"Pappa Ante Portas: The effect of the husband's retirement on the wife's mental health in Japan

Pappa Ante Portas: The Effect of the Husband's Retirement on the Wife's Mental Health in Japan[§]

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§ *Pappa Ante Portas* is a 1991 German movie directed by Loriot and Renate Westphal-Lorenz, describing the conflicts between a retired husband and his wife and son. The title alludes to *Hannibal ante portas!* ("Hannibal before the gates!"), a Roman call referring to Carthaginian commander Hannibal on his way to Rome in 211.

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Pappa Ante Portas: [§]

The Effect of the Husband's Retirement on the Wife's Mental Health in Japan

Abstract

The "Retired Husband Syndrome", that affects the mental health of wives of retired men around the world, has been anecdotally documented but never formally investigated. Using Japanese micro-data and the exogenous variation across cohorts in the maximum age of guaranteed employment induced by a 2006 Japanese reform, we estimate that the husband's earlier retirement significantly increases the probability that the wife reports symptoms related to the syndrome. We also find that retirement has a negative effect both on the household's economic situation and on the husband's own mental health, and that the higher economic distress contributes to reducing the wife's mental health.

JEL codes: D1, I1, I3, J14, J26

Keywords: Japan; mental health; stress; couples; retirement; pension reforms

[§] *Pappa Ante Portas* is a 1991 German movie directed by Loriot and Renate Westphal-Lorenz, describing the conflicts between a retired husband and his wife and son. The title alludes to *Hannibal ante portas!* ("Hannibal before the gates!"), a Roman call referring to Carthaginian commander Hannibal on his way to Rome in 211.

1. Introduction

The "Retired Husband Syndrome" (RHS hereafter) is a stress-induced condition affecting wives of retired husbands, with symptoms that include headaches, depression, agitation, palpitations, and lack of sleep. These symptoms typically appear after the husband's retirement (see Johnson, 1984). Health economists have investigated in depth the effects of retirement on the mental health of retiring individuals (see for instance Belloni et al., 2016, and Bonsang and Klein, 2012), but have overlooked so far to examine the possibility that cross-partner mental health effects exist. This is rather surprising, given the large body of literature that studies the joint retirement decision of couples (see for instance Hurd, 1990, and Gustmann and Steinmeier, 2000) and the cross-partner effects of retirement on time use, home production and the incidence of divorce (see for instance Stancanelli and Van Soest, 2012a and 2012b, Stancanelli, 2014, Bonsang and Van Soest, 2015, and Ciani, 2016).

Thus, in spite of the potential relevance of RHS for the well-being and health of older married women, no empirical evidence exists to date documenting whether the husband's retirement has a *causal* negative effect on the wife's mental health. Cross-partner mental health effects of retirement are considered in psychological studies (see for instance Smith and Moen, 2003, and Szinovacz and Davey, 2004), but these studies do not estimate causal effects.

In this paper, we try to fill this gap. Our empirical investigation focuses on Japan. While stories about RHS are by no means restricted to a single country, the international press has often looked at Japan, because of its alleged diffusion there (see for instance Kenyon, P., *Retired Husband Syndrome*, BBC, 11/13/2006). Our data are drawn from the Preference Parameters Study (PPS), an annual survey conducted by the University of Osaka on a representative sample of the Japanese population, which contains detailed information on individuals and their spouses, including retirement and self-reported measures of mental health.

We identify the causal impact of the husband's earlier retirement on the wife's RHS using the exogenous variation generated by the 2006 revision of the Elderly Employment Stabilization Law (EESL), which mandated Japanese employers to guarantee continuous employment between mandatory retirement age (at age 60) and full pension eligibility age. We use the exogenous changes in maximum guaranteed employment age across cohorts as an instrumental variable for the husband's retirement, carefully controlling for potential confounding factors, and find that the husband's retirement has a causal effect on the wife's RHS symptoms by increasing her stress, depression and inability to sleep.

The intensity of this effect may vary with the wife's labour market status. On the one hand, an inactive wife may have to share longer time with the retired husband, and be stressed by his requests. On the other hand, a working wife may have to add to the stress for the job the stress associated to a retired husband. We estimate that employed wives are the ones suffering the most, suggesting that the latter effect prevails.

Coping with a retired and estranged husband is a natural candidate explanation of our results. However, other mechanisms could also be at play. On the one hand, the wife's RHS symptoms may increase when the husband himself experiences these symptoms, because of his own retirement (see Hashimoto, 2013). On the other hand, early retirement may generate economic distress by reducing income and wealth – the "early retirement trap" discussed for instance by Angelini et al., 2009 – with negative effects on the wife's mental health. We present evidence on the latter mechanism by showing that the inclusion of indicators of economic distress in our empirical model reduces the magnitude of the estimated RHS effect by about 15 percent. We also document that the husband's retirement negatively affects his own mental health. Unfortunately, since our data do not allow us to verify whether this is a relevant mechanism, our view that the attention and care required by a depressed husband matters for the wife's RHS remains speculative at best. We conclude that – in the absence of better data – much of the estimated overall effect remains unaccounted for.

2. Institutional Background

In Japan, long term employment contracts typically terminate at age 60 with mandatory retirement (see for instance Ichimura and Shimizutani, 2012, and Kondo and Shigeoka, 2014). The two-tier pension scheme for private sector employees (see Okumura and Usui, 2014, for an overview) was reformed by the 2001 Pension Reform Act, which gradually increased the minimum eligibility age for full pension benefits above mandatory retirement age at 60. For men, and starting in 2001, the cohorts born between 1941 and 1943 could draw the flat-rate pension benefit (first tier) only from age 61, while retaining the right to draw the wage proportional benefit (second tier) from age 60. Kondo and Shigeoka, 2014, report that the average share of the first tier component on total pension benefits was equal to 37.5 percent. Younger male cohorts were progressively exposed to even higher increases in the minimum eligible age for the flat-rate component of pension benefits, until age 65 was reached in 2013 for the cohorts born in 1949 or later.

Before 2006, the increase in the minimum eligible age for the first tier of pension benefits was not accompanied by changes in mandatory retirement age. Therefore, individuals belonging to the exposed cohorts reached mandatory retirement age without being able to draw full pension benefits. To address this problem, in 2006 the Japanese government passed a revision of the Elderly Employment Stabilization Law, which mandated firms to introduce measures to guarantee employment until eligibility for full pension benefits was reached. This additional reform affected private sector employees born from 1946 onwards, by raising the maximum age of guaranteed employment from 60 to 63 for those born in 1946, to 64 for those born between 1947 and 1948, and to 65 for those born from 1949 onwards.

For the cohorts born between 1940 and 1952, Table 1 shows mandatory retirement age, the age until which employers are legally obliged to continue employment and the eligibility age for the flat-rate pension benefit. To illustrate with an example the consequences of the EESL revision, consider two employees born in 1945 and 1946. For the former, maximum guaranteed employment and full pension eligibility age were 60 and 63 respectively. For the latter, both ages were set at 63.

Firms could comply with the employment guarantee either by raising mandatory retirement age, a costly option given the steeply rising age-earnings profiles in Japan (see Hashimoto and Raisian, 1985), or by re-employment after mandatory retirement until guaranteed age, typically at a lower wage. To further encourage retention, the government provided a subsidy to employers offering re-employment to all retiring employees. Not surprisingly, as of 2008 only 15 percent of employers chose to extend mandatory retirement, while 85 percent selected re-employment.

Importantly, the practice of re-employing workers after mandatory retirement pre-dates the EESL revision. However, according to the "Personnel Management Survey" conducted in 2004, only 15.7 percent of Japanese firms continued to employ all those who wished to be employed after reaching age 60. Employment opportunities for males aged 60+ increased after the EESL revision. According to a governmental survey quoted by Fujimoto, 2008, two thirds of the sampled firms reported that they employed almost all those who wished to stay on, and the number of regular employees aged 60+ increased by more than 30 percent because of the EESL revision.

3. Data

We use data from the Japanese Preference Parameters Study (PPS), conducted by the University of Osaka. The PPS is a nationally representative panel survey on behaviours, risk attitudes, habits formation and time preferences of the Japanese population, implemented yearly from 2003 onwards, with refreshment samples in 2006 and 2009. Interviews are carried out via paper-and-pencil questionnaires that are delivered and picked up by interviewers at the interviewees' homes, with very high response rates (always above 70 percent and in recent years close to 90 percent) in both the longitudinal and the refreshment samples.

The PPS is not a household survey, as only one individual per household is interviewed. However, the questionnaire asks married interviewees to report information also on their partners, including year of birth, education, employment status and, if the partner is retired, age at retirement. By focusing on the sub-sample of interviewed married females, we obtain information both on the wife's mental health and on the husband's retirement, as reported by the wife. Since questionnaires are left at interviewees' homes for completion, partners may fill them in together, reducing the risk that the wife misreports the husband's retirement age. In our data, there are 74 cases of wives reporting the year of husband's retirement inconsistently over time. In these cases, we use the retirement year most frequently reported by the wife as the correct one. Removing these observations from the sample does not affect our estimates.

In the waves from 2008 to 2013 the interviewees were asked how they felt about the following statements describing their mental health:

- *I have been feeling stressed lately.*

- *I have been feeling depressed lately.*

- *I haven't been sleeping well lately.*

Responses had to be provided on a 5-point discrete scale, with 5 indicating something particularly true for the respondent and 1 something that does not hold true at all. Figure A1 in the Appendix shows the distribution of individual evaluations of each statement for the sample of wives used in the estimates.

While our data do not include information on diagnosed depression or on validated scales that measure self-reported depressive symptoms, such as the CES-D or the Euro-D scales (Radloff, 1977; Guerra et al., 2015), the statements we consider provide useful information on the key symptoms of RHS described by Johnson, 1984, in his clinical article. In addition, self-reported depression, lack of sleep and stress (or irritability) are components of both the CES-D and the Euro-D scales.

We code the answers to each statement as dummies equal to 1 for values 4 and 5 and to 0 for the remaining values to make sure that our results reflect differences in mental health rather than differences due to over- or under- reporting, which is more likely to occur when distinguishing between near values at the extremes of the scale, such as 1 and 2 or 4 and 5. A similar approach is adopted by Haas, 2007, Angelini and Mierau, 2014, and [Anonymous et al., 2015].

Our outcome variable is the dummy MH^w, equal to 1 if at least one of the three dummy variables indicating the presence of RHS symptoms is equal to 1, and to 0 otherwise. Since this definition is somewhat arbitrary, as alternative definitions we: a) treat each symptom separately rather than aggregating them; b) use the original coding of the answers (from 1 to 5); c) extract from the three symptoms a single index of mental health using principal component analysis, that exploits the correlation among symptoms to generate aggregation weights. Reassuringly, as shown in Table A4 in the Appendix, our empirical results do not vary qualitatively with the selected definition.

Our initial sample consists of married women interviewed between 2008 and 2013 whose husbands were born between 1940 and 1952, a six-year span on each side of the 1946 cohort, first affected by the 2006 EESL reform. We drop outliers in the distribution of the age difference between partners by excluding married couples where the wife was born either before 1940 or after 1961, which account for about 2 percent of the initial sample.

The final sample is a yearly unbalanced panel consisting of 817 wives and 3,192 wife-year observations with non-missing values in the outcome and treatment variables. The final number of observations is affected by the fact that close to 50 percent of individuals in our final sample entered the survey in the 2009 refreshment sample, by attrition and by a few cases of wives completing the questionnaire only in some waves. Attrition is a problem in our setup if it correlates with the wife's mental condition. To check whether this is the case, we regress the probability of replying to the survey at time t but not in the successive year on the dummy MH^w at time t and age dummies and – reassuringly – find that the coefficient attached to MH^w is small and not statistically significant.

Descriptive statistics for the relevant variables are shown in Panel A of Table 2. It turns out that 46 percent of the wives in our sample report at least one of the three RHS symptoms: 41 percent have been feeling stressed, 23 percent have been feeling depressed and 16 percent have experienced sleeping difficulties. In our sample, 21 percent of husbands are retired. The unconditional average number of years since retirement is 1.33. Conditional on retirement, the average increases to 5.42. Furthermore, 51 percent of husbands are older than the cohort-specific maximum guaranteed employment age, and the number of years since this maximum age ranges from 0 to 14, with a mean of 7.19 years, conditional on eligibility.

4. Methods

As described in detail in the Appendix, we assume that the wife's mental health MH^w – where the superscript w is for the wife and h is for the husband – depends on the husband's time since retirement (in years) YR^h, the wife's labour market status NLF^w – defined as a dummy equal to one if the wife is out of the labour force and to zero otherwise (we use labour force status rather than retirement in the case of females because, contrary to the case of males, retirement in the relevant age group is not the main source of disengagement from work) – and a vector X of predetermined variables, including the couple's education, age and cohort of birth and the number of children. We further assume that the wife's labour market status depends on vector X and the exogenous variable Z^w, that will be introduced below. Our empirical model is

$$MH_{jt}^{w} = \alpha + \beta YR_{jt}^{h} + \gamma X_{jt} + \delta Z_{jt}^{w} + \xi_{jt} \quad (1)$$

where the vector X_{jt} includes the number of children and the education of both partners, indicators of early life conditions and linear trends in the husband and the wife's age and in the husband's cohort of birth, the subscripts *j* and *t* are for the household and time, ξ is the error term and we have used the assumption that NLF^w depends on X and Z^w to replace the former with the latter.

Since the wife's RHS syndrome is likely to be affected not only by the husband's transition into retirement but also by the time he has spent in retirement, in our preferred specification we measure YR^h as the number of years spent in retirement plus 1 if the husband has already retired and as zero otherwise, or, more formally:

$$YR_{jt}^h = \begin{cases} 0 & if\ Age_{jt}^h < R_{jt}^h \\ Age_{jt}^h + 1 - R_{jt}^h & if\ Age_{jt}^h \geq R_{jt}^h \end{cases} \quad (2)$$

where Age_{jt}^h and R_{jt}^h are the husband's current age and retirement age respectively. Mazzonna and Peracchi, 2012, use a similar specification. We add 1 to current age to avoid assigning the same value to husbands who have just retired (age equal to retirement age) and who have still to retire. As a robustness check, we estimate an alternative specification of (1) where YR^h is a dummy for whether the husband is retired or not, and show that results are comparable (see Table A3 in the Appendix). As discussed below, we also experiment with a quadratic specification and with a specification that uses YR^h bins rather than a linear trend in YR^h.

The estimation of equation (1) by OLS is unlikely to uncover causal effects for at least three reasons. First, the husband's retirement is a choice variable, which is likely to be affected by shocks influencing the wife's mental health (see Coile, 2004). Second, our measure of time since retirement could be influenced by measurement error if some husbands keep working after formal retirement. Last but not least, there may be variables affecting both the husband's retirement and the wife's mental health that are omitted from (1).

We address these problems using an instrumental variables (IV) identification strategy. Similarly to Kondo and Shigeoka, 2014, our selected instrument for YR^h, Z^h, exploits the exogenous variation across cohorts induced by the revision of the EESL in 2006, which progressively guaranteed additional employment after age 60 to the cohorts of men born between 1946 and 1952 (treated cohorts), but not to earlier cohorts born between 1940 and 1945 (control cohorts) – see Table 1. Since our data are longitudinal, we maximize the variability of the instrument Z_{jt}^{h} by defining it as

$$Z_{jt}^{h} = \begin{cases} 0 & if\ Age_{jt}^{h} < CEG_c \\ Age_{jt}^{h} + 1 - CEG_c & if\ Age_{jt}^{h} \geq CEG_c \end{cases} \tag{3}$$

where CEG_c (guarantee of continued employment) is the maximum age of guaranteed employment for employees born in cohort c – see column (2) in Table 1. The variable Z_{jt}^{h} is equal to zero whenever the husband's age is below maximum age CEG, and equal to the distance between age and CEG plus 1 when age is above or equal to the maximum age. We define in a similar fashion Z_{jt}^{w} for wives.

The discontinuous jumps in CEG, from 60 to 63 for those born in 1946, from 63 to 64 for those born between 1947 and 1948, and from 64 to 65 for those born later, induce discontinuous jumps in Z^h and generate exogenous variation in YR^h. To illustrate, consider individuals aged 64 who have been born between 1944 and 1948. Their value of Z^h is equal to 5 for those born in 1944 and 1945, falls to 2 for those born in 1946, further to 1 for those born in 1947 and 1948 and to 0 for those born after 1948.

To qualify as valid, the instrument should affect wives' mental health only via its impact on husbands' retirement, and should therefore be uncorrelated with variables entering in the error term of equation (1). Since our identification strategy is based on comparisons across cohorts, a potential threat to identification is that treated cohorts, who have been born from 1946 onwards, may have experienced different early life conditions than control cohorts, born before 1946. If childhood conditions affect mental health, and these effects are persistent (see for instance Almond and Chay, 2006, [Anonymous, 2015], and Layard et al., 2014), we may incorrectly attribute changes in the wife's mental health to variations in the duration of husband's retirement, when these changes are driven instead by omitted differences among cohorts.

Having panel data, we could model time-invariant early life conditions as individual fixed effects and use a fixed effect estimator. By using fixed effects, however, we would absorb the variability in Z^h between cohorts conditional on age, and only very limited within-cohort variation in Z^h would remain – concentrated among those born between 1946 and 1948, who changed their CEG status during our sample period.

We control instead for individual differences in early economic conditions by including in all regressions a linear trend in the husband's year of birth and the log of real GDP per capita in the year of birth of both partners. In addition, we capture differences in family background and childhood conditions by including also the variables listed in Panel B of Table 2. Since the main coefficients of interest change very little after adding these variables, we conclude that omitted variable bias due to uncontrolled differences in early life conditions among cohorts is not driving our results. To further dispel this concern, we apply the methodology proposed by Altonji, Elder and Taber, 2005, and Oster, 2015, and use selection on observables to estimate the degree of selection on un-observables that would be required to explain away our first stage and reduced form estimates. Results (available upon request) show that un-observables are not a plausible source of bias.

We estimate Equation (1) both by Two Stage Least Squares (2SLS) and using an IV probit model, to account for the fact that the dependent variable MH^w is binary. We cluster standard errors at the same level of variation of the instrument, that is, cohort by year, for a total of 78 clusters (13 cohorts by 6 years). Estimates are robust to clustering by individual, as shown in Table A2 in the Appendix, where we also present other robustness tests, described in the notes to the table.

Since we expect that the effect of the husband's retirement on the wife's mental health varies with her labour market status, we estimate a version of Equation (1) that includes the interaction of YR^h with the wife's employment status (1 for employment, 0 for inactivity). We also augment Equation (1) with a vector of indicators of the household's economic situation, that we interpret as potential mediators of the overall effect, and verify whether the impact of the husband's retirement on the wife' mental health is altered. This vector includes dummies indicating whether the household owns the house, whether the wife's self-reported current standard of living is low, whether the husband's income is below median and the presence of financial distress – due to lower than median assets or to the presence of debt. Panel C of Table 2 reports the descriptive statistics of these variables.

5. Results

5.1. First-stage regressions

Our identification strategy relies on the exogenous variation in the timing of the husband's retirement induced by changes in maximum guaranteed employment age (CEG) across contiguous cohorts. We first regress years since husband's retirement YR^h and the wife's labour market status NLF^w on Z^h, Z^w and the vector of predetermined variables X – see Table 3. All regressions include linear trends in the husband and the wife's age, the husband's year of birth and real GDP per capita at birth for both partners, and we progressively add to the baseline specifications reported in columns (1) and (4) controls for demographic characteristics and early life conditions – see columns (2) and (3) for YR^h and columns (5) and (6) for NLF^w.

We always find that the instrument Z^h has a positive and statistically significant effect on YR^h, but no statistically significant effect on NLF^w. Furthermore, adding other covariates affects only marginally the estimated first stage effects. A threat to causal inference would be the presence of an effect of Z^h on the wife's labour market status NLF^w, that depends also on Z^w. In this event, we could not tell whether the reduced form effect of Z^h on MH^w is to be attributed to its effect on YR^h or on NLF^w. Reassuringly for our identification strategy, columns (4) to (6) report that the instrument Z^h is unrelated to the wife's labour market status. NLF^w depends instead on her own time since eligibility to retirement, Z^w, which we always include as control in our empirical analysis.

Focusing on the least parsimonious specification, reported in column (3), we estimate that adding one year to Z^h increases time into retirement by 0.391 years, a sizeable effect. As documented by the first stage F statistic – equal to 109.0 – our instrument is not weak. As reported in the bottom two lines of Table A1 in the Appendix, these estimates are robust to several specification tests, including the use of age dummies and higher order polynomials for age and cohort trends. We also verify the quality of the data reported by wives on their husbands by estimating the effect of Z^h on YR^h when retirement age is reported by the husbands on themselves rather than by their wives and find very similar results: the estimated first stage effect of Z^h including all controls is 0.345*** (F statistic: 36.3).

5.2. Main results

The OLS, 2SLS, probit, and IV probit estimates of Equation (1) using the full set of available controls are reported in Table 4. Separate results for stress, depression, lack of sleep (using both the binary and the original coding of the outcomes) and for a standardized index of mental health obtained using principal component analysis are presented instead in Table A4 in the Appendix, and are in line with those in Table 4. Considering first the OLS and probit specifications, reported in Columns (1) and (3) of the table, we find no evidence that the husband's retirement and the wife's RHS symptoms are associated, as marginal effects are close to zero and not statistically significant in either estimate. However, when we address the endogeneity of years since husband's retirement YR^h – see Columns (2) and (4) – our estimates change markedly: we find that an additional year of the husband's retirement increases the wife's RHS symptoms by 5.5 to 6.4 percent, depending on whether we use a linear or a probit model. This corresponds to an 11.9 to 13.9 percent increase with respect to the sample average value (0.46).

These estimates indicate that the husband's retirement has a causal effect on the wife's RHS symptoms, measured by stress, depression and lack of sleep. The natural question arises as to whether the reverse is also true. However, when we estimate the effect of the wife's labour market status on the husband's RHS symptoms using Eq. (1), our estimates (available upon request) show no evidence that a statistically significant effect exists.

5.3. Specification Tests

Our baseline specification assumes that the marginal effect of the husband's retirement on the wife's mental health is constant. This assumption may be overly restrictive. Therefore, we relax it by estimating two alternative specifications, one using a quadratic in YR^h and the other using four YR^h bins (for distance to retirement equal to 0-2, 3-5, 6-8 and above 8 years). The quadratic specification shows that the marginal effect increases with time since retirement until 6 to 8 years, when it peaks and starts to decline – see column (2) of Table 5. However, when we test whether the quadratic term is statistically significant, we cannot reject the null (constant marginal effect) at the five percent level of confidence.

In a similar fashion, the coefficients associated to the YR^h bins are small for low distances (0-2), become larger as distance from retirement increases, reach a peak 6 to 8 years since retirement and decline afterwards – see column (3) of Table 5. Again, when we test whether the cumulative effects at different years since retirement implied by the linear specification and the one using bins are statistically different from each other, we fail to reject the null hypothesis of a constant marginal effect. Not reported in the table, we also experiment with a specification that includes a retirement dummy and distance from retirement (starting from zero rather than from one). Both variables attract positive but statistically insignificant coefficients. We conclude that the linear specification used in our baseline estimates does a reasonably good job in fitting the data.

If the EESL law targets full time employees rather than the self-employed or temporary workers, and if the type of husband's job directly affects the wife's mental health, omitting the former from model (1) may invalidate the instrument. To address this concern, we use the available information to generate dummies for white or blue collar jobs, employment in the private sector, public sector, or self-employment, and full versus part time jobs. When available, we use the information on the previous job for retirees and the unemployed, and control for missing values with specific dummies.

As shown in Table A5 in the Appendix, our IV estimates with and without these additional controls are very similar, suggesting that if any bias exists because of their omission, it is small.

5.4. Heterogeneous effects and mediation analysis

Table A6 in the Appendix presents estimates of a specification of Equation (1) that includes the interaction of time since retirement and a dummy indicating whether the wife is currently in employment. We find that this interaction attracts a positive and statistically significant coefficient, suggesting that working wives suffer more than inactive wives the consequences of having a retired husband. We also test for the presence of other heterogeneous effects (based on the age difference between husband and wife and on whether the wife has a college degree) but do not find significant differences.

As in Hashimoto, 2013, we also find evidence that the husband's retirement increases his own RHS symptoms. The estimated effect, however, is smaller in magnitude than the one for wives (3.6% vs. 6.4%) and statistically significant only at the ten percent level of confidence. It suggests that concerns with the "Retired Husband Syndrome" should not be limited to wives, as both partners may be affected, and that the attention and care required by a retired (and potentially depressed) husband is likely to increase pressure on the wife. It is worth remembering that, since we only observe the mental health of the respondent – and not of the partner, this effect is estimated using the sample of males, who report both their own mental health and their own retirement status.

Zhao et al., 2012, show that retirement in Japan affects health behaviours by reducing smoking and increasing exercising. We find positive and statistically significant – at the five or ten percent level of confidence – effects of retirement on the following indicators of household economic distress: does not own the house, financial distress, and low standard of living (as reported by the wife), but no effect on the husband's income, the wife's housework and her self-assessed general health, probably because of the small sample size due to the presence of many missing values (detailed results are available from the authors).

When we add these indicators of economic distress to the baseline regression – see Table 6 – we find that they are positively and significantly associated to the outcome both jointly (p-value < 0.01) and – except for the husband's income – individually. Their inclusion reduces the marginal effect of the husband's years since retirement on the wife's mental health by about 15 percent suggesting that they act as mechanisms that account for a relatively small part of the overall effect. Unfortunately, since wives are not asked to report about their husband's mental health, we cannot establish whether the latter also contributes to the estimated effect.

6. Discussion and conclusions

We have found that the husband's retirement has a causal effect on the wife's RHS symptoms by increasing her stress, depression and inability to sleep. We have shown that, conditional on both partners' age, adding one year to the time spent in retirement by the husband increases the probability that the wife develops at least one of the RHS symptoms by 5.5 to 6.4 percentage points, depending on the estimation method. In our sample, average years since retirement for those retired are about 5.4. Evaluated at this mean value, the effect of the husband's time since retirement on the probability of the wife having RHS is close to 30 percentage points, not a small effect given that the prevalence of RHS among wives in our sample is 46 percent. This result is robust to changes in the definition of the dependent variable and to a battery of specification tests.

A candidate explanation of the large gap between the OLS and 2SLS (and between probit and IV probit) estimates of the husband's retirement effect is endogenous selection: if the husbands whose wives are not depressed select into earlier retirement, this would impart a negative bias on the OLS estimates, reducing them below the consistent 2SLS estimates. Another candidate is the attenuation bias induced by the fact that the time spent in retirement is a noisy measure of the husband's disengagement from work, a common finding in the literature on the effects of retirement on health (see e.g. Celidoni, Dal Bianco and Weber, 2013).

We have considered mechanisms that may help explaining the negative effect of the husband's retirement on the wife's mental health and found that the husband's retirement is a source of stress for both partners, partly because it reduces financial and economic security. Although we cannot formally test whether the husband's depression affects the wife's mental health, we suspect that the attention and care required by a retired and potentially depressed husband is likely to increase pressure on the wife. However, stronger conclusions on the mechanisms explaining our results must await for better data.

Our study is not without shortcomings. The first limitation is in terms of external validity. Since our estimates are based on instrumental variable regressions, they are only informative about the sub-population complying with the selected instrument. However, in the absence of randomization of the age of retirement, we believe that this is as close as we can get to estimating a causal effect. Another drawback is that we measure mental health by relying upon self-assessed symptoms rather than on a diagnosis by qualified physicians. Needless to say, future research that has access to more comprehensive data may improve upon these limitations.

While our findings are based on a single country, they highlight the need to study retirement as a joint process affecting the couple, and show that failure to consider cross-partner effects can lead to underestimating the negative consequences of retirement on mental well-being. While much debate surrounding retirement in economics is centred around the financial preparedness to retire, our results suggest that attention should also be paid to preparing for retirement from a psychological point of view, so as to avoid or attenuate the consequences on the mental health and well-being of married couples.

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Tables

Table 1. Mandatory retirement age, age until employment is legally continued and eligibility age for the flat – rate benefit.

Cohort	Mandatory retirement age	Minimum age for guaranteed continued employment (CEG) after mandatory retirement.	Eligibility age for the flat – rate pension benefit
1940	60	60	60
1941	60	60	60
1942	60	60	60
1943	60	60	61
1944	60	60	61
1945	60	60	63
1946	60	63	63
1947	60	64	64
1948	60	64	64
1949	60	65	65
1950	60	65	65
1951	60	65	65
1952	60	65	65

Note: see Kondo and Shigeoka, 2014, and Okumura and Usui, 2014.

Table 2. Descriptive statistics. Sample size: 3,192.

Variable	Mean	Std. Dev.
Panel A. Main Variables		
Dependent variables		
Wife has RHS symptoms (MH^w)	0.46	0.5
Wife is stressed	0.41	0.49
Wife is depressed	0.22	0.42
Wife lacks sleep	0.16	0.37
Explanatory variables		
Husband has retired	0.21	0.41
Husband's years since retirement (YR^h)	1.33	2.95
Wife is out of the labour force	0.54	0.50
Husband's age plus 1 minus maximum guaranteed age (Z^h)	3.64	4.44
Wife's age plus 1 minus maximum guaranteed age (Z^w)	1.69	3.23
Husband's age	63.86	3.88
Wife's age	60.8	4.38
Husband's year of birth – 1900	46.62	3.60
Wife has college degree	0.28	0.45
Husband has college degree	0.34	0.48
Number of children	2.11	0.79
Parents live in the same household	0.14	0.35
Panel B. Family background and childhood conditions		
At least one parent with high school or higher education	0.14	0.34
At least one parent of spouse with high school or higher education	0.19	0.39
Both parents with high school or higher education	0.33	0.47
Both parents of spouse with high school or higher education	0.36	0.48
High self-reported wellbeing at age 15	0.29	0.46
Low self-reported wellbeing at age 15	0.21	0.41
Lived in metropolitan area at age 15	0.32	0.47
Mother was not working at age 3	0.50	0.50
Spouse's mother was not working at age 3	0.57	0.50
Oldest son or daughter	0.41	0.49
Number of siblings at age 15	2.5	1.63
Log real GDP in thousand dollars at husband's birth	0.71	0.25
Log real GDP in thousand dollars at wife's birth	0.76	0.25

Panel C. Mediating factors		
Husband has RHS symptoms	0.38	0.49
Not homeowner	0.09	0.29
Low wife's self-reported current standard of living	0.25	0.43
Husband's income below median	0.22	0.41
Financial distress – household has lower than median assets or has debt	0.55	0.49

Table 3. First stage OLS estimates. Dependent variables: years since husband's retirement YR^h and a dummy indicating whether the wife is out of the labour force NLF^w.

	(1) Years since husband's retirement	(2) Years since husband's retirement	(3) Years since husband's retirement	(4) Wife out of the labour force	(5) Wife out of the labour force	(6) Wife out of the labour force
Husband's age plus 1 minus maximum guaranteed age (Z^h)	0.401***	0.395***	0.391***	0.002	0.001	-0.004
	(0.038)	(0.038)	(0.037)	(0.007)	(0.007)	(0.007)
Wife's age plus 1 minus maximum guaranteed age (Z^w)	0.056	0.055	0.071	0.017**	0.016**	0.020***
	(0.083)	(0.082)	(0.083)	(0.006)	(0.006)	(0.006)
Basic controls	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	No	Yes	Yes	No	Yes	Yes
Family background and childhood controls	No	No	Yes	No	No	Yes
Observations	3,192	3,192	3,192	3,192	3,192	3,192
R-squared	0.287	0.297	0.313	0.061	0.078	0.111
F-stat for husband's Z	109.3	108.3	109.0	0.1	0	0.2
F-stat for wife's Z	0.5	0.4	0.7	6.5	6.7	12.4

Notes: Basic controls: husband and wife's age, husband's year of birth, real GDP per capita at birth for both partners. Additional controls: the couple's education, number of children and whether the couple's parents also live in the household. Family background and childhood controls: couple's parental education, wife's self-reported wellbeing at age 15, order of birth, whether the mother was working at age 3, whether the individual lived in a metropolitan area at age 15, dummies for number of siblings of the wife. In case of missing values, we set the value of the relevant variable to zero and introduce a corresponding dummy for missing values to 1. Robust standard errors clustered by cohort and year within parentheses. One, two and three stars for statistical significance at the 10, 5 and 1 percent level of confidence.

Table 4. The effect of the years since husband's retirement on the wife's retired husband syndrome. Ordinary least squares, two-stages least squares, probit and instrumental variables probit estimates. Marginal effects. Dependent variable: dummy MH^w for the presence of the wife's retired husband symptoms.

	(1) OLS	(2) 2SLS	(3) Probit	(4) IV probit
Husband's years since retirement (YR^h)	-0.002	0.064***	-0.003	0.055***
	(0.004)	(0.018)	(0.004)	(0.012)
Wife's age plus 1 minus maximum guaranteed age (Z^w)	0.003	-0.009	0.003	-0.008
	(0.007)	(0.010)	(0.007)	(0.009)
Basic controls	Yes	Yes	Yes	Yes
Additional control	Yes	Yes	Yes	Yes
Family background and childhood controls	Yes	Yes	Yes	Yes
Observations	3,192	3,192	3,192	3,192

Notes: see Table 3.

Table 5. The effect of the years since husband's retirement on the wife's retired husband syndrome. Two-stages least squares estimates. Linear, quadratic specifications and specification with bins. Dependent variable: dummy MH^w for the presence of the wife's retired husband symptoms.

	(1) Linear specification	(2) Quadratic specification	(3) Bins in years since husband's retirement
Husband's years since retirement	0.064***	0.143***	0.059***
	(0.018)	(0.055)	(0.021)
Husband's years since retirement squared		-0.009*	
		(0.005)	
0-2 years since husband's retirement			0.026
			(0.541)
3-5 years since husband's retirement			0.409
			(0.249)
6-8 years since husband's retirement			0.547***
			(0.173)
More than 8 years since husband's retirement			0.333
			(0.354)
Basic controls	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Family background and childhood controls	Yes	Yes	Yes
Observations	3,192	3,192	3,192

Notes: See Table 3. All regressions also include the wife's age plus 1 minus maximum guaranteed age as control. The linear specification uses a linear trend in Z^h as instrument, the quadratic specification uses a quadratic polynomial in Z^h, the bins specification a set of dummies for each level of Z^h.

Table 6. The effect of years since the husband's retirement on the wife' mental health. With and without mediating factors as explanatory variables. Two-stages least squares estimates. Dependent variable: dummy MH^w for the presence of the wife's retired husband symptoms.

	(1) Baseline estimate	(2) With mediators
Husband's years since retirement	0.064***	0.054***
	(0.018)	(0.018)
Not homeowner		0.100***
		(0.030)
Financial distress		0.069***
		(0.021)
Husband's income below median		0.011
		(0.026)
Self-reported standard of living		0.162***
		(0.026)
Basic controls	Yes	Yes
Additional controls	Yes	Yes
Family background and childhood controls	Yes	Yes
Observations	3,192	3,192

Notes: See Table 3. All regressions also include the wife's age plus 1 minus maximum guaranteed age as control, as well as dummies for missing values.