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Social protection for happiness? The impact of social pension reform on subjective well-being of the Korean elderly

Tae-Young Pak, Ph.D.[1]

Abstract

Expanding the scope of the traditional indicators used to assess economic progress with measures of subjective well-being has been of growing importance in policy modeling. A recent Stiglitz-Sen-Fitoussi report argued that policy decisions and welfare evaluations should be approached from a broader perspective on well-being and take into account metrics derived from self-reports of living conditions. To this end, we evaluate the efficacy of the 2014 social pension reform in South Korea by examining various domains of subjective well-being. The South Korean government introduced a basic old-age pension in 2008 and doubled its monthly benefits in 2014. The reform in 2014 represents one of the largest social welfare expansions in South Korean history, with an aim to improve seniors' quality of life. Using data from the Korean Longitudinal Study of Aging, we estimate a matched difference-in-differences model that isolates the causal effects of benefit increase from underlying trend changes in well-being. Results show that the reform was associated with an average of 4.8-5.7% increase in financial satisfaction among beneficiaries, and that this correlation was more pronounced for retirees, seniors above age 70, and those at the bottom of wealth distribution. However, there is insufficient evidence to conclude that satisfaction with health status, parent-child relationship, and overall quality of life has improved in response to the reform. Our findings are in contrast to the past evidence on objective welfare gains associated with pension expansion. Taken together this study calls for the use of self-reported well-being data in policy evaluation.

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1. Introduction

Since the publication of a report by the Commission on the Measurement of Economic Performance and Social Progress (Stiglitz et al., 2009), self-reports of subjective welfare has drawn renewed interests from policymakers as an alternative yardstick to measure societal progress. The essence of the report was that present measures of economic performance such as gross domestic product (GDP) or unemployment rate are insufficient indicators of societal progress, and that subjective well-being and other quality of life measurements should also be taken into account. Aggregate statistics that currently play a prominent role in policymaking does not reflect non-material dimensions of well-being. For many years, researchers have been puzzled by an apparent paradox that rising income is only weakly correlated with happiness in the long run (Easterlin, 1974). How well off people feel about their life depends on many factors that conventional income-based measures do not capture, such as health, social connections, environment, and political governance. The advocacy for objective indicators has encouraged an unbalanced focus on economic growth and policies that have proven to be unequal and not poverty-reducing. In order to assess whether policy intervention flourishes one's life, policy modeling should employ measures that tell us the extent of improvements in human well-being.

This study evaluates the efficacy of social pension expansion in South Korea using measures of subjective well-being. 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 basic old-age pension (BOAP) in 2008 and doubled its monthly benefits beginning in July 2014. The BOAP is a non-contributory social pension that transfers flat-rate cash benefits to seniors at the bottom 60-70% of the income distribution, with an aim to stabilize their economic conditions. Prior research showed that cash transfer through social pension has improved the lives of the poor by increasing consumption, economic productivity, and access to healthcare. To date, there has been limited evidence on the association between poverty-targeted social protection programs and subjective well-being.

We exploit the expansion of the BOAP in 2014 to isolate the causal effect of pension benefit increase on subjective well-being. Using data from the Korean Longitudinal Study of Aging (KLoSA), the policy effect is estimated by comparing pre- vs. post-expansion changes in

well-being outcomes between beneficiaries and non-beneficiaries. Since the BOAP was established as voluntary benefits, a difference-in-differences (DD) model combined with propensity score matching (PSM) is used to identify treatment effects. The combination of these two methods accounts for time-invariant unobservables that lead to self-selection into the program, and compares only those with similar observable characteristics (Rosenbaum and Rubin, 1983). Subjective well-being is measured as a broad assessment of life in general, as well as regarding three specific domains: financial situation, health, and parent-children relationship. Examining financial satisfaction along with other measures of happiness allows us to investigate whether the well-being effect of income is limited to the financial domain or extends to other aspects of life.

There are two main findings. First, the reform to the BOAP was associated with higher financial satisfaction among beneficiaries. Through 2012 and 2014 a richer non-beneficiary group experienced a significant drop in financial satisfaction, whereas beneficiaries reported nearly the same assessment of their financial situation. Compared to the pre-expansion average, the reform was responsible for approximately a 4.8-5.7% increase in financial satisfaction. Second, correlations between the reform and other domains of satisfaction were small in magnitude and not robust to estimation methods. The correlations were statistically insignificant even in retirees, seniors above age 70, and those at the bottom of wealth distribution for which financial satisfaction increased to a greater extent. Overall, our results suggest that increases in public transfer income led to the financial peace of mind but did little to improve happiness.

This study contributes to the existing literature along several dimensions. First, our measures of subjective well-being encompass multiple domains. Most previous studies used a single measure of happiness and failed to examine which domains of subjective well-being responds to government support. Second, we use a nationally representative sample and a large policy change that has affected majority of seniors. We advance the previous studies that have often used a local-level policy change or small and selective samples. Third, this study examines the well-being effect of social pension in an industrialized and urbanized context. Most prior research studied African and Latin American experiences where non-contributory pensions were given more attention with broader coverage. Fourth, 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, however, the universal coverage clause was removed and coverage was limited to the bottom 70% of the income

distribution. Because this change was unannounced prior to implementation, given the plausible assumption that the original plan should have raised expectations in all age-eligible seniors our quasi-experimental framework yields estimate that is not confounded by changes in consumption behaviors in anticipation of an upcoming reform.

2. Background

2.1. 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. The 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) was introduced in January 2008 with a goal to complement the NPS and to provide 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

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

married couple (Pak, 2019). 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 president Park's electoral promise to provide more income support for retirees. Though the original plan was to expand coverage to all citizens aged at 65 or higher, budgetary considerations led to retaining the same asset-based eligibility rules as the BOAP. 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. Despite these changes, this reform was a significant improvement in scope and depth over its predecessor.

There has been mixed evidence regarding 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). The only study on subjective well-being found a small increase in financial satisfaction (about 2%) associated with the introduction of the BOAP (Kang and Moon, 2013).

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 lead to underestimated treatment effect. 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 of the BP could be further downward biased. A recent study by Pak (2019) examined the BP reform in 2014 and found significant increases in tourism spending among those aged 65 or higher. Our study builds upon this research and links the BP reform to changes in subjective well-being.

2.2. Impact of social pension on economic security and well-being

Most social pensions are designed as a non-contributory program to target individuals who are not covered by traditional pay-as-you-go contributory pensions (Willmore, 2007). This contributory structure has proved to be difficult to scale up in economies with large informal labor markets (Dethier et al., 2010; Galiani and Weinschelbaum, 2012). Compared to workers in the formal sector, individuals in small or family-based business have no access to an employer-provided pension or are not required to make regular contributions. In Africa, mandatory pension scheme coverage ranges from 1% to 35%, with an average 1% of GDP being dedicated to the pension system (Abels and Guven, 2016). As a result, the non-contributory pension is designed to be universal and spreads benefits more thinly over a large segment of the population (Salinas-Rodriguez et al., 2014).

Cash transfer through non-contributory pension has been an efficient policy intervention to alleviate the depth of poverty as well as its incidence. The evidence from Brazil and South Africa showed that households with a social pension recipient have a 12-18% lower probability of being poor than those not receiving benefits (Barrientos, 2003). Similarly, the non-contributory pension in Argentina was associated with a significant reduction in poverty rates (Bertranou and Grushka, 2002). The beneficiaries of social pension exhibit greater financial stability and are less likely to fall into poverty in subsequent periods (Barrientos and Lloyd-Sherlock, 2002). The policy simulation in sub-Saharan African countries showed that universal non-contributory pensions would reduce indigence headcount and gap when employed (Kakwani and Subbarao, 2007).

Social pension has been shown to have spillover effects on well-being and health. In a study of South Africa's state old age pension, beneficiaries reported higher self-rated health after receiving transfers (Case, 2004). The subsequent studies in Mexico and Peru found similar evidence of material well-being and mental health benefits among beneficiaries (Bando et al., 2016; Galiani et al., 2016). In recent Chinese evidence, the introduction of the Rural Pension Scheme was responsible for improvements in mental health and economic satisfaction (Cheng et al., 2018). The benefits of social pension also extend to non-recipients in a family. Households with social pension beneficiaries are more likely to migrate elsewhere for a job opportunity (Barrientos and Lloyd-Sherlock, 2003), or use resources to provide better health care to their children (Duflo, 2000). The presence of a pensioner was associated with more educational opportunities and nutrition for grandchildren (Case and Menendez, 2007).

Several limitations of non-contributory pensions have been reported. First, public income transfers often displace private transfers from adult children to parents. Some studies argued that public pension programs would do little to poverty reduction if they only replace private income support (Cox and Jimenez, 1989; Juarez, 2009). Second, they may distort labor market (Cuesta and Olivera) and discourage labor supply in old age (Bertrand et al., 2003). Third, social protection is not affordable and sustainable as it has to compete with other government expenditure priorities (Beattie, 2000; Nelissen, 1999).

3. Methods

3.1. Data description

The data for this study comes from 2012 and 2014 waves of the Korean Longitudinal Study of Aging (KLoSA). The KLoSA is a biannual longitudinal survey that has tracked community-dwelling adults aged 45 years or older and their spouse since 2006. The study is designed to capture various aspects of population aging necessary to understand economic, social, and health situations at the end of life. The interviews have been administered primarily in the second half of the year, with the 2014 survey fielded in September through November. Since the BP scheme was implemented in July 2014, comparing 2012 and 2014 waves allows us to examine the short-term and immediate impact of the benefit increase.[2]

Following the previous studies, the sample is limited to respondents aged 65 or older in 2014 (Kang and Moon, 2013). This sample selection excludes age-ineligible respondents at the time of reform. Since individual characteristics might substantially differ across age, limiting the sample to age-eligible households gives a more homogeneous sample and reduces the potential for omitted variable bias. After dropping observations with missing values, the final sample includes a balanced panel of 4,113 individuals.

3.2. Measures of subjective well-being

Empirical analyses consider satisfaction with (a) financial situation, (b) health condition, (c) parent-children relationship, and (d) overall quality of life. The well-being questions run as follows: *How happy are you at present with your (financial situation / physical health /*

[2] Refer to the KLoSA website (http://survey.keis.or.kr) for detailed information about survey procedures.

relationship with spouse / relationship with your children / overall quality of life?.[3] The response takes the form of probability and ranges from 0 to 100 in the multiples of 10. Higher values correspond to higher levels of perceived satisfaction. Satisfaction with the parent-children relationship was asked only to respondents with living children.

We expect to estimate the direct impact of the reform from changes in financial satisfaction, and indirect spillover effect from other domains of well-being. Satisfaction with financial condition is likely to increase if the reform alleviated poverty and financial burden. Likewise, health satisfaction would have improved if the expansion increased access to healthcare and resulted in better health. One might anticipate a more intimate relationship between parents and children as the expansion could have displaced private transfers from children to parents and eliminated potential conflicts over the adequate amount of financial support. Lastly, increases in domain satisfaction may contribute to higher satisfaction with life.

3.3. Treatment and control group

The control group includes respondents aged 65 and older and did not receive benefits from the BOAP in both 2012 and 2014 (1,395 individuals). Since our sample is limited to age-eligible individuals, this group consists of individuals who could not pass the asset-based income test and those who were income-eligible but did not join the program. The treatment group includes individuals who were age-eligible and received cash transfers from the BOAP (2,718 individuals). This is made up of two subgroups; (a) those who joined the program prior to 2012 and stayed being enrolled in 2014 (1,988 individuals), and (b) new beneficiaries who joined the program in 2014 (730 individuals). In the new beneficiary group (category b), only 216 individuals became age-eligible in 2014, while 514 individuals passed the threshold age prior to the expansion. According to our preliminary analysis, approximately half of these 514 individuals could have passed the income test if they applied for the program in 2012. This indicates that assignments into the treatment and control groups are likely non-random.

3.4. Empirical framework

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

Since enrollment in the BP is voluntary, beneficiaries are likely to be different from the control group in unobserved ways. Under this setting, a naive comparison of the enrolled and non-enrolled would capture both treatment and selection effect and produce biased estimates of the policy effect. The preferred approach is to calculate the expected outcome among program participants minus the expected outcome they would have had if they had not participated in the program – known as the average treatment effect on the treated (ATT). Formally,

$$ATT = E(Y_{i1} - Y_{i0}|D_i = 1) = E(Y_{i1}|D_i = 1) - E(Y_{i0}|D_i = 1), \quad (1)$$

where *i* indexes respondents; D_i is indicator for enrollment in the BP; Y_{i1} is the well-being outcome of a participant who had benefited from the BP reform in 2014; and Y_{i0} is the well-being outcome of the same individual if he/she had not benefited from the expansion. The major problem is that we do know what the level of outcomes would have been if individuals had not participated in the BP. Counterfactual Y_{i0} cannot be observed in data.

Rosenbaum and Rubin (1983) proposed the propensity score matching (PSM) to mimic an experimental study using observational data. The basic intuition is to create a counterfactual group of non-participants who are similar to program participants in terms of observed characteristics and take the counterfactual outcomes as the approximation of $E(Y_{i0}|D_i = 1)$. This approach builds on the conditional independence assumption, $E(Y_{i0}|Z_i, D_i = 1) = E(Y_{i0}|Z_i, D_i = 0) = E(Y_{i0}|Z_i)$, where Z_i denotes observed characteristics. That is, conditional on observed characteristics, enrollment in the BP in 2014 can be treated as random. If this assumption is satisfied, there exists a set of matched control analogs for each treated individuals, and $E(Y_{i0}|Z_i, D_i = 0)$ can be substituted for $E(Y_{i0}|D_i = 1)$. Though theoretically appealing, finding the matched analogues in the control group is computationally prohibitive when the dimension of Z_i is high. Therefore, matching is performed on a single dimension propensity score, $p(Z_i) = Pr(D_i = 1|Z_i)$. This conditional probability is estimated by a probit regression of the BP enrollment on observed characteristics.

The limitation of PSM is that treatment and control groups are balanced with respect to observed covariates only. If there is any unobserved covariate that correlates with program participation and well-being outcomes, the estimates of the policy effect would still be subject to selection bias. However, we can take first differences in the outcomes and eliminates the effects of individual-specific time invariants (e.g., personality, attitudes, time preference, etc.) on program

participation. If t and $t+1$ denote before and after the reform (i.e., 2012 and 2014), the PSM can be combined with difference-in-differences (DD) estimator as follows,

$$ATT^{DD-PSM} = E(Y_{i1}^{t+1} - Y_{i1}^{t} \big| D_i = 1) - E(Y_{i0}^{t+1} - Y_{i0}^{t} \big| p(Z_i), D_i = 0). \quad (2)$$

3.5. Matching and estimation strategy

This study employs two matching methods: nearest-neighbor (*NN*) and kernel matching. Under the *NN* method, each treated individual is matched with observations in the control group that are considered nearest neighbors in terms of propensity score. A potential drawback of *NN* matching is that matching quality could be reduced if the closest neighbor is far away. The *NN* matching with caliper avoids the risk of bad matches by matching only comparison observations whose propensity score falls within a pre-determined tolerance level. For *k*-to-*1 NN* matching, *k* controls within a caliper are matched with each treated subject. The estimation can be implemented either with replacement or without replacement. When matching is performed with replacement, the same comparison group observation can be used repeatedly as a match. In general, it is advised to allow replacement to avoid bad matches (Caliendo and Kopeinig, 2008; Dehejia and Wahba, 2002). This study uses *5*-to-*1* and *2*-to-*1 NN* matching with replacement and caliper 0.01.

The kernel matching matches treated subjects with a weighted average of all controls where the weights are inversely proportional to the distance between the treated and control group's propensity scores. The choice of threshold distance (bandwidth parameter) is important as the tradeoff between variance and bias arises (Heckman et al., 1997). Small bandwidth exacerbates the problem of common support but improves matching quality. High bandwidth reduces variance between the estimated and underlying density function at the cost of larger bias in the estimated density. Given a lack of empirical guidance, we select different levels of bandwidths (0.01 and 0.05). The estimation results remain robust to matching algorithms and parameter settings.[4]

Blundell and Costa-Dias (2000) showed that the ATT combined with the PSM can be estimated by the following expression,

$$\widehat{ATT}^{DD-PSM} = \frac{1}{N_{D_1}} \sum_{i \in D_1 \cap S} \left[(Y_{i1}^{t+1} - Y_{i1}^{t}) - \sum_{j \in D_0 \cap S} w_{ij} (Y_{j0}^{t+1} - Y_{j0}^{t}) \right], \quad (3)$$

where w_{ij} denotes the weight assigned to matched individual *j* when compared with treated individual *i*. *S* is the area of joint common support, defined as the subset of treated individuals who

[4] The results with alternative matching algorithms will be made available upon request.

are matched with the counterfactual group, and D_1 and D_0 denote the treatment and control group. N_{D_1} is the number of treated individuals who belong to joint common support *S*. That is, only observations in the common support are used for estimation.

3.6. Variable selection

The covariates include age, gender, education background, marital status, labor supply (retirement status), household income, home ownership, public and private health insurance ownership, self-reported health, activities of daily living (ADL) score, and location of residence. As suggested by Caliendo and Kopeinig (2008), we select covariates that are known to determine subjective well-being or correlate with the BP enrollment. Age is modeled in a quadratic form to account for the quadratic effect of age. Education background is captured by indicators for middle school degree and high school/college degree, with less than middle school being omitted as a reference group. Marital status takes one if married and zero if separated, divorced, widowed, and never married. Following Bonsang et al. (2012), individuals are classified as being retired if he/she is out of the labor force with the intention of staying out permanently. Household income is the sum of all incomes received by both spouses over the last 12 months, including capital gains, pension income, and benefits from other government welfare programs. Home ownership is included as a proxy for household wealth. Our preliminary analysis showed that a binary indicator of home ownership is more predictive of enrollment in the BOAP than the noisy wealth reports. We also include self-reported health and ADL score to capture changes in health condition and disability status. Since medical expenditure is a major spending category for retirees, those with poor health or disabilities would be more likely to enroll in the BP. All covariates are measured using 2012 data to avoid potential endogeneity problems.

4. Results

Table 1 shows sample characteristics prior to matching and probit estimates for propensity score. Panel A is based on a full sample of 4,113 respondents, and panel B is limited to 2,137 respondents with living children. The first three columns in each panel present the sample mean of covariates in 2012 and corresponding *t*-test results. Looking at panel A, the control group is 47% female, 85% married, 41% high school graduate, and has an average age of 69.8. The treatment group is predominantly retirees and females in their mid-70s and have less financial resources than

the control group. In both panels, average differences in demographic and socioeconomic characteristics between the two groups are statistically significant at the 1% or 5% level.

Columns (4) and (8) report probit coefficient estimates for propensity score estimation. The magnitude and sign of the coefficients are expected in the sense that economically disadvantaged groups are more likely to participate in the BP. The enrollment status exhibits strong correlations with variables related to the asset-testing such as household income, home ownership, and private health insurance ownership. We find that gender, retirement status, public health insurance ownership, and ADL score are not predictive of the BP enrollment.

[Insert Table 1 about here]

Table 2 presents a series of test statistics concerning matching quality. If covariates are well balanced between the two groups, all three statistics (absolute standardized bias, pseudo R^2, and likelihood-ratio χ^2) should be reduced close to zero (see Sianesi, 2004 for more details). We first find that mean and median absolute standardized bias in the matched sample is below or around the commonly accepted threshold of 3% (Caliendo and Kopeinig, 2008). The next columns show that pseudo R^2 fall to almost zero after matching, and likelihood-ratio tests fail to reject the null hypothesis at the 10% level (5% level for the *NN* matching in panel A). These results all point to no systematic difference in the distribution of the covariates between the treated and matched control groups. The last four columns report how many persons are dropped after matching. Because matching is implemented with replacement, imposing a common support condition does not exclude a significant number of observations. In the analysis not shown, we examined standardized difference in regressors between the two groups in a matched sample. On all covariates, standardized differences are much smaller than a commonly accepted threshold of 10% (Austin, 2009).

[Insert Table 2 about here]

Table 3 presents estimation results for equation (3). We report estimates based on the kernel and *NN* matching as well as the unmatched sample. Column (1) shows that the reform in 2014 was associated with 2.21-2.63 points increase in financial satisfaction. The ATT estimates are different from zero at the 5% significance level and about twice as large as the unmatched estimate. Compared to the 2012 sample mean of financial satisfaction in the treatment group (Table A1), these correspond to an average of 4.8-5.7% increase. Regarding health satisfaction, the estimated ATTs represent 4.5-6.4% improvements from the mean pre-intervention level in the treatment

group. Similar to column (1), these results are robust to different matching methods in terms of statistical and economic significance. For relationship satisfaction and life satisfaction, the ATT estimates carry the positive sign but are not estimated with enough precision to reject the null hypothesis at the desired significance level. Comparing the matched and unmatched results, we find evidence of a downward bias in the estimated treatment effect when selection bias is unaddressed. Overall, the unmatched estimates are 40-90% smaller in magnitude than the corresponding estimates with matching.

[Insert Table 3 about here]

Table 4 shows the differential impact of the BP reform by age, retirement status, and household net worth. Panels A and B split the sample by age and retirement status in 2012 and re-estimate equation (3). Except for column (3), the estimated ATTs are much larger for persons above age 70 and retirees as compared to those of younger and working individuals (differences significant at the 5% level). Since these groups have fewer income sources, increases in monthly benefits from the expansion could have a fairly large impact on subjective well-being. In panel C, the sample is stratified by net worth in 2012. The results in the first row come from individuals whose net worth is less than or equal to the first quartile of the mean net worth. The second row uses individuals that are above the first quartile of the mean net worth. Not surprisingly, the estimated ATTs decrease with net worth. Among those with below the first quartile net worth, the reform was associated with 7.59 points increase or 21% rise in financial satisfaction from the mean pre-intervention level. In the wealthier group, the reform explained only 0.59 points increase in financial satisfaction, which is equivalent to 1.1% improvements. It is also important to note that correlations between the reform and life satisfaction are not significant at the 10% level across all panels. Even in the subsamples limited to older, retired, and poor respondents, the estimated ATTs are not much different from zero.

[Insert Table 4 about here]

Our analysis so far used enrollment in the BP for treatment assignments. The caveat of this approach is that enrollment status could be the outcome of dissaving in the previous periods in anticipation of the reform. Research has shown that means-tested social welfare creates a strong disincentive for saving and labor supply and increases consumption expenditures (Hubbard et al., 1995; Powers, 1998). If potential beneficiaries spent down their assets to qualify for the benefits, it is difficult to distinguish whether changes in subjective well-being are attributable to the social

pension expansion or means testing effect. Alternatively, we can use a threshold age (age ≥ 65) to separate treated from controls. Change in age is strictly exogenous and cannot be manipulated unless age report is systematically incorrect. The DD model combined with this treatment assignment identifies "intent-to-treat" estimate and yields underestimated policy effect.

Table 5 presents DD estimates based on age-based identification strategy. We estimate the following linear DD model, $Y_i = \beta_0 + \beta_1 \cdot Treated_i + \beta_2 \cdot Year_{i,2014} + \beta_3 \cdot (Treated_i \times Year_{i,2014}) + Z_i \cdot \boldsymbol{\gamma} + \varepsilon_i$, where $Treated_i$ is treatment indicator, $Year_{i,2014}$ is dummy for 2014 survey, Z_i is a vector of covariates, and ε_i is *i.i.d.* error term, and report estimates for β_1, β_2, and β_3 in table. The sample is expanded to persons aged 50-80 in 2012 and 2014 to construct treatment and control groups of comparable size. Therefore, the control group includes respondents aged 50-64, and the treatment group consists of those aged 65-80. The regressions are estimated by pooled OLS with standard errors clustered at the individual levels.[5]

Column (1) in panel A shows that the DD term is positively associated with financial satisfaction. The coefficient estimate is different from zero at the 1% level, and explains about 2.8% improvement from the pre-mean of financial satisfaction among the age-eligible group. Compared to the ATT estimation results, this point estimate is significantly smaller at the 1% level but yields the same inference. From columns (2) through (4), correlations between the DD term and well-being outcomes are no longer statistically significant. These results contradict the previous estimation results, meaning that the ATT estimates for the reform-health satisfaction correlation do not represent causal impact.

The primary identification assumption underlying the DD analysis is that unobserved factors are not inducing a trend break in subjective well-being between the treatment and control groups. While our regressions include a wide array of covariates, their estimates do not account for the impact of potential confounders that differ by treatment assignment. To check the validity of our identifying assumption, regressions in panel B include interaction terms between treatment indicator and covariates. Specifically, a treatment indicator is interacted with marital status, labor force participation, household income, home ownership, health insurance ownership, self-rated

[5] We prefer pooled OLS over individual fixed effects (FE) model. Notice that the individuals FE models capture changes in subjective well-being for persons aged from 64 to 65 years in 2014 but miss variations for persons who were already age-eligible and received increased benefits in 2014. As discussed in section 3.3, those who became age-eligible for the program in 2014 are of a small number and do not provide enough within variation.

health, ADL score, and dummy for urban residence. These interactions terms allow for the covariates to have a differential impact on the well-being outcomes by treatment assignment. Across all columns, inferences about the effect of the reform remain unchanged.

The remaining concern is that not all age-eligible individuals are exposed to the reform. Since the treatment group consists of beneficiaries and age-eligible individuals who could not pass the income test, our results could be driven by changes in outcomes for the non-beneficiaries that are unrelated to the reform. Therefore, we attempt to recover the local average treatment effect (LATE) using the instrumental variable (IV) technique. Our strategy is to include the average monthly public transfer income as an endogenous regressor in the second-stage regression, and use a DD term as an instrumental variable. 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. From 2012 to 2014, the mean of public transfer income in our sample increased from 46,000 KRW to 110,000 KRW. The related estimation results are presented in panel C. The estimates are quite similar to the upper panels; the reform was significantly associated with financial satisfaction but not with other domains of satisfaction. Evaluated at the mean increase in average public transfer between surveys, the reform explained 0.9 points increase in financial satisfaction.

[Insert Table 5 about here]

5. Conclusions

We found robust evidence that the 2014 expansion of the BOAP improved subjective financial well-being among beneficiaries. Using a matched difference-in-differences approach, we estimated an average of 4.8-5.7% increase in financial satisfaction associated with the reform. For other domains of well-being, we could not find any significant improvement in response to the reform; the estimated policy effects were either statistically insignificant (relationship satisfaction and life satisfaction) or significant but not robust to different estimation methods and modeling assumptions (health satisfaction). Further analyses highlighted that the well-being benefits accrued to a small portion of older respondents, retirees, and those with less wealth. Similar to the main findings, changes in non-financial domains of subjective well-being were not associated with the reform across subpopulations.

The heterogeneous response of financial and non-financial domains of well-being suggests that fulfilling basic needs through public transfer does little to improve happiness. One possible explanation is that income is not a major determinant of life satisfaction for older ages (Pinquart and Sorensen, 2000). It has been argued that happiness in later life depends on various factors other than income, such as health, marital status, and social network (Krueger and Schkade, 2008), and that the importance of income in subjective well-being declines with age (Hsieh, 2011). Another explanation is that the reform may have had a limited influence on relative income. Individuals feel much happier if their income is higher in comparison with the income of their reference group (Clark and Oswald, 1996; Ferrer-i-Carbonell, 2005). In the case of social pension expansion, there may have been very small changes in the income of a reference group because one's social network would include people of similar characteristics (Christakis and Fowler, 2014). If a majority of peers in a social group were already entitled to the BP, the reform would have a little impact on how much money an individual earns relative to others.

A few limitations need to be considered when interpreting the findings. First, our estimates concern only the short-term effect of the reform. Research has shown that the influence of income on subjective well-being operates indirectly through changes in non-financial factors such as social activities and network, which expands slowly with additional income (George, 1992). Since the 2014 KLoSA data was surveyed 2-5 months after the expansion, not enough time had passed for our respondents to build social relationships and realize greater happiness. Second, indicators for subjective well-being are subject to measurement problems. Subjective measures are known to be less reliable and valid than objective indicators, and can be biased by social comparisons, aspirations, and social norms.

The relationship between public transfer and happiness has been under-explored within the literature, particularly for the elderly. An important but less-discussed implication of Easterlin (1974) is that public transfers have gradually increased while the economy grows and happiness remains stagnant. The fact that higher social spending and a decrease in poverty are not followed by a happier life suggest that government may play a limited role in promoting subjective social welfare. As this study showed, the mechanism through which public transfer impacts subjective well-being is multi-faceted and likely differs by domains of subjective welfare considered. Further research is needed to develop a more nuanced understanding of the public transfer-happiness nexus and to extend this study into longer time frames and countries with different institutional settings.

6. Policy implications

A key implication of this study is that evaluating and assessing public policy is not a straightforward recognition and should be accompanied by a variety of yardsticks that measure both objective and subjective well-being. As shown in this study, monitoring subjective well-being could give us a good idea of how public policy is doing and whether people are feeling better in response. One of the arguments to prefer subjective indicators over objective ones is that what matters to a good life differs by people (Kroll and Delhey, 2013). While objective indicators reflect a few materialistic conditions that might be critical for an ideal nation, progress in those factors does not necessarily indicate equal benefits for all individuals. Some people would draw more utility from a policy that enhances personal safety, while others would put environment in front of other social matters. Often, indicators for social and economic progress are determined by a few researchers and policymakers in a top-down process, without considering how people feel about development. Indeed, individual is the best judge of how surrounding circumstances affect his/her life.

Subjective indicators are particularly useful in policy modeling when changes in subjective well-being are not moving together with material life circumstances. A good example is low levels of happiness in wealthy nations that have a strong social safety net. Countries like Japan and South Korea are considered wealthy measured in real GDP but have had low average happiness and a high suicide rate. The unbalanced focus on economic growth and policies that have proven to be unsustainable, unequal and not poverty-reducing resulted in the nationwide dissonance between objective and subjective well-being. Policymakers should then ask, why are these people unhappy? does the current policy provide enough benefits or direct resources to the right place? how does the current policy affect non-material factors such as social connections and relationships? and which aspect of policy should be prioritized in reform to increase well-being? Examining divergence between objective and subjective indicators informs policymakers a population subgroup that has been neglected in prior policy discussions and choosing the right strategy that enhances human well-being.

A potential pitfall of using happiness in policy modeling is that any policy is not likely to have a major impact on life satisfaction. A cursory look at the happiness research suggests that subjective well-being is a stable construct and rarely changes at the national level (Myers, 2000). The drivers of subjective well-being are complex, and various treadmill effects - known as the

positional and hedonic treadmill - make it nearly impossible to raise happiness at the same rate as economic growth. The fact that national happiness remains stagnant does not necessarily mean that a policy has failed to achieve its objectives. It might be the case that the benefits of policy accrue to a particular domain of happiness, or objective welfare is slowly spreading to subjective well-being. In contrast, having no benefits across domains of subjective welfare and population subgroups is a clear alarm signal that requires immediate action. Incorporating subjective well-being into policy evaluation thus complements existing objective measures and deepens our understanding of how policies affect various domains of life other than economic conditions.

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Tables

Table 1
Descriptive statistics before matching and propensity score probit estimates.

	Panel A: Full sample (*N*=4113)				Panel B: Sample with children (*N*=2137)			
	Treated group (mean)	Control group (mean)	Difference (1)-(2) (*t*-values)	Propensity score probit (coefficients)	Treated group (mean)	Control group (mean)	Difference (1)-(2) (*t*-values)	Propensity score probit (coefficients)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age	74.5	69.8	21.6***	0.394***	75.6	70.4	15.8***	0.412***
Age squared	5580	4912	21.0***	-0.002***	5758	5001	15.2***	-0.002***
Female	0.63	0.47	9.8***	0.035	0.74	0.55	8.4***	0.042
Middle school graduate	0.12	0.20	-6.9***	-0.401***	0.11	0.19	-5.2***	-0.389***
HS or college graduate	0.15	0.41	-19.7***	-0.715***	0.11	0.40	-16.2***	-0.814***
Married	0.62	0.85	-15.2***	-0.315***	0.36	0.67	-13.6***	-0.321***
Retired	0.77	0.67	6.4***	-0.046	0.81	0.68	6.7***	0.090
HH income (10000 KRW)	1665	2595	-13.5***	-0.001***	1720	2550	-9.0***	-0.001***
Own home	0.79	0.92	-10.3***	-0.547***	0.79	0.90	-6.1***	-0.468***
Own public health insurance	0.91	0.97	-7.0***	-0.088	0.91	0.97	-4.8***	-0.134
Own private health insurance	0.08	0.23	-13.0***	-0.308***	0.08	0.22	-9.3***	-0.243**
SR health: fair or better	0.56	0.72	-9.7***	-0.021	0.55	0.76	-9.2***	-0.164**
ADL score	0.25	0.15	3.0***	-0.006	0.26	0.10	3.2***	0.011
Urban area	0.39	0.43	-2.5**	-0.049	0.38	0.43	-2.2**	-0.136**
Likelihood ratio χ^2 statistic				992.3				550.7
McFadden's pseudo R^2				0.188				0.217

Notes: Household income is adjusted to 2016 KRW using the Korean Consumer Price Index. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 2
Quality of matching.

	Absolute standardized bias				Pseudo R^2		LR χ^2		Treated group		Control group	
	Median		Mean						Observations		Observations	
	Before	After	Before	After	Before	After	Before	After	Before	After	Before	After
Panel A: Full sample												
K (*b*=0.01)	34.2	1.0	37.6	1.5	0.188	0.002	992.3	14.0	2718	2712	1395	1395
K (*b*=0.005)	34.2	1.6	37.6	1.9	0.188	0.002	992.3	16.2	2718	2712	1395	1395
NN (*m*=2, δ=0.01)	34.2	1.9	37.6	2.2	0.188	0.003	992.3	22.5	2718	2712	1395	1377
NN (*m*=5, δ=0.01)	34.2	2.0	37.6	2.3	0.188	0.003	992.3	22.3	2718	2712	1395	1377
Panel B: Sample with children												
K (*b*=0.01)	39.8	1.8	42.4	2.6	0.217	0.003	550.7	12.7	1537	1526	600	581
K (*b*=0.005)	39.8	2.1	42.4	2.9	0.217	0.003	550.7	14.4	1537	1526	600	581
NN (*m*=2, δ=0.01)	39.8	2.2	42.4	2.8	0.217	0.004	550.7	16.6	1537	1526	600	581
NN (*m*=5, δ=0.01)	39.8	3.1	42.4	3.0	0.217	0.004	550.7	15.8	1537	1526	600	581

Notes: Matching methods; *K*, kernel; *NN*, nearest-neighbor. *b* refers to bandwidth for kernel matching. *m* refers to the number of nearest neighbor matches involved. δ refers to the size of caliper. All matching is with replacement and on the common support.

Table 3
ATT estimates.

Outcome:	Financial satisfaction (1)	Health satisfaction (2)	Relationship satisfaction (3)	Life satisfaction (4)
Unmatched	1.23 (0.62)**	1.40 (0.68)**	1.03 (0.84)	1.07 (0.58)*
K (*b*=0.01)	2.34 (0.99)**	2.17 (1.16)*	1.64 (1.04)	1.07 (0.75)
K (*b*=0.005)	2.44 (0.95)***	2.31 (1.14)**	1.61 (1.09)	1.03 (0.76)
NN (*m*=2, δ=0.01)	2.21 (0.95)**	3.10 (1.26)**	1.59 (1.23)	1.46 (0.88)*
NN (*m*=5, δ=0.01)	2.63 (1.04)**	2.65 (1.25)**	1.80 (1.19)	1.41 (0.84)*

Notes: Matching methods; *K*, kernel; *NN*, nearest-neighbor. *b* refers to bandwidth for kernel matching. *m* refers to the number of nearest neighbor matches involved. δ refers to the size of caliper. Standard errors in parentheses are bootstrapped with 50 iterations. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 4
ATT estimates by age, retirement status, and net worth.

Outcome:	Financial satisfaction (1)	Health satisfaction (2)	Relationship satisfaction (3)	Life satisfaction (4)
Panel A: stratified by age in 2012				
Age $\leq$ 70	-0.37 (1.36)	0.97 (1.17)	-1.48 (1.33)	0.16 (1.30)
Age $>$ 70	2.77 (1.14)**	2.17 (1.32)	1.89 (1.46)	1.17 (0.87)
Panel B: stratified by retirement status in 2012				
Retired	2.50 (0.92)***	2.40 (1.18)**	1.10 (1.54)	1.27 (0.80)
Not retired	0.68 (1.60)	0.78 (2.07)	2.40 (2.48)	-0.37 (1.29)
Panel C: stratified by household net worth in 2012				
Net worth $\leq Q_1$	7.59 (2.18)***	1.83 (2.82)	2.69 (3.13)	3.09 (2.50)
Net worth $> Q_1$	0.59 (0.88)	1.08 (1.02)	0.26 (1.04)	0.02 (0.75)

Notes: ATT estimates using kernel matching and 0.01 bandwidth. Q_1 is the first quartile of total net worth. Standard errors in parentheses are bootstrapped with 50 iterations. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 5
Robustness checks using age-based identification strategy.

Outcome:	Financial satisfaction	Health satisfaction	Relationship satisfaction	Life satisfaction
	(1)	(2)	(3)	(4)
Panel A: Control group aged below 65 and treatment group aged 65 or above				
Treated	-0.57 (0.72)	-0.43 (0.67)	0.53 (0.84)	-0.69 (0.60)
Year 2014	-1.80 (0.35)***	1.01 (0.34)***	-0.31 (0.43)	-0.43 (0.31)
Treated × Year 2014	1.45 (0.52)***	0.01 (0.53)	-0.21 (0.63)	0.58 (0.46)
Observations	11938	11938	6969	11938
Panel B: Control group aged below 65 and treatment group aged 65 or above				
Regressions include interactions between a treatment indicator and time-varying covariates				
Treated	-1.10 (2.17)	3.43 (2.17)	-3.20 (3.55)	2.56 (2.04)
Year 2014	-1.76 (0.35)***	1.02 (0.35)***	-0.30 (0.43)	-0.44 (0.31)
Treated × Year 2014	1.37 (0.52)***	0.02 (0.53)	-0.23 (0.63)	0.62 (0.47)
Observations	11938	11938	6969	11938
Panel C: Control group aged below 65 and treatment group aged 65 or above				
Local average treatment effect estimated by 2SLS				
Public transfer income	0.14 (0.05)***	0.01 (0.05)	-0.02 (0.06)	0.06 (0.05)
Observations	11938	11938	6969	11938
IV validity test:				
Underidentification[a]	668	427	668	668
Weak instruments[b]	398	254	398	398

Notes: All regressions include a basic set of covariates. Regressions in panel B include the covariates plus interactions between treatment indicator and marital status, labor force participation, household income, home ownership, health insurance ownership, self-rated health, ADL score, and dummy for urban residence. Public transfer income is 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. The instrument for panel C is the interaction between treatment indicator and post dummy. [a]Kleibergen-Paap rank LM-statistic tests the null hypothesis of underidentification. [b]Cragg-Donald Wald F statistic tests the null hypothesis of weak instruments. Standard errors in parentheses are clustered at the individual levels. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Appendix A

Table A1
Mean differences before and after matching.

Outcome:		Financial satisfaction		Health satisfaction		Relationship satisfaction		Life satisfaction	
		Treated	Control	Treated	Control	Treated	Control	Treated	Control
Unmatched	2012	46.01	60.44	48.43	58.54	65.06	70.65	54.61	63.48
	2014	45.26	58.46	48.61	57.32	64.12	68.68	54.54	62.34
	Diff.	-0.75	-1.98	0.18	-1.22	-0.94	-1.97	-0.07	-1.14
K (*b*=0.01)	2012	46.04	55.91	48.46	51.46	65.20	67.33	54.65	58.68
	2014	45.25	52.79	48.60	49.44	64.18	64.67	54.53	57.49
	Diff.	-0.79	-3.12	0.14	-2.02	-1.02	-2.66	-0.12	-1.19
K (*b*=0.005)	2012	46.04	55.85	48.46	51.42	65.20	67.42	54.65	58.67
	2014	45.25	52.62	48.60	49.25	64.18	64.80	54.53	57.52
	Diff.	-0.79	-3.23	0.14	-2.17	-1.02	-2.62	-0.12	-1.15
NN (*m*=2, δ=0.01)	2012	46.04	55.88	48.46	52.05	65.20	68.08	54.65	59.30
	2014	45.25	52.88	48.60	49.09	64.18	65.48	54.53	57.72
	Diff.	-0.79	-3.00	0.14	-2.96	-1.02	-2.60	-0.12	-1.58
NN (*m*=5, δ=0.01)	2012	46.04	56.16	48.46	51.64	65.20	67.52	54.65	58.97
	2014	45.25	52.74	48.60	49.14	64.18	64.70	54.53	57.44
	Diff.	-0.79	-3.42	0.14	-2.50	-1.02	-2.82	-0.12	-1.53

Notes: Matching methods; *K*, kernel; *NN*, nearest-neighbor. *b* refers to bandwidth for kernel matching. *m* refers to the number of nearest neighbor matches involved. δ refers to the size of caliper.