The Health Effects of Non-Contributory Pension Expansion: Korean Evidence

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Abstract

Non-contributory social pension has been widely used to provide basic income support for the elderly. Despite its effectiveness in reducing old-age poverty, little is known about the extent to which these welfare gains improve population health. In this study, we exploit a reform to the social pension program in South Korea to estimate the impact of unconditional cash transfer on physical and mental health. This reform represents one of the largest social welfare expansions in South Korean history and provides an excellent natural experiment for isolating exogenous variation in supplementary income. We use data from the Korean Longitudinal Study of Aging and estimate a series of difference-in-differences models. Results show that the expansion is associated with an average of 9.5-15.2% reductions in depressive symptoms, and that this effect operates mainly through increased satisfaction with financial condition and overall quality of life. For other health outcomes (physical health, cognitive health, and functional limitations), we find no evidence that the reform has had a health-preserving impact on beneficiaries. Overall, our findings suggest that public transfers through social pensions improve mental health but do little to improve physical health and functionality.

Keywords: Grip strength, Cognitive functioning, Depression, Public transfer, Non-contributory pension, Elderly poverty

JEL codes: H55, I12, I38, J14

1. Introduction

Aging population poses a great challenge for governments seeking to ensure the well-being of older people. In developing countries, contributory social security systems could not scale up with rapid economic growth and left a large segment of seniors without a fixed income. Low pension coverage is a major threat that undermines the well-being of retired people and places a heavy financial burden on adult children. In response, social pension schemes have been widely adopted in an effort to extend coverage to all members of the elderly population. Social pension is a government-provided cash transfer program in which access to benefits does not require a contributory record. Cash transfer through social pension has proved to be an efficient way to alleviate the depth of poverty as well as its incidence (Barrientos and Lloyd-Sherlock, 2002; Bertranou and Grushka, 2002; Deaton and Case, 1998; Kaushal, 2014).

An important, yet underexplored, hypothesis is whether the benefits of non-contributory pension programs extend to population health. Elders and their family members may use their pension benefits to increase access to medical services and nutritious meals that they need to maintain good health (Duflo 2000; 2003). Increased pension benefit is known to discourage labor supply (Gruber and Wise, 1998), and this effect could reduce exposure to occupational hazards (Gasparini et al., 2010) and job-related stress (Galiani et al., 2016). On the other hand, access to social pension could have impaired health if transfer income increases appetite for nutrition-empty and high-calorie food among low-income households. Theory predicts an inverted U-shape relationship between unearned income and weight (Lakdawalla and Philipson, 2009); as income increases, individuals increase food consumption and beyond a certain threshold, wealthier individuals pursue higher quality foods that have lower calories per dollar. Indeed, evidence from conditional cash transfer programs has reported positive associations of welfare benefits with higher body mass index and blood pressure (Fernald et al., 2008). Moreover, the health benefits of pensions may be contingent on health care accessibility and availability, as well as on health awareness, particularly in rural residents (Lloyd-Sherlock and Agrawal, 2014). Although many developing countries, including South Korea, have greatly expanded their national health insurance system, poor households still pay a significant amount out-of-pocket and face barriers to utilizing those services (Azam, 2018; Ruger and Kim, 2007; Wagstaff et al., 2009).

There have been only a few empirical studies on the health effect of non-contributory pension. In a study of the South African state old age pension, Case (2004) showed that exogenous increase in pension income is positively associated with self-rated health in households that pool income, and that this effect operates through nutritious meals, improved living conditions, and reduced stress regarding day-to-day life. In Colombia, the introduction of unconditional cash transfer program enhanced self-rated health and reduced hospitalizations only in vulnerable older men but not in women (Hessel et al., 2018). Russian evidence by Jensen and Richter (2004) found that the pension loss, associated with the Russian pension crisis, has led to higher mortality rates among pensioners. Galiani et al. (2016) focused on mental health outcomes and found a significant reduction in depressive symptoms among pension beneficiaries in rural Mexico. In Cheng et al. (2018), China's New Rural Pension scheme was positively associated with physical health and cognitive function of the rural elderly but has had no significant impact on self-rated health and psychological well-being. To date, empirical evidence in this strand vastly differ by health outcomes and remains inconclusive.

We exploit social pension reform in South Korea to identify the effects of public transfer. The elderly poverty rate in South Korea is the highest among OECD countries (OECD, 2011), due in large part to the late introduction of the contributory national pension scheme (Lee, 2017) and high levels of labor market informality (United Nations, 2016). To alleviate high elderly poverty, The South Korean government introduced their first social pension scheme in 2008 and doubled monthly benefits in July 2014. The reform in 2014 represents one of the largest social welfare expansions in South Korean history and provides an excellent opportunity to set up a quasi-experimental study. We draw upon this natural experiment to isolate the health effect of public transfer income. Using data from the Korean Longitudinal Study of Aging (KLoSA), the policy effect is estimated by comparing pre vs. post-expansion changes in health outcomes between the age-eligible and non-eligible groups. Respondents' health status is measured by grip strength, cognitive impairment, self-rated health, disability scores, pain-related symptoms, and depression score, which encompass both objective and self-reported health indicators.

Results show that the 2014 expansion of the Korean social pension was associated with 9.5-15.2% improvements in mental health. The correlations were more pronounced for subpopulations with limited retirement income – e.g., females, low-income households, and those without national pension benefits. We also found suggestive evidence that increased financial satisfaction and quality of life are the potential pathways underlying these findings. Other potential mediators, such as access to health care, food expenditures, and leisure consumption, are examined but found to be uncorrelated with the reform. For other measures of health, there is no evidence that the reform has had a positive impact on beneficiaries. The mental health benefits associated with social pension would have played an important role in preventing elderly suicide and reducing social costs accordingly.

This study contributes to the literature along several dimensions. First, we employ a Korean sample in a study of social pension in contrast to most previous studies that examined African and Latin American evidence. This institutional setting offers an opportunity to explore the health effect of social pension in a more industrialized and urbanized context. Second, taking advantage of our rich dataset we adopt extensive measures of health outcomes. We study both objective (e.g., grip strength, cognitive impairment, disability score, and depression score) and subjective health indicators (e.g., self-rated health and pain-related symptoms), as well as the physical and mental component of health. Third, the 2014 social pension expansion in South Korea yields causal inference. The original proposal for the reform was to expand pension coverage to all seniors above age 65. As budgetary concerns arose, the universal coverage clause was removed and coverage was limited to the disadvantaged elderly. Because the original plan should have raised expectations in all age-eligible seniors, our quasi-experimental framework yields estimate that are not confounded by changes in consumption behaviors in anticipation of an upcoming reform.

2. Background: Basic Old-Age Pension in South Korea

The primary source of old-age income in South Korea is the National Pension Scheme (NPS). The NPS was established through the National Pension Act of 1988 to extend pension coverage to the general public. This scheme is a partially-funded contributory pension that provides monthly income at the full benefits age (see Moon, 2009 for more details). Participants in the NPS are required to contribute for at least ten years in order to be eligible for benefits. Due to its late start, much of the current elderly population could not meet the ten-year minimum contribution requirement. In 2012, about 43% of the working-age population was contributing to the NPS, and only 29% of the elderly population was receiving pension benefits (Jones and Urasawa, 2014). As a result, nearly half of the Boomer generation entered retirement without a source of fixed income and fell below the poverty line (Kim et al., 2016).

The basic old-age pension (BOAP) scheme was introduced in January 2008 to complement the NPS and guarantee a stable income in old age. The BOAP is a means-tested, non-contributory pension for the elderly population whose income is below a specified threshold. In its first year, the program covered seniors above age 70 who were in the bottom 60th percentile of income. Since January 2009, eligibility was expanded to include those aged 65 and older who were below the 70th percentile. To qualify for the BOAP, monthly household income plus income-equivalent wealth in 2009 Korean won (KRW) should be less than 680,000 KRW ($633) for a single person and 1,080,000 KRW ($1,005) for a married couple (Shin and Do, 2015).[1] The monthly benefit was set at 5% of the average monthly income of NPS participants, which in 2008 KRW equals 84,000 KRW ($78) for a single person household and 139,000 KRW ($129) for a married couple (Lee et al., in press). This pension amount covered only one-fifth of the minimum cost of living (Moon, 2009).

Beginning in July 2014, the basic pension (BP) scheme replaced the BOAP and doubled the maximum monthly benefits to 168,000 KRW ($156) for singles and 269,000 KRW ($250) for married couples. This reform was part of the new president Park's electoral promise to provide more realistic income support that matches rising minimum cost of living. Though the original promise had been to expand coverage to all senior citizens, budgetary considerations led to retaining the same asset-based eligibility rules as the BOAP (Lee et al., in press). Under the BP scheme, monthly benefit is designed to decrease by the amount of benefits from the NPS and other social welfare programs. For instance, beneficiaries of the National Basic Livelihood Security (NBLS) program who also qualify for the BP receive less benefits from the NBLS after deducting the BP amount. Likewise, pension income from the NPS leads to proportionately smaller benefits from the BP. These changes were mainly driven by the political considerations to maintain wide coverage with limited social spending (Moon, 2009). Despite these changes, this reform was a significant improvement in scope and depth over its predecessor.

[1] As of June 9th 2018, 10,000 KRW is equivalent to $9.31.

There has been mixed evidence on the efficacy of the BOAP. The previous studies have linked several domains of financial well-being, including household income, consumption, and poverty, to the introduction of the BOAP in 2008. Among studies that employed a quasi-experimental design, correlation between the BOAP and increased financial security was found to be either statistically insignificant (Lee and Cho, 2015; Lee and Moon, 2014) or significant but not meaningful in terms of magnitude (Lee and Kwon, 2016; Park and Kim, 2015; Shin and Do, 2015).

A major weakness of existing studies is the inclusion of the 2007-2008 financial crisis during the study period. If low-income households were more severely affected by the recession through early retirement, any positive policy effect would be canceled out by the unobserved business cycle effect and therefore underestimated in empirical analyses. More importantly, the BP reform was accompanied by broader social welfare reform, which included the establishment of Korean long-term care insurance (LTCI). Since disability is more pronounced for the economically disadvantaged group, the income effect attributable to the BP could be masked by the extent to which other policy changes affect well-being outcomes. A recent study by Lee et al. (in press) examined the BP reform in 2014 and found significant reductions in poverty and increases in consumption among beneficiaries. Our study builds upon Lee et al. and examines if the welfare benefits of the BP reform extend to improved health outcomes.

3. Research Design and Methods

3.1 Data Description

The data is drawn from the 2008-2016 waves of the Korean Longitudinal Study of Aging (KLoSA). The KLoSA is a nationally representative longitudinal survey of individuals over 45 years of age and their spouses. The study has been conducted every even years, and collects detailed information on sociodemographic characteristics, health changes, and economic circumstances of elderly Korean residents. The interviews were administered in the second half of each survey year, with the 2014 survey conducted from September through November (after the implementation of the BP reform).[2]

A number of sample selection criteria are applied. First, the sample is limited to individuals aged 50 to 80 years in 2014.[3] This sample selection yields a relatively homogeneous sample in terms of health and economic characteristics and reduces the potential for omitted variable bias. Second, smokers are excluded to avoid the confounding effects of a cigarette tax increase in January 2015. Since those who continued to smoke would have spent more on cigarettes, the increase in the price of cigarettes could have a negative impact on healthcare spending and offset the BP effect. Third, observations with missing values for the outcome variables and covariates are deleted. Lastly, individuals with only one observation during the study periods are dropped to implement pre-post study design. The final sample is an unbalanced panel of 22,772 observations for 4,745 individuals.

3.2 Treatment and Control Group

The means-tested structure of public transfer adds additional complexity to the determination of treatment and control groups. Potential beneficiaries around the income threshold have an incentive to spend down their assets to become eligible for public transfer. Indeed, research has shown a strong positive association between means testing and consumption expenditures (Powers, 1998). If a treatment group is determined by beneficiary status at the time of reform, it is difficult to distinguish whether changes in leisure consumption are attributable to BP reform or the effect of means testing. Alternatively, one can use the age threshold (age $\geq$ 65) to isolate age-eligible individuals. Change in age is strictly exogenous and cannot be manipulated by respondents unless their report of age is systematically incorrect. This approach identifies "intent-to-treat" estimates and yields a lower bound for the policy effect.

The treatment group consists of households where the eldest member is aged 65 years or older. Respondents are assigned to the treatment group if (a) they are age-eligible singles or (b) they are married and their eldest household member is age-eligible. Likewise, the control group

[2] Details on the sampling procedure, questionnaire contents, and fieldwork methodology are reported at the KLoSA website (http://survey.keis.or.kr).
[3] Our regression estimates are robust to the choice of age bands. We re-fit the regressions with narrower and wider age bands and present estimates on Table A1.

includes (a) singles who are age-ineligible and (b) those whose eldest household member is age-ineligible. This classification builds upon the assumption that spouses pool their household income and wealth. In analyses not shown, we found that the regression results remain unchanged in terms of statistical inference when the treatment assignment does not consider the eligibility of other household members.

3.3 Measures of Physical and Mental Health

Grip strength

Grip strength is known to signal the risk of developing chronic disease and mortality in old age (Alfaro-Acha et al., 2006; Fujita et al., 1995; Metter et al., 2002; Silventoinen et al., 2009). It has been used to proxy for individual's general health condition (Christensen et al., 2001) and linked to various measures of economic behaviors (Decker and Schmitz, 2016; Kalwij and Vermeulen, 2008). In the KLoSA, grip strength is measured using a handheld dynamometer – where respondents are asked to press a lever as hard as they can, and recorded in kilograms on a scale of 0-100. Of a total four measurements (two measurements for each hand), this study uses the largest recorded value. This measure has advantages over the self-reported measures in that its values are measured on a clearly defined scale and independent to reporting style (Decker and Schmitz, 2016).

Cognitive functioning

Cognitive abilities are assessed by the Korean Mini-Mental State Examination (K-MMSE). The MMSE is a brief screening test for dementia and has been used to evaluate the severity and progression of cognitive impairment (Folstein et al., 1975). The K-MMSE includes 19 items that assess multiple domains of cognitive functioning, such as orientation to time (5 points), orientation to place (5 points), attention and calculation (5 points), registration of three words (3 points), recall of three words (3 points) language (8 points), and visual construction (1 point). The total K-MMSE score ranges from 0 to 30 point, with a high score representing better global cognitive performance.

Self-rated health

Self-assessed health is a global measure of health that captures a full range of possible diseases and limitations. This measure is based on a survey question, "*How would you rate your health condition?*".[4] Answers are recorded so that higher values represented better overall health, and range from 1 (representing poor health) to 5 (representing excellent health). Close to 30% of the sample report poor to fair health with the majority of individuals reporting good health condition. Though subjective, research has repeatedly found it to be correlated with objective measures of health (Idler and Benyamini, 1997; DeSalvo et al., 2006; Phillips et al., 2010).

Mental health

Mental health is measured by the Center for Epidemiologic Studies Depression (CES-D) scale. The CES-D is a reliable measure of depression and anxiety that is associated with physician diagnoses and psychiatric treatment (Turvey et al., 1999). The test includes questions on negative feelings (like having the blues, experiencing life as a failure, feeling lonely or sad, crying), on positive thoughts (as being hopeful about the future, feeling happy, enjoying life), on somatic activity (like losing appetite, suffering from a restless sleep, talking less), and on social contacts (experiencing other persons as unfriendly, and thinking that people dislike the respondent). The KLoSA used CES-D 10 and yielded a score of depression on a 0-30 scale.

Activities of daily living (ADL) and instrumental activities of daily living (IADL)

ADLs measure limitations in the following daily activities and functional mobility: bathing, dressing, toilet use, transferring (in and out of a bed or chair), urine and bowel continence, and eating. The ADL score counts the number of functional limitations and ranges from 0 to 7, with a higher score representing more disabilities. IADLs measure the following skills to support an independent lifestyle: shopping, cooking, doing common and less common household chores, doing administrative tasks, managing medication, moving around in all of the rooms of a floor, leaving home, using transportation, finding your way, using a telephone. Similarly, the IADL score is defined as the number of limitations in these activities and scaled on 0-10.

[4] Survey questions are translated from Korean by authors.

Chronic pain

Chronic pain is constructed in two steps. First, respondents are asked if they have any pain on head, shoulder, arm, wrist, finger, chest, abdomen, waist, heap, leg, knee, ankle, and toe, and second, they are asked if any of these symptoms limit their daily activities. Persons who answered yes to this summary question is coded one for chronic pain, and those who reported no is coded zero.

3.4 Estimation Strategy

The health effect of the BP is estimated by comparing changes in health outcomes of the treatment group to those of the control group before and after the reform in 2014. We estimate a linear difference-in-differences (DD) model in the following form,

$$y_{i,t} = \beta_0 + \beta_1 T_{i,t} + \beta_2 P_t + \beta_3 (T_{i,t} \cdot P_t) + X'_{i,t}\Psi + \tau_t + \varepsilon_{i,t}, \quad (1)$$

where i denotes individual respondent; $t \in \{2008, 2010, 2012, 2014, 2016\}$ indexes survey years; $y_{i,t}$ is the measures of physical and mental health; $T_{i,t}$ is a treatment indicator for the eldest household member being 65 years or older in year t; P_t is a dummy for post-intervention periods; τ_t is time fixed effects; $\varepsilon_{i,t}$ an error term; and β and Ψ are parameters to be estimated. The coefficient β_1 represents differences in health outcomes between the treatment and control groups before 2014, and the coefficient β_2 captures differences in health before and after 2014 among the untreated individuals. The treatment effect of the reform is identified by β_3 on the interaction term between $T_{i,t}$ and P_t.

Regressions are estimated by the pooled OLS with standard errors clustered at both individual and age levels (Cameron et al., 2012). While individual fixed effects model can be considered to account for unobserved individual heterogeneity, such approach identifies policy effect using only changes in health outcomes between age 64 and 65, and fails to capture improvements in health for those who were already older than age 65 when the BP was expanded. This would lead to a significantly impoverished policy effect if a majority of the respondents in our sample were above age 65 prior to 2014. As robustness checks, the baseline regression is estimated with alternative standard errors clustered at the combination of individual, household, age, year, and sampling stratum levels (Table A.2). The standard error estimates fluctuate across specifications but remain qualitatively unchanged in terms of statistical inference.

The covariate vector X_{ijt} includes age, gender, education background, marital status, number of children, private health insurance ownership, entitlement to national pension and other public welfare programs, labor force participation, home ownership, household income, total net worth, urban/rural indicator, cohort fixed effects, month-of-survey fixed effects, and quarterly real GDP. Age effect is modeled by age and its squared term to allow physical functioning decline at an increasing rate. Private health insurance ownership takes one if respondents own private health insurance and zero otherwise. Household income is the total income received by both spouses in the last year, not including benefits from the BP. Total net worth is defined as the sum of checking and savings accounts, bonds, stocks, retirement accounts, vehicles, home equity, other real estates, and business equity, minus debts and mortgages. Income and wealth measures are log-transformed by $ln(x + .01)$ for nonnegative x and $-ln(-x)$ for negative x. We also include (quarterly) real GDP to net out differential effects of the business cycle on the treatment and control groups. This variable is obtained from the Federal Reserve Economic Data at the Federal Reserve Bank of St. Louis, and match-merged with the primary data according to survey dates.

Our DD estimate is valid only if there is no discrepancy in the underlying trend in health outcomes unrelated to the BP expansion. That is, changes in health over time should be reasonably similar for both treatment and control groups in the absence of BP expansion. A formal test of parallel trends is to examine the joint significance of interaction terms between the treatment condition and year dummies in the pre-intervention periods. To get the estimates for such tests, we re-fit Eq. (1) in the following form,

$$y_{i,t} = \gamma_0 + \gamma_1 T_{i,t} + \sum_{t=2008}^{2016} \gamma_{2,t} \cdot \tau_t + \sum_{t=2008}^{2016} \gamma_{3,t}(T_{i,t} \cdot \tau_t) + X'_{i,t}\Phi + \varepsilon_{i,t}, \qquad (2)$$

If estimates on the interaction terms involving pre-2014 dummies are jointly significant, we would have to conclude that unobserved factors or events contributed to a trend break in health. If such estimates are not significant, it is reasonable to assume that the parallel trend would have continued for both groups when there was no BP expansion.

4. Results

Table 1 presents summary statistics for the full sample, and separately for the treatment group (respondents whose eldest household member is 65 years or older) and control group (respondents whose eldest household member is 64 years or younger). On average, respondents are about 62.7 years old; females make up 60% of the sample; 84% are married; and 20% are collecting benefits from the national pension scheme. The average household earned 32,190,000 KRW ($29,969) per year, and accumulated 232,420,000 KRW of net worth ($216,383). The average of grip strength, MMSE score, self-rated health, and pain outcome remains unchanged after the intervention, while ADL and IADL scores are significantly higher for the post periods. The CES-D score generally declines with a larger drop for a treatment group. In terms of covariates, we find a significant reduction in the share of social welfare beneficiaries in the treatment group, which shows the substitution between BP and other social welfare benefits. As discussed, this result confirms our prediction that beneficiaries of social welfare programs could have switched to the BP for larger monthly benefits. Overall, pre-post differences in the covariates are negligible or exhibit similar changes over time between the two groups except for pension outcomes and mental health.

[Insert Table 1 about here]

Table 2 presents OLS coefficient estimates of the determinants of health outcomes. For the sake of brevity, the coefficients for urban/rural indicator, cohort fixed effects, month-of-survey fixed effect, and quarterly GDP are omitted from the table. All regressions are weighted using sampling weights in the KLoSA, and robust standard errors are clustered at the individual and age levels. The coefficient estimates for the interaction between a treatment indicator and a post-2014 dummy identify the treatment effect of the BP reform.

In columns (1) through (6), the DD term is not significantly associated with the health outcomes. The coefficient estimates are very small in magnitude and carry an unexpected sign. Unlike the past research, we find no evidence that physical health was improved with the exogenous increase in public transfers. In column (7), the coefficient estimate for the DD term is positive and exceeds its standard error by more than a factor of three. Quantitatively, the BP expansion is associated with 0.638 points decrease in mean CES-D score among treated persons relative to the untreated persons. Compared to the pre-2014 mean CES-D score of 6.69, this represents approximately 9.5% decrease in depressive symptoms.

The coefficient estimates for the control variables generally carry the expected sign. Health outcomes decline at an increasing rate with age, and are positively correlated with education, income, wealth, and health insurance ownership. Depressive symptoms, in particular, are much lower for the married couples, those with children, and those who live in their own house. Retirement is the single most important predictor of physical and mental health decline.

[Insert Table 2 about here]

Our definition of treatment group consists of beneficiaries and age-eligible individuals who could not pass the income test. Since non-beneficiaries were not exposed to the reform, our DD estimates in Table 2 are supposed to be under-estimated. To recover the true effect size, we estimate the local average treatment effect (LATE) using the instrumental variable (IV) technique. Our strategy is to use average monthly public transfer income as an endogenous regressor in the second-stage regression, and use a DD term as an instrumental variable in the first-stage regression. The KLoSA provides information on average monthly benefits from unemployment compensation, industrial accident compensation, national basic livelihood security, Veterans benefits program, basic old-age pension, and other social welfare programs. In our cleaned sample, the mean of household public transfer income increased from 226,528 KRW in 2012 to 342,258 KRW in 2014. The related estimation results are presented in Table 3. The coefficient estimates are quite similar to the previous table; the reform was significantly associated with the CES-D score but not with other domains of health. Evaluated at the mean increase in average public transfer between surveys, the reform explained 1.01 points decrease in CES-D score. Using the pre-2014 mean of the CES-D score, we recover an average of 15.2% decline in depressive symptoms associated with the reform.

[Insert Table 3 about here]

Table 4 presents the coefficient estimates for Eq. (2) with 2012 as a reference year. Across all seven columns, the coefficient estimates for $\gamma_{3,1}$ and $\gamma_{3,2}$ are not different from zero at the 5% significance level. This shows that there is little difference in health outcomes between the treatment and control groups in 2008 and 2010 relative to 2012 level, and thus the parallel trends assumption holds. In column (7), the coefficient estimates for $\gamma_{3,3}$ and $\gamma_{3,4}$ indicates that the reform reduced depressive symptoms for the treated persons by 0.324 points (4.8% over the pre-2014 mean) in 2014 and by 0.842 (12.6% over the pre-2014 mean) in 2016, relative to the ineligible persons. This larger policy effect in subsequent years could be attributed to a gradual increase in enrollment after the reform.

[Insert Table 4 about here]

Our DD model is valid only if unobserved factors arose during or after the reform have equal impact on the treatment and control groups. Otherwise, the treatment effect could be driven by changes in factors unrelated to the BP that were occurred at the same time with the reform. While our regressions include a wide array of covariates, those variables do not account for the impact of potential confounders that differ by treatment status or time periods. This threat to identification is evaluated in Table 5. Our empirical strategy is to include additional interactions between a treatment indicator (or, a post dummy) and covariates to account for deviations from a trend due to the differential impact of covariates on treated and untreated individuals (or, on observations before and after the expansion). In column (1), we include an interaction between a treatment indicator and a linear year variable to allow for differential trends in health outcomes for the treatment group relative to the control group. Column (2) includes all covariates in column (1) plus interactions between a treatment dummy and marital status indicator, health insurance ownership, dummies for national pension and social welfare receipt, labor force participation, household income, home ownership, total net worth, rural/urban indicator, and quarterly GDP. Column (3) includes all covariates in column (1) plus interactions between a post dummy and marital status indicator, health insurance ownership, dummies for national pension and social welfare receipt, labor force participation, household income, home ownership, total net worth, rural/urban indicator, and quarterly GDP. Column (4) controls for all two-way interactions and a basic set of covariates. Across all columns, the negative impact of the BP expansion on depression remains unchanged. Although the magnitude of the DD estimates varies significantly across models, the null hypothesis associated with the DD coefficient is rejected at the 5% level. For other health outcomes, the coefficient estimates are not consistently significant across specifications.

[Insert Table 5 about here]

The remaining concern is that the results presented so far could be attributed to unobserved factors that have differential impacts on beneficiaries relative to younger non-participants. If this claim holds, our DD model is merely picking up underlying changes in health that differ systematically by age. We conduct a series of placebo tests to nullify this argument (Table 6). For this test, we identify households whose eldest spouse is aged 50-62 in 2014 and use a variety of age cut-offs to create a fake treatment group.[5] Because the reform had either no impact or equal impact on the two age groups, the DD estimates should not be statistically significant unless unobserved factors confound the results. The age cut-offs for treatment assignment is presented in column headings. Overall, the DD estimates from these placebo tests are statistically insignificant and in many cases have alternating signs. Although there are a few estimates that are significant at the 5 or 10% level, there are no cases in which the estimates are consistently negative-significant or positive-significant across age cut-offs. Thus, it seems unlikely that differential trends by age are driving our main results.

[Insert Table 6 about here]

Table 7 explores the potential mechanisms through which social pension leads to reduced depressive symptoms. We investigate five domains – healthcare access, food consumption, leisure consumption, subjective well-being, and labor force participation – that are likely to improve mental health. Specifically, the dependent variables include out-of-pocket expenditures for medical services, out-of-pocket expenditures for prescription drugs, spending on food-at-home and food-away-from-home, leisure expenditures, financial satisfaction, life satisfaction, and employment status. The consumption variables are defined as the total household spending over the last 12 months from the date of survey. The well-being questions run as follows: *How happy are you at present with your (financial situation / overall quality of life?*, and responses ranges from 0 to 100 in the multiples of 10. Higher values correspond to higher levels of perceived satisfaction. Employment outcome is modeled separately by whether respondents work for salary or work without pay.

The regression models correspond to Eq. (1), and control for a basic set of covariates. In columns (6) and (8), the coefficient estimates for the DD term are different from zero at the 5% level, and in column (7) the coefficient estimate is marginally significant at the 10% level. Column (8) suggests that the expansion reduced labor supply among pensioners by 3.9%. As retirement is positively associated with a CES-D score in Table 2, this estimate shows that retirement is not a pathway that improves mental health. Columns (6) and (7) shows a 2.6 and

[5] The age band for sample selection excludes those aged 63 or 64 in 2014, as they become age-eligible in 2016 survey.

1.7% increase in financial satisfaction and life satisfaction from the sample means in the pre-periods. These well-being indicators are negatively associated with a CES-D score, and are the most likely pathways that mediate the mental health benefits of the reform.

[Insert Table 7 about here]

Table 8 shows the effect of the BP expansion on mental health by gender, entitlement to NPS, household income, and marital status. We only present regression results for a CES-D score as other health outcomes are uncorrelated with the reform in the main regressions. The estimates show that a majority of the mental health benefit accrues disproportionately to disadvantaged subpopulations who are more likely to benefit from social pension expansion. In columns (1) through (4), the estimates for the DD term are different from zero at the 5% level for females and those not receiving benefits from the NPS. By household income, we find that the effect of the BP expansion is more pronounced for households with below-mean household income. The last two columns show that the BP effect is statistically significant only for married couples.

[Insert Table 8 about here]

5. Conclusion

We found robust evidence that the 2014 expansion of the BP improved mental health among beneficiaries. Using a difference-in-differences approach, we estimated an average of 9.5-15.2% decrease in depressive symptoms attributable to the reform. The depression-reducing impact of the BP was sustained through 2016 and much larger in magnitude in subsequent years. We could not, in contrast, find any significant changes in grip strength, cognitive functioning, self-rated health, disability scores, and pain-related symptoms. The estimated policy effects were statistically insignificant and not robust to different estimation methods and modeling assumptions. We also found that improvements in financial satisfaction and life satisfaction are the likely pathways that lead to lower depressive symptoms. Further analyses highlighted that the mental health benefits accrued to females, those without contributory pension benefits, households with below-mean income, and married couples.

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Tables

Table 1. Summary statistics

	Age-ineligible group (Eldest HH member aged 50-64)		Age-eligible group (Eldest HH member aged 65-80)		
	Pre	Post	Pre	Post	Full sample
Health outcomes:					
Grip strength†	26.5	27.5	23.2	24.1	25.4
MMSE score (0-30)	27.5	27.8	25.2	25.2	26.5
Self-rated health (1-5)	3.37	3.06	2.86	2.46	3.00
ADL (0-7)	0.02	0.03	0.10	0.15	0.07
IADL (0-10)	0.15	0.15	0.36	0.49	0.27
Pain limits daily activity (0,1)	0.13	0.11	0.29	0.27	0.19
CES-D score (0-30)	5.85	5.32	7.62	6.68	6.35
Covariates:					
Age	56.2	58.5	68.4	71.0	62.7
Female (0,1)	0.59	0.60	0.61	0.61	0.60
No high school degree (0,1)	0.43	0.32	0.73	0.67	0.53
High school graduate (0,1)	0.57	0.68	0.27	0.33	0.47
Married (0,1)	0.90	0.89	0.79	0.76	0.84
Number of children	2.30	2.11	3.22	2.96	2.62
Own private health insurance (0,1)	0.55	0.64	0.18	0.23	0.41
National pension beneficiary (0,1)	0.09	0.09	0.29	0.37	0.20
Social welfare beneficiary (0,1)	0.03	0.02	0.25	0.07	0.09
Retired (0,1)	0.47	0.44	0.74	0.75	0.59
Household income (yearly)‡	3,909	4,438	2,115	2,206	3,219
Own home (0,1)	0.81	0.80	0.84	0.83	0.82
Total net worth‡	27,723	21,223	23,657	17,016	23,242
Observations	7,645	3,035	6,311	5,781	22,772
Persons	2,958	1,790	2,512	3,215	4,745

Notes: Mean values are calculated using weights provided in the KLoSA. Minimum and maximum are presented in parentheses if possible values are bounded. †Grip strength is measured in Kilogram. ‡All money figures are adjusted to the 2016 Korean won using the Korean Consumer Price Index and expressed in 10,000 won. Social welfare benefits consider benefits received from unemployment compensation, industrial accident compensation, national basic livelihood security program, Veterans benefits, and other social welfare programs.

Table 2. Regressions for health outcomes

Outcome:	Grip (1)	MMSE (2)	SR health (3)	ADL (4)	IADL (5)	Pain (6)	CES-D (7)
$I(Age \geq 65)$	0.192	0.070	0.022	-0.017	-0.080**	-0.004	0.322**
	(0.171)	(0.095)	(0.022)	(0.016)	(0.039)	(0.013)	(0.161)
Post	3.167	-0.832	-1.482***	0.662*	0.680	-0.091	3.869**
	(3.840)	(2.072)	(0.406)	(0.361)	(0.643)	(0.184)	(1.865)
$\boldsymbol{I(Age \geq 65)} \times$ **Post**	**-0.081**	**0.075**	**-0.028**	**0.001**	**0.009**	**-0.002**	**-0.638*** **
	(0.226)	**(0.125)**	**(0.030)**	**(0.020)**	**(0.047)**	**(0.014)**	**(0.193)**
Age	0.234*	0.382***	-0.026	-0.059***	-0.155***	-0.010	-0.171
	(0.125)	(0.077)	(0.016)	(0.015)	(0.033)	(0.007)	(0.120)
Age squared	-0.004***	-0.004***	0.001	0.001***	0.001***	0.001*	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Female	-11.61***	-0.272***	-0.032	-0.086***	-0.382***	0.075***	-0.021
	(0.214)	(0.102)	(0.026)	(0.019)	(0.043)	(0.011)	(0.141)
Middle school graduate	0.586***	1.270***	0.137***	-0.003	-0.060	-0.077***	-0.531***
	(0.150)	(0.127)	(0.025)	(0.017)	(0.041)	(0.012)	(0.174)
High school graduate	0.792***	1.746***	0.279***	-0.019	-0.124***	-0.103***	-0.959***
	(0.161)	(0.123)	(0.027)	(0.019)	(0.046)	(0.011)	(0.168)
Married	-0.156	0.313**	-0.009	0.037**	0.068*	-0.034**	-1.030***
	(0.173)	(0.146)	(0.025)	(0.017)	(0.039)	(0.014)	(0.186)
Number of children	-0.001	-0.194***	0.016*	-0.003	-0.025	0.002	-0.154***
	(0.062)	(0.051)	(0.008)	(0.007)	(0.017)	(0.005)	(0.056)
Own private health insurance	0.420***	0.496***	0.056***	-0.029***	-0.075***	-0.003	-0.815***
	(0.133)	(0.073)	(0.018)	(0.009)	(0.022)	(0.007)	(0.115)
National pension beneficiary	-0.110	0.051	-0.010	0.016	-0.019	0.007	-0.151
	(0.175)	(0.111)	(0.020)	(0.020)	(0.037)	(0.012)	(0.150)
Social welfare beneficiary	-0.088	-0.311	-0.181***	0.018	0.126**	0.079***	-0.020
	(0.219)	(0.208)	(0.027)	(0.031)	(0.064)	(0.016)	(0.213)
Retired	-0.824***	-0.352***	-0.217***	0.100***	0.270***	0.040***	1.002***
	(0.125)	(0.099)	(0.017)	(0.016)	(0.036)	(0.007)	(0.122)
Log(HH income)	0.452***	0.263***	0.118***	-0.009	-0.022	-0.034***	-0.306***
	(0.069)	(0.052)	(0.010)	(0.010)	(0.020)	(0.004)	(0.070)
Own home	-0.027	0.087	0.150***	-0.012	-0.026	-0.062***	-0.761***
	(0.156)	(0.120)	(0.026)	(0.015)	(0.035)	(0.011)	(0.187)
Log(Total net worth)	0.037***	0.043***	0.009***	-0.008***	-0.018***	-0.003***	-0.025**
	(0.013)	(0.012)	(0.002)	(0.002)	(0.004)	(0.001)	(0.011)
Observations	20,435	21,903	22,771	22,772	22,772	22,772	22,685
R^2	0.572	0.222	0.306	0.026	0.058	0.123	0.096

Notes: Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for urban/rural dummy, cohort fixed effects, month and year-of-survey fixed effects, and quarterly GDP. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 3. Regressions for health outcomes, IV estimates

Outcome:	Grip	MMSE	SR health	ADL	IADL	Pain	CES-D
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HH Social welfare benefits	**0.001**	**0.019**	**-0.004**	**-0.001**	**-0.002**	**-0.001**	**-0.120*****
	(0.038)	**(0.022)**	**(0.005)**	**(0.003)**	**(0.008)**	**(0.002)**	**(0.036)**
Observations	16,565	17,581	18,261	18,261	18,261	18,261	18,177
Underidentification†	62.7	73.9	78.8	78.8	78.8	78.8	78.7
Weak instrument‡	118.6	131.5	141.7	141.7	141.7	141.7	141.6
Stock-Yogo 10% critical value	16.4	16.4	16.4	16.4	16.4	16.4	16.4

Notes: Robust standard errors in parentheses are clustered at the individual levels. Regressions control for a full set of covariates. Regression estimates are based on the 2008, 2010, 2012, and 2016 wave of the KLoSA. †Kleibergen-Paap rank LM-statistic tests the null hypothesis of underidentification. ‡Cragg-Donald Wald F statistic tests the null hypothesis of weak instruments. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 4. Regressions for health outcomes, with year fixed effects

Outcome:	Grip	MMSE	SR health	ADL	IADL	Pain	CES-D
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$I(Age \geq 65)$	0.423*	0.127	0.011	0.001	-0.070	-0.005	0.232
	(0.219)	(0.113)	(0.023)	(0.019)	(0.046)	(0.013)	(0.162)
Year 2008	-0.865	0.587	0.422**	-0.348*	-0.370	0.039	-2.759***
	(1.971)	(1.056)	(0.213)	(0.191)	(0.340)	(0.099)	(0.918)
Year 2010	-1.047	-0.075	0.155**	-0.109*	-0.105	0.015	-0.677*
	(0.673)	(0.390)	(0.069)	(0.065)	(0.120)	(0.035)	(0.348)
Year 2014	1.267	-0.162	-0.165	0.149*	0.153	-0.051	0.517
	(0.976)	(0.513)	(0.102)	(0.085)	(0.151)	(0.045)	(0.491)
Year 2016	2.090	-0.289	-1.035***	0.305*	0.326	-0.054	1.345
	(1.871)	(1.019)	(0.193)	(0.167)	(0.293)	(0.086)	(0.945)
$\gamma_{3,1}$: $I(Age \geq 65) \times$ Year 2008	-0.378*	-0.049	0.057*	-0.037	-0.005	-0.011	0.133
	(0.210)	(0.091)	(0.033)	(0.024)	(0.041)	(0.015)	(0.173)
$\gamma_{3,2}$: $I(Age \geq 65) \times$ Year 2010	-0.393*	-0.134	-0.011	-0.025	-0.023	0.012	0.182
	(0.215)	(0.098)	(0.034)	(0.017)	(0.034)	(0.010)	(0.145)
$\gamma_{3,3}$: $\boldsymbol{I(Age \geq 65) \times}$ **Year 2014**	**-0.366**	**-0.019**	**-0.021**	**-0.008**	**0.021**	**0.003**	**-0.324****
	(0.251)	**(0.102)**	**(0.023)**	**(0.018)**	**(0.039)**	**(0.015)**	**(0.135)**
$\gamma_{3,4}$: $\boldsymbol{I(Age \geq 65) \times}$ **Year 2016**	**-0.169**	**0.072**	**-0.024**	**-0.022**	**-0.026**	**-0.004**	**-0.842*****
	(0.318)	**(0.176)**	**(0.039)**	**(0.031)**	**(0.068)**	**(0.016)**	**(0.230)**
Observations	20,435	21,903	22,771	22,772	22,772	22,772	22,685

Notes: Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 5. Robustness checks

Covariates:	Baseline + Linear time trend × $I(Age \geq 65)$	Baseline + Linear time trend × $I(Age \geq 65)$ + Interactions with $I(Age \geq 65)$	Baseline + Linear time trend × $I(Age \geq 65)$ + Interactions with Post	Baseline + Linear time trend × $I(Age \geq 65)$ + Interactions with $I(Age \geq 65)$ + Interactions with Post
	(1)	(2)	(3)	(4)
Grip	-0.507*	-0.635**	-0.234	-0.345
	(0.292)	(0.269)	(0.294)	(0.285)
MMSE	-0.019	0.212*	0.119	0.309*
	(0.100)	(0.123)	(0.153)	(0.159)
SR health	0.016	0.033	-0.034	-0.010
	(0.027)	(0.031)	(0.029)	(0.034)
ADL	-0.024	-0.063**	-0.032	-0.071**
	(0.025)	(0.028)	(0.025)	(0.027)
IADL	0.024	-0.054	0.009	-0.069
	(0.038)	(0.045)	(0.049)	(0.052)
Pain	-0.006	-0.013	0.017	0.012
	(0.018)	(0.020)	(0.025)	(0.027)
CES-D	-0.273**	-0.366***	-0.338**	-0.450**
	(0.122)	(0.135)	(0.158)	(0.181)

Notes: Table presents estimates for an interaction between $I(Age \geq 65)$ and Post. Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 6. Placebo tests

Sample:	Eldest HH member aged 50 and 62 in 2014						
Age threshold:	$I(Age \geq 54)$	$I(Age \geq 55)$	$I(Age \geq 56)$	$I(Age \geq 57)$	$I(Age \geq 58)$	$I(Age \geq 59)$	$I(Age \geq 60)$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Grip	0.115	-0.306	0.053	0.394	0.259	-0.343	-0.757*
	(0.279)	(0.254)	(0.301)	(0.291)	(0.368)	(0.487)	(0.426)
MMSE	0.071	-0.180	-0.016	0.021	0.191	-0.160	0.071
	(0.124)	(0.181)	(0.164)	(0.188)	(0.179)	(0.136)	(0.124)
SR health	0.010	0.062**	0.028	-0.051	-0.011	-0.014	0.010
	(0.050)	(0.029)	(0.030)	(0.039)	(0.045)	(0.039)	(0.050)
ADL	0.028	0.007	0.022**	0.023**	0.013	0.006	0.028
	(0.021)	(0.013)	(0.011)	(0.010)	(0.014)	(0.024)	(0.021)
IADL	0.074	0.004	0.017	0.052**	0.050	0.047	0.074
	(0.085)	(0.035)	(0.029)	(0.020)	(0.043)	(0.040)	(0.085)
Pain	-0.025	-0.026**	-0.010	-0.007	-0.023	-0.013	-0.025
	(0.019)	(0.012)	(0.015)	(0.012)	(0.016)	(0.020)	(0.019)
CES-D	0.143	-0.070	0.259	-0.273	-0.187	0.356	0.143
	(0.311)	(0.215)	(0.268)	(0.375)	(0.394)	(0.272)	(0.311)

Notes: Table presents estimates for an interaction between $I(Age \geq 65)$ and Post. The sample is limited to persons whose eldest household member is aged 50 and 62 in 2014. Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 7. Potential mechanisms

Outcome:	Log (medical OOP)	Log (drug OOP)	Log (food spending)	Log (leisure spending)	Exercise	Financial satisfaction	Life satisfaction	Work for pay	Work without pay
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$I(Age \geq 65)$	-0.095	-0.014	0.022	0.161	0.033*	-0.288	-0.647	0.020	0.014
	(0.080)	(0.142)	(0.015)	(0.137)	(0.018)	(0.517)	(0.518)	(0.016)	(0.009)
Post	-0.316*	-0.378*	-0.004	1.112***	-0.655***	7.256	-6.155	0.186	-0.066
	(0.182)	(0.217)	(0.024)	(0.296)	(0.250)	(8.796)	(6.937)	(0.176)	(0.100)
$\boldsymbol{I(Age \geq 65)}$ **× Post**	**0.092**	**0.241**	**-0.018**	**-0.122**	**0.025**	**1.408****	**1.065***	**-0.039****	**0.009**
	(0.136)	**(0.172)**	**(0.029)**	**(0.288)**	**(0.017)**	**(0.646)**	**(0.557)**	**(0.016)**	**(0.008)**
Observations	18,261	17,953	11,789	18,261	22,772	22,769	22,767	22,772	22,772

Notes: Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 8. Regressions by group status

Outcome:	CES-D score							
	By gender		By HH income		By national pension		By marital status	
	Female	Male	< mean	≥ mean	Not receiving benefits	Receiving benefits	Not married	Married
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I(Age \geq 65)$	0.475**	-0.009	0.230	0.252	0.358**	0.494	0.647	0.204
	(0.210)	(0.244)	(0.209)	(0.254)	(0.167)	(0.369)	(0.483)	(0.171)
Post	1.972	6.247*	8.052***	0.292	3.741*	1.713	2.007	4.314*
	(2.654)	(3.243)	(2.708)	(2.726)	(2.224)	(4.670)	(6.791)	(2.252)
$\boldsymbol{I(Age \geq 65)} \times$ **Post**	**-0.789*** **	**-0.391**	**-0.712** **	**-0.590** **	**-0.799*** **	**-0.065**	**-1.180* **	**-0.534*** **
	(0.245)	**(0.277)**	**(0.297)**	**(0.272)**	**(0.213)**	**(0.382)**	**(0.671)**	**(0.181)**
Observations	14,122	8,563	11,324	11,361	17,940	4,745	3,780	18,905

Notes: Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Appendix

Table A1. Regressions with different sample selection

Outcome:	Grip (1)	MMSE (2)	SR health (3)	ADL (4)	IADL (5)	Pain (6)	CES-D (7)
Panel A: eldest HH member aged 60 and 70 in 2014							
$I(Age \geq 65)$	0.345*	0.123	0.019	0.005	-0.022	-0.002	0.428***
	(0.183)	(0.105)	(0.026)	(0.013)	(0.037)	(0.013)	(0.156)
Post	-1.013	-0.943	-2.222***	1.036*	1.767	-0.155	1.705
	(6.309)	(2.477)	(0.733)	(0.576)	(1.170)	(0.286)	(2.503)
$\boldsymbol{I(Age \geq 65)}$ **× Post**	**-0.082**	**0.011**	**0.044**	**0.021**	**0.065**	**-0.024**	**-0.518****
	(0.355)	**(0.203)**	**(0.031)**	**(0.020)**	**(0.060)**	**(0.021)**	**(0.247)**
Observations	8,391	8,860	9,151	9,151	9,151	9,151	9,126
Panel B: eldest HH member aged 55 and 75 in 2014							
$I(Age \geq 65)$	0.167	0.066	0.017	0.005	-0.041	-0.008	0.327**
	(0.164)	(0.104)	(0.024)	(0.018)	(0.040)	(0.014)	(0.153)
Post	3.299	-0.064	-1.376***	0.759*	0.815	-0.236	2.389
	(4.335)	(2.170)	(0.506)	(0.424)	(0.777)	(0.197)	(2.040)
$\boldsymbol{I(Age \geq 65)}$ **× Post**	**-0.188**	**-0.038**	**-0.029**	**-0.013**	**0.004**	**0.010**	**-0.428****
	(0.249)	**(0.140)**	**(0.033)**	**(0.022)**	**(0.049)**	**(0.017)**	**(0.215)**
Observations	15,841	16,794	17,422	17,423	17,423	17,423	17,347
Panel C: eldest HH member aged 45 and 85 in 2014							
$I(Age \geq 65)$	0.169	0.117	0.014	-0.010	-0.062	-0.007	0.297*
	(0.169)	(0.103)	(0.021)	(0.020)	(0.049)	(0.013)	(0.154)
Post	2.414	-1.431	-1.376***	0.901**	1.040*	-0.089	3.648**
	(3.806)	(2.034)	(0.385)	(0.356)	(0.627)	(0.178)	(1.729)
$\boldsymbol{I(Age \geq 65)}$ **× Post**	**-0.067**	**0.109**	**-0.019**	**-0.006**	**-0.015**	**-0.008**	**-0.691*****
	(0.215)	**(0.127)**	**(0.028)**	**(0.021)**	**(0.046)**	**(0.013)**	**(0.187)**
Observations	22,305	24,201	25,203	25,204	25,204	25,204	25,108

Notes: Robust standard errors in parentheses are clustered at the individual and age levels. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A2. Robustness of standard error estimates

Outcome:	Grip (1)	MMSE (2)	SR health (3)	ADL (4)	IADL (5)	Pain (6)	CES-D (7)
Baseline estimates	-0.081	0.075	-0.028	0.001	0.009	-0.002	-0.638***
Clustered at the household and age levels	(0.226)	(0.125)	(0.030)	(0.020)	(0.047)	(0.014)	(0.193)
Clustered at the individual levels	(0.224)	(0.136)	(0.030)	(0.022)	(0.050)	(0.014)	(0.195)***
Clustered at the household levels	(0.236)	(0.145)	(0.031)	(0.022)	(0.050)	(0.014)	(0.216)***
Clustered at the age levels	(0.216)	(0.112)	(0.027)	(0.018)	(0.038)	(0.013)	(0.175)***
Clustered at the PSU levels	(0.191)	(0.154)	(0.022)	(0.019)	(0.029)	(0.013)	(0.189)***
Clustered at the individual and PSU levels	(0.193)	(0.154)	(0.023)	(0.020)	(0.034)	(0.013)	(0.192)***
Clustered at the individual and year levels	(0.136)	(0.104)	(0.023)	(0.023)	(0.042)	(0.010)	(0.263)**
Clustered at the household and age levels	(0.236)	(0.134)	(0.031)	(0.020)	(0.047)	(0.014)	(0.213)***
Clustered at the household and PSU levels	(0.194)	(0.153)	(0.023)	(0.020)	(0.034)	(0.013)	(0.192)***
Clustered at the household and year levels	(0.139)	(0.106)	(0.024)	(0.023)	(0.042)	(0.010)	(0.265)**

Notes: Table presents estimates for an interaction between $I(Age \geq 65)$ and Post. Robust standard errors in parentheses. Regressions control for a full set of covariates. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.