Revisiting Public Income Replacing Private Transfers: A Regression Discontinuity Design

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Abstract

I estimate the extent that a provision of a public pension scheme displaces, or 'crowds out', remittance that the elderly recipients originally collected from non-cohabitating family members or other private sources. I explore the introduction of the Farmer's Pension Program (FPP) of Taiwan, using an estimation strategy based on a regression-discontinuity (RD) design with specification errors. The results suggest that a dollar of the FPP benefit has displaced around 31 to 39 cents of private transfers. Despite the moderate level of pension benefit and the significant crowding out, the pension appears to increase recipients' medical expense and utilization of medical service.

Key Words: Private transfers; Crowding out; public pensions; farmers

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1. Introduction

An important general issue in public economics concerns the unanticipated private responses to public policy interventions. Figuring out the often-unpredicted private responses primarily constitutes the core of policy evaluation, as ignoring those responses leads to under- or over-statement of policy outcomes. Given the aging of the population in developed and developing countries alike, policies relating to the design and reform of public pension programs are prominent in policy debates.[3] Studying the effects of reforms in these countries not only sheds light on private responses to the introduction of public pension programs, of relevance to the general issue in public economics of how public and private sector responses interact. Lessons from these experiences are also exceptionally relevant to emerging economies, such as China and India, where adoptions of similar types of public programs seem highly likely in the near future.

Previous studies have attempted to measure the extent to which a provision of an old-age pension program induced 'crowding out' of transfers made by non-coresiding family members to the elderly. The main question of interest is: when an elderly pensioner receives a dollar of social security benefit financed by general government revenues, will his/her income go up by the full amount? A larger crowding out implies a smaller net benefit effectively delivered to the pensioners, and the intended policy impact is entirely neutralized in the case of a complete (dollar to dollar) crowding out.

In this paper, I apply a regression discontinuity (RD) design to refining the assessment of the extent of such a crowding-out effect as an extension of previous studies that mostly

[3] In the US context, for example, there were discussions of private consequences of privatizing the state-provided Social Security, focusing on its impacts on risk management and savings (Feldstein and Ranguelova, 2001; Enger and Gale, 1997), retirement decisions (Orszag and Stiglitz, 1999), and intergenerational redistribution (Mulligan, 2000). In many developing countries, the reverse issue is pertinent. Many middle-income countries such as Taiwan and South Korea have already taken steps to replace traditional family-based support for the elderly with state-funded programs, to date targeted at groups such as civil servants, low-income households, and overall population.

rely on other empirical approaches.[4] While many previous studies explore the correlation between incomes and interhousehold private transfers,[5] only a few directly investigate whether public cash transfers are associated with a reduction in private transfers.[6] While data and identification strategies differ across these studies, it is common that endogeneity of different forms potentially constitutes a concern. For example, as pointed out by Juarez (2009), income is likely to be endogenous because individuals, in response to a provision of public income, would adjust their income by working less. Applying the instrumental variables (IV) strategy, she finds that this endogeneity is likely to be a significant concern, as suggested by the IV estimate being much larger than the OLS estimate. Jensen (2003), for another example, exploits a reform of the Old Age Pension (OAP) program in South Africa – a means-tested scheme where benefits are determined by the beneficiaries' income and employment histories.[7] The same data and experiment have been explored by Maitra and Ray (2003), who conducted a formal test with a result showing that the endogeneity of the OAP payment cannot be rejected.

A few studies, such as Jansen (2003) and Fan (2009), address the endogeneity issue using the difference-in-difference (DD) method. One limitation of this method is that the

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[4] The primary advantage of regression-discontinuity (RD) design is that, when the data generation process (DGP) produces a discontinuity in the forcing variable at the threshold point, the requirements for an application of a RD design to be valid can be less stringent for points around the threshold – it demands that individuals are unable to precisely manipulate, though still have some influence on, the forcing variable.

[5] This refers to a set of studies, such as Cox (1987) and Altonji et al. (1992 and 1997), that examine the motivations that drive private transfers by testing the 'altruism hypothesis' against the 'self-interest (or exchange) hypothesis'.

[6] The results from these studies are generally mixed, offering estimates of crowding out that vary in a wide scope Also, studies based on data from low or middle-income countries suggest stronger crowding out than those based on data for developed countries. Cox and Jimenez (1992) estimate the effect of the social security benefits in Peru on private transfers and find crowded out about 20 percent of private transfers. Jensen (2003) examines the Old Age Pension program of South Africa in 1987, and finds that a dollar of public income displaced around 25 to 30 cents of remittance. Using the same data, Maitra and Ray (2003) apply the 3SLS method and find evidence of crowding out only for poor households. Juarez (2009) uses Mexican data and finds crowding out as large as 87 percent based on an IV strategy, which is much larger than the corresponding OLS estimate. Using U.S. data, Cox and Jakubson (1995) investigate the anti-poverty effectiveness of public transfers while the private-transfer responses are taken into account. They find no evidence indicating that public transfers supplant private ones to the elderly. In research on the UK, Ward-Batts (2000) explores the responses of single-mother households to an increase in Child Benefit and finds no evidence of crowding out.

[7] This endogeneity problem might be less of a major concern in the case of OAP, as argued by the author, as it provided a nearly universal entitlement for African households with an age-qualified member.

reliability of estimation crucially depends on the stringent validity requirements that the treatment and control groups adopted by researchers often fail to fully satisfy.[8] In practice, it is potentially difficult to find a control group that is ideal for netting out the effects from other policies.[9]

Using a RD design, in this paper I estimate the crowding-out effect by revisiting Farmers' Pension Program (FPP) in Taiwan. Using FPP as the policy experiment provides a unique opportunity to apply RD design to assessing the crowding out, because its primary eligibility rule was an age threshold at 65 years, leading to a noticeable discontinuity in the likelihood of receiving the FPP payment between individuals who just turn 65 years old and those who are just about to turn 65. A RD design can therefore be used to measure the policy effect of FPP without invoking any particular control group needed for the DD strategy. Further, the experiment of FPP allows estimating the crowding-out effect using the two-stage least squares (2SLS) method and the results can be compared to the RD estimates. Combined, they provide a boarder perspective of the policy effect.

An additional advantage of exploring FPP is that its benefit level is designed to be fiscal friendly, thus is more likely to be replicated in other developing countries contemplating providing public cash transfers to finance the need for the elderly living in poor households. Farmers also demand special attention for transition economies where agricultural production is losing its ground, leaving elderly farmers in poverty. Therefore, a more profound understanding about the behavioral and welfare implications of a fiscally

[8] Specifically, the treatment needs to be exogenously determined so self-selection into the treatment group cannot be exercised. Further, for a control group to be valid to mimic the counterfactual outcome facing the treatment group at the absence of the treatment, it needs to be exempted from the effect of the treatment or exposed to a weaker degree of the treatment effect so the differential in the effect remains significant enough to be identified. Also, it is necessary to meet the requirement that all other influential factors are common to the treatment and control groups, and no other factors affect the outcomes of the two groups in a similar way as the treatment does.

[9] The general problem of inappropriately chosen control groups has been highlighted by Lemieux and Milligan (2008), who find Canadian evidence suggesting that more generous social assistance benefits reduce employment, and the commonly used DD estimators may perform poorly when inappropriate control groups are adopted.

feasible scheme, such as Taiwan's FPP, should be of great importance for policy makers in the middle and low income countries.

Findings from this paper suggest that a dollar of FPP payments displaced 31 to 39 cents of remittance that would otherwise be received by the FPP pensioners, while the IV strategy proposed by Juarez (2009) performs poorly as the IV formed by eligibility rules is fairly weak. The RD estimates are highly consistent with the figures, 30-39 cents, obtained by Fan (2010) whose estimations rely on the DD method. However, it is important to note the possibility that the RD estimates are understated, as other old-age programs seem to induce farmers to terminate cohabitating with adult children when they turn 65 years old, leading to an increase in remittance given to the elderly farmers. Despite the moderate pension benefit and the significant crowding out, the pension appears to increase recipients' medical expense and utilization of medical services. These health implications of FPP suggest that a fiscally friendly pension program with minor benefit level can still lead to welfare improvements for the pensioners – an encouraging news for developing countries in the urge of addressing poverty among the elderly.

2. Background

<u>The Farmers' Pension Program in Taiwan</u>

Up to early 1990s, around 70 percent of individuals aged 65 years or over were not covered by any pension program in Taiwan. The FPP represented the first government initiative provide a public pension scheme targeting elderly farmers. Commencing in 1995, the program granted all eligible farmers a monthly stipend of NT$3,000.[10] Amendments to the Agricultural Development Act were passed to increase the benefit level to NT$4,000 in 2004, and further up to $5,000 in 2006.

[10] This is equivalent to about US$111 based on the exchange rate in 1995.

The scheme started with means-tested restrictions on both income and wealth, which were later completely removed in a reform in 1999. Thereafter, the entitlement to the pension is confined to those who (i) had reached the age of 65; (ii) had been covered by the Farmers' Health Insurance (FHI) program for more than six months; and (iii) were not receiving any other old-age pensions from social insurance, living allowances, or other types of government assistance. Fan (2010) offers a detailed discussion on the remaining three eligibility requirements and concludes that self-selection is unlikely to form a major concern for years after 1999.[11]

Compared to recipients' income level, the amount of FPP benefit is far from being generous. For years 1999 to 2006, as suggested by Table 1, the average annual FPP benefit received by farmers was $47,646, accounting for around 8 percent of average household income. This benefit level is particularly small if compared to the case of South Africa's Old Age Pension, of which the maximum monthly payment was greater than twice the median per capita monthly household income among Africans.

In this paper, I exploit the 1999-2006 data to carry out the estimations, with observations of years 1995 to 1998 excluded from the sample although these individuals were interviewed post-intervention. These data are dropped because they lack information about a key variable, individuals' birth month, in the data of these years.[12] For a farmer who just turns 65 years old, birth month determines the total amount of the FPP benefit that, in the beginning year, a newly eligible farmer is entitled to. Thus, for the application of a RD design to be valid, the treatment for the fresh beneficiaries needs to be adjusted to accommodate the various degrees of being treated. A natural way to do so is to assume that the treatment for a

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[11] The main reasons are that there are stringent limitations in acquiring the farmer status for an elderly non-farmer, and other old-age social security payments are typically more generous than FPP. Also, statistics show that the proportion of FHI insurants in the population aged 65 or over remained virtually constant from 1990 to 200. As the FHI coverage is a pre-requisite for the FPP entitlement, this finding suggests that it is unlikely that the introduction of FPP had induced mass transitions between the farmer group and the non-farmer group.

[12] Information about individuals' birthdays is absent in the data.

new beneficiary is proportional to the eligible months in his/her beginning year. Later in Section 4, I will introduce the formula used to make such an adjustment.

Data

The data to be used in this paper are drawn from the *Taiwan Household Income and Expenditure Survey* (THIES), which is conducted annually since 1976. For the purpose of this project, I will focus on data from 1995 to 2006, and use 1990-2004 data for statistical comparisons. The THIES data are nationally representative and the sample entails 13,600 to 16,000 households each year. The data detail different sources of income at the household level, including interhousehold private transfers, the FPP benefit, and payments from other public schemes.

Since both private transfers and FPP benefits are measured at the household level, but the identity of farmer is recorded at the individual level, it is necessary to define the treatment at the household level. In this case, the sample is restricted to households with at least one farmer as a household member. The forcing variable, therefore, refers to the age of the oldest farmer in the household.

3. Methodologies – RD with specification errors

The primary task of this paper is to carry out the RD estimation of the effect of FPP benefits on displacing private transfers that would otherwise be given to the pension beneficiaries. Consider the following equation:

$$PT_i = \alpha + \tau D_i + \delta X_i + u_i \tag{1.1}$$

where PT_i refers to private transfers received by for farmer's household i; D_i is a dummy variable indicating whether the household is treated (equal to 1 if receiving FPP benefits) or not (equal to 0 if not receiving FPP benefits); X_i is a vector of baseline covariates; and u_i is the error term. The OLS estimation of Equation (1.1) may provide a biased estimate of τ

because ε_i may include components, such as preferences, which are correlated with D_i. In this paper, we try to mitigate this problem using a RD design.[13]

The basic idea of the RD design is to utilize an exogenous force that produces a discontinuity in the probability of receiving the treatment between individuals who are located in just above the threshold and those who are located in just below the threshold point. If individuals are unable to precisely manipulate the forcing variable, the discontinuous jump in the outcome can be reasonably attributed to the causal effect of the treatment (Lee and Lemieux, 2010). Under the continuity assumption,[14] the average treatment effect can be written as:

$$\tau = \lim_{c \downarrow \bar{c}} E(PT \mid C = \bar{c}) - \lim_{c \uparrow \bar{c}} E(PT \mid C = \bar{c}) \tag{1.2}$$

where C refers to the forcing variable (in our case, the age); c is a certain value of C; and $\bar{c}$ is the cutoff point, i.e., the age of 65 years. in the case of FPP, I estimate the following regression:

$$\begin{aligned} PT_i &= \alpha + \tau D_i + k(C_i) + \delta X_i + u_i \\ D_i &= 1\{C_i + u_i \geq 0\} \end{aligned} \tag{1.3}$$

where C_i is the age of the oldest farmer of household i, k is a flexible function of C_i, which can be a vector of high-order polynomial terms, and $\bar{c}$ is the cutoff age. The second line of equation (1.3) indicates the eligibility rule: if the oldest farmer's age is equal to or greater than $\bar{c}$, he or she will claim the benefit; otherwise, he or she does not.

When the forcing variable does not relate to the treatment receipt in a deterministic way (u_i is a stochastic variable), which is the case of farmers receiving the FPP benefits, we have a 'fuzzy' RD design, and the OLS estimator of τ in equation (1.3) is biased. In this paper, I

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[13] Note that the estimate of τ does not represent the crowding-out effect directly. However, the average crowding-out effect can be derived from it using the average FPP benefits received by farmers' households.

[14] For the RD design to be legitimate for estimating the treatment effect, it is necessary to assume that all factors other than the treatment and outcome variables are continuous with respect to the forcing variable.

adapt the IV strategy to estimate the treatment effect for a group of farmers whose treatment participation is induced by their eligibility statuses, which is proposed as a natural candidate for the instrument by Hahn (2001) and Lee and Lemieux (2010).

The RD design also needs to be adapted to accommodate the limitation due to the discrete forcing variable. Since the data only allow distinguishing whether people are age 64 or age 65 on the date of the survey, there is no direct information for a comparison between farmers who 'just turned 65' and farmers who 'just about to turn 65'. Because of the discrete nature of the way that individuals' ages are recorded in the data set, it is important that the regression approach is able to extrapolate that data to where the discontinuity would precisely be. To this end, a method proposed by Lee and Card (2007) is employed in this paper to deal with the errors in the RD designs caused by discrete covariates. Specifically, equation (1.3) can be re-written as:

$$PT_{ia} = \alpha + \tau D_{ia} + k(C_i) + \delta X_{ia} + u_{ia} \tag{1.4}$$

where the subscript *a* denotes a certain age. Following the strategy proposed by Lemieux and Milligan (2008), all the information at the individual level can be summarized in the age-specific means of the variables, and equation (1.4) reduces to:

$$PT_a = \alpha + \tau D_a + k(C) + \delta X_a + u_a \tag{1.5}$$

Lemieux and Milligan (2008) argue that estimates of equation (1.4) at the *i* level are the same as weighted estimates of equation (1.5) at the age level if the number of observations in each age group is used as the weights.

A few considerations are important for implementing the estimation of equation (1.5). First, a fuzzy design should be considered due to the existence of non-compliers. The standard procedure to address the fuzziness is to apply an IV, for which a natural candidate is the eligibility determined by an individual's age being equal to or higher than 65.[15] Throughout this paper, this instrumental variable is denoted by '*eligible*'.[16] Second, in estimating equation (1.5), I control for no baseline covariates X as it is theoretically unnecessary to impose any such variable in the regressions if the discontinuity in the treatment variable at the threshold point is exogenously determined. Third, the traditional way to determine the bandwidth for the sample to be used for the RD estimation is to apply the cross-validation (CV) method.[17] Given the discrete nature of the forcing variable in this paper, however, an alternative way is to examine the robustness of the RD estimates to a range of different bandwidths. I adopt this strategy in this paper.

Finally, a potential threat to the validity of the RD estimates from equation (1.5) lies in the permanent income hypothesis (PIH), which predicts that consumption and relevant decisions are determined by a change in permanent income, rather than change in temporary income. In the case of Taiwan's FPP, it is then possible that the introduction of FPP not only crowded out the private transfers received by farmers age 65 or older, but also reduced those received by younger farmers, particularly those approaching the age of 65, through boosting their permanent income. If so, the crowding-out effect estimated by equation (1.5) would be downward-biased. To examine the extent of this potential risk, in Figure A1 in the Appendix I demonstrate the average private transfers received by farmers aged 60-64 across the pre- and post-intervention years. The graph shows no sign of a clear discontinuity at year1995,

[15] Hahn, Todd and van der Klaauw (2001) show that the standard RD fuzzy design estimator based on discontinuities is numerically equivalent to an IV estimator using the eligibility as the exclusive instrumental variable.

[16] The IV estimate can be interpreted as the average treatment effect (ATE) for the entire population only if the treatment effect is homogeneous across subpopulations of various compliance types. Otherwise it can be interpreted as a weighted local average treatment effect (LATE) for the compliers.

[17] The cross-validation function, as proposed by Lee and Lemieux (2010), is specified as $CV_Y(h) = \frac{1}{N}\sum_{i=1}^{N}(Y_i - \hat{Y}_i(X_i))^2$ where $\hat{Y}_i(X_i)$ is the predicted value of Y_i based on a regression using observations with a bin of width *h* on either the left (for observations on left of the cutoff) or the right (for observations on the right of the cutoff) of observation *i*, but not including observation *i* itself.

though the average private transfers fluctuate over the years. Thus, the potential bias caused by the permanent income effect is most likely to be limited.

2SLS

The experiment of FPP also allows for estimating the crowding-out effect using both ordinary least squares (OLS) and two-stage least squares (2SLS) methods. Comparisons of these results and the RD estimates are intriguing because they will make a substantial contribution to the debate on whether OLS results are subject to a certain degree of bias due to self-selection. They will also shed light on whether the IV strategy, as employed by Juarez (2009), can properly address the endogeneity issue in the case of Taiwan's FPP. Further, these results can also be weighed against the difference-in-difference (DD) estimates of the crowding-out effect by Fan (2010).

Following the IV strategy proposed by Juarez (2009), a 2SLS estimation can be carried out by estimating the following two-stage system:

$$\begin{aligned} PT_i &= \alpha + \tau Y_i + \delta Z_i + v_i \\ Y_i &= \alpha' + \gamma \cdot eligible_i + \delta' Z_i + \rho_i \end{aligned} \tag{1.6}$$

where Y_i denotes total income (including the FPP benefits) for household *i* with private transfers excluded; Z_i is a vector of control variables; *eligible* is the same as defined in equation (1.5), and in the system it is used as the exclusive instrumental variable; v_i is the error term. To the extent that *eligible* meets the requirements for a valid IV, $\hat{\tau}_{2SLS}$ captures the crowding-out effect. As the dependent variable PT_i has is left-censored at zero, in this paper the 2SLS estimations are carried out using both a linear model and the Tobit model.[18]

This IV strategy described by equation (1.6), as argued by Juarez (2009), has the following two advantages. First, it helps address selection biases caused by potentially

[18] I use the maximum likelihood estimation to obtain the IV Tobit estimator. In addition, I also estimate the Newey's efficient estimator using the two-step procedure proposed by Newey (1987).

endogenous income variable. Second, instead of estimating the effect of a particular program, it generates estimates that capture the causal effect of income on private transfers.

4. Results

Summary statistics

Table 1 reports the summary statistics for farmers aged 65 or older in years 1990-1994 and in years 1999-2006 from outside of the household. The main dependent variable used in this study is annual private transfers received by these farmers. On average, the amount of private transfers is slightly lower for years 1999-2006 than that for years 1990-1994. The average FPP benefit was NT$47,646 for years 1999-2006, which is smaller than total private transfer, NT$76,113, received in the same period. Compared to farmers in 1990-1994, farmers in the post-1999 sample are also slightly higher in labor force participation, are less educated, more likely to live alone or with spouse only, to live in a smaller household, and to be the household head.

Figure 2 presents the average FPP benefit received by the households across the age of the oldest farmer in the household. The data used here are drawn from the Taiwan Household Income and Expenditure Survey (THIES), 1990-2006, with the pre- and post-1999 data being used separately. Part A suggests households with at least one eligible farmer (that is, farmers aged 65 or over) received positive amount of the pension after 1999, while households with only younger farmers claim no benefit.[19] It is important to note that the benefit level is particularly low at the age of 65, if compared to the average benefits for older ages. This reflects a limitation of the THIES data because the respondents were asked to report the annual FPP benefits they received in the corresponding year. Thus, farmers who turn 65 in the middle of the year are entitled only to a prorated amount of the FPP benefit that is

[19] The fitted line is graphed by carrying out a locally weighted regression of y variable (FPP benefit associated with each age) on the x variable (age).

proportional to the eligible days of the year. Given that only individuals' birth months, not birth days, are available in the data, my strategy for adjusting the treatment for 65 years old farmers is to average the degrees of eligibility, denoted by $eligible_i$ as the IV for the treatment, over all individuals of a certain birth month these using the following formula:

$$\begin{aligned} eligible_i &= 0, \quad \text{if } age \le 64 \\ &= (12.5 - birthm_i)/12, \quad \text{if } age = 65 \\ &= 1, \quad \text{if } age \ge 66 \end{aligned} \tag{1.7}$$

where $birthm_i = 1,\ 2, \ldots\ldots,\ 12$. To apply this formula, I assume that individuals' birthdays are uniformly distributed within any month, so on average individuals born in a certain month received only half monthly payment from FPP for the beginning month. To examine the validity of formula (1.7), in Figure 1 I demonstrate the month distributions of annual FPP benefits received by households with the oldest farmers being 64, 65, and 66 years old, using 1999-2006 data. It is obvious that the average benefit for the age 65 (represented by the red curve) declines along the birth month to the right, and the curve tightly follow a straight line (the dash black line) that describes the FPP benefit predicted by formula (1.7). Meanwhile, the curves for the other two ages exhibit no such a declining trend, implying that farmers aged 66, despite their birth months, reported a full annual FPP payment in the survey, and farmers aged 64 acquired no entitlement just yet.

Going back to Figure 2, in part A the average benefit appears to be increasing in age for ages over 65. This is mainly because the FPP benefit is measured at the household level and it is common that a household has more than one farmer. The increasing trend directly reflects the nature that the number of eligible farmers in a household increases with the age of the oldest farmer in that household. Part B of figure 2 simply suggests that the benefit was not available before 1995 when the FPP has yet been introduced.

The story told by Figure 2 is corroborated by Figure 3, which shows the probability of receiving FPP benefit for the same sample. Again, the results using post-1999 data illustrate an abrupt jump in the likelihood at the age of 65, followed by a further increase at the age of 66.

The primary outcome variable of interest is the inter-household transfers. Figure 4 demonstrates the average amount of private transfers received by the households, again using data from 1999-2006 years (part A) and 1990-1994 years (part B) separately. Part A shows a steadily increasing trend of the average benefit amount for the ages of 55 to 64. The trend exhibits an evident drop from age 64 to age 66, with the value for age 65 lying between the values of its two adjacent years. This drop, potentially implying the possibility of the crowding-out effect, does not appear in part B where the pre-1995 data are used, though the average value seems to be jumpy for ages over 65.

In Figure 5, I display the incident of receiving a positive amount of inter-household transfers for households in the same sample. In contrast to Figure 4, both parts A and B in Figure 5 show no clear discontinuity in the incidence around the age of 65. Combined with finding of the sharp decline in the FPP benefit around age 65, the fact that the incidence is smooth over the cutoff point implies that the private transfers received by the FPP beneficiaries were mostly reduced, but rarely completely ceased, due to the provision of FPP.

Validity tests

I begin with validity tests for the RD design by examining two implicit features of the underlying assumption for RD designs. First, if individuals do not have precise control over the forcing variable in the neighbourhood of the cutoff point, the density of the forcing variable should not exhibit any discontinuity at the cutoff. A jump in the density at the cutoff implies some degree of sorting around the threshold, such as non-farmers registering themselves as farmers as to claim the FPP benefit, and should cast doubt on the reliability of the RD design (Lee and Lemieux, 2010). Second, the mean values of the observable characteristics in relation to the outcome variable should be continuous and smooth through the cutoff. Any such discontinuity in these characteristics at the cutoff implies that at least some individuals with particular characteristics are able to self-select them into treatment.[20] In Figure 6 I graph the frequency distribution of the household oldest farmers aged 55 to 74 years, using post-1995 data. The graph exhibits no sign of discontinuity in the frequency distribution around age 65.[21]

For the second test, I follow Lee and Lemieux (2010) to estimate regressions (equation 1.5 in the paper) using six different observable characteristics as dependent variables to test whether their means are continuous through the cutoff point. These variables refer to labor force participation, years of schooling, gender, years of schooling, household head status, household size, and living arrangement.[22] The estimation results are summarized in Table 2, where none of the estimates of τ is statistically significant, suggesting that the means of all the six variables are smooth over the age of 65. However, the graphs tell a different story. In Figure 7 I present the means of the six characteristics for the oldest framers in the sample. While most variables appear to be smooth through the cutoff point, the likelihood of living alone or with spouse only shows a clear leap at age 65, and household size exhibits a drop at the threshold, though less obviously. While the reason for the discontinuities of the two variables remains unclear, one conjecture is that other low-income policies targeting at the elderly might be at play here. For example, individuals (farmers or non-farmers) aged 65 or older are eligible for applying for the Low Income Status (LIS), which provides deductions in health insurance premium and aids in medical expense for the status holders. Since LIS is means-tested in household income, it is plausible that the scheme would induce young family members who originally co-resided with low income elderly members to leave the household

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[20] See Lindo et al. (2010) and Carpenter and Dobkin (2009) for applications of this type of test.

[21] Please note that the test proposed by McCrary (2008), which is often used in RD papers, is not applicable for cases with discrete forcing variables.

[22] The variable for labor force participation refers to a dummy variable equating to 1 if the respondent's status is currently working regardless the hours worked; gender is a dummy variable equating to 1 for the males; household head status is a dummy variable equating to 1 if the respondent is the head of the household; living arrangement is a dummy variable equating to 1 if the farmer lives alone or with spouse only.

when an elderly member reaches age 65. As a result, the proportion of independently lived elderly increases and the household size decreases at the age of 65.

To the extent of these effects of other policies on elderly farmers' living arrangements, the RD estimates of crowding out using equation (1.5) are understated because it is most likely that elderly farmers would received more interhousehold transfers after a young member moves out of the household. In the case of the sample in this paper, the average private transfers received by farmers who live alone or with spouse only is NT$ 84,388, which is significantly higher than the amount, NT$ 59,642, received by those coresiding with other family members. Thus, to the extent of this possible bias caused by other policies, the crowding out estimates based on the RD design should be regarded as a lower bound.

The RD results

Table 3 summarizes the 2SLS results from estimating equation (1.5) using *eligible* as the instrumental variable and the oldest farmer's age minus 65 as the forcing variable. The IV estimator is equivalent to the fuzzy RD estimator, which can be interpreted as the Local Average Treatment Effect (LATE) of the provision of the FPP on private transfers. The estimations are carried out at the age level, and the mean values of both the dependent and independent variables of each age are used in the regressions, which are weighted by the number of observations of each age. For now, the bandwidth is set to be 10 years on both side of the cutoff, but the robustness of the estimated LATEs will be examined for narrower bandwidths from 9 to 7 years.

The IV results from estimating equation (4) are presented in Panel 1 of Table 3. To examine the robustness of the IV estimates, I present the results based on four different regression specifications. Column (1) shows that, when only the linear term of the forcing variable together with a linear spline variable (the interaction term of the forcing variable and *eligible*) are imposed in the regression specification, the LATE estimate is -0.31, which is negatively significant. This implies that a dollar of the FPP benefit provided to farmers aged 65 or over displaces 31 cents of remittances that these farmers would otherwise receive from private resources. Columns (2) to (4) suggest that the estimated LATEs range from -0.31 to -0.39 when higher order forms of the forcing variable and the quadratic term of the spline variable are imposed in the regression.

It is important to note that, in Table 3, numbers reported in the parentheses are robust standard errors. As the point estimates of the LATE are obtained from regressions at the group (age) level, the standard errors need to take into account the cluster nature of these specifications. On this regard, Lee and Card (2008) show that non-clustered variance must be inflated, and robust standard errors are valid under some conditions (Dong, 2012).

The lower panel of Table 3 displays the results from the first stage results, using the same four functional specifications. In all the four cases, the results show that the instrument is very strong as suggested by both the significant coefficients of *eligible* and the large F values from testing on the coefficient of the instrument in the first stage regressions.

In Table A1 of the Appendix, I summarize the RD estimates based on narrower bandwidths. As suggested by the table, the estimate of crowding out increases to 0.43 when the bandwidth shrinks to 8 years on each side of the cutoff, then decreases to 0.23 when the bandwidth further shrinks to 6 years. These results imply that the RD estimate of the crowding out is relatively robust to narrower bandwidths.

In Table 4, I present the results from examining the effect of the provision of the FPP on the incidence that a farmer receives a positive amount of private transfers. The estimations are implemented by replacing the dependent variable of equation (1.5) with a dummy variable indicating receiving private transfers or not. Consistent with the story told by Figure 5, all the estimated LATEs on the incidence are small and insignificant. Together, the findings from Tables 3 and 4 suggest that, while the provision of FPP leads to a reduction of the remittance transferred to farmers aged 65 or above, it is uncommon that the private transfers were completely terminated due to the introduction of the scheme.

Health related outcomes

Since the RD estimates obtained from estimating equation (1.5) suggests that a dollar of FPP pension crowds out around 31 to 39 cents of private transfers, this implies that the provision of FPP still benefits the recipients financially, and thus would likely enhance their wellbeing. In this section, using the same RD design described by equation (1.5), I explore this possible welfare implication of FPP by estimating its effects on recipients' medical expense and utilization of medical services. The only relevant variables that can be retrieved from the THIES data are (1) number of doctor visits during the survey year (at the individual level), and (2) per capita household medical expense.

The results are summarized in Table 5, where the coefficient estimates represent the effects of a provision of NT$10,000 of the FPP pension. In column (1), the coefficient estimate of the FPP benefit suggests that NT$10,000 of the FPP pension leads to a significant increase in doctor visits by 0.44 time. This implies that an average FPP payment (that is, NT$47,646) would induce 2.1 times of doctor visits – equivalent to an 11 percent increase if compared to the average number of doctor visits (19.65) for the recipients. The estimate in column (2), moreover, suggests a significantly positive effect on household per capita medical expense. The corresponding estimate ($249) implies an increase of NT$1,185 due to the provision of FPP at its mean value, amounting to a 12 percent increase in per capita medical expense.

Both of the effects can be detected by graphical analyses. Figures 8 and 9 demonstrate the discontinuities in doctor visits and per capita medical expense, respectively. Both graphs show an evident discontinuity at the age of 65 for post-1999 years, but not for pre-1995 years.

The 2SLS results

As proposed by Juarez (2009), an alternative way to estimate the causal effect of FPP on private transfers is to carry out the estimation of the 2SLS model described in equation (1.6). In this paper I conduct the estimations using both the linear model and two IV Tobit models based on the Newey's two-step procedure. In all cases, '*eligible*' is adopted as the sole exclusive IV. The sample used here is identical to those used in the RD estimations, though the 2SLS estimations are performed at the household level, instead of the group level. The results are summarized in Table 6.

Columns (2) presents the estimates from the linear IV model, where the coefficient of household income suggests a crowding out of 2.2 dollars due to a dollar increase in household income caused by the provision of FPP. While this estimate appears to be unrealistically large, it is important to note that '*eligible*' servers as a weak instrumental variable here, as suggested by the small F-value (in the lower panel of the table) for testing the IV in the first-stage estimation. The weak-IV problem also happens to the estimation based on the two-step Tobit models, as suggested by the corresponding estimates and F-value displayed in column (3).[23] Given such a weak IV, potential biases of these 2SLS estimates constitute a serious concern.

While it is difficult to divulge why '*eligible*' fails to function as a strong IV here, one conjecture is that the average amount of the FPP payment is rather small relative to the large variation of household income. Recall Table 1 where the statistics summary is presented, the average FPP benefit is around NT$48,000 for years 1999-2006, which is fairly small if compared to the standard deviation (NT$484,000) of household income for the same period. For a comparison, the Mexican pension scheme (Nutrition Transfer for Senior Adults) analyzed by Juarez (2009), who employs the same IV model, offers recipients an average of

[23] At the bottom of columns (2) and (3) in Table 6 are Wald test results of the exogeneity of the instrumented variable. If the test statistic is not significant, there is no sufficient information in the sample to reject the null hypothesis of no endogeneity.

34 pesos, which is three times as large as the standard deviation of the household non-transferred income.[24]

Thus, to the extent that the speculation above is valid, the problem of weak IV highlights the advantage, in terms of applicability, of the RD design for estimating the crowding-out effect. For pension schemes such as Taiwan's FPP that offer moderate amounts of stipend that accounts for a small portion of recipients' income, the IV strategy might lose its ground as the increase in total income caused by the pension is out-weighted by variations from other income sources. In these cases, the RD design could provide more compelling evidence about the crowding out, when a discontinuity in the likelihood of receiving the pension is significant and can be readily detected.

5. Conclusions

This study explores the private responses to a provision of old age pension using the Farmers' Pension Program in Taiwan as the policy experiment. I focus on estimating the extent that the pension displaces interhousehold private transfers and the effects of the pension on recipients' utilization of medical services. The estimation strategy relies on a RD design, which exploits the discontinuity in the likelihood that farmers receive the pension payments when they turn 65 years old. The results indicate a substantial and significant crowding out – a dollar of pension replaces 31–42 cents of private transfers. On the other hand, the impact on the incidence that a farmer receives a positive amount of private transfers is minimal and insignificant. These results are qualitatively and quantitatively consistent with the findings from Fan (2010), who estimates the crowding-out effects using a difference-in-difference strategy with the same data. However, the graphical analyses show discontinuities in recipients' living arrangements and household size at the age of 65, plausibly caused by other

[24] Figures are drawn from Table 1 in Juarez (2009).

low income public policies. To the extent of these confounded effects, the RD estimates of crowding out are understated in the paper.

I have also estimated the crowding out using the IV strategy recently developed by Juarez (2009). While the resulting 2SLS estimates appear to be much larger than the RD estimates, the instrumental variable employed for the estimations performs very poorly as it has little explanatory power on total household income in the first stage estimations. Thus, the 2SLS estimates from this procedure may be subject to serious biases.

The comparisons of these three approaches suggest that each estimation method has its pros and cons, and the powers of these methods crucially depend on the designs of the pension programs that researchers are intended to analyze, as well as the nature of the available data used for the estimations. In the case of Taiwan's FPP, where a compelling IV is unavailable, the RD design gains its weight, especially given that the treatment in the neighborhood of the cutoff point can be measured in a relatively precise way.

Despite the finding of a large crowding out that implies a heavily discounted efficacy of the public pension scheme that aims to finance the need of elderly farmers, the provision of FPP appears to increase medical expense and doctor visits. Whether these effects imply an improvement of health conditions remains unknown and demands further research to better understand.

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Table 1: Statistics summary for elderly farmers aged 65 or over

	Years 1990-1994		Years 1999-2006	
	Mean	Std. Dev.	Mean	Std. Dev.
Proportion of males	0.78	0.41	0.75	0.43
Labor force participation	0.49	0.50	0.52	0.50
Years of schooling	4.89	4.59	4.39	3.71
Household size	3.73	2.26	2.70	1.89
Living arrangement[a]	0.33	0.47	0.67	0.47
Number of income makers	0.73	0.44	0.84	0.37
Household head	0.61	0.49	0.75	0.43
FPP benefit	0	0	47,646	28,904
Private transfers	78,372	128,364	76,113	83,211
Total household income	699,473	655,808	587,637	484,339
Per capita consumption	149,161	96,870	195,337	99,924
Per capita medical expense	16,711	13,682	10,973	23,726
Per capita household income	196,516	170,763	237,178	151,341
No. of observations	1,374		3,744	

Notes: (a) A dummy variable indicating that the respondent lives alone or with his/her sponse only. All money values are measured in terms of New Taiwan Dollars (NT$), and are deflated to dollar values in 2006.

Table 2: Testing the smoothness of means of observable characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Labor force part.[a]	Living arrangement[b]	Male	Year of schooling	Household head[c]	Household size
FPP benefit	-0.01	0.01	-0.00	0.04	0.00	-0.06
	(0.01)	(0.01)	(0.00)	(0.05)	(0.00)	(0.04)
age-65	-0.03***	0.02**	-0.00	-0.22***	-0.00	-0.08
	(0.01)	(0.01)	(0.01)	(0.04)	(0.01)	(0.06)
$(\text{age-65})^2$	-0.00	0.00	0.00	0.00	0.00	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
(age-65) x *eligible* [d]	0.00	-0.02	-0.00	0.06	-0.00	0.12
	(0.01)	(0.02)	(0.02)	(0.09)	(0.01)	(0.11)
Constant	0.69***	0.59***	0.76***	4.56***	0.74***	3.01***
	(0.02)	(0.03)	(0.02)	(0.11)	(0.02)	(0.17)
Observations	20	20	20	20	20	20

Notes: (a) A dummy variable indicating that the respondent is currently working (=1) or not (=0). (b) A dummy variable indicating that the respondent lives alone or with his/her sponse only (=1) or not (=0). (c) A dummy variable indicating that the respondent is the head of the household (=1) or not (=0). (d) A dummy variable indicating whether the respondent is 65 years old or older (=1). Regressions are carried out using mean values at the age level, weighted by the number of observations of each age.

Standard errors are in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Estimated crowding-out effects on private transfers received by farmers (RD design)

	(1)	(2)	(3)	(4)
TSLS results:				
FPP benefit	-0.31***	-0.31***	-0.39*	0.32
	(0.09)	(0.09)	(0.22)	(0.36)
age-65	4,003.03***	2,640.89	3,487.23	-9,572.59
	(606.43)	(1,622.23)	(2,125.50)	(7,558.19)
$(\text{age-65})^2$		-139.68	-58.99	-3,014.86
		(151.01)	(186.40)	(1,788.92)
$(\text{age-65})^3$				-185.04
				(112.59)
(age-65) x *eligible* [a]	-2,064.66*	602.37	953.45	-1,049.45
	(1,007.17)	(3,101.21)	(3,703.94)	(2,998.52)
$(\text{age-65})^2$ x *eligible*			-188.29	5,767.63
			(459.70)	(3,544.90)
Constant	82,262.65***	80,192.03***	81,769.04***	67,711.88***
	(2,023.71)	(3,121.92)	(4,393.49)	(7,721.35)
First stage results:				
eligible	43,217.56***	43,277.18***	36,805.04***	32,988.01***
	(2,272.47)	(1,903.97)	(1,901.18)	(3,026.73)
age-65	-86.59	-1,603.82***	183.84	2,074.56
	(95.39)	(429.05)	(238.58)	(1,192.06)
$(\text{age-65})^2$		-155.57***	15.25	445.92*
		(44.89)	(19.69)	(250.23)
$(\text{age-65})^3$				27.06
				(15.29)
(age-65) x *eligible*	1,715.07***	4,687.90***	4,628.88***	4,441.71***
	(330.73)	(938.84)	(550.38)	(523.96)
$(\text{age-65})^2$ x *eligible*			-344.34***	-1,179.59**
			(82.58)	(493.03)
Constant	-554.23	-2,861.18**	450.78	2,459.67
	(615.87)	(975.41)	(599.36)	(1,601.81)
F-test	261.8	516.7	374.1	118.8
Observations	20	20	20	20

Notes: (a) A dummy variable indicating whether the respondent is 65 years old or older (=1). Regressions are carried out using mean values at the age level, weighted by the number of observations of each age. The variable 'eligible' is used as the exclusive IV in the 2SLS estimations. All money values are measured in terms of New Taiwan Dollars (NT$), and are deflated to dollar values in 2006.

Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Estimated effects of FPP on the incidence of farmers receiving priviate transfers

	(1)	(2)	(3)	(4)
TSLS results:				
FPP benefit	0.03	0.03	0.11	0.15
	(0.42)	(0.39)	(0.94)	(1.97)
age-65	0.02***	0.01	0.01	0.01
	(0.00)	(0.01)	(0.01)	(0.03)
$(\text{age-65})^2$		-0.00	-0.00	-0.00
		(0.00)	(0.00)	(0.01)
$(\text{age-65})^3$				-0.00
				(0.00)
(age-65) x *eligible* [a]	-0.01***	-0.00	-0.00	-0.00
	(0.00)	(0.01)	(0.01)	(0.02)
$(\text{age-65})^2$ x *eligible*			0.00	0.00
			(0.00)	(0.01)
Constant	0.90***	0.89***	0.89***	0.89***
	(0.01)	(0.02)	(0.03)	(0.04)
First stage results:				
eligible $(\text{x}10^{-7})$	43,217.56***	43,277.18***	36,805.04***	32,988.01***
	(2,272.47)	(1,903.97)	(1,901.18)	(3,026.73)
age-65	-86.59	-1,603.82***	183.84	2,074.56
	(95.39)	(429.05)	(238.58)	(1,192.06)
$(\text{age-65})^2$		-155.57***	15.25	445.92*
		(44.89)	(19.69)	(250.23)
$(\text{age-65})^3$				27.06
				(15.29)
(age-65) x *eligible* $(\text{x}10^{-7})$	1,715.07***	4,687.90***	4,628.88***	4,441.71***
	(330.73)	(938.84)	(550.38)	(523.96)
$(\text{age-65})^2$ x *eligible* $(\text{x}10^{-7})$			-344.34***	-1,179.59**
			(82.58)	(493.03)
Constant	-554.23	-2,861.18**	450.78	2,459.67
	(615.87)	(975.41)	(599.36)	(1,601.81)
F-test	0.35	0.71	0.71	0.25
Observations	20	20	20	20

Notes: (a) A dummy variable indicating whether the respondent is 65 years old or older (=1). For individuals aged 65, this variable is adjusted by their birth months using equation (1.7) in the paper. Regressions are carried out using mean values at the age level, weighted by the number of observations of each age. The variable 'eligible' is used as the exclusive IV in the 2SLS estimations. All money values are measured in terms of New Taiwan Dollars (NT$), and are deflated to dollar values in 2006.

Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Effects of $10,000 of FPP on medical service utilization and expense

	(1)	(2)	(3)
	Doctor visits	Per capita medical expense	Per capita consumption
sample average	19.65	9,719	200,292
FPP benefit	0.44***	248.65**	1,406.06
	(0.14)	(116.18)	(1,233.61)
age-65	0.56**	150.98	-2,004.67
	(0.21)	(163.10)	(1,289.16)
$(age-65)^2$	0.03	-4.31	107.69
	(0.02)	(16.36)	(120.33)
(age-65) x *eligible* [c]	-0.34	-47.38	-584.98
	(0.39)	(326.00)	(2,354.07)
Constant	17.03***	7,494.51***	190,701.43***
	(0.47)	(464.01)	(4,398.41)
Observations	20	20	20

Notes: (a) A dummy variable indicating that the respondent is currently working (=1) or not (=0). (b) A dummy variable indicating that the respondent lives alone or with his/her sponse only (=1). (c) A dummy variable indicating whether the respondent is 65 years old or older (=1). Regressions are carried out using mean values at the age level, weighted by the number of observations of each age.

Standard errors are in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

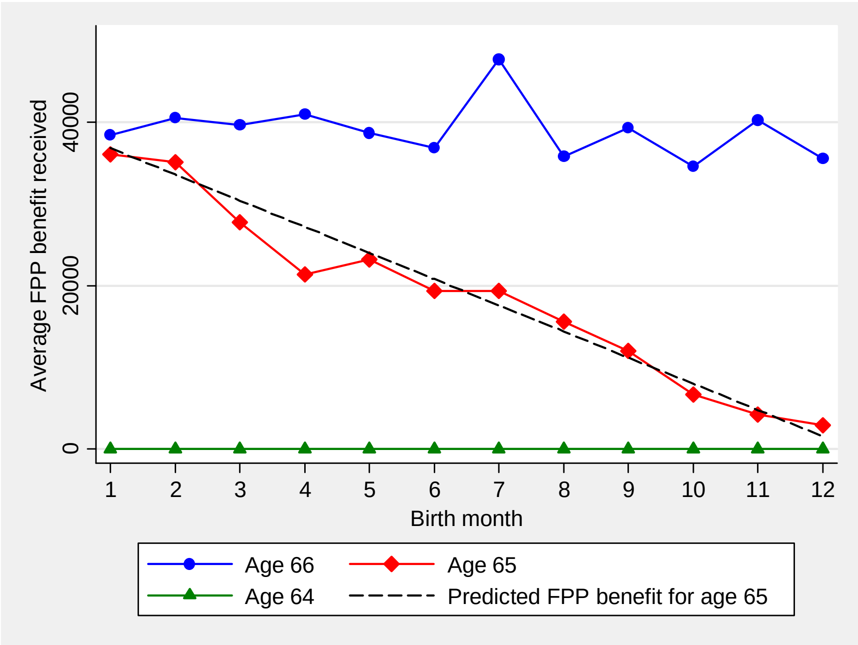
Table 6: Estimated crowding-out effects on priviate transfers received by farmers (OLS and 2SLS)

	OLS	2SLS (linear)	2SLS (two-step)
	(1)	(2)	(3)
TSLS results:			
household income	-0.022***	-2.193	-2.286
	(0.005)	(7.073)	(8.992)
age	87	-20,845	-21,315
	(210)	(68,008)	(86,771)
sex	8,732***	-241,297	-251,861
	(2,759)	(815,975)	(1,036,503)
Years of schooling	1,724***	54,347	56,482
	(330)	(171,887)	(218,062)
household size	-2,640***	390,860	406,878
	(893)	(1,282,695)	(1,630,335)
Constant	95,131***	1,420,051	1,447,447
	(14,691)	(4,304,837)	(5,492,884)
First stage results:			
eligible [a]		6,124	6,124
		(24,329)	(24,329)
age		-10,126***	-10,126***
		(2,278)	(2,278)
sex		-115,195***	-115,195***
		(11,594)	(11,594)
Years of schooling		24,235***	24,235***
		(1,837)	(1,837)
household size		181,327***	181,327***
		(3,063)	(3,063)
Constant		638,500***	638,500***
		(141,678)	(141,678)
F-test for IV		0.0625	0.0625
Wald test of exogeneity			p-value = 0.003
Observations	6,014	6,014	6,014

Notes: (a) A dummy variable indicating whether the respondent is 65 years old or older. For individuals aged 65, this variable is adjusted by their birth months using equation (1.7) in the paper. Regressions are carried out using mean values at the household level. Other ovariates controlled in the regressions are a full set of county dummies. The variable 'eligible' is used as the exclusive IV in the 2SLS estimations. All money values are measured in terms of New Taiwan Dollars (NT$), and are deflated to dollar values in 2006.

Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%

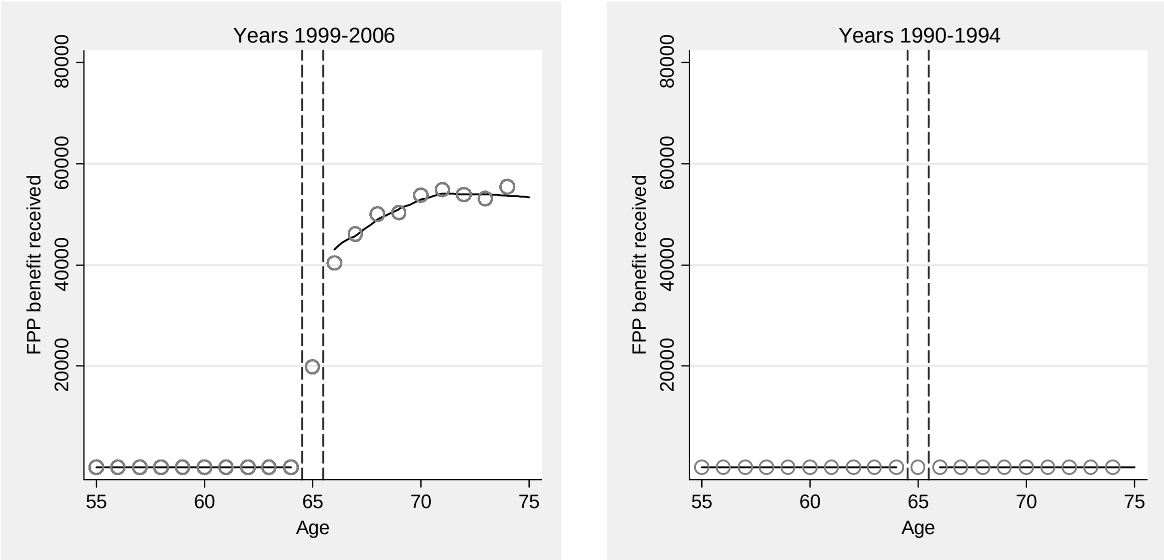
Figure 1: Comparing average annual FPP benefit received by farmers across birth months



Data source: Taiwan Household Income and Expenditure Survey, 1999-2006.

Figure 2: Average farmer's pension received by farmers

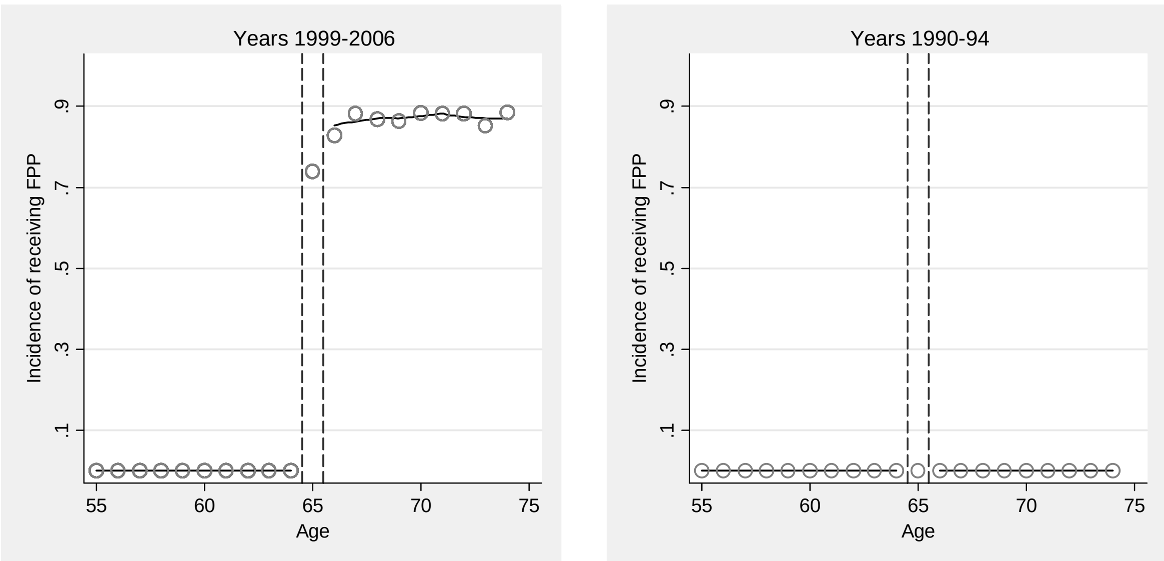
A B



Data source: Taiwan Household Income and Expenditure Survey, 1990-2006.

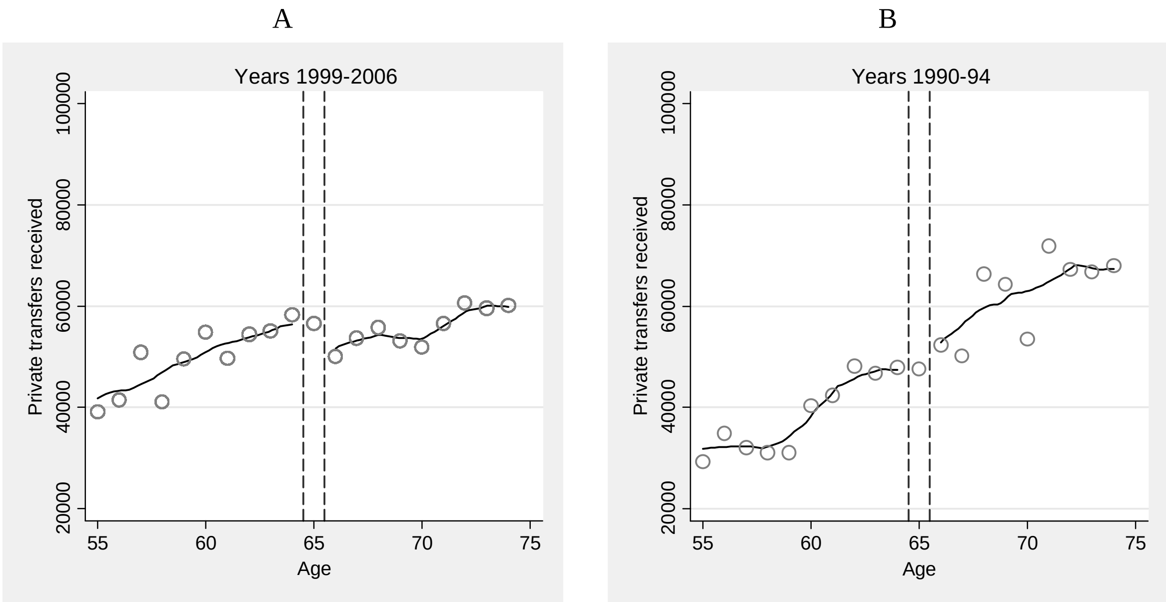
Figure 3: Incident of receiving farmer's pension for farmers

A B



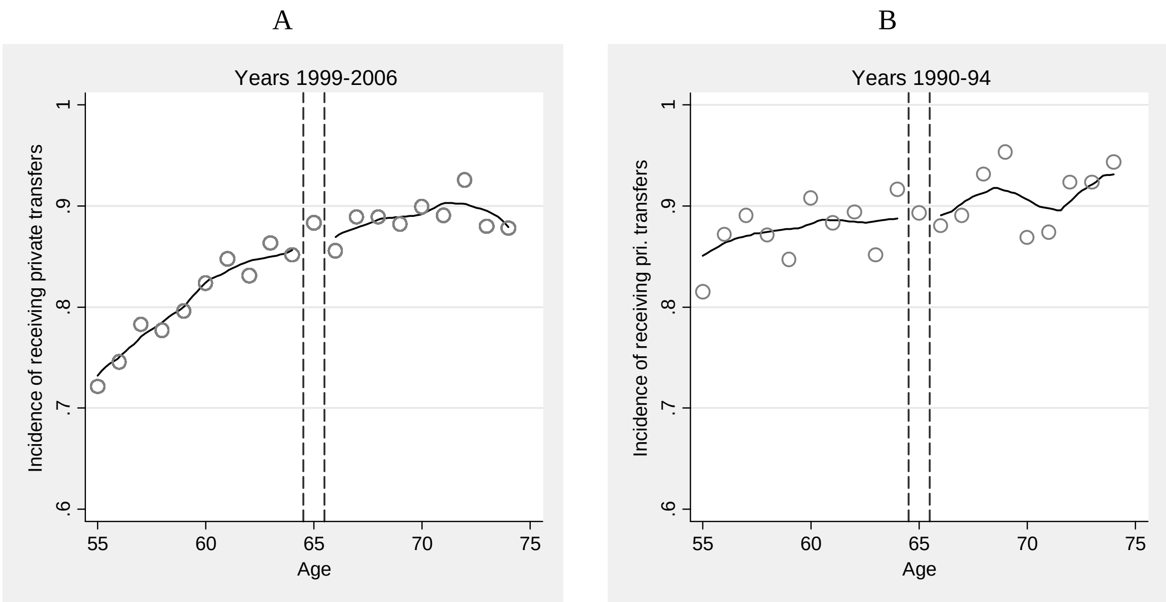
Data source: Taiwan Household Income and Expenditure Survey, 1990-2006.

Figure 4: Average private transfers received by farmers



Data source: Taiwan Household Income and Expenditure Survey, 1990-2006.

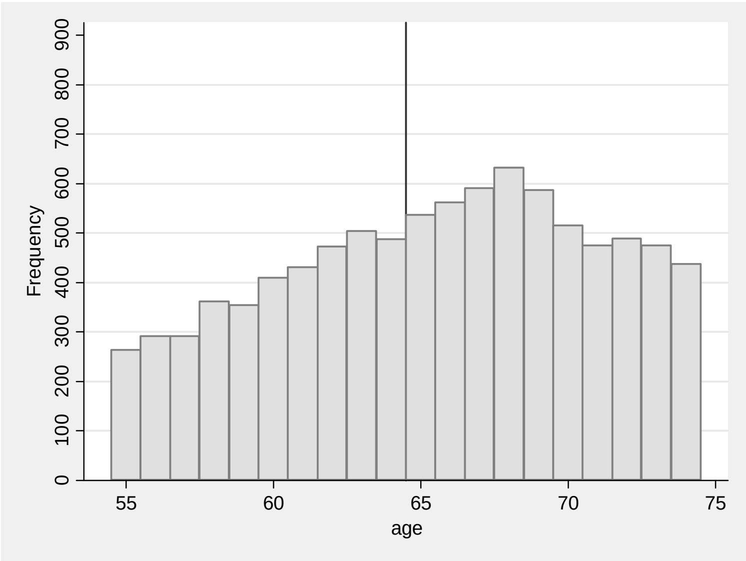
Figure 5: Incident of farmers receiving private transfers



Data source: Taiwan Household Income and Expenditure Survey, 1990-2006.

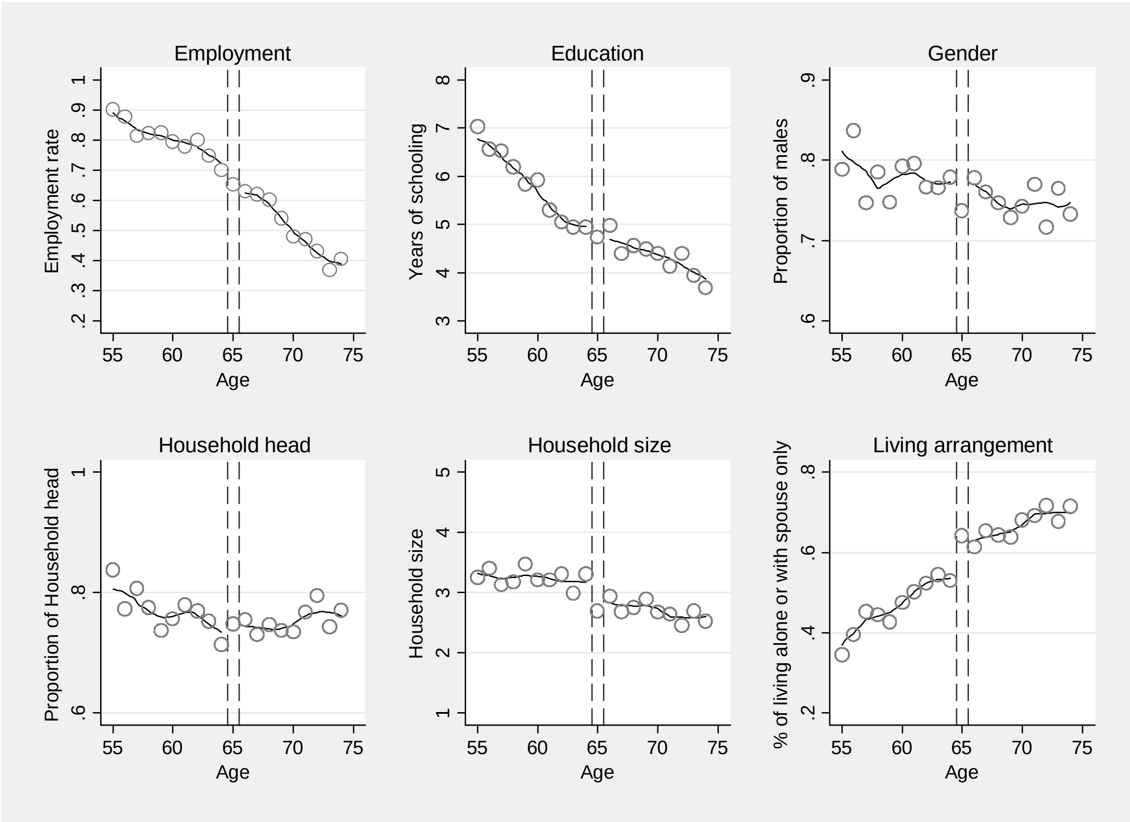
Figure 6: Frequency distribution of the oldest farmers in the household across ages

(55 to 74 Years Old)



Data source: Taiwan Household Income and Expenditure Survey, 1995 to 2006.

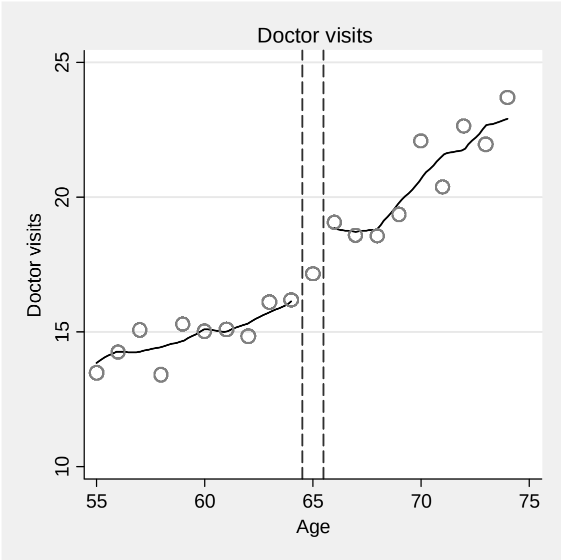
Figure 7: Distribution of the observables relative to the cutoff across ages
(55 to 74 Years Old)



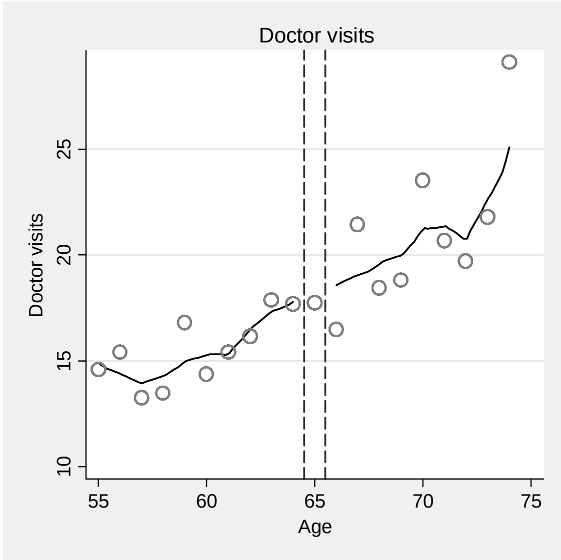
Data source: Taiwan Household Income and Expenditure Survey, 1995 to 2006.

Figure 8: Doctor visits

1999-2006



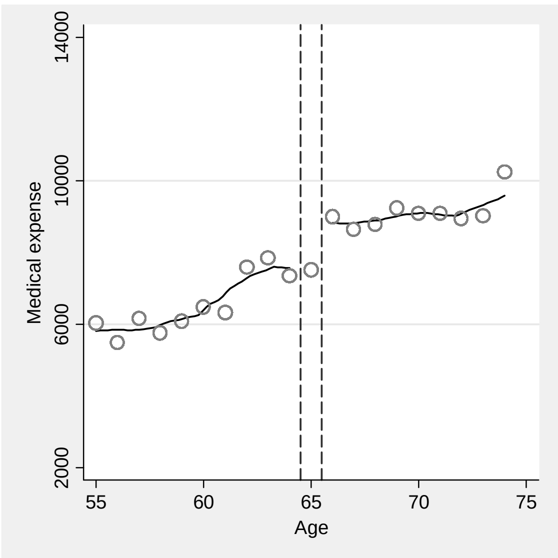
1990-1994



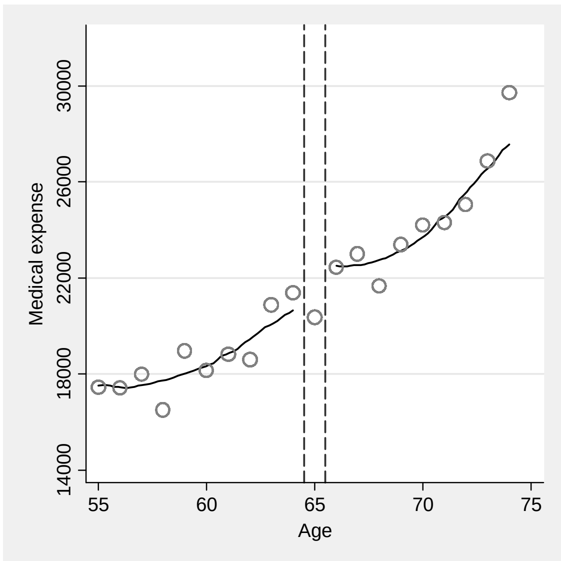
Data source: Taiwan Household Income and Expenditure Survey, 1990-2006.

Figure 9: Per capita medical expense

1999-2006



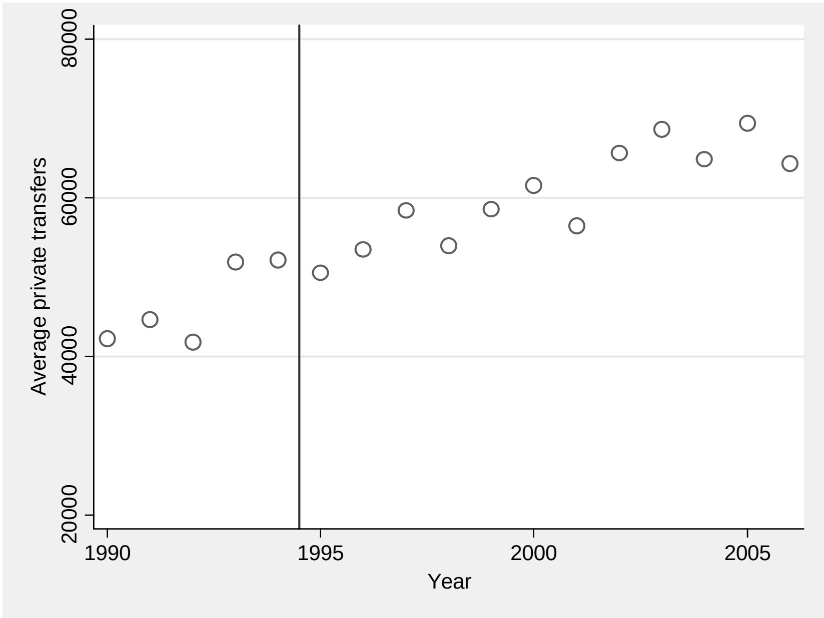
1990-1994



Data source: Taiwan Household Income and Expenditure Survey, 1990-2006.

Appendix

Figure A1: Average private transfers received by households with the oldest farmer aged 62 to 64
(1990-2006)



Data source: Taiwan Household Income and Expenditure Survey, 1990 to 2006.

Table A1: Testing the robustness of RD estimates to bandwidths

	(1)	(2)	(3)	(4)	(5)
	Ages 55 to 74	Ages 56 to 73	Ages 57 to 72	Ages 58 to 71	Ages 59 to 70
FPP benefit	-0.31***	-0.36**	-0.43***	-0.41**	-0.23*
	(0.09)	(0.12)	(0.12)	(0.14)	(0.11)
age-65	2,640.89	3,592.94	7,664.74***	5,178.27*	3,343.14
	(1,622.23)	(2,049.54)	(1,810.07)	(2,425.82)	(1,895.74)
$(age-65)^2$	-139.68	-54.54	514.19*	99.08	-11.68
	(151.01)	(235.18)	(242.38)	(336.58)	(322.27)
(age-65) x *eligible*[d]	602.37	-487.84	-7,478.05*	-2,924.73	-2,765.55
	(3,101.21)	(3,538.33)	(3,481.74)	(4,054.85)	(3,287.60)
Constant	80,192.03***	81,940.11***	86,578.78***	84,336.93***	80,659.71***
	(3,121.92)	(3,696.01)	(3,396.17)	(3,994.20)	(2,813.49)
Observations	20	18	16	14	12

Notes: Regressions are carried out using mean values at the age level, weighted by the number of observations of each age. The variable 'eligible' is used as the exclusive IV in the 2SLS estimations. All money values are measured in terms of New Taiwan Dollars (NT$), and are deflated to dollar values in 2006. Robust standard errors in brackets, * significant at 10%; ** significant at 5%; *** significant at 1%