t},g_t),$$

$$a_{t+1}=(1+r_t)a_t+\sum_{j=1}^{2}(1-\tau_{1t})(1-l_{jt}-h_{jt})w_{jt}-g_t \quad (t=1,2),$$

$$b_{j,t+1}=(1+r_t)b_{jt}+\tau_{1t}(1-l_{jt}-h_{jt})w_{jt},$$

$$0\le l_{jt}+h_{jt}\le 1, \quad l_{j3}+h_{j3}=1,$$

where θ_{jt} is match-specific utility, w_{jt} is j's market wage, r_t is the risk-free interest rate,[13] and the total time is normalized to one. Since the couple marries in period 1, the initial-period participation condition is summarized in the Pareto weight μ. Solving this problem backwardly, we observe that the household allocation plan is contingent on total assets $a_t+b_{1t}+b_{2t}$ but not on the assets and pension benefits divided upon divorce, $(a_t^D, b_{1t}^D, b_{2t}^D)$. Hence, the solution to the problem is:

$$x_{jt}(\Omega_t)=\tilde{x}_{jt}(a_t+b_{1t}+b_{2t},\theta_{1t},\theta_{2t},w_{1t},w_{2t}) \quad (x\in\{c,l,h\};j\in\{1,2\}), \quad (2)$$

where $\Omega_t=(a_t,a_{1t}^D,a_{2t}^D,b_{1t},b_{2t},b_{1t}^D,b_{2t}^D,\theta_{1t},\theta_{2t},w_{1t},w_{2t})$ is the set of state variables.

The household problem under limited commitment is similar to the full commitment household problem (1), but also requires the participation conditions in periods 2 and 3:

$$u_j(c_{j2},l_{j2},\theta_{j2})+\beta u_j(c_{j3},l_{j3},\theta_{j3})\ge V_{j2}^D\left(a_{j2}^D+b_{j2}^D,w_{j2}\right), \quad (3)$$

$$u_j(c_{j3},l_{j3},\theta_{j3})\ge V_{j3}^D\left(a_{j3}^D+b_{j3}^D\right) \quad (j\in\{1,2\}), \quad (4)$$

where $b_{jt}^D=b_{jt}$ under the pre-reform regime and $b_{jt}^D=\frac{b_{1t}+b_{2t}}{2}$ under the post-reform regime. $V_{j3}^D(a)$ is the value of divorce with asset a at period 3, which is defined as:

$$V_{j3}^D(a)=\max u_j(c_{j3},l_{j3}) \quad \text{s.t.} \quad c_{j3}=F(h_{j3},g_3), \quad g_3=a, \quad l_{j3}+h_{j3}=1. \quad (5)$$

[13] Although, for simplicity, the amount of pension benefits is assumed to be accumulated in the same way as savings, the model identification result does not rely on this assumption.

The period 3 problem is:

$$\begin{aligned} V_3(\Omega_3) = \max\ & \mu u_1(c_{13}, l_{13}, \theta_{13}) + (1-\mu) u_2(c_{23}, l_{23}, \theta_{23}), \\ \text{s.t. } & c_{13} + c_{23} = F(h_{13}, h_{23}, g_3), \\ & g_3 = a_3 + b_{13} + b_{23}; \quad l_{j3} + h_{j3} = 1 \quad (j \in \{1,2\}), \\ & u_j(c_{j3}, l_{j3}, \theta_{j3}) \geq V_{j3}^D\left(a_{j3}^D + b_{j3}^D\right) \quad (j \in \{1,2\}). \end{aligned}$$

Denoting the Lagrange multiplier on the participation conditions by λ_j (≥ 0), this problem is rewritten as:

$$\begin{aligned} \max\ & (\mu + \lambda_1) u_1(c_{13}, l_{13}, \theta_{13}) + (1 - \mu + \lambda_2) u_2(c_{23}, l_{23}, \theta_{23}), \\ \text{s.t. } & c_{13} + c_{23} = F(h_{13}, h_{23}, g_3), \\ & g_3 = a_3 + b_{13} + b_{23}; \quad l_{j3} + h_{j3} = 1 \quad (j \in \{1,2\}), \end{aligned}$$

where $\lambda_j = 0$ if member j's participation condition is not binding. Thus, the couple re-bargains only when the participation condition of one member binds.

The main difference from the full commitment case is that the pension division rule upon divorce, b_{j3}^D, matters, and this in turn implies that so does asset composition (b_{1t}, b_{2t}). Indeed, we observe in general that:

$$\lambda_j = \lambda_j(a_{13}^D, a_{23}^D, b_{13}, b_{23}, \theta_{13}, \theta_{23}) \neq \lambda_j(a_{13}^D, a_{23}^D, b_{13} + b_{23}, \theta_{13}, \theta_{23}).$$

Since a similar argument applies to the period 1 and 2 problems, the household allocation plan cannot be written as a function of total assets:

$$\begin{aligned} x_{jt}(\Omega_t) &= x(a_t, a_{1t}^D, a_{2t}^D, b_{1t}, b_{2t}, b_{1t}^D, b_{2t}^D, \theta_{1t}, \theta_{2t}, w_{1t}, w_{2t}) \\ &\neq \tilde{x}_{j,t}(a_t + b_{1t} + b_{2t}, \theta_{1t}, \theta_{2t}, w_{1t}, w_{2t}) \quad (x \in \{c, l, h\}; j \in \{1,2\}). \end{aligned} \tag{6}$$

Equations (2) and (6) give the test of the full commitment model using the variation in the amount of pension benefits divided upon divorce. In particular, the household consumption plan is not affected by how the pension benefits would be divided upon divorce under the full commitment; that is, $\frac{\partial x_{jt}}{\partial b_{jt}^D} = 0$. On the other hand, the household consumption plan is affected by the pension division rule under the limited commitment, $\frac{\partial x_{jt}}{\partial b_{jt}^D} \neq 0$. Since the 2007 pension reform affects the amount of pension benefits upon divorce, it is used to identify the model. To see the impact of the pension reform in detail, suppose that the reform is unexpectedly implemented at

the beginning of period 2. Under limited commitment, the initial allocation plan satisfies:

$$u_j(c_{j2}, l_{j2}, \theta_{j2}) + \beta u_j(c_{j3}, l_{j3}, \theta_{j3}) \geq V_{j2}^D\left(a_{j2}^D + b_{j2}, w_{j2}\right),$$
$$u_j(c_{j3}, l_{j3}, \theta_{j3}) \geq V_{j3}^D\left(a_{j3}^D + b_{j3}\right) \quad (j \in \{1, 2\}),$$

where $V_{j2}^D(a, w)$ is the value of divorce with asset a and wage w at period 2, which is the value function when a member remains single in periods 2 and 3, and is defined similarly to equation (5). However, due to the change in the pension division rule, the new participation conditions are:

$$u_j(c_{j2}, l_{j2}, \theta_{j2}) + \beta u_j(c_{j3}, l_{j3}, \theta_{j3}) \geq V_{j2}^D\left(a_{j2}^D + \frac{b_{12} + b_{22}}{2}, w_{j2}\right),$$
$$u_j(c_{j3}, l_{j3}, \theta_{j,3}) \geq V_{j3}^D\left(a_{j3}^D + \frac{b_{13} + b_{23}}{2}\right) \quad (j \in \{1, 2\}),$$

and the period 2 participation condition is now more restrictive for the lower-income spouse if $b_{12} > b_{22}$.

Taking this into consideration, other things being equal, the greater is $(b_{12} - b_{22})$, the more likely the participation condition for spouse 2 will bind. Since the pension division upon divorce applies only to the pension records during the marital period, a young couple does not have many divisible pension benefits (i.e. b_{12} is small). Furthermore, since pension benefits are not, in practice, considered to be collateral, a divorcing spouse cannot rely on future pension benefits until retirement. As a result, the reform is unlikely to substantially increase the value of divorce for a young spouse, especially one with limited labor market experience and a low market wage rate. For these reasons, the participation condition before retirement is less likely to be binding for young households. If the initial allocation plan satisfies the period 2 participation condition but does not satisfy the period 3 condition, the main impact is on the period 3 allocation because the period 3 participation constraint cannot hold without adjusting the allocation. The period 2 allocation is then affected secondarily due to consumption smoothing.

The above discussion implies that the main impact of the pension reform should be observed after the reform is implemented, and young households are unlikely to show a substantial change in allocation in the reform year even if they re-bargain their future resource allocation. Furthermore, given the concavity of the value function, the value of divorce is sensitive to the amount of pension benefits when the amount of other assets available upon divorce is small. This suggests that a couple with few assets other than pension benefits will be most affected by the reform. In Appendix A, we conduct numerical exercises to illustrate how the policy impact varies depending on the value of the husband's pension benefits and household assets, and it indeed gives results consistent with the above argument. See Appendix A for details.

3.2 Caveats

Our model, which is a simplified version of Mazzocco (2007), has only three periods whereas the data in our empirical setting is annual. Thus, although the numerical exercises in Appendix A illustrate some empirically testable implications, those results might not apply to our data due to the discrepancy between the model and the data. However, we do not extend our model to a T-period life-cycle model because our main point is that the pension reform serves as the identification source for the dynamic collective model, and this result is unchanged in a T-period life-cycle model. In addition, the remaining implications such as the heterogeneity of the policy impact by household age and assets are intuitive and empirically testable. For these reasons, we have used our theoretical model to motivate the specification of our empirical model.

It is worth noting that Mazzocco (2007) assumes that bargaining power is not a function of an asset in obtaining identification of the degree of commitment whereas we have used asset composition for identification. We can obtain empirically testable implications without making the former assumption because we instead implicitly assume that the couple does not ever divorce. When the probability of divorce is non-zero, the objective function of a household member consists of the value of marriage and the value of divorce, weighted by the probability of divorce. In other words, household members are concerned about their welfare after any potential divorce, which makes the household behavior contingent on the share of assets upon divorce even under full commitment. Therefore, when bargaining power is a function of an asset *and* the probability of divorce is non-zero, we cannot identify the degree of commitment. Since our assumption rules out such divorce-concern behavior, the model is identified by investigating the effect of the asset composition on household resource allocation.

While we acknowledge that our assumption that the couple does not ever divorce is restrictive, this assumption does not mean that the threat of divorce is merely an empty threat but rather means that the couple can always find a new resource allocation for which both members are better off staying married than divorcing. In Appendix B, we discuss the extent to which divorce-concern behavior would complicate our result in practice, and find that the model without divorce seems a reasonable approximation of the real world. This is firstly because the divorce rate among elderly households in Japan is quite low, with the lifetime divorce rate conditional on being married at age 55 being only about 3 percent. Second, the future value of divorce is discounted which, combined with the very low probability of divorce, suggests that the concern about future divorce is unlikely to be the main driver of the household resource allocation. Furthermore, given that the probability of divorce could be highly heterogeneous across households, as a further check of the robustness of our empirical analysis, we excluded households that were divorced between 2005 and 2012.

4 Data and Identification Strategy

4.1 Data

This study utilizes data from the *Keio Household Panel Survey* (KHPS) provided by the Keio University Panel Data Research Center. The KHPS is an annual household-level panel survey of households beginning in 2004 and consisting of 4,000 households (3,000 married and 1,000 single). Each year, the survey is conducted at the end of January and respondents are asked about their usual time allocation as well as background information such as age, sex, family composition and employment status. As Japan's fiscal year begins in April, each implementation of the survey occurs in the last quarter of the previous fiscal year, with KHPS 2004, conducted in January 2004, asking about respondent behavior in FY2003, for example. Accordingly, KHPS 2004 includes data on the socioeconomic status of respondents before the 2004 pension reform approval and KHPS 2008 and succeeding waves represent household behavior after the enactment of the reform on April 1, 2007.

Although ten waves (KHPS 2004 through 2013) were available, we restricted our main sample to KHPS 2005 through 2008. As KHPS 2004 does not include information about time spent on childcare, the domestic labor supply in this wave is inconsistent with other waves so we used this first wave only to obtain background information. We also excluded KHPS 2009 through 2013 from our sample because of two external events, the global financial crisis of 2008 and the Great East Japan Earthquake of 2011, for it would be difficult to identify separately the effects on households of these two major events from the effect of the 2007 pension reform. With this in mind, the set of households chosen for the analysis sample was selected according to the following criteria: (1) spouses who married before 2004[14], who live together, and who were both aged 30–59 in 2007; (2) a time allocation that meets the constraint of 168 hours; and (3) the lack of any missing key variables for our analysis.[15] In our empirical analysis, we used family size, the number of children, and an indicator variable for children aged less than or equal to six to control for household heterogeneity. The variables of interest are time allocation, leisure, market labor supply, and domestic labor supply[16], which are measured in average hours per week.

Difference-in-differences (DD) estimation, which separates the impact of a specific policy from the counterfactual time trend that the treatment group would have faced had there not been any treatment, requires that the analysis sample of households be divided into a treatment group and a control group. We defined the treatment group as households in which the husband was

[14]We did not include newly married couples in our sample because in 2004 these couples may have known about the pension reform at the time of their marriage and so within-household variation in bargaining position is not identifiable for those households.

[15]The second condition for the sample restriction drops about 10 percent of the observations while the third condition has little impact on the sample.

[16]Domestic labor supply consisted mainly of meal preparation, laundry, grocery shopping, cleaning, and childcare.

Table 1: Descriptive statistics of KHPS sample

	Wife		Husband		Household	
	Treatment (1)	Control (2)	Treatment (3)	Control (4)	Treatment (5)	Control (6)
Leisure	105.86	105.51	118.46	120.59		
	(32.63)	(29.02)	(19.14)	(21.96)		
Market labor supply	12.49	27.65	46.72	43.85		
	(15.30)	(21.71)	18.41	(20.79)		
Domestic labor supply	49.65	34.84	2.82	3.56		
	(35.23)	(29.22)	(5.82)	(7.16)		
Age	42.87	44.72	45.06	47.24		
	(7.64)	(7.67)	(8.01)	(7.83)		
Family size					4.04	4.07
					(1.21)	(1.39)
Number of children					1.72	1.52
					(0.96)	(1.00)
Children under 6 years old					0.24	0.17
					(0.43)	(0.38)
Marital period					15.77[i]	17.53 [ii]
					(8.25)	(8.66)
Observations	2,868	1,812	2,868	1,812	2,868	1,812

Note: The table shows means and standard deviations, with the latter in parentheses. Time allocation was measured as the average hours per week. i) Sample size was 2836. ii) Sample size was 1776.

permanently employed but the wife was not because those households would have been most affected by the pension reform. The control group consists of the remaining households; that is, households in which the husband was self-employed[17] and *dual-permanent* households in which both members are permanent employees, so households in which the wife worked part-time are excluded. The treatment status of each household was fixed throughout the sample period by using the employment status in 2003, one year before the approval of the reform.[18]

4.2 Sample and Treatment Group Validity Checks

The sample descriptive statistics in Table 1 show some differences between the treatment and control households, with the average treatment household being slightly younger and having more children than the control household. Further, while leisure time is similar, wives in treatment households work less in the market and longer in domestic production than those in control

[17]Households in which the wife was a permanent employee and the husband was not would also have been affected by the pension reform, but as the impact would be in the opposite direction of that of our treatment group, and since the size of this subset was very small, we included this type of household in the control group.

[18]A change in employment status is relatively rare, and nearly 90 percent of observations would be assigned the same treatment status if current employment status were used instead of employment status in 2003.

households, while husbands in treatment households work longer in the market and less in domestic production.[19] Although these differences may signal potential heterogeneity between the two groups, our identification strategy is robust to this heterogeneity because DD estimation with household fixed effects controls for heterogeneity in preferences as long as the differences demonstrate the same trend over time. However, as a further control, we also implemented a triple differences (DDD) estimation (discussed in Section 6.5) that is robust to a violation of this parallel trend assumption.

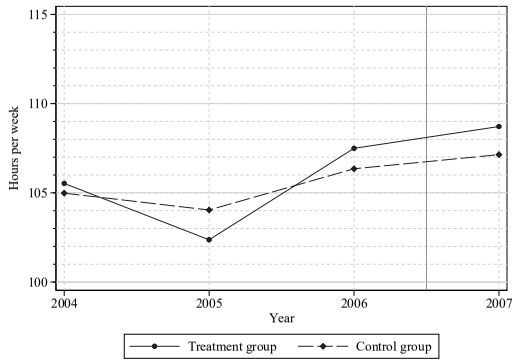
Before implementing our DD estimation strategy, we visually checked to see if the parallel trend assumption required to identify the average treatment effect on treated (ATT) was likely to hold by comparing the trends in the pre-treatment period. Figure 1 shows trends in the time allocation of each spouse and highlights several points relevant to our analysis. First, as is also seen in the descriptive statistics, the amount of time allocated to various activities differs between the treatment and control groups. In particular, there are relatively large differences between the two groups in the market and domestic labor supplies of wives, with the typical wife in the treatment group working more hours in the household and fewer in the market than the typical wife in the control group. However, with control variables and fixed effects, DD estimation can account for any differences in the absolute levels of time allocation as long as the parallel trend assumption holds. Second, and more importantly, the treatment and control groups show a similar trend before the pension reform, which supports the parallel trend assumption required for our DD estimation.

Since the pension reform is expected to affect the time allocation of elderly households more than young households, we also show the trends for leisure time of the wife by age group (Figure 2). For the oldest age group, the leisure of the treated wife was stable (120–121 hours/week) between 2004 and 2006 before increasing to 124 hours/week in 2007. In contrast, the leisure of the untreated wife was almost the same in 2007 as in 2004. We acknowledge an apparent increase in the leisure of the untreated wife in 2006, which could possibly lead to a spurious policy impact. However, our placebo analysis with the baseline year as 2004 shows that this apparent increase in leisure is statistically insignificant, so it is likely to be sampling error (Section 6). For the middle age group, the leisure of the treated wife increased in 2006 but not in 2007 while the leisure of the untreated wife was stable during the analysis period. For the youngest age group, we did not observe any relation between the pension reform and a change in the wife's leisure time.

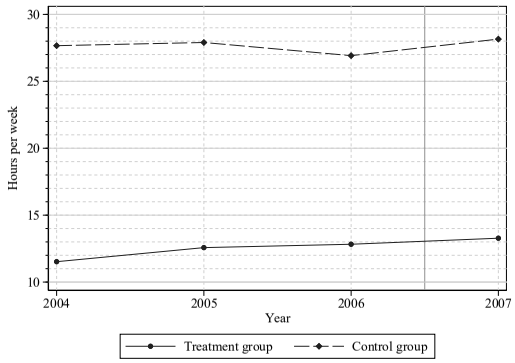
4.3 Regression Framework for Difference-in-Differences Analysis

We applied a difference-in-differences (DD) estimation strategy to investigate the causal impact of the 2007 Japanese pension reform, with the treatment group households in which the hus-

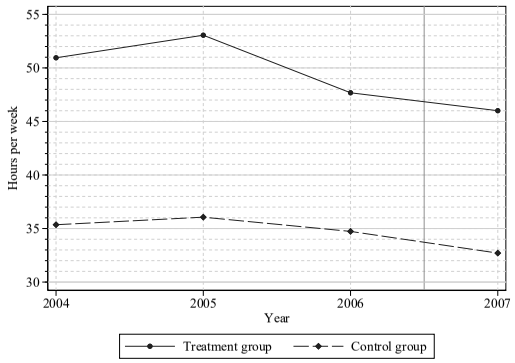
[19] See Table 10 for descriptive statistics of time allocation before and after the reform.



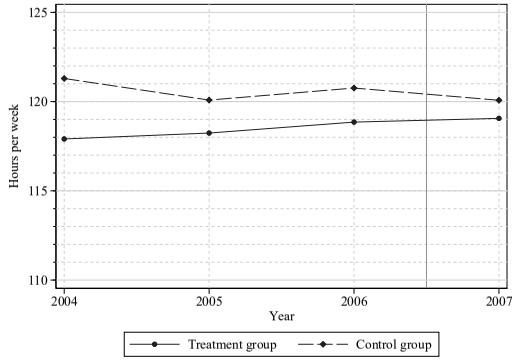
(a) Leisure: Wife



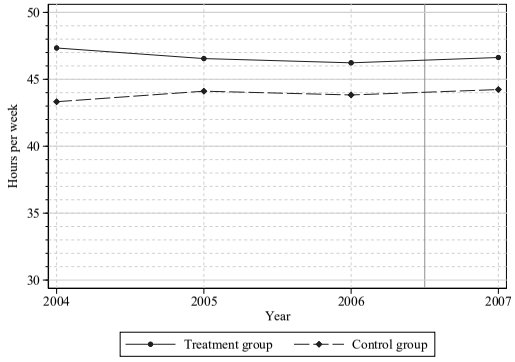
(c) Market Labor Supply: Wife



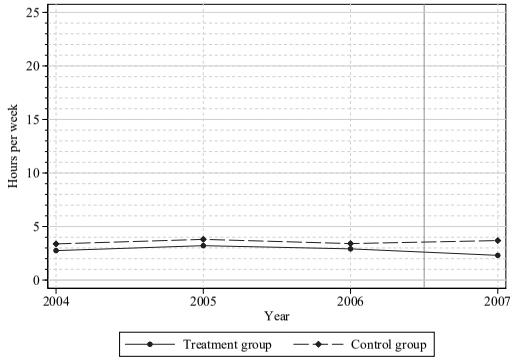
(e) Domestic Labor Supply: Wife



(b) Leisure: Husband



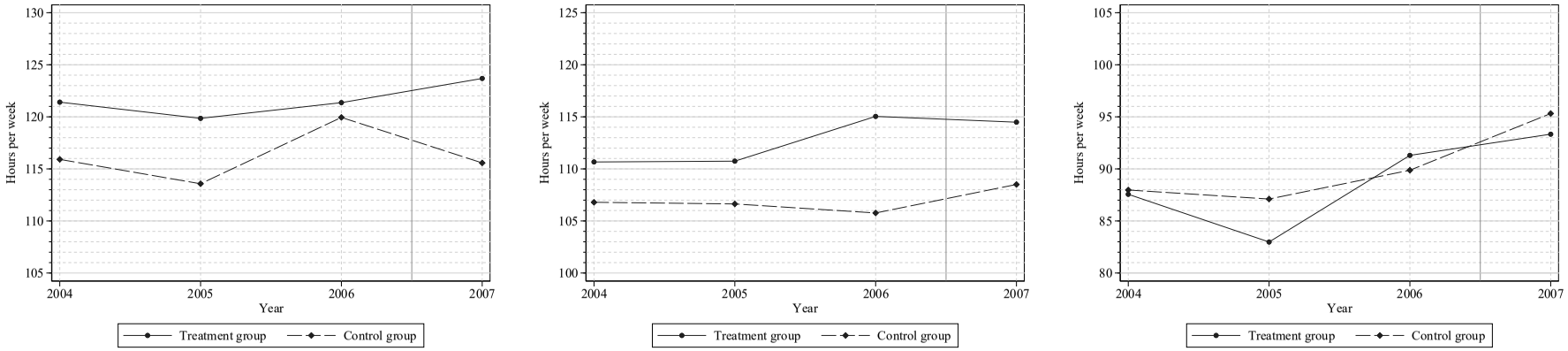
(d) Market Labor Supply: Husband



(f) Domestic Labor Supply: Husband

Figure 1: Time trend in market labor supply, domestic labor supply, and leisure

Data Source: The *Keio Household Panel Survey*
Note: This figure shows the yearly trend in the time allocation of wives and husbands. The solid lines represent the time allocation of individuals in the treatment group while the dotted lines represent individuals in the control group. The vertical lines highlight 2006, one year before the pension reform was enacted.



(a) Wife Aged 50-59 (b) Wife Aged 40-49 (c) Wife Aged 30-39

Figure 2: Time trend in time allocated to leisure by age of wife

Data Source: The *Keio Household Panel Survey*
Note: Yearly trend in the time allocated to leisure by wives is presented for each age group. Solid lines represent the treatment group and dotted lines represent the control group. The vertical lines indicate 2006, one year before the pension reform was enacted.

band was a permanent employee in 2003 and the wife was not, and the control group all other households. The estimation equation is

$$y_{it}^{j} = \delta_1^{j} After_t \cdot Treatment_i + x_{it}'\beta^{j} + \pi_t^{j} + c_i^{j} + u_{it}^{j}, \tag{7}$$

where i, j and t denote household, household member and year, respectively. Dummy variables include $After_t$, which takes one if $t = 2007$, and $Treatment_i$, which takes one if household i is in the treatment group. The control variables x_{it} are a constant and household characteristics, which include the squared ages of both spouses, family size, number of children, and a dummy variable indicating whether the household has children aged 6 or younger.[20] π_i^{j} is year fixed effects and c_i^{j} is household fixed effects. Note that $After_t$ is equivalent to d_{2007}, and household fixed effects absorb the treatment dummy variable, $Treatment_i$, which is fixed according to employment status in 2003. Our control variables do not include the level of age because it is collinear with household fixed effects and year fixed effects. The dependent variable, y_{it}^{j}, is the allocation to leisure, market labor supply, and domestic labor supply of spouse j of household i in year t.

The coefficient of interest is δ_1^{j}, which represents the household time allocation response to the pension reform and is key to testing the degree of commitment. Under full commitment, a couple's consumption plan is contingent on its total assets, so asset composition is irrelevant given that amount. As the couple does not respond to the reform, δ_1^{j} is expected to be zero. Under limited commitment, however, the couple's consumption plan is contingent on each member's asset share upon divorce, so δ_1^{j} is not equal to zero, assuming that the participation conditions are violated in some households due to the reform. Hence, if δ_1^{j} is different from zero, we reject the full commitment model. Since the theoretical model suggests that the pension reform will have heterogeneous effects according to the wife's age, we estimated equation (7) by dividing the sample into three groups: an older group of households with wives aged 50–59 in 2007, a middle-aged group with wives aged 40–49, and a younger group with wives aged 30–39.[21]

Estimating equation (7) by the age of the wife allows us to investigate how spouses might update their bargaining positions. For example, if we find no policy impact on the time allocation of younger households, this suggests that those households display some degree of commitment, which is consistent with the model in which the re-bargaining occurs only when the participation

[20] Although we did not include wage rates in the empirical model since those of non-labor participants are unobserved, our estimates do not suffer from any bias as long as the (potential) wage rates are uncorrelated with the treatment status, conditional on household fixed effects, year dummy variables and other control variables. Since the treatment status was fixed over the period, we believe that this conditional independence assumption is not restrictive.

[21] Another approach would have been to categorize households by marriage duration instead of age, as the amount of divisible pension records depends on marriage duration, but this would have been problematic because the marital period is likely to be correlated with match quality. Indeed, in equations (3) and (4), we see that the higher the match-specific utility $\theta_{j,t}$, the less likely the participation conditions are to be binding.

Table 2: The effect of pension reform: Difference-in-differences estimation

Wife				
Age Group	Leisure (1)	Market Labor Supply (2)	Domestic Labor Supply (3)	Observations [Households]
50–59	4.976**	-2.363	-2.613*	1,333
	(2.509)	(2.136)	(1.435)	[442]
40–49	-0.322	2.166	-1.844	1,804
	(1.878)	(1.491)	(1.578)	[575]
30–39	-3.265	1.475	1.790	1,543
	(3.445)	(1.637)	(3.153)	[490]
All	0.797	0.695	-1.492	4,680
	(1.530)	(0.997)	(1.288)	[1,507]
Husband				
Age group	Leisure (4)	Market labor supply (5)	Domestic labor supply (6)	Observations [Households]
50–59	1.014	-0.171	-0.842	1,333
	(2.180)	(2.109)	(0.567)	[442]
40–49	-0.935	1.259	-0.324	1,804
	(2.056)	(2.004)	(0.504)	[575]
30–39	1.615	-0.811	-0.805	1,543
	(2.361)	(2.293)	(0.874)	[490]
All	0.376	0.312	-0.688*	4,680
	(1.281)	(1.243)	(0.371)	[1,507]

Note: The table shows the estimation results of equation (7) by the age group of the wife. Only the estimated values of δ_1^j are reported, with standard errors clustered by each household in parentheses.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$.

condition is binding. We must remember, however, that this specific test may lack statistical power, for even if the younger household does re-bargain its resource allocation plan, the effect may be too small to detect due to the small policy impact on the outside option of a young wife.

5 Results

5.1 Baseline Results

Table 2 reports the empirical results of the DD estimation showing the household response to the reform by age group. In the oldest group, consisting of households with wives aged 50–59, the wife's allocation to leisure increased by 5.0 hours per week, or 4.8 percent, which is accompanied by a roughly equal decrease in market labor supply and domestic labor supply, though these latter

effects are not statistically significant. The estimates in this age group are jointly different from zero at a 10 percent significance level, so our statistical inference is not a consequence of testing multiple hypotheses one by one.

These estimation results imply that the older spouses fail to fully commit to their initial allocation plan due to conflicting incentives, which leads us to reject the full commitment model of household behavior. While one might consider complete commitment as a good approximation if re-bargaining effects were negligible, we find substantial re-bargaining in the form of a 5-percent change in the wife's leisure. We thus conclude that the model with limited commitment is a better approximation of actual household behavior. Additionally, we find that home production played a non-negligible role in re-bargaining. As shown in Blundell et al. (2005), the level of home production depends on the marginal willingness of each spouse to pay, and thus the contribution to home production is an important bargaining domain.[22]

In order to gauge the magnitude of the policy impact, we consider the amount of pension benefits that a wife can access from her husband upon divorce. Suppose that the annual value of the divisible pension benefits for a wife aged 55 is 4,800 US dollars, which was the average amount among those who were eligible for benefits in 2014 (i.e., wives who were older than 53 when the pension reform was enacted)[23], the wife's age of eligibility for pension benefits is 60, and she marries at 28 and dies at 86, which reflects the average lifespan of women in Japan.[24] Setting the interest rate at $r = 0.01$, the present value of benefits for a wife aged 55 is USD 90,500. Given that the annual amount of her own national pension benefits is USD 6,500 beginning at age 65, the pension reform roughly doubles the present value of her total pension benefits from USD 92,000 to USD 182,600. This back-of-the-envelope calculation indicates that the wife's leisure, market work, and domestic work elasticities to the lifetime pension benefits upon divorce are 0.05, 0.20, and 0.05, respectively. Since the reform does not change total benefits, these effects are not due to any wealth effect but can be attributed solely to the re-bargaining effect. Furthermore, since the wife may have other assets at her disposal, the elasticity to the assets at her disposal upon divorce could be even larger.

In contrast to our results for older couples, we do not find any statistically significant changes in time allocation among the younger households, and the estimates for the two younger groups are also jointly insignificant (Table 2). It should be noted that these two younger groups correspond to the households in the Sakamoto (2008) study, which does not find any policy impact of the reform. This is consistent with the prediction of our model that the current-period participa-

[22]Blundell et al. (2005) discuss the provision of public goods under a cooperative framework. Although we specify the home production good as a private good, our model can be easily modified so that both private and public goods are produced at home without changing the main predictions.

[23]Source: Japanese Ministry of Health, Labour and Welfare; http://www.mhlw.go.jp/stf/seisakunitsuite/bunya/0000106808_1.html.

[24]Data Source: http://www.mhlw.go.jp/toukei/saikin/hw/jinkou/geppo/nengai07/ and http://www.mhlw.go.jp/toukei/saikin/hw/life/life07/index.html.

tion condition is unlikely to bind for younger households because the pension division applies only to the records tracked during marriage, which is short for young households. Furthermore, since pension benefits do not count as collateral, a divorcing wife cannot immediately receive the present value of any future pension benefits but must wait until retirement.[25] Finally, the discounted future value of the divorce receipts for a young wife would be low, again making it unlikely that the current-period participation condition would bind. For all these reasons, the possible division of pension benefits is less relevant to a younger wife, resulting in almost no impact of the reform on her household resource allocation. Even if the participation conditions at future periods bind under the current allocation, the impact on the current-period resource allocation is only through consumption-smoothing, not through the direct impact of the re-bargaining. As a result, the resource allocation would be gradually adjusted to minimize the deviation from the *ex-ante* efficient allocation.

This discussion is in line with Lise and Yamada (2018), who argue that small shocks do not trigger re-bargaining. Since any possible improvement in welfare is too small for a young wife to initiate divorce, her bargaining position is not updated. Consequently, the reform has virtually no impact on young households, for it affects neither the budget set nor the utility weight on each member. However, we must recall that this view of the method to update bargaining power should be treated with caution since it could possibly be due to a lack of statistical power in testing very small changes in time allocation.

5.2 Home Ownership and Net Housing Value

Another prediction from our model is that the impact of the reform depends on the amount of household assets other than pension benefits because the assets obtained through the pension division would be negligible for high-net-worth households. In Japan, the property division rule ensures divorcing spouses an equal division of assets they both contributed to obtaining, and one of the most important nonfinancial household assets is the family home. The interpretation of the law is that a home purchased during the marital period is divided among the spouses upon divorce, but if it was acquired either before marriage or through an inheritance or gift, it belongs to one spouse and is not divided.

The impact of the pension reform on a dependent wife's option outside of marriage is therefore likely to also depend on the value of any property that she would obtain upon divorce. If the value of a property is high, the pension benefits divided from her husband would be of only marginal importance to her. Thus we would expect the pension reform to have a negligible impact on a wife with other real assets but a substantial impact on a wife with no property. Another

[25] Exceptions to this include borrowing against future benefits through the Welfare and Medical Service Agency and the Japan Finance Corporation. For more detail, see `http://hp.wam.go.jp/home/tabid/36/Default.aspx` and `https://www.jfc.go.jp`.

Table 3: Subsample analysis by housing value and property rights (Wives aged 50–59)

	Housing ≤ 0 (1)	Housing > 0 (2)
Dependent variable		
Leisure	9.116**	-0.593
	(4.342)	(4.487)
Market work	-4.083	-0.314
	(3.319)	(4.200)
Domestic work	-5.033*	0.906
	(2.753)	(2.006)
p-value of joint test	0.086	0.900
p-value of H_0: same impact in (1) and (2)	0.156	
Observations	495	500
Households	165	164

Note: The table shows the estimation results of equation (7) for the oldest age group of the wife, by subsamples defined by net housing value, where the top and bottom two percent of the property values of wives are excluded to eliminate outliers. Only the estimated value of δ_1^j is reported. Standard errors clustered by each household are in parentheses. The p-value for the joint test against the null hypothesis that the response to the reform is in terms of time allocation the same across the two groups is less than 0.1.
***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$.

possible interpretation is that home ownership could also work as a commitment device that makes divorce undesirable relative to staying married. In such cases, a household with home ownership would likely have a higher degree of commitment than one without.

To test this prediction, we divided the oldest age group into two subgroups according to the household's net housing value and home ownership.[26] To this end, we first calculated the net value of all houses and plots of land using the self-evaluated value of these properties less the remaining debt from acquiring them.[27] We then assigned a property value to each spouse according to the property rights from housing and land, and then divided the oldest group into two subgroups: wives with positive property values and those without. Given the above discussion, we would expect the reform to have a substantial impact only on wives without a positive property value.

Table 3 highlights the heterogeneous effects of the pension reform on wives according to the value of the property they own. The leisure of wives whose net housing value was non-positive increased substantially by 9 hours per week while their market and domestic labor supply

[26]Although it would have been possible to conduct this subsample analysis based on household assets or savings, we chose home ownership because the titles to properties other than housing were not available in our dataset. Consequently, we could not distinguish the fraction of savings divided upon divorce from the fraction of savings accumulated before marriage or accumulated through inheritance.

[27]We omitted the top and bottom two percent of the property values to alleviate any influence of outliers.

decreased by 4 and 5 hours, and these estimates are jointly significantly different from zero. In contrast, the hours allocated to both leisure and production by wives with positive property value were not affected in any statistically significant way and, furthermore, the point estimates are almost zero. Although we cannot reject the null hypothesis that the response to the reform is the same across the two groups in terms of time allocation at the 10 percent level (with p-value 0.16), we believe that this is due to the relatively small sample size. These estimation results thus suggest that those who were mainly affected by the reform were wives with poor options outside of marriage, which is well explained by the collective model with limited commitment. To sum up, this subsample analysis further supports the rejection of the full commitment model.

6 Validity Check of the Identification Assumption

6.1 Placebo Test

While the parallel trend assumption required for DD estimation is not directly testable, it is worth considering what might occur if this assumption were violated. For example, if the results in Table 2 were driven entirely by a business cycle unique to the treatment group, we would observe its impact throughout all age groups. However, we find significant changes in the time allocation in the oldest group but not in the other groups, so our results are unlikely to be explained by an economic shock that would have influenced only the treatment group.

To further confirm the validity of the parallel trend assumption, we estimated the placebo effects of the policy by counterfactually assuming that the reform was enacted in 2005 and 2006 as well as in 2007. Specifically, we estimated the following equation for each age group:

$$y_{it}^{j} = \sum_{t=2005}^{2007} \delta_{1t}^{j} d_t \cdot Treatment_i + x_{it}'\beta^{j} + \pi_t^{j} + c_i^{j} + u_{it}^{j}. \tag{8}$$

Since $\delta_{1,2005}^{j}$ and $\delta_{1,2006}^{j}$ represent placebo policy effects, they should be zero when time trends are common across groups. If they are different from zero, the parallel trend assumption may be violated. In the following analysis, we focus on the time allocation of the wife since we found close to null effects for the husband in our DD analysis (Columns 4–6 in Table 2).[28]

Table 4 shows the estimates of δ_{1t}^{j} in equation (8) for the wives. Consistent with our baseline results, we find a significant impact on the oldest group but a smaller impact on the other age

[28]As the husband in the treatment group was typically a full-time employee, it appears to have been difficult for him to have changed his hours worked. Furthermore, there were many households in which the husband did not engage in any household production. As a result, changes in bargaining power caused by the pension reform may not have been substantial enough for these husbands to deviate from the corner solution. However, our estimation results do not immediately suggest that the pension reform had no impact on husbands, as their levels of consumption may have declined, both in terms of private and public goods.

Table 4: Placebo test for the wives

Age group: 50–59	Leisure	Market labor supply	Domestic labor supply
	(1)	(2)	(3)
$Treatment \times d_{2007}$	5.733*	-1.659	-4.074**
	(2.998)	(2.527)	(1.882)
$Treatment \times d_{2006}$	-0.992	2.957	-1.965
	(2.587)	(1.944)	(2.009)
$Treatment \times d_{2005}$	3.199	-0.832	-2.367
	(2.656)	(2.158)	(1.890)
Observations	1333	1333	1333
Households	442	442	442
Age group: 40–49	Leisure	Market labor supply	Domestic labor supply
	(4)	(5)	(6)
$Treatment \times d_{2007}$	1.138	3.103	-4.240*
	(2.641)	(1.894)	(2.193)
$Treatment \times d_{2006}$	4.705*	0.378	-5.084**
	(2.751)	(1.729)	(2.376)
$Treatment \times d_{2005}$	-0.431	2.412	-1.981
	(2.546)	(1.718)	(2.109)
Observations	1804	1804	1804
Households	575	575	575
Age group: 30–39	Leisure	Market labor supply	Domestic labor supply
	(7)	(8)	(9)
$Treatment \times d_{2007}$	-3.809	2.032	1.777
	(4.623)	(2.201)	(4.135)
$Treatment \times d_{2006}$	0.902	0.745	-1.647
	(4.342)	(2.109)	(4.022)
$Treatment \times d_{2005}$	-2.635	0.882	1.753
	(3.887)	(1.895)	(3.679)
Observations	1543	1543	1543
Households	490	490	490

Note: The table shows the estimation results of equation (8), checking the pre-time trend for the wives. Standard errors clustered by each household are in parentheses. Only the estimated values of the interaction terms between the treatment dummy variable and year dummy variables are reported.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

groups.[29] In terms of the coefficients on the placebo years, the estimates of these coefficients are not statistically different from zero in the oldest and youngest households, supporting the parallel trend assumption. Although the leisure of the wife significantly increased and the domestic labor supply decreased in 2006, which could potentially indicate a violation of our identification assumption, this must be interpreted carefully, as we are testing multiple hypotheses one by one. If we tested a total of 12 hypotheses that each placebo coefficient is zero, it is not unlikely that one of them might be rejected at a 5 or 10 percent significance level even when all of them are true.[30] In fact, we cannot reject the null hypothesis that the placebo coefficients are all zero (p= 0.36).

In addition, following Bertrand et al. (2004), we conducted a Monte Carlo simulation in which we randomly assigned treatment status to each household by using the empirical share of treated households as the assignment probability and then replicated Table 4 a thousand times. Due to this random assignment, the true "treatment effect" is zero. Table 11 shows the distribution of the number of statistically significant estimates out of 18 pre-treatment coefficients (i.e., $Treatment \times d_{2006}$ and $Treatment \times d_{2005}$). For example, the first row shows that we do not have any statistically significant estimate in 26.5 percent (49.8 percent) of simulation exercises, with significance level $\alpha = 0.1$ ($\alpha = 0.05$). Table 11 clearly indicates that it is not rare to find at least two statistically significant estimates in pre-treatment years, as is also the case with our results shown in Table 4 in which the share of two or more significant estimates is 52.8 percent (24.8 percent) with $\alpha = 0.1$ ($\alpha = 0.05$). Therefore, this simulation analysis suggests that it is likely to have some statistically significant estimates even if the true parameter is zero due to the multiple testing problem. Furthermore, we counted the number of the simulation results with two or more significant estimates in 2007 but with 2 or less significant estimates in 2005 or 2006, which is the realization consistent with Table 4, and we found only 5.5 percent and 1.9 percent of the simulation results satisfy such condition with $\alpha = 0.1$ and $\alpha = 0.05$, respectively. Therefore, our estimation result in Table 4 is unlikely to be an artifact of a sampling error.

However, we still cannot completely negate the possibility of the violation of the parallel trend assumption so, as a further validity check, we explicitly allowed for a linear time trend specific to the treatment group. The first column in Table 5 shows the estimation results. We see that the estimate for leisure is quantitatively similar to the baseline result and although the market and domestic labor supply estimates differ from the baseline, the signs are the same and the standard errors are large. Considering that we have only one treatment year out of the four sample periods, it seems natural that the estimates would become imprecise, and so the robust

[29]The estimates are jointly significant in the oldest group. Moreover, we reject the null hypothesis that the 9 coefficients on $Treatment \times d_{2007}$ are all zero, where the three restrictions are redundant since the time allocation sums up to 168.

[30]While we have 18 placebo estimates (3 outcomes × 2 placebo years × 3 age group), 6 estimates are redundant in terms of hypothesis testing due to the identity: $\hat{\delta}_{\text{Leisure}} + \hat{\delta}_{\text{Market work}} + \hat{\delta}_{\text{Domestic work}} = 0$.

result for leisure thus supports our contention that our results are not driven by a violation of the identification assumption.

Another possible explanation for the significant estimates for the pre-treatment years in Table 4 is an anticipation effect. Considering that the policy discussion on the pension division started in the late 1990s (Figure 8) and the pension reform was approved in 2004, it is possible that a couple may have started to re-bargain their resource allocation before the enactment of the policy. However, even if a couple had already responded to the reform before its approval in 2004, this would cause us to underestimate the policy effects by partialling out the anticipation effect. We discuss in detail in Section 6.3 how policy anticipation, particularly at the time of marriage, affects the interpretation of our results.

Table 5: Robustness checks using households with wives aged 50–59

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable							
Leisure	5.837*	5.198**	5.833*	4.930*	4.976**	5.890**	5.654*
	(3.531)	(2.530)	(3.052)	(2.574)	(2.509)	(2.509)	(3.162)
Market work	-5.229*	-2.608	-2.524	-2.164	-2.363	-2.935	-4.361*
	(2.863)	(2.201)	(2.697)	(2.225)	(2.136)	(2.180)	(2.400)
Domestic work	-0.608	-2.590*	-3.309**	-2.767*	-2.613*	-2.955**	-1.293
	(2.223)	(1.378)	(1.681)	(1.464)	(1.435)	(1.411)	(2.280)
p-value of joint test	0.173	0.069	0.076	0.088	0.090	0.035	0.146
Method	DD	DD	DD	DD	DD	DD	DDD
Group-specific linear trend	X						X
Flexible family structure		X					
Wives with permanent employment	X	X		X	X	X	X
Wives with husbands in large firms	X	X	X		X	X	X
Couples divorced btw. 2005 and 2012	X	X	X	X		X	X
Couples married before 1998	X	X	X	X	X		X
Observations	1333	1333	1104	1219	1328	1287	4680
Households	442	442	364	413	439	426	1507

Note: The table shows the estimation results of equation (7) and (9) with various specifications and sample restrictions for the oldest age group. Only the estimated value of δ_1^j is reported, and standard errors clustered by household are in parentheses. In the DDD analysis, the two younger groups were used as the control.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.2 Inference under a small number of clusters with group-specific errors

While we reported the standard errors clustered by each household, we did not allow the group-specific error term (i.e., $Treatment \times Year$). Donald and Lang (2007) point out that the group-specific error complicates the statistical inference, particularly when the number of groups is small, since the standard asymptotic result requires the number of groups to go to infinity. Consequently, the standard statistical inference can be misleading. Given that we have only 8 groups (2 treatment statuses and 4 years), our result can also be sensitive to this issue.

The recent literature review by Roth et al. (2023) suggests three types of approaches that apply to a small number of groups. The first approach is to directly model the error structure, and Donald and Lang (2007) propose statistical inference with few clusters (both in terms of treated and control groups), assuming the number of observations per cluster is sufficiently large to have Gaussian group-specific errors. However, according to their Monte Carlo simulation, the convergence of t-statistics to t-distribution is slow, and the distribution of their t-statistics with 250 observations per group is substantially different from the t-distribution, resulting in the statistical test being too conservative.[31] Since our sample has only 200 observations per group, it seems difficult to make correct inferences based on this approach. Additionally, Roth et al. (2023) comment that the homoskedastic Gaussian assumption of Donald and Lang (2007) is a strong assumption in practice. Second, Roth et al. (2023) recommend using the permutation test, which computes the distribution of the test statistics with many permutations of the treatment assignment. However, since we have only one treated group and seven untreated groups, the permutation of this data cannot generate sufficient variation of the test statistics, so the permutation test is not applicable to our data.

Finally, a suggested method that is applicable to our setting is the clustering wild bootstrap. According to the numerical simulation by Cameron et al. (2008), the clustering wild bootstrap performs well even with 5 clusters.[32] Since we have only 8 groups, we implemented two types of the clustering wild bootstrap, one with two weights, $\{-1, 1\}$, and another with 6 weights,$\{-\sqrt{1.5}, -1, -\sqrt{0.5}, \sqrt{0.5}, 1, \sqrt{1.5}\}$. Table 12 shows the results for each outcome, and we find that the distribution of t-statistics obtained is notably different from a standard normal distribution. Specifically, the critical value is higher than the standard normal for leisure and domestic labor supply whereas it is lower for market labor supply. However, the policy effects on leisure and domestic labor supply are still statistically significant at the 10 percent level. Therefore, our statistical inference is robust to the small cluster problem to some extent.

[31] When we implemented their method, our results were no longer statistically significant. The result is available upon request.

[32] Canay et al. (2021) formally analyze the validity of the test statistics based on the clustering wild bootstrap with few clusters and show that the wild bootstrap could be unreliable with substantial heterogeneity in treatment effects across clusters.

6.3 Sensitivity to Specification and Sample Restrictions

One potential cause of differing time trends between the treatment and control groups could be a change in family structure. In particular, the typical treatment household tends to have a larger family and younger children than the control household (Table 1). Although in the baseline specification we controlled for family size, the number of children, whether households had a child under 6, and household fixed effects, it is possible that these were insufficient to completely remove the impact of changes in family structure. Thus, we conducted an additional analysis controlling for family structure by using fourth-order polynomials of family size and the number of children, dummy variables indicating whether or not the household had a child aged 0–6, 7–12, and 13–18, and interaction terms between these polynomials and the dummy variables. Despite this flexible specification, we obtain estimation results that are qualitatively and quantitatively identical to the baseline results (Column 2 in Table 5). Therefore, any potential changes in family structure seem not to be a concern for our identification strategy.

A second potential violation of the parallel trend assumption could occur because the control group in our baseline estimation included wives who were permanent employees in 2003 whereas the treatment group did not. Since it is conceivable that the time allocation trend of wives with permanent employment differs from those with part-time or no employment, as an additional confirmation of our baseline results, we excluded wives with permanent employment from our sample and re-estimated the baseline DD equation (7). The third column of Table 5 shows that the leisure time of the wife in the oldest group increased while her market hours and domestic work hours decreased in response to the pension reform. Furthermore, the magnitude of the estimates is comparable with those from the baseline result. As before, none of the estimates for the younger two groups are statistically significant. These findings suggest that differences in time use are not the main driver of our baseline results.

As discussed in Section 3, the key to identifying the degree of commitment that spouses have to an initial resource allocation plan is the intra-household variation in bargaining position caused by the pension reform. Though approved in 2004 and enacted into law in 2007, discussion of the reform began several years earlier, with published newspaper articles about the reform appearing as early as 1998. It is thus possible that some people aware of the potential reform before 2000 (Figure 8) may have married in the early 2000s already anticipating that the division of pension records upon divorce might be allowed in the future. If this were the case, then the pension reform of 2007 would be unsuitable as an identification strategy, as it would be difficult to separate the changes in bargaining position after marriage from the bargaining position at marriage. Consequently, as couples that may have anticipated the pension division reform would need to be excluded from the analysis, we implemented a subsample analysis that limited the estimation sample to wives in the oldest group who married in 1997 or earlier. However, as the results (Column 6 in Table 5) are identical to the baseline, the issue of couples anticipating the

pension reform at the time of their marriage appears to be inconsequential to our analysis.

On the other hand, it is also possible that a couple may have married without anticipating the pension reform of 2007 but, becoming aware of it in 2004 when the policy was announced, changed the household resource allocation before 2007. However, as long as the effect of policy anticipation is smaller than the effect of policy enactment, this results in an underestimation of the entire policy effect because what we estimated is the policy effect net of the anticipation effect. Therefore, even if there was an anticipation effect, our results still reject the full-commitment model.[33]

Finally, to ensure that our estimates on the heterogeneous policy impact across the age of the wife are not an artifact of idiosyncratic errors, we re-estimated equation (7) by starting with the 10-year age bin of "50–59" and sequentially reducing the age by one year, first to "49–58," then "48–57," and so on, and we did the same for the 5-year age bin.[34] If the heterogeneity that we found is systematic and not idiosyncratic, we would expect the size of the estimates to become gradually smaller as we shift to a younger group, and this is what we indeed find (Tables 13 and 14). The policy impact on leisure is largest in the oldest age group and, roughly speaking, it becomes monotonically smaller as the estimation sample becomes younger. We also find a similar trend for the market and domestic labor supply, though the pattern for the market labor supply is somewhat noisy. Therefore, this exercise indicates that the heterogeneity in the policy effect is systematic and consistent with the prediction that the older the household, the larger the policy impact.

6.4 Other Policy Changes

6.4.1 Pension Reforms of 2004

The pension division rule upon divorce that is the focus of our interest was only part of a broader series of pension reforms legislated in 2004, whose main purpose was to ensure that the pension system would remain sustainable amid the rapidly aging population in Japan. From the viewpoint of households, the pension reforms in 2004 consisted mainly of (1) an increase in pension premiums and (2) a decrease in the income replacement rate. First, the monthly pension premiums of the national pension insurance increased by about 2.5 USD (280 JPY) in each year from 2005 to 2017, and the contribution to the employee pension insurance increased by 0.354 percentage points in each year from 2004 to 2017, so the rate of the employee pension premium increased gradually from 13.58 percent to 18.30 percent. Second, the income replacement rate of pension benefits was planned to decrease from 60 percent to 50 percent depending on economic

[33]As further support, in Appendix A.3, we conduct a numerical exercise to show that the anticipation effect is smaller than the enactment effect. However, we acknowledge that the model used for this numerical analysis does not coincide with our empirical setting, so this result might not apply to our real-world data.

[34]We thank the anonymous referee for suggesting this robustness check.

growth and population growth. However, due to sluggish economic growth, the adjustment of the income replacement was not implemented until 2014. As a result, the income replacement rate was still around 60 percent even in 2019.

Although these other aspects of the 2004 pension reforms could potentially affect our treatment and control groups differently, they are unlikely to be the main driver of our empirical results for three reasons. First, the increase in pension premiums started in 2004 and 2005, but we did not find any policy impact in 2005 or 2006 but only in 2007. Furthermore, if the change in pension premium mattered, we would observe a change in household behavior in young households as well as elderly households since the pension premium payment does not depend on age. Second, the increase in the employee pension premium has a negative income effect on the wife in the treatment group, and thus the policy impact on her leisure would, if any, be negative as long as leisure is a normal good. However, we find a positive impact on the wife's leisure. Finally, one might argue that although the income replacement rate did not actually change as had been planned, the policy announcement itself changed household expectations of their future pension benefits downwardly, resulting in a change in household resource allocation. This explanation is again inconsistent with our empirical results as it would lead to a negative income effect, which is not what was observed.

6.4.2 The Mandatory Retirement Age

Another consideration in interpreting our estimation results is the 2006 enactment of the Elderly Employment Stabilization Law (EESL) affecting the mandatory retirement system in Japan. From 2006, the Japanese government has required firms to comply with a scheme to raise the mandatory retirement age to 65 by gradually filling the gap between the existing mandatory retirement age of 60 at that time and the new pension eligibility age of 65. Specifically, the EESL allows those born in 1946 to remain employed until age 63, those born in 1947 or 1948 until age 64, and those born in 1949 or later until age 65. The new law did not force firms to raise the mandatory retirement age but instead provided them with three options to continue to employ workers who would have otherwise needed to retire: (1) raise the mandatory retirement age; (2) extend or renew employment contracts; or (3) abolish mandatory retirement. According to the Ministry of Health, Labour and Welfare, more than 80 percent of firms chose option 2 rather than either raising or abolishing the mandatory retirement age.[35]

It is expected that the EESL would have increased the labor force participation rate of elderly people hired as full-time employees, and while Kondo and Shigeoka (2017) show that it has indeed increased the ratio of salaried workers over 60 years of age, the impact is rather small at only 3 percentage points and is seen only in large firms ($\geq$ 500 employees), perhaps because small and medium firms with less standardized employment practices had already abolished or

[35]Data source: `http://www.mhlw.go.jp/toukei/list/11-23c.html`.

raised the retirement age. In addition, they find that these contract renewals tend to be associated with a substantial decline in wages, which discourages employees from continuing to participate in the labor force. Despite the apparent limited impact of the EESL, it could nonetheless bias our estimates due to the similarity of the target populations. Those born in 1946 became 60 years old in 2006, and the main target of the EESL was full-time employees, so the pension reform enacted in 2007 presumably affected the same employees. Given that the effect of the EESL was to increase the future earnings of the husband, this wealth effect could potentially decrease the current market labor supply of the wife and increase her leisure. Since those affected by the EESL are employees at large firms, to test this, we excluded them from the sample and re-estimated the baseline empirical model (7). However, as this subsample analysis (Column 4 in Table 5) replicates the baseline results, the EESL also does not seem to be the main driver of our findings.

One further possibility to consider is that the EESL may potentially have had an indirect effect on younger employees. Since the EESL requires firms to continue to employ workers who would otherwise retire at age 60, it is conceivable that firms might accommodate this new requirement by decreasing the number of young employees, either by not hiring new graduates or by letting go of part-time employees. Conversely, if elderly workers and young workers are viewed as complementary, then firms could increase the number of young employees as a result of the EESL. Ohta (2012) and Kondo (2016) report negative correlations between the proportions of employees aged over 60 and of female part-time workers, raising the possibility that the EESL might have created a crowding-out effect. However, since our control group includes part-time employees, such an indirect impact of the EESL is controlled by our DD estimation to a certain degree. Furthermore, as long as the EESL affects younger and older female part-time employees in a similar manner, DDD estimation (described next) partials out any potential indirect impact of the EESL so that potential biases caused by the EESL are likely to be small.

6.5 Triple Differences Estimation

DD estimation requires the assumption that the trends in the wife's time allocation are the same for the treatment and control households in the absence of the policy reform. This seems restrictive since the treatment status depends mainly on whether the husband is employed or self-employed. To relax this assumption, we performed a DDD estimation, introducing two younger age groups as additional controls and a dummy variable, Old_i, which takes one if the wife was over 50 in 2007. We then estimated

$$y_{it}^j = \delta_1^j DDD_{it} + \delta_2^j After_t \cdot Treatment_i + \delta_3^j After_t \cdot Old_i + x_{it}'\beta^j + \pi_t^j + c_i^j + u_{it}^j, \quad (9)$$

where $DDD_{it} = After_t \cdot Treatment_i \cdot Old_i$, and δ_1^j represents the impact of the pension reform. To accommodate any arbitrariness in the choice of the control group, we estimated equation (9) using the entire sample and two subsamples, with one subsample consisting of the oldest group and the youngest group and the other consisting of the oldest group and the second youngest group.

This DDD estimation requires two identification assumptions. First, the degree of violation of the parallel trend assumption is the same for older and ($Old_i = 1$) and younger households ($Old_i = 0$), which makes the DDD estimation robust to the violation of the parallel trend assumption to some extent. However, we must be careful in interpreting the DDD estimates because we also need to assume that there is no policy effect on younger households. In particular, as some estimates for the oldest and middle-aged groups are similar (Table 2), DDD estimation could underestimate the policy impact, thus making the null hypothesis of full commitment difficult to reject. This would cause the resulting hypothesis test to be rather conservative.

In estimating equation (9), we find that the leisure of the wife statistically significantly increased by more than 5 hours per week regardless of the control group (Table 6). While the estimate using wives aged 30–39 as the control group is relatively larger (Column 7), the size of the other two estimates is comparable to the results of the DD estimation (Columns 1 and 4). Hence, we are confident that the results from both DDD and DD estimation provide sufficient evidence to reject the full commitment model. Meanwhile, the market labor supply of the typical wife decreased by 4–4.5 hours, a result that is also robust to change in control groups. Moreover, the sign of the DDD estimate is in line with those of the baseline DD estimates, though its magnitude is somewhat greater than the baseline and similar to the DD estimate allowing for a linear time trend specific to the treatment group. All in all, considering the robustness of our findings, it appears clear that wives' market hours decreased with the increased value to them of divorce.

In contrast to the results for market work, we observe relatively large variations in the DDD estimates of the impact on domestic work and, moreover, these estimates are not statistically significant. However, the sign of the coefficient is still negative in all three cases, as it is with the DD estimate. Furthermore, the estimate becomes smaller when we use the second oldest group as the control group, which is consistent with our earlier discussion that DDD estimation possibly underestimates the policy impact by filtering out effects common among older and younger households.

While our DDD analysis maintained the identification assumption that there was no time trend specific to the treatment group in the oldest group, the DDD estimates would fail to recover the ATT if this assumption did not hold. Thus, to check the sensitivity of our estimates, we implemented a further DDD estimation while allowing for a linear time trend specific to the oldest group and also to the treatment group. Under the identification assumption, the result-

Table 6: The effect of the pension reform on wives: Triple differences estimation

Control group: Wife aged 30–49			
Dep. Var	Leisure	Market labor supply	Domestic labor supply
	(1)	(2)	(3)
DDD	5.658*	-4.278*	-1.380
	(3.147)	(2.399)	(2.265)
Observations	4,680	4,680	4,680
Households	1,507	1,507	1,507
Control group: Wife aged 40–49			
Dep. Var	Leisure	Market labor supply	Domestic labor supply
	(4)	(5)	(6)
DDD	5.233*	-4.565*	-0.669
	(3.113)	(2.597)	(2.117)
Observations	3,137	3,137	3,137
Households	1,017	1,017	1,017
Control group: Wife aged 30–39			
Dep. Var	Leisure	Market labor supply	Domestic labor supply
	(7)	(8)	(9)
DDD	7.636*	-3.911	-3.725
	(4.380)	(2.701)	(3.665)
Observations	2,876	2,876	2,876
Households	932	932	932

Note: The table shows the estimated values of δ_1^j in equation (9) for wives, with standard errors clustered by each household in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

ing estimates would likely be similar to the DDD estimates obtained from the empirical model without those group-specific time trends and, indeed, we find an almost identical result with this specification (Column 7 in Table 5). To sum up the above discussion, a comprehensive series of robustness checks further supports our rejection of the full commitment model.

7 Conclusion

Using a Japanese pension reform as a natural experiment, this study fills a gap in the literature by testing the full commitment model of household decision-making without imposing any assumptions about specific functional forms for preferences or home production technology. The results lead us to reject the full commitment model, as elderly wives exploited their improved

outside options to enjoy more leisure by reducing market and domestic labor. Consistent with the model's prediction, this impact is most striking among low-net-worth couples. While it appears that couples have difficulty committing to an initial resource allocation plan, incomplete commitment does not mean no commitment, and we find suggestive evidence that a couple does not respond to small shocks unless the participation condition binds. A lack of commitment thus seems to distort resource allocation and typically makes the first-best allocation difficult, if not impossible, to achieve. Therefore, future work should address the size of the distortion in long-term decision making contexts such as human capital accumulation, investment in children and risk sharing. Finally, as we find a non-negligible impact of re-bargaining, we believe that model-based studies should incorporate this feature of family decision making.

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A Numerical Example

A.1 Household Decision Problem

This section provides some numerical examples to further illustrate the effect of the pension reform on the wife's time allocation in the household decision model. The model considered in this section is a simple two-period model without home production, so it is less complex than the model presented in Section 3 of our main analysis. Nonetheless, we believe it is sufficient for highlighting the effect of the pension reform on the leisure time of the wife. In this model, each household member works in the market in period 1 and retires in period 2. The household problem is

$$
\begin{aligned}
\max \mu \left[u_1^M(c_{11}, l_{11}; \theta_1) + u_1^M(c_{12}, l_{12}; \theta_1)\right] + (1-\mu)\left[u_2^M(c_{21}, l_{21}; \theta_2) + u_2^M(c_{22}, l_{22}; \theta_2)\right] \\
\text{s.t.} \quad a_2 = a_1 + \sum_{j=1}^{2}(1-\tau)(1-l_{j1})w_j - c_{11} - c_{12} \\
b_{j2} = b_{j1} + \tau(1-l_{j1})w_j \\
a_2 + b_{12} + b_{22} = c_{12} + c_{22} \\
0 \le l_{j1} \le 1, \quad l_{j2} = 1 \\
uj^M(c_{j1}, l_{j1}; \theta_j) + u_j^M(c_{j2}, l_{j2}; \theta_j) \ge DV_{j1}(a_1, b_{11}, b_{21}, w_j; \phi), \\
u_j^M(c_{j2}, l_{j2}; \theta_j) \ge DV_{j2}(a_2, b_{12}, b_{22}, w_j; \phi),
\end{aligned}
$$

where $u_j^M(c, l; \theta) = u_j(c, l) + \theta$, $u_j(c, l) = \ln c + \gamma_j \ln l$, θ_j is the match quality for spouse j, and $\phi(a_t, b_{1t}, b_{2t}, j)$ is the asset division rule for spouse j, given the household asset a and each spouse's accumulated pension benefits b_{jt}. In particular, the asset division rule before the pension reform is $\phi_0(a_t, b_{1t}, b_{2t}) = \frac{a_t}{2} + b_{jt}$, and the asset division rule after the pension reform is $\phi_1(a_t, b_{1t}, b_{2t}) = \frac{a_t + b_{1t} + b_{2t}}{2}$. Since the pension reform increases the value of divorce only for the wife (decreasing its value for the husband), we consider here only the participation conditions of the wife.

Denoting the Lagrange multipliers on the intertemporal budget constraint, the initial-period participation condition and the 2nd-period participation condition as λ_0, λ_1 and λ_2, respectively,

the above maximization problem can be written as

$$\begin{aligned}\max \mu \left[u_1^M(c_{11}, l_{11}) + u_1^M(c_{12}, l_{12})\right] + (1-\mu)\left[u_2^M(c_{21}, l_{21}) + u_2^M(c_{22}, l_{22})\right] \\ + \lambda_0 \left[a_1 + b_{11} + b_{21} + w_1 + w_2 - \sum_{j=1}^{2}\sum_{t=1}^{2} c_{jt} - \sum_{j=1}^{2} w_j l_j\right] \\ + \lambda_1 \left[u_2^M(c_{21}, l_{21}) + u_2^M(c_{22}, l_{22}) - DV_{21}(a_1, b_{11}, b_{21}, w_2; \phi)\right] \\ + \lambda_2 \left[u_2^M(c_{22}, l_{22}) - DV_{22}(a_2, b_{12}, b_{22}, w_2; \phi)\right],\end{aligned}$$

with the pension accumulation rule $b_{j2} = b_{j1} + \tau(1 - l_{j1})w_j$ and the complementary slackness conditions

$$\begin{aligned}&\lambda_0 \left[a_1 + b_{11} + b_{21} + w_1 + w_2 - \sum_{j=1}^{2}\sum_{t=1}^{2} c_{jt} - \sum_{j=1}^{2} w_j l_j\right] = 0, \\ &\lambda_1 \left[u_2^M(c_{21}, l_{21}) + u_2^M(c_{22}, l_{22}) - DV_{21}(a_1, b_{11}, b_{21}, w_2; \phi)\right] = 0, \\ &\lambda_2 \left[u_2^M(c_{22}, l_{22}) - DV_{22}(a_2, b_{12}, b_{22}, w_2; \phi)\right] = 0.\end{aligned}$$

Given the Lagrange multiplies λ_1 and λ_2, the first order conditions and intertemporal budget constraint implies that the solution to this problem, $x(\lambda_1, \lambda_2)$, is given by the following system of equations:

$$\begin{aligned}0 &= \frac{\mu}{c_{11}(\lambda_1, \lambda_2)} - \frac{\mu}{c_{12}(\lambda_1, \lambda_2)} + \frac{\lambda_2}{A_2(\lambda_1, \lambda_2)}, \\ 0 &= \frac{\mu\gamma_1}{l_1(\lambda_1, \lambda_2)} - \frac{\mu w_1}{c_{12}(\lambda_1, \lambda_2)} + \frac{\lambda_2 w_1}{A_2(\lambda_1, \lambda_2)}, \\ 0 &= \frac{(1-\mu) + \lambda_1}{c_{21}(\lambda_1, \lambda_2)} - \frac{\mu}{c_{12}(\lambda_1, \lambda_2)} + \frac{\lambda_2}{A_2(\lambda_1, \lambda_2)}, \\ 0 &= \frac{(1-\mu) + \lambda_1\gamma_2}{l_2(\lambda_1, \lambda_2)} - \frac{\mu w_2}{c_{12}(\lambda_1, \lambda_2)} + \frac{\lambda_2 w_2}{A_2(\lambda_1, \lambda_2)}, \\ 0 &= \frac{[(1-\mu) + \lambda_1 + \lambda_2]\,\gamma_2}{c_{22}(\lambda_1, \lambda_2)} - \frac{\mu_1}{c_{12}(\lambda_1, \lambda_2)}, \\ 0 &= A_2(\lambda_1, \lambda_2) - c_{12}(\lambda_1, \lambda_2) - c_{22}(\lambda_1, \lambda_2),\end{aligned}$$

where $A_2(\lambda_1, \lambda_2) = a_0 + b_{11} + b_{21} + w_1 + w_2 - c_{11}(\lambda_1, \lambda_2) - c_{21}(\lambda_1, \lambda_2) - w_1 l_1(\lambda_1, \lambda_2) - w_2 l_2(\lambda_1, \lambda_2)$. Then, denoting the value of the 2nd-period asset and pension benefits derived from this solution by $a_2(\lambda_1, \lambda_2)$, $b_{12}(\lambda_1, \lambda_2)$ and $b_{22}(\lambda_1, \lambda_2)$, respectively, the Lagrange multipliers

Table 7: Parameter values in simulation analysis

Comparative statics	Husband's pension benefits (1)	Asset (2)
(γ_1, γ_2)	$(0.2, 0.8)$	$(0.2, 0.8)$
(θ_1, θ_2)	$(\ln 2, \ln 2)$	$(\ln 2, \ln 2)$
(w_1, w_2)	$(1, 0.5)$	$(1, 0.5)$
τ	0.18	0.18
a_1	2	$\in [0.5, 4.0]$
b_{11}	$\in [0.5, 2.5]$	1
b_{21}	0	0

λ_1 and λ_2 solves

$$0 = \lambda_1 \left[u_2^M \left(c_{21}(\lambda_1, \lambda_2), l_{21}(\lambda_1, \lambda_2) \right) + u_2^M \left(c_{22}(\lambda_1, \lambda_2), l_{22}(\lambda_1, \lambda_2) \right) - DV_{21}(a_1, b_{11}, b_{21}, w_2; \phi) \right],$$
$$0 = \lambda_2 \left[u_2^M \left(c_{22}(\lambda_1, \lambda_2), l_{22}(\lambda_1, \lambda_2) \right) - DV_{22} \left(a_2(\lambda_1, \lambda_2), b_{12}(\lambda_1, \lambda_2), b_{22}(\lambda_1, \lambda_2), w_2; \phi \right) \right],$$

with $\lambda_1 \geq 0$ and $\lambda_2 \geq 0$.

A.2 Comparative Statics

In order to analyze the effect of the pension reform on household behavior, we numerically solve this model and investigate the heterogeneity of the policy impact across the husband's pension benefits and household assets. Throughout this simulation, we normalize the Pareto weight μ so that the wife is indifferent between marriage and divorce under the pre-reform regime, ϕ_0. Other parameters used in the simulation are given in Table 7. We admit that the preference parameter was chosen somewhat arbitrarily to achieve a high labor supply for the husband and a low labor supply for the wife, and a relatively high match quality is required to induce both members into marriage, for this model does not have any other channels to make family formation attractive such as specialization or economies of scale in home production. The rate of the pension premium, τ, is set to the actual rate.[36] The range of comparative statics of the husband's pension benefits and household asset was chosen so that a couple stays married before and after the policy reform, given other parameters.

Figure 3 summarizes the result of the comparative statics with respect to the husband's pension benefits, b_{11}. Intuitively, the Pareto weight on the wife is decreasing in the husband's pension benefits, as it suggests that the value of divorce is lower for her than for her husband before the pension reform. However, since the pension reform gives her access to her husband's pen-

[36] In reality, half of the pension premium is paid by the employer, but we ignore this feature for simplicity. Changing this parameter does not qualitatively affect the simulation result.

sion benefits upon divorce, she would choose to divorce under the current consumption plan. Consequently, she can threaten her husband to modify the household resource allocation in her favor. In fact, the value of marriage to her is less than the value of divorce with a pre-reform allocation plan (Figure 3c), while the husband has a large gain from the marriage (Figure 3b). The adjustment of the resource allocation is made through the Pareto weight, the positive value of λ_1 and λ_2 (Figure 3a). Furthermore, the period 1 Lagrange multiplier is increasing in the husband's pension benefits, which is consistent with the discussion in Section 3; that is, the larger her husband's pension benefits, the more the wife can benefit from the reform. Although the period 2 Lagrange multiplier is insensitive to the husband's pension benefits, we note that the change in the 2nd-period Pareto weight is $\lambda_1 + \lambda_2$ and it is increasing in the husband's pension benefits.

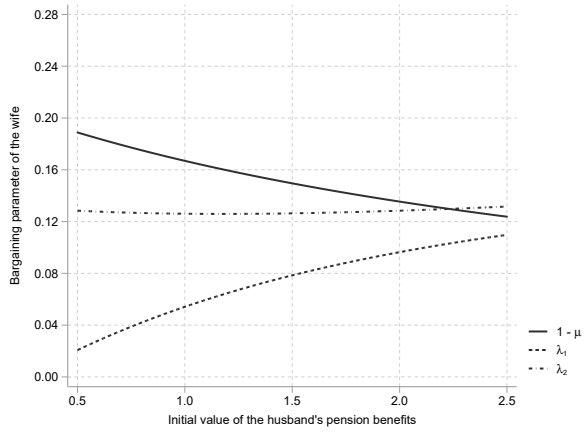
As a result of the change in the Pareto weight, the consumption and leisure of the wife increase while those of the husband decrease after the reform (Figures 3d–3f). Due to the log utility assumption, the 1st-period consumption is proportional to 1st-period leisure, so the policy impacts on them are the same in terms of percent change. Since the change in the Pareto weight is increasing in the husband's pension benefits, so are the impacts of the pension reform. Consistent with the discussion in Section 3, the policy impact on a household with large pension benefits is larger than those on a household with small pension benefits. Although this is only a 2-period model, an elderly household can be regarded as the former type of household while a young household as the latter type of household. In this sense, our empirical results on the heterogeneous policy impact across the wife's age (Table 2) are consistent with the prediction from the simulation analysis.

Next, Figure 4 shows the comparative statics with respect to the household initial asset, a_1. In this case, a large household asset indicates a better outside option for the wife even before the pension reform, as the asset is divided equally upon divorce and the pension reform does not affect this asset division rule. Consequently, the Pareto weight of the wife, $1-\mu$, is increasing in the initial asset. On the other hand, the Pareto weight after the reform is decreasing in the initial asset because the pension benefits divided from the husband are marginal to her if she has a large asset and thus the change in the value of divorce to her after the reform is marginal as well. As a result, her threat to divorce is so weak that only a small adjustment in resource allocation is sufficient for her to stay married. Consequently, the simulation analysis shows that the policy impact is larger for a household with small assets than for one with large assets. Therefore, this validates our empirical results on the heterogeneity across housing values (Table 3).

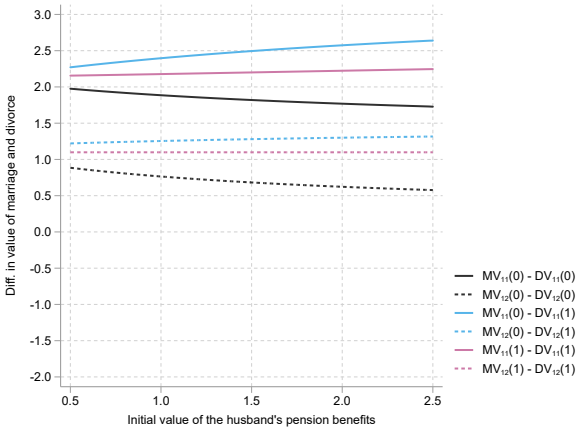
A.3 Anticipation of the Reform

We next demonstrate how anticipation of the policy affects the policy impact. To that end, we consider the case in which the policy is announced in period 1 but the new policy applies only to

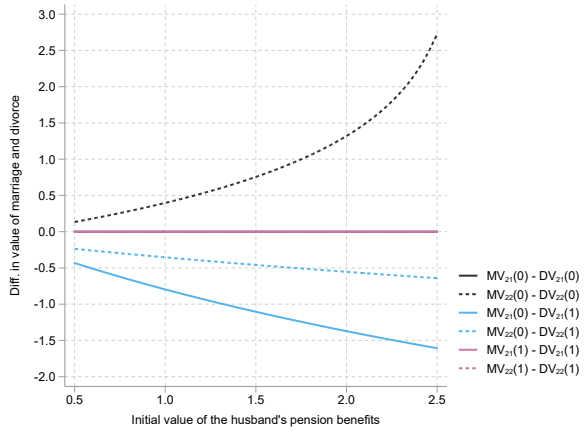
couples that divorce in period 2. In this case, while the reform affects the participation condition in period 2 as in the above exercise, the reform does not affect the participation condition in period 1 since the old pension division rule is applied only if a couple divorces in period 1. We numerically solve the model in a way similar to that above, and the results are shown in Figures 5 and 6. An important difference from the previous exercise is that the period 1 participation condition is not binding (i.e., $MV_{21}(1) - DV_{21}(1) > 0$) and thus the Lagrange multiplier on the period 1 participation condition is zero. Instead, the period 2 participation condition is more binding, so the Lagrange multiplier on the period 2 participation condition becomes larger. As a result, we observe a large increase in the wife's consumption in period 2. Interestingly, the period 1 consumption and leisure of both spouses slightly increase because the announcement of the future reform gives the husband an incentive to reduce the accumulation of assets and pension benefits because they increase the value of divorce for the wife in the next period. Consequently, more household resources are consumed at the period 1 compared to the baseline case without the reform. Nonetheless, this anticipation effect is relatively small, and the main policy impact is observed after the policy implementation. As a caveat, we note that our theoretical model has only three periods, and thus, the result of this numerical exercise might not apply to our real-world data.



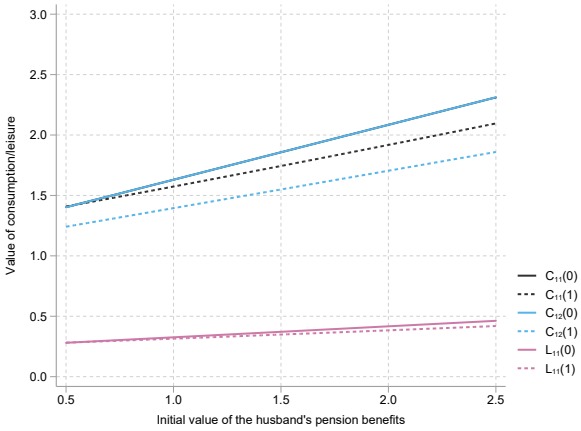
(a) Bargaining parameters



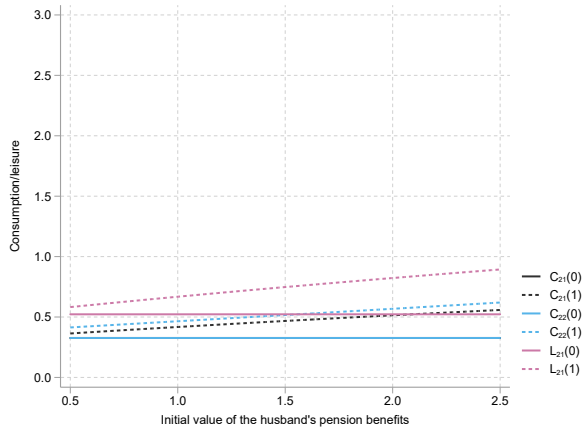
(b) Relative value of marriage: Husband



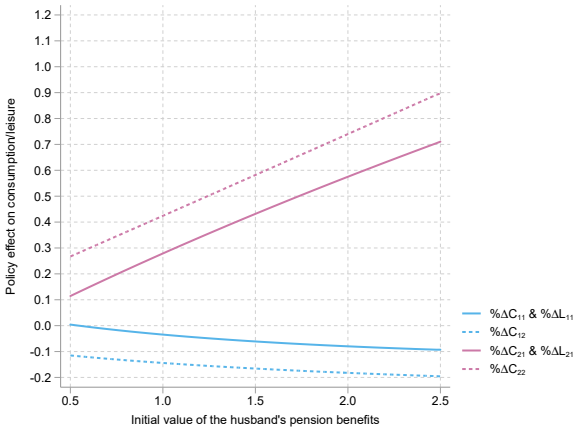
(c) Relative value of marriage: Wife



(d) Consumption and leisure: Husband

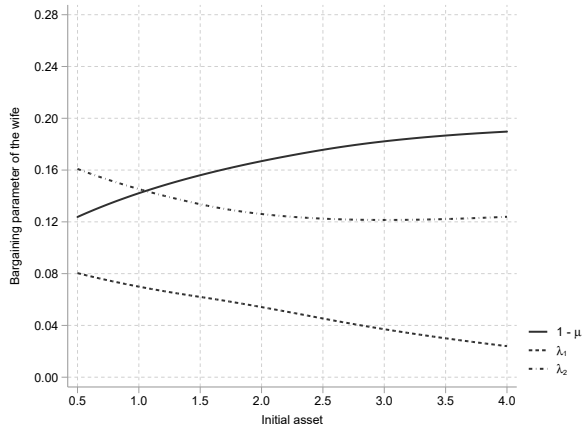


(e) Consumption and leisure: Wife

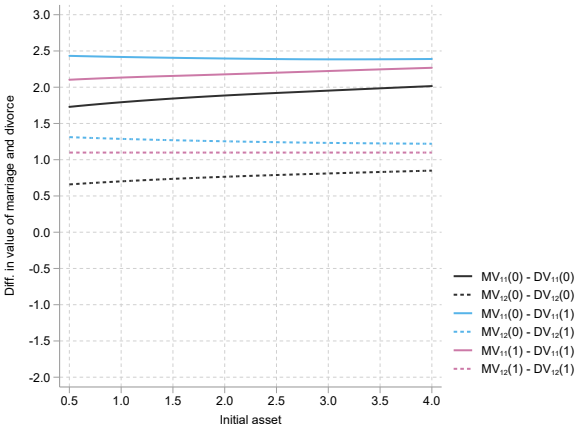


(f) Policy effects on consumption and leisure

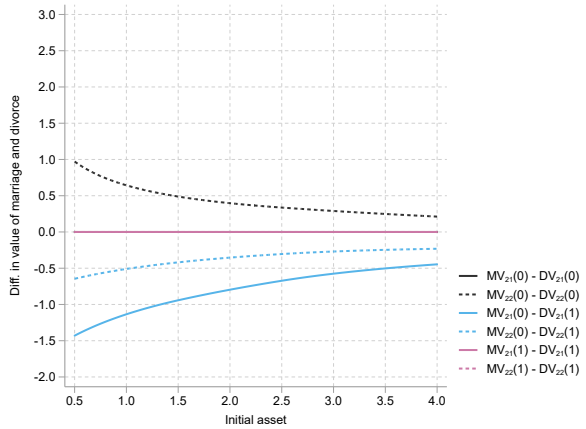
Figure 3: Comparative statics with respect to the husband's pension benefits



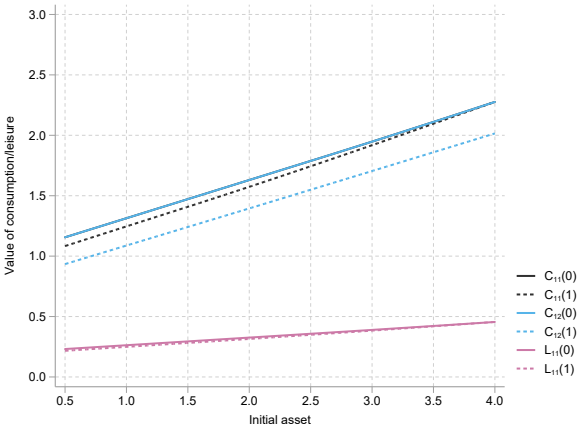
(a) Bargaining parameters



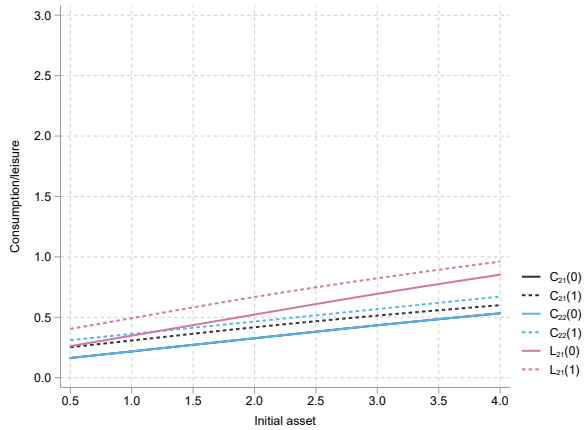
(b) Relative value of marriage: Husband



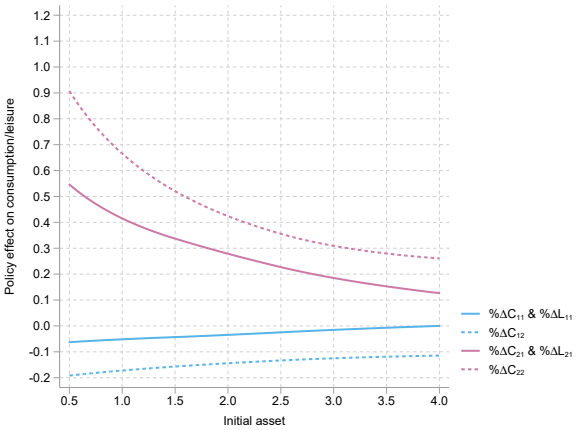
(c) Relative value of marriage: Wife



(d) Consumption and leisure: Husband

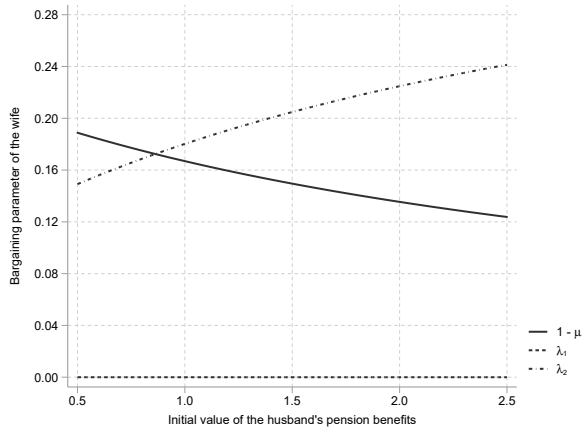


(e) Consumption and leisure: Wife

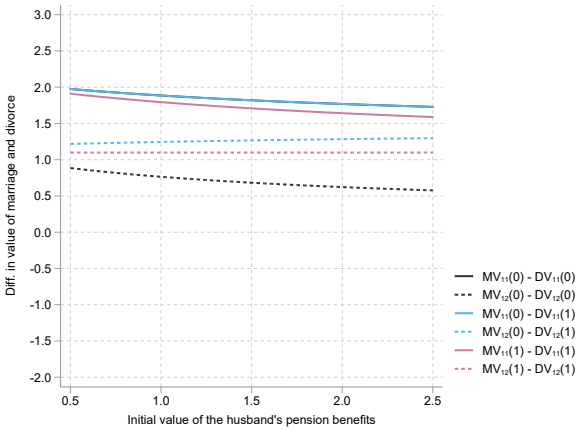


(f) Policy effects on consumption and leisure

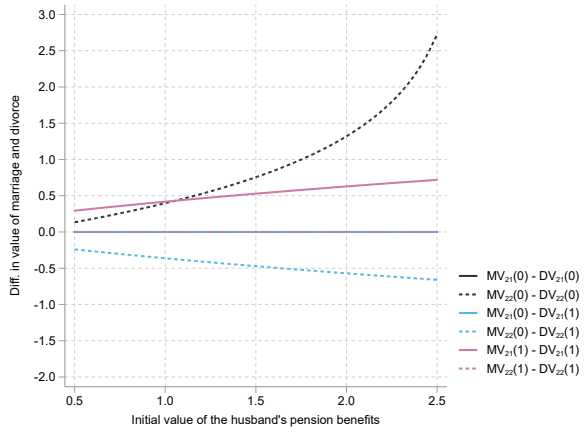
Figure 4: Comparative statics with respect to the initial household asset



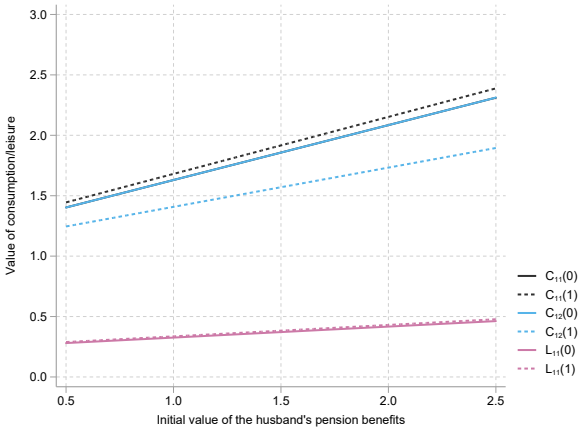
(a) Bargaining parameters



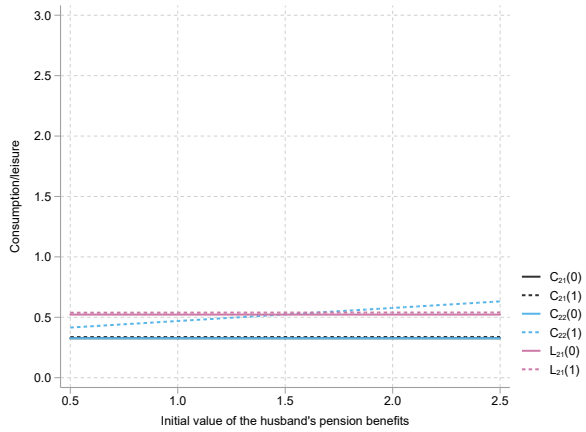
(b) Relative value of marriage: Husband



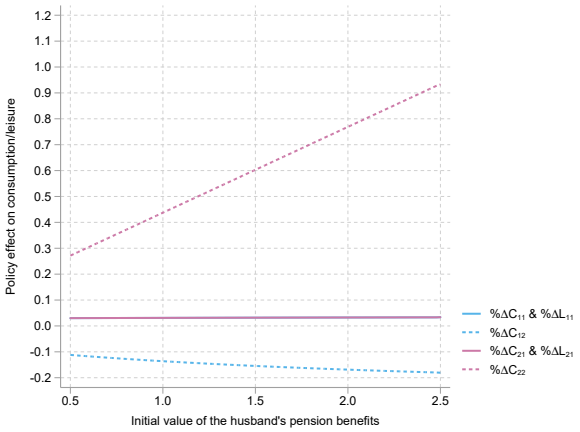
(c) Relative value of marriage: Wife



(d) Consumption and leisure: Husband

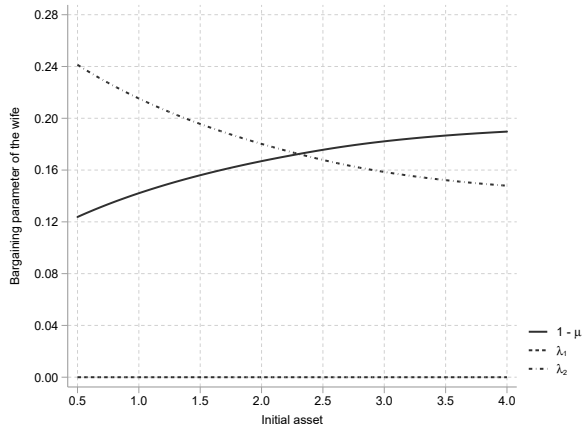


(e) Consumption and leisure: Wife

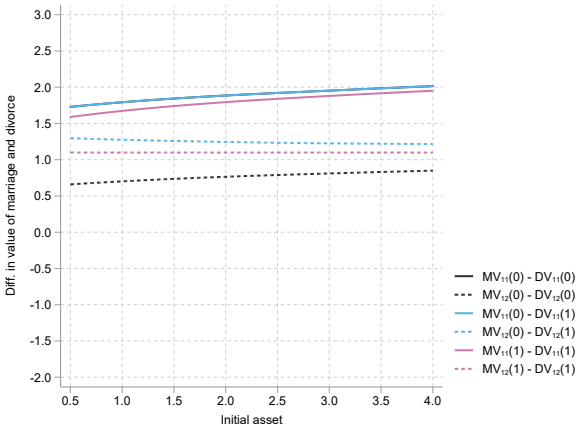


(f) Policy effects on consumption and leisure

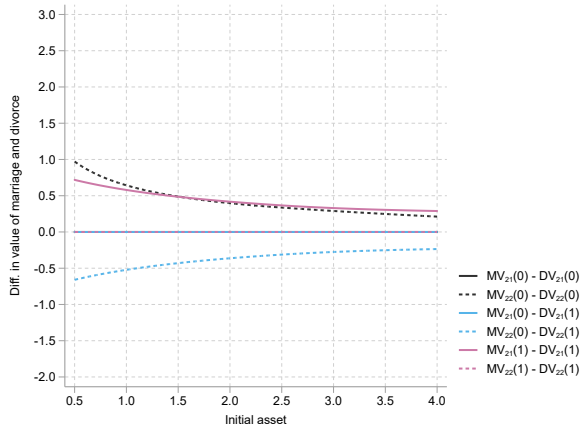
Figure 5: Policy anticipation: Comparative statics with respect to husband's pension benefits



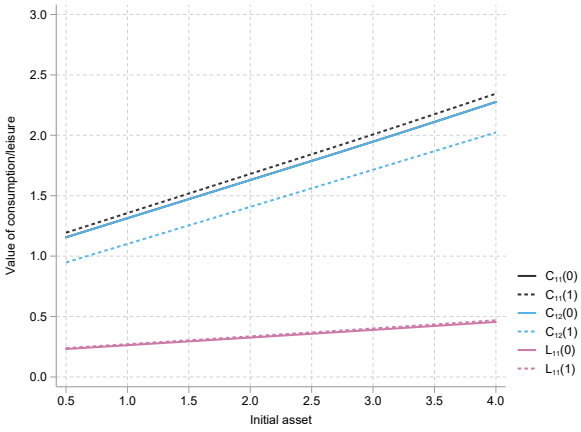
(a) Bargaining parameters



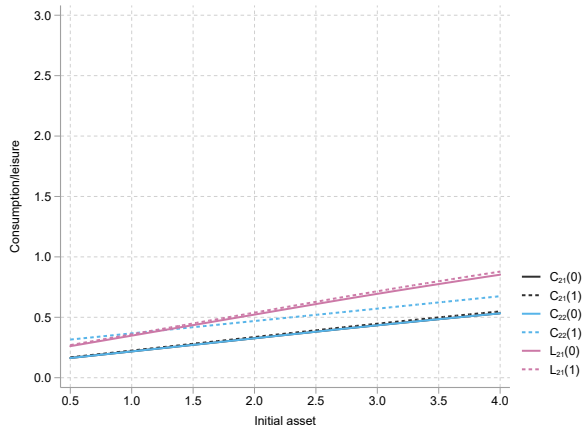
(b) Relative value of marriage: Husband



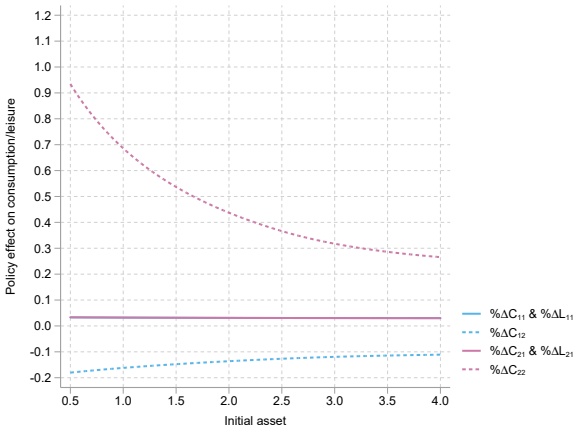
(c) Relative value of marriage: Wife



(d) Consumption and leisure: Husband



(e) Consumption and leisure: Wife



(f) Policy effects on consumption and leisure

Figure 6: Policy anticipation: Comparative statics with respect to the initial household asset

B Household Decision Model with Divorce

In Section 3, we assumed that a couple does not eventually divorce, and the identification result indeed relies on that assumption. In this section, we thus examine the case when divorce does occur. With a positive probability of divorce, the household objective function becomes

$$\mu E\left[\sum_{t=1}^{3}\beta^{t-1}\left\{(1-D_t)u_1(c_{1t},l_{1t};\theta_{1t})+D_tV_{1t}^{D}\left(a_{1t}^{D}+b_{1t}^{D}\right)\right\}\right] + (1-\mu)E\left[\sum_{t=1}^{3}\beta^{t-1}\left\{(1-D_t)u_2(c_{2t},l_{2t};\theta_{2t})+D_tV_{2t}^{D}\left(a_{2t}^{D}+b_{2t}^{D}\right)\right\}\right],$$

where D_t is a decision to divorce, which is an absorbing state, and $D_1 = 0$ as we focus on a married couple. In this case, each spouse cares about their welfare after potential divorce even under full commitment and, consequently, this concern about divorce makes household behavior contingent on the share of assets upon divorce. Note that this caveat is not unique to this study but is typical in the literature.[37]

One possible interpretation of this is that the pension division system might have affected the wife's actions prior to divorce and thus our estimates reflect that instead of the effects of re-bargaining. For example, it seems plausible that a housewife planning to divorce may begin working in order to prepare for her life after divorce, and Mazzocco et al. (2006) suggest that wives in the United States tend to start working about two years before divorce so as to accumulate human capital. Since the Japanese pension reform provided an additional income source after divorce, this might eliminate the need for a wife to work before (and possibly after) the divorce. In this scenario, her market hours would decline after the reform and her leisure would increase, which is consistent with our estimation results but unrelated to changes in her bargaining position.

Reflecting on the above situation, however, it seems difficult to explain our findings entirely by such "divorce-concern" behavior due to the low probability of divorce in Japan, particularly among elderly households. Figure 7a illustrates the annual divorce rate of elderly households before and after the reform and shows, for example, that 3 in 1000 couples with wives aged 55 divorced the next year. This probability declines rapidly as the age of the couples rises, becoming less than 0.1 percent at age 70. Although we are referring here to cross-sectional data, from this we calculated a lifetime divorce rate at each age of the wife (Figure 7b). While the lifetime probability of divorce is relatively high for young wives because both the annual divorce rate and life expectancy are high, this is not true of elderly wives, whose lifetime divorce rate at age

[37] Our limitation is the same as that of Blau and Goodstein (2016), while Mazzocco (2007) does not allow assets to be used for bargaining, and Lise and Yamada (2018) do not incorporate endogenous divorce behavior or human capital accumulation in their structural model so the "divorce-concern" behavior discussed by Mazzocco et al. (2006) is not addressed.

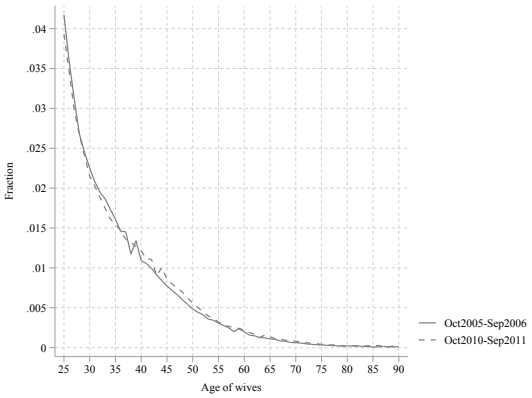
55 is only about 3 percent. Since the value of divorce under full commitment affects household behavior only through the decision to divorce, these low probabilities of divorce suggest that our baseline model without divorce approximates reality well and our estimation results do not seem to be driven entirely by behavior precipitated by divorce concerns.

We also calculated the weight ω_2^D put on the divorce value in the wife's value function. The weight of the wife aged t would be approximated by

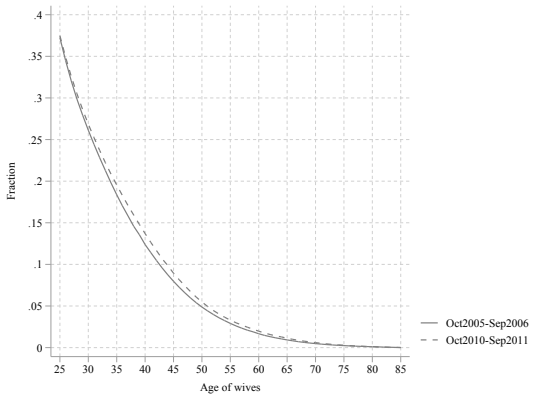
$$\begin{aligned}\omega_2^D(t) &= \frac{\sum_{s=t}^{T} \beta^{s-1} \Pr(D_s = 1 | D_{s-1} = 0)}{\sum_{s=t}^{T} \beta^{s-1} \Pr(D_s = 1 | D_{s-1} = 0) + \sum_{s=t}^{T} \beta^{s-1} \Pr(D_s = 0 | D_{s-1} = 0)} \\ &= \frac{\sum_{s=t}^{T} \beta^{s-1} \Pr(D_s = 1 | D_{s-1} = 0)}{\sum_{s=t}^{T} \beta^{s-1}},\end{aligned}$$

with the imprecision due to the decision to divorce depending on a distribution of uncertainties. Figure 7c plots ω_2^D at each age, showing that this weight is minute at all age points. It is still possible, however, that the weight on the divorce state could be large particularly among couples on the verge of divorce. To partially check if those couples are the driving force of our estimation results, we re-estimated our baseline regression excluding couples that divorced between 2005 and 2012, but the results are unchanged (Column 5 in Table 5), further suggesting that our results are difficult to explain simply by divorce concern behavior.

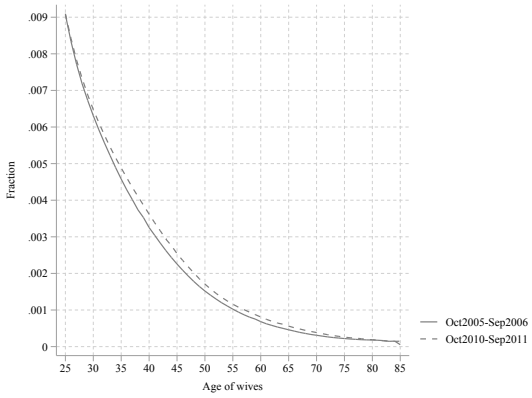
If we can accept that the full commitment model is rejected, our findings underline how difficult it is to make a commitment, even for elderly couples in Japan whose marital relationship tends to be stable and divorce is rare. While a lack of substantial exogenous variation did not allow us to test the degree of commitment of young couples, we suspect that their comparatively less stable marital relationships would make a commitment even more difficult. In fact, a high divorce rate may suggest that the participation conditions are likely to bind, so a young couple may have more opportunities to threaten divorce or to bring about a hold-up problem. We thus believe that a lack of commitment is a key feature of family decision making, and its economic consequences are worth considering in future empirical studies.



(a) Yearly divorce probability



(b) Life-time divorce probability



(c) Utility weight on the divorce state

Figure 7: Divorce probability and utility weight put on the divorce value

Source: *Vital Statistics* and *Census*.

C Supplementary Figures and Tables

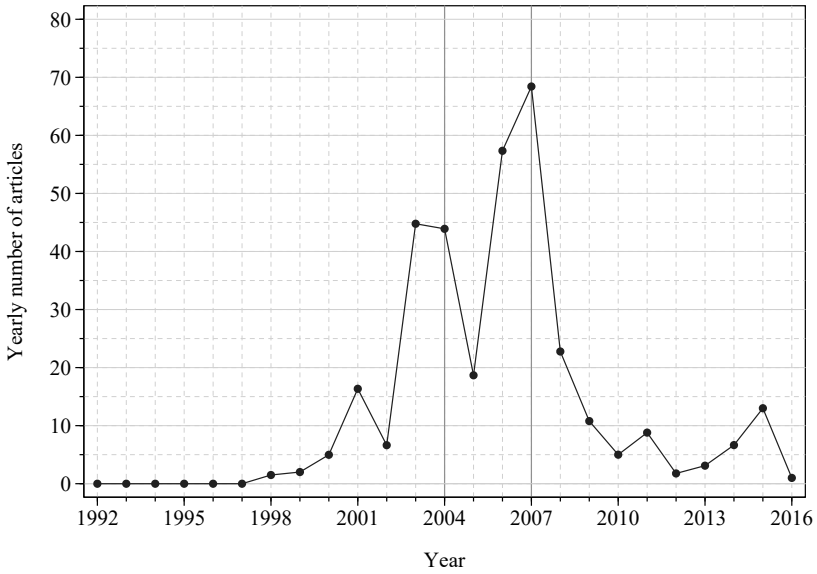


Figure 8: The yearly number of newspaper articles about pension division

Data Source: *Kikuzo II Visual*, *Maisaku*, *Semi-Annual Newspaper Issuer Report*, *Nikkei Telecom* and *Yomidasu Rekishikan*
Note: This figure shows the yearly number of newspaper articles about pension division on divorce published in the *Asahi*, *Mainichi*, *Nikkei* and *Yomiuri* newspapers, which are the prime national newspapers in Japan. The vertical axis shows the weighted sum of the number of the articles, where the annual yearly circulation of each newspaper was used as the weight and the weight on the number of the articles of the *Yomiuri* newspaper was normalized to one. Due to data availability, the total number of articles in 2015 and 2016 was not weighted. The pension reform was approved in 2004 and enacted in 2007.

Table 8: Age of eligibility for employee pension benefits

Birth cohort	Basic part		Proportional part	
	Men	Women	Men	Women
1940	60	60	60	60
1941	61	60	60	60
1942	61	60	60	60
1943	62	60	60	60
1944	62	60	60	60
1945	63	60	60	60
1946	63	61	60	60
1947	64	61	60	60
1948	64	62	60	60
1949	65	62	60	60
1950	65	63	60	60
1951	65	63	60	60
1952	65	64	60	60
1953	65	64	61	60
1954	65	65	61	60
1955	65	65	62	60
1956	65	65	62	60
1957	65	65	63	60
1958	65	65	63	61
1959	65	65	64	61
1960	65	65	64	62
1961	65	65	65	62
1962	65	65	65	63
1963	65	65	65	63
1964	65	65	65	64
1965	65	65	65	64
1966	65	65	65	65

Table 9: Summary of the 2007 pension reform

Date of approval	June 2004
Date of enactment	April 2007
Applies to	Employee pension insurance (*kosei nenkin*)
	Mutual Aid pension insurance (*kyosai nenkin*)
Does not apply to	National pension insurance (*kokumin nenkin*)
Maximum division rate	50% of household total pension benefits

Table 10: Changes in the time allocation of the treatment group and the control group

	Wife				Husband			
Leisure	Before	After	D	DD	Before	After	D	DD
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Treatment group	105.09	108.71	3.63**	1.57	118.3	119.06	0.76	1.41
	[33.48]	[29.10]	(1.49)	(2.30)	[19.41]	[18.09]	(0.87)	(1.49)
Control group	105.09	107.14	2.06		120.73	120.08	-0.65	
	[29.60]	[26.70]	(1.68)		[22.10]	[21.45]	(1.27)	
Market labor supply	Before	After	D	DD	Before	After	D	DD
	(5)	(6)	(7)	(8)	(5)	(6)	(7)	(8)
Treatment group	12.27	13.28	1.01	0.37	46.74	46.63	-0.11	-0.60
	[15.34]	[15.15]	(0.70)	(1.33)	[18.55]	[17.89]	(0.84)	(1.43)
Control group	27.51	28.15	0.64		43.74	44.23	0.49	
	[21.57]	[22.22]	(1.25)		[20.84]	[20.61]	(1.20)	
Domestic labor supply	Before	After	D	DD	Before	After	D	DD
	(9)	(10)	(11)	(12)	(9)	(10)	(11)	(12)
Treatment group	50.64	46.01	-4.64***	-1.94	2.96	2.31	-0.65**	-0.81*
	[36.26]	[30.92]	(1.60)	(2.43)	[6.16]	[4.26]	(0.27)	(0.47)
Control Group	35.4	32.71	-2.70		3.53	3.69	0.16	
	[30.06]	[25.74]	(1.69)		[7.19]	[7.05]	(0.41)	

Note: The table shows the means of leisure before and after the pension reform, their difference within each group, and a DD estimate, with standard deviations in brackets and standard errors in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Placebo test: Monte Carlo simulation

# of statistically significant estimates for pre-treatment years	Significance level $\alpha = 0.1$	$\alpha = 0.05$
0	26.5%	49.8%
1	20.7%	25.4%
2	22.7%	14.7%
3	13.4%	5.6%
4	8.6%	2.6%
5	4.7%	1.5%
6	1.9%	0.2%
7+	1.5%	0.2%

Note: The table shows the results of a Monte Carlo simulation in which the treatment status was randomly assigned using the empirical share of treated households and replicated Table 4 a thousand times.

Table 12: Clustering wild bootstrap results

	Leisure	Market labor supply	Domestic labor supply
	(1)	(2)	(3)
$Treatment \times d_{2007}$	5.733	-1.659	-4.074
	(2.998)	(2.527)	(1.882)
t-statistics	1.912	0.657	2.165
Wild bootstrap with two weights $\{-1, 1\}$			
p90	1.683	0.804	2.100
p95	2.037	0.957	2.231
p99	2.307	1.061	2.309
Wild bootstrap with six weights $\{-\sqrt{1.5}, -1, -\sqrt{0.5}, \sqrt{0.5}, 1, \sqrt{1.5}\}$			
p90	1.817	0.822	2.102
p95	1.993	0.951	2.199
p99	2.270	1.102	2.386

Note: The table shows the results of the clustering wild bootstrap for the coefficient on $Treatment \times d_{2007}$ in equation (8), where the cluster is defined by treatment status and year. Two types of the wild bootstrap were conducted, one with two random weights $\{-1, 1\}$ and another with six random weights $\{-\sqrt{1.5}, -1, -\sqrt{0.5}, \sqrt{0.5}, 1, \sqrt{1.5}\}$, as suggested by Cameron et al. (2008). For the first type, all possible combinations of the weight assignment were implemented since there are only 8 groups. For the second type, the weights were randomly assigned with 1000-time bootstrap replications.

Table 13: Robustness check of heterogeneity of policy impact by wife's age (10-year bin)

Age	Leisure (1)	Market labor supply (2)	Domestic labor supply (3)	Observations [Households]
50–59	4.976**	-2.363	-2.613*	1333
	(2.509)	(2.136)	(1.435)	[442]
49–58	3.939*	-1.498	-2.441*	1468
	(2.308)	(1.978)	(1.322)	[481]
48–57	2.395	0.085	-2.481*	1643
	(2.078)	(1.795)	(1.288)	[533]
47–56	1.842	0.020	-1.862	1748
	(1.932)	(1.652)	(1.233)	[564]
46–55	0.786	1.121	-1.908	1803
	(1.871)	(1.624)	(1.208)	[584]
45–54	0.907	0.701	-1.608	1845
	(1.849)	(1.622)	(1.150)	[595]
44–53	0.866	0.208	-1.075	1852
	(1.807)	(1.545)	(1.226)	[596]
43–52	-0.513	1.270	-0.757	1849
	(1.805)	(1.544)	(1.264)	[595]
42–51	0.256	0.674	-0.931	1823
	(1.798)	(1.518)	(1.386)	[585]
41–50	0.957	1.517	-2.474	1836
	(1.851)	(1.482)	(1.555)	[587]

Note: The table shows the estimation results of equation (7) by the age group of the wife. Only the estimated values of δ_1^j are reported, with standard errors clustered by each household in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 14: Robustness check of heterogeneity of policy impact by wife's age (5-year bin)

Age	Leisure (1)	Market labor supply (2)	Domestic labor supply (3)	Observations [Households]
55–59	7.863*	-1.999	-5.864*	490
	(4.364)	(3.406)	(2.996)	[169]
54–58	8.079*	-3.343	-4.736**	625
	(4.319)	(3.714)	(2.340)	[207]
53–57	7.953**	-4.397	-3.556*	736
	(3.581)	(3.070)	(2.002)	[242]
52–56	5.417	-2.928	-2.489	794
	(3.383)	(2.854)	(1.753)	[258]
51–55	3.425	-1.612	-1.813	845
	(3.162)	(2.713)	(1.681)	[275]
50–54	4.195	-2.760	-1.434	843
	(3.049)	(2.722)	(1.548)	[273]
49–53	1.417	-0.453	-0.964	843
	(2.629)	(2.293)	(1.573)	[274]
48–52	-1.897	3.521	-1.624	907
	(2.450)	(2.145)	(1.707)	[291]
47–51	-1.213	2.424	-1.211	954
	(2.213)	(1.916)	(1.741)	[306]
46–50	-1.922	3.610**	-1.688	958
	(2.101)	(1.812)	(1.776)	[309]

Note: The table shows the estimation results of equation (7) by the age group of the wife. Only the estimated values of δ_1^j are reported, with standard errors clustered by each household in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.