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Transfer Benefits, Implicit Taxes, and the Earnings of Welfare Recipients: Evidence from Public Assistance Programs in Japan

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Abstract
In this study, we investigate the impact of a transfer program on the earnings of welfare recipients. The case we consider is Public Assistance (PA) in Japan where we can utilize a dataset that contains complete observations of its recipients along with detailed information on their characteristics. Following the literature on the elasticity of taxable income, we also exploit the 2013 reform of the PA system to construct instruments to estimate the earnings responses of PA recipients. With some exceptions, we find small positive price effects and large negative income effects, the latter of which suggests that PA recipients are quite responsive to lump-sum changes in PA benefits. Our study thus highlights the importance of the income effect when we consider earnings responses in the lower tail of income distribution.

Keywords: public assistance, elasticity of taxable income, welfare effects, labor supply
JEL Classification: H24, I38, J22

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1. Introduction

The optimal design of a welfare program necessitates the evaluation of its effect on the choices of low-income households. A transfer program usually affects both the resources (or "virtual income") its recipients are endowed with and relative prices they are subjected to. The literature describes their response either in terms of labor supply (Chan and Moffit 2018) or earnings (Saez et al. 2012). It highlights the importance of the two sorts of response, one to changes in marginal tax/subsidy rate (i.e., price effect) and the other to changes in lump-sum benefit (i.e., income effect). The price effect is identified through variations in net-of-tax (or gross-of-subsidy) wage rate or share, while the income effect is obtained through variations in virtual income. At first glance, we may find these two types of variations difficult to observe since low-income households typically earn less than the minimum level of taxable income. Nonetheless, a close examination of a transfer scheme helps us obtain variations in price and virtual income. As such, a series of studies in the literature have examined transfer schemes in various countries to obtain important policy implications (Moffit 1992, 2016).

This study contributes to the literature by examining the earnings response of welfare recipients in the Japanese system of income guarantee, called Public Assistance (PA). Analogous studies on the topic in Japan are scarce due to a lack of good data at the household level.[1] To our knowledge, there are only two studies with aggregate data. For example, Yugami et al. (2017) examine the effect of PA benefits on PA recipients' labor supply, using data aggregated at the municipal level. Meanwhile, Tamada and Ohtake (2004) investigated the impact of activation policy on PA recipients using data aggregated at the prefectural level. We improve on the current state of inquiry by using a large and rich dataset from the Survey on Public Assistance Recipients (SPAR) compiled by the Ministry of Health, Labour and Welfare (MHLW). With this dataset, which covers *all* PA recipients in Japan, we can conduct an analysis that would otherwise be difficult to implement. Hopefully, by tackling the issues below, this study could also address more general interest than a mere single-country study is supposed to be able to.

One limitation of the SPAR is, however, that it does not provides us with information on the hours worked of PA recipients, but information on their earnings. We thus use the latter as the outcome variable, which makes our study analogous to that of the literature on the elasticity of taxable income (ETI). The ETI literature argues that the response of taxable income not only

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[1] Typical accessible data sources at the household level contain only less than one hundred PA recipients, reflecting the fact that they survey a few thousands from general population among which the share of PA recipients is around 1.6 percent.

reflects changes in hours worked but also those in other response variables, including work intensities, income shifting, and tax evasion. These multiple responses, however, should not be so plausible for PA recipients. First, for the very reason of being poor, the recipients should not have multiple income sources among which they can shift their earnings. Second, while they may wish to underreport their earnings, they are subjected to the scrutiny by welfare offices which is considered more stringent than that by tax offices. We thus argue that, since the recipients could only control hours worked, their labor response is reasonably inferred from their earnings response.

In many cases, a transfer program implicitly “taxes” the earnings of its recipients (Barr and Hall 1975; Rowlatt 1972). The Japanese PA system is not an exception as it provides its recipients with benefits whose amount is equal to their “basic cost of living (BCL)” in excess of their “revenue.” If all earnings are included in the “revenue,” such a gap-filling formula imposes a 100% marginal tax on earnings (Freidman 1965; Tobin 1966), yielding no variations in net-of-tax share. In practice, however, the PA system deducts some of the earnings out of the “revenue,” making the implicit marginal tax rates less than 100%. Before the August 2013 reform, the deduction schedule in the PA system had entailed approximately 10 different implicit marginal tax rates with the corresponding number of earnings brackets. The reform then reduced the number of the rates and the brackets to two. Complemented with the institutional information on the 2013 reform and the mechanism of the PA system, the SPAR dataset indeed helps us construct a measure of price (i.e., net-of-tax share) faced by low-income households.

The SPAR dataset also helps us construct a reliable measure of virtual income whose variation is used to identify the income effect. Meanwhile, the ETI literature pays less attention to the income effect, partly because one of the seminal studies (Saez and Gruber 2002) found little evidence for its existence. In addition, focusing on the top of the income distribution and removing them from its samples, the literature does not necessarily capture the impact on low-income households. Contrariwise, we focus on the bottom of the income distribution where the income effect is expected to exert meaningful impacts. As such, it is essential for us to obtain good data for virtual income. It has been long recognized that measurement errors in virtual income bias the estimation substantially. In particular, nonlabor income, typically capital income, is considered the main source of the errors (Blomquist 1996; Eklöf and Sacklén 2000). However, this concern does not apply to this study since only the BCL constitutes non-labor income for PA recipients. Since the BCL is calculated by welfare offices that follow the rules set by the central government, the data should be free from any significant errors that are comparable to those found in capital income. Another notable merit of the SPAR dataset is that it records and provides BCL data for each PA household.

Therefore, with the household-level data from the SPAR, we can construct data for net-of-tax share and virtual income through which we can gauge the impact of welfare payments on the earnings of low-income households in Japan. However, since good data are only necessary but not sufficient, we have to address the following additional issues.

First, since the budget constraint of a PA recipient is piecewise-linear, price and income variables are endogenous (Hall 1973; Hausman 1985; Moffitt 1986, 1990). The piece-wise linearity makes net-of-tax share dependent on earnings, which also makes virtual income contingent on them. The classic literature on labor supply response provides several methods to address this endogeneity (Blomquist 1996; Hall 1973; Hausman 1979; MaCurdy et al. 1990; Van Soest 1995; Zabalza 1983). The more recent literature on the ETI tackles the issue utilizing instrument variables that exploit exogenous changes in a tax-transfer system. Starting with Gruber and Saez (2002), several types of such instruments are proposed (e.g., Weber 2014; Burns and Ziliak 2017; Kumar and Liang 2020). Since we use earnings as the output variable with the availability of the 2014 reform for constructing the instruments, we follow the latter approach employing the three types of instruments by Gruber and Saez (2002), Weber (2014), and Burns and Ziliak (2017).[2]

Second, there is the issue of confounding factors. In particular, the ETI literature is concerned about bias in estimation caused by the mean reversion of income and the secular trends in income inequality.[3] However, while the two issues may be typical for those located at the top of the income distribution, they should be less of concern at the bottom of the income distribution. The mean reversion, which is a transitory change where an increase in earnings is followed by their subsequent drop, is hardly observed among PA recipients (Yuzawa et al. 2011). Moreover, while different secular trends among income groups obviously matter when we compare individuals across income groups, it may not so when we compare them within a single income group (i.e., the group of PA recipients). Instead, we are concerned about different trends of earnings across regions as labor market conditions may be geographically different. We allow

[2] The instruments by Kumar and Liang (2020) are not applicable to our case. Their instrument is only applicable when the shape of the tax-transfer schedule differs among households with an identical level of before-tax earnings. The PA system has no equivalent of income deductions and instead addresses the issue by adjusting the level of the BCL according to household characteristics.

[3] The literature tries to mitigate these two issues by including polynomials or splines of the log of base-year income. However, this inclusion may cause another source of endogeneity when we perform a regression with differenced dependent and independent variables from a large number of cross-section units. See Wooldridge (2010, pp. 368–374). As such, if we are to include such polynomials or splines, they also have to be instrumented.

for this possibility by including region-specific drifts in our regression models.

The issue of confounders is also related to the handling of socio-economic characteristics at the household level. The ETI literature typically uses data on tax returns that have little information on household characteristics, except for tax-related variables. Against this backdrop, studies have started to examine changes in estimates with additional demographic covariates. While Kleven and Schultz (2014) show that such changes are small if income distribution is stable and tax variations are rich over years, Burns and Ziliak (2015) find 20% reduction in estimate values when the covariates are included. In any case, it may be preferable to allow for the effects of socio-economic characteristics at the individual level. The SPAR dataset provides us with a rich set of PA recipients' characteristics which we can utilize as covariates for our estimation. It also provides information on which we can construct a panel of PA recipients to adjust for unobserved heterogeneity at the individual level.

Lastly, sample selection may matter. The ETI studies that use tax return data exclude from their samples those who do not file tax returns, which obviously depends on the earnings of tax filers. In many cases, they also truncate observations with earnings less than a certain threshold. The use of panel data complicates the matter. In addition to the selection in base year, there may be additional exclusions or inclusions of observations in later years. If these attritions and additions depend on the earnings of observations, we face another form of the selection problem. These issues also apply to this study. There are two avenues to address this selection problem. First, we alternatively allow for sample selection in every year using the method developed by Semykina and Wooldridge (2010). However, this approach is the computationally difficult for our case due to the large size of our samples. Second, we may define our population of interest as a subset of the entire PA recipients in base year, and then allow for the attrition of observations toward later years, which we adopt in this study. Note that, since studies in the ETI literature ignore the issue of sample selection, they effectively follow this approach *without* allowing for the attritions towards latter periods. An exception is Burns and Ziliak (2015) who only allow for the selection in base year. In contrast, the current study allows for the selection in years that follows, conditioned on a well-defined subsample in base year.

The remaining sections are outlined as follows. Section 2 provides the institutional and theoretical backgrounds on which we build our analysis. Section 3 describes an econometric model that takes advantage of the August 2013 reform. Section 4 presents and discusses the results, and Section 5 concludes.

2. Backgrounds

2.1. Institutional background

PA intends to guarantee the BCL for its recipients. Following formulae set by the MHLW, welfare offices calculate the BCL that allows for the characteristics and needs of each household.[4] To be eligible for PA benefits, applicants must demonstrate that they cannot earn more than their BCL and have exhausted all of their resources, including savings and assets, supports from family and relatives, and other benefits provided by the public sectors. If considered eligible, the recipients receive an amount equal to their BCL in excess of what the government considers their "revenue." As such, the benefit schedule is based on a gap-filling formula, which might otherwise imply a 100% marginal tax rate on earnings. However, the PA system deducts some amount from the actual earnings of its recipients, which renders the marginal tax rate less than 100%.

The deduction for recipient *i*, D_i, is expressed as a function of earnings Y_i:

$$D_i = d(Y_i), \tag{1}$$

where $d(Y_i)$ is the deduction schedule. The amount of PA benefits for recipient *i* is

$$B_i = M_i - \{[Y_i - d(Y_i)] + N_i\}, \tag{2}$$

where M_i is the amount of the BCL, N_i is the amount of *i*'s revenues other than Y_i, and $[Y_i - d(Y_i)] + N_i$ is the "revenue." Since *i*'s consumption (or after-tax income) is given as $c_i \equiv Y_i + N_i + B_i$, Eq. (2) yields:

$$c_i = M_i + d(Y_i). \tag{3}$$

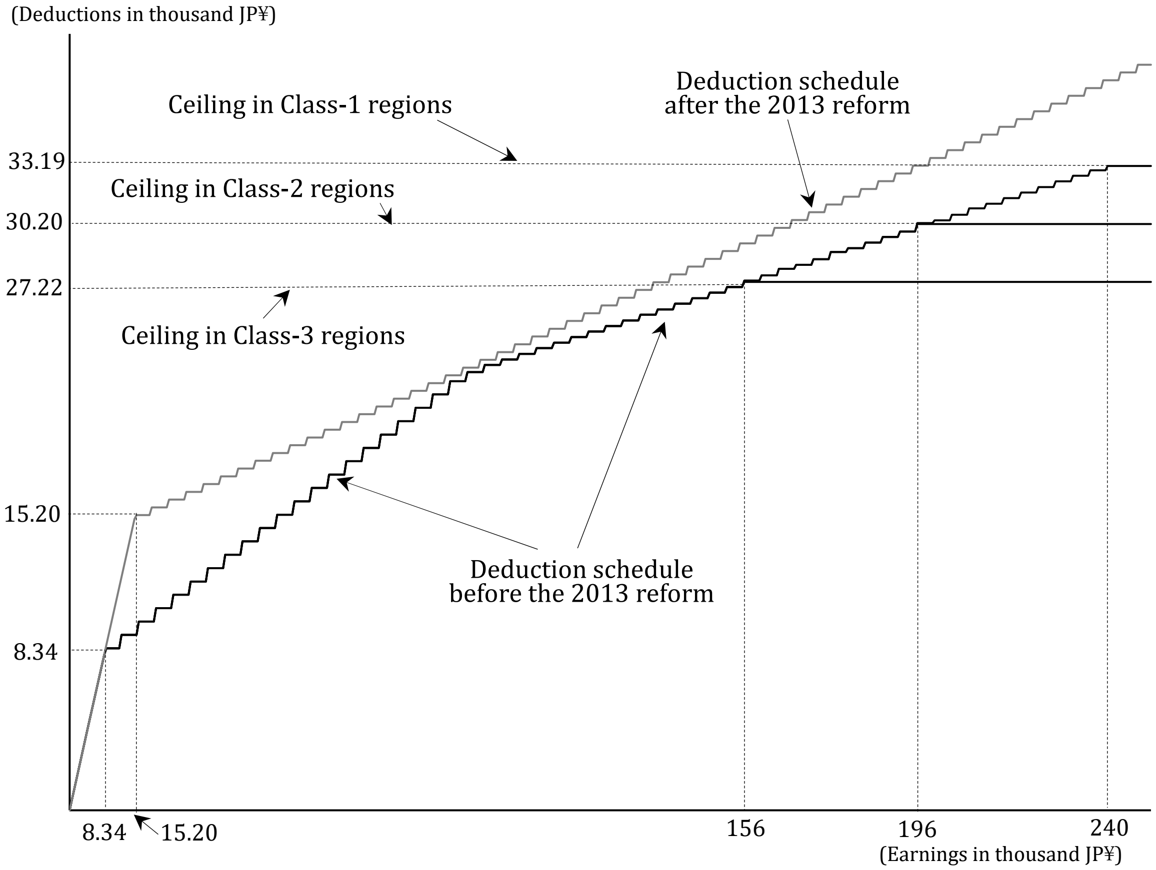
Provided that the schedule is differentiable, the slope of this budget is given as $d'(Y_i)$ and the marginal tax rate is given as $1 - d'(Y_i)$.

Figure 1 illustrates two monthly deduction schedules $d(Y_i)$ for the principal earner in a PA recipient household before and after the reform in August 2013. The black line refers to the schedule before August 2013 (i.e., until the end of July 2013). All monthly earnings of up to 8,340 Japanese Yen (JP¥) were deducted before the reform. Above this threshold, the deduction phased out discretely for additional earnings of JP¥4,000, except for the first interval whose amount was JP¥3,660. The deduction amount plateaus at a certain earnings threshold. The combination of the deduction ceiling and earnings threshold levels was different among the three classes of local areas, which were set according to regional price levels. The combinations of the ceiling and threshold values were JP¥27,220 and JP¥156,000 for Class 3, JP¥30,200 and JP¥196,000 for

[4] For the details of the calculation of the BCL, see Hayashi (2010).

Class 2, and JP¥33,190 and JP¥240,000 for Class 1. The 2013 reform modified the schedule to a gray line by expanding the earnings threshold from JP¥8,340 to JP¥15,200 for the 100% deduction while abolishing the three ceilings of deduction. Above this new threshold, the deduction now increases discretely by JP¥400 for additional earnings of JP¥4,000, except that the additional amount was JP¥3,800 for the first interval above JP¥15,200.

Figure 1. Deduction schedules before and after the August 2013 reform.



We may approximate the two stepped lines in **Figure 1** as piecewise linear lines, as depicted in **Figure 2**, where the originals of **Figure 1** are also replicated. The pre-reform schedule is approximated as lines with six kinks, whereas the post-reform schedule is so with two kinks. Each line segment between two adjacent kinks has a different slope (or price) p_i, with $\tau_i \equiv 1 - p_i$ being the marginal tax rate on *i*'s earnings. As such, the approximated slope p_i or net-of-tax share also indicates the generosity of the deduction system. **Table 1** lists the net-of-tax shares (slopes) for different ranges of monthly earnings, along with their changes after the reform. The line segment in **Figure 2** yields the corresponding intercept of a straight dotted line that extends from that line segment. This intercept measures the difference between virtual income F_i and the BCL M_i (i.e., net-of-BCL virtual income $F_i - M_i$). **Table 2** lists the net-of-BCL virtual incomes (i.e., intercepts) for the monthly earning ranges, along with their changes after the reform.[5]

Figure 2. Piecewise linearization of deduction schedules.

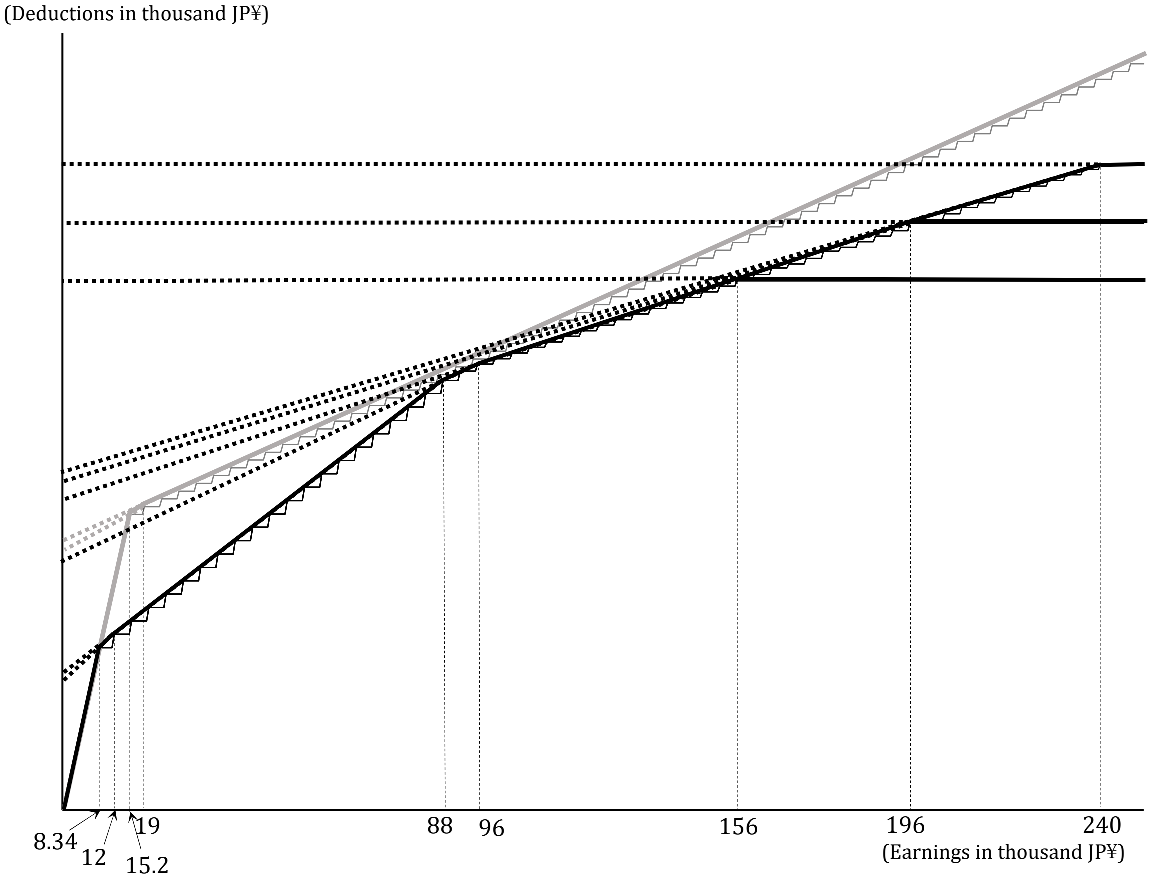


Table 1 shows that PA recipients face more than 80% tax rate for earnings above JP¥8,340 before the reform and around 90% for earnings above JP¥15,200 after it. Despite these high tax rates, their labor participation rate is high according to international standards (Tamada and Ohtake 2004). As shown in **Table 3**, 25% of those aged 20 to 64 participated in the labor markets in 2016. The ratio reduces to 13% if those aged 65 and older are included (i.e., their share is as high as 47%). We can still regard this figure as effectively high, considering that more than 80%

5 Both in **Tables 1** and **2**, bracket R7 applies only to Class-2 and Class-1 regions, whereas R8 and R9 apply to those in Class 1. Also, two additional brackets, R10 and R11, only apply to earnings greater than JP¥156,000 in Class 3 and JP¥196,000 in Class 2.

of the heads of PA households were either ill, injured, disadvantaged, or old.

Table 1. Changes in net-of-tax share before and after the August 2013 reform.

<table>
<tr><th>Monthly Earnings (JP¥)</th><th>Before</th><th>After</th><th>Increase</th></tr>
<tr><td>R0: 0 < Y < 8,340</td><td>1.000</td><td rowspan="3">1.000</td><td>0.000</td></tr>
<tr><td>R1: 3,340 ≤ Y < 12,000</td><td>0.189</td><td>0.801</td></tr>
<tr><td>R2: 12,000 ≤ Y <15,200</td><td rowspan="3">0.172</td><td>0.828</td></tr>
<tr><td>R3: 15,200 ≤ Y <19,000</td><td>0.105</td><td>-0.067</td></tr>
<tr><td>R4: 19,000 ≤ Y < 88,000</td><td rowspan="8">0.100</td><td>-0.072</td></tr>
<tr><td>R5: 88,000 ≤ Y < 96,000</td><td>0.105</td><td>-0.005</td></tr>
<tr><td>R6: 96,000 ≤ Y < 156,000</td><td>0.072</td><td>0.028</td></tr>
<tr><td>R7: 156,000 ≤ Y < 196,000</td><td>0.074[b,c]</td><td>0.026[b,c]</td></tr>
<tr><td>R8: 196,000 ≤ Y < 240,000</td><td>0.067[c]</td><td>0.033[c]</td></tr>
<tr><td>R9: 240,000 ≤ Y</td><td>0.000[c]</td><td>0.100[c]</td></tr>
<tr><td>R10: 156,000 ≤ Y</td><td>0.000[a]</td><td>0.100[a]</td></tr>
<tr><td>R11: 196,000 ≤ Y</td><td>0.000[b]</td><td>0.100[b]</td></tr>
</table>

Notes: Superscripts *a*, *b*, and *c* refer to the amounts applicable to PA recipients in the Class-3, Class-2, and Class-1 regions, respectively.

Table 2. Changes in net-of-BCL virtual income before and after the August 2013 reform.

<table>
<tr><th>Monthly Earnings (JP¥)</th><th>Before</th><th>After</th><th>Increase</th></tr>
<tr><td>R0: 0 < Y < 8,340</td><td>0</td><td rowspan="3">0</td><td>0</td></tr>
<tr><td>R1: 3,340 ≤ Y < 12,000</td><td>6,768</td><td>-6,798</td></tr>
<tr><td>R2: 12,000 ≤ Y <15,200</td><td rowspan="3">6,966</td><td>-6,966</td></tr>
<tr><td>R3: 15,200 ≤ Y <19,000</td><td>13,600</td><td>6,634</td></tr>
<tr><td>R4: 19,000 ≤ Y < 88,000</td><td rowspan="8">13,700</td><td>6,734</td></tr>
<tr><td>R5: 88,000 ≤ Y < 96,000</td><td>12,860</td><td>840</td></tr>
<tr><td>R6: 96,000 ≤ Y < 156,000</td><td>15,996</td><td>-2,296</td></tr>
<tr><td>R7: 156,000 ≤ Y < 196,000</td><td>15,736[b,c]</td><td>-2,036[b,c]</td></tr>
<tr><td>R8: 196,000 ≤ Y < 240,000</td><td>17,099[c]</td><td>-3,399[c]</td></tr>
<tr><td>R9: 240,000 ≤ Y</td><td>33,190[c]</td><td>-20,490[c]</td></tr>
<tr><td>R10: 156,000 ≤ Y</td><td>27,220[a]</td><td>-13,520[a]</td></tr>
<tr><td>R11: 196,000 ≤ Y</td><td>30,200[b]</td><td>-12,760[b]</td></tr>
</table>

Notes: Superscripts *a*, *b*, and *c* refer to the amounts applicable to PA recipients in the Class-3, Class-2, and Class-1 regions, respectively. The net of the BCL virtual income is given as $F_i - M_i$. The unit of measurement was JP¥.

Table 3. Labor market participation of PA recipients.

Age (yeas old)	(1) PA Recipients (in thousand)			(2) Working PA Recipients (in thousand)			(3) Working Ratio (2)/(3) (in percent)		
	2012	2014	2016	2012	2014	2016	2012	2014	2016
All	2,090	2,128	2,110	242	262	274	12	12	13
20-64	952	917	855	194	210	212	20	23	25
65-	833	925	1,001	31	36	45	4	4	5
Average	54.0	55.3	56.8	47.3	47.8	48.8			

Source: The Survey on Public Assistance Recipients (relevant years)

2.2 Theoretical background

For reference, we briefly review the standard model of consumer decision to characterize the response of PA recipients' earnings with price elasticity η and income elasticity ξ. Assume that a PA recipient, indexed by *i*, has the following utility

$$U_i = U(c_i, l_i; \boldsymbol{a}_i) = U(c_i, H - h_i; \boldsymbol{a}_i), \tag{4}$$

where c_i is the consumption, as defined in Eq. (3), l_i is leisure hours, and $\boldsymbol{a}_i$ is a vector of demographic factors that affect *i*'s preferences (i.e., preference shifters). With time endowment *H*, we can express *i*'s leisure in terms of labor supply h_i (hours worked) as $l_i = H - h_i$. We assume the standard properties of the utility function.

Figure 2 suggests that the linearized version of Eq. (3) is as follows:

$$c_i = F_i + p_i \cdot Y_i. \tag{5a}$$

Since $Y_i \equiv W_i \cdot h_i$, with W_i being *i*'s wage rate (ability) and $l_i = H - h_i$, we alternatively have:

$$c_i + p_i \cdot W_i \cdot l_i = F_i + p_i \cdot W_i \cdot H. \tag{5b}$$

Also, note that virtual income F_i is given as

$$F_i \equiv M_i + [d(Y_L) - p_i \cdot Y_L], \tag{6}$$

where Y_L is the lower bound of the bracket containing recipient *i*.

Recipient *i* chooses c_i and h_i to maximize Eq. (4) subject to Eq. (5b). Setting aside recipients' choices at kinks, we obtain the standard labor supply function $h_i = h(w_i, F_i)$, where $w_i = p_i \cdot W_i$. Assuming that the recipient stays in the same bracket, a change in p_i yields $dh_i/dp_i = (\partial h_i/\partial w_i) \cdot W_i - (\partial h_i/\partial F_i) \cdot Y_L$. In the elasticity form,

$$\frac{dh_i}{dp_i}\cdot\frac{p_i}{h_i}=\eta_i-p_i\cdot\frac{Y_L}{F_i}\cdot\xi_i, \tag{7}$$

where $\eta_i \equiv (w_i/h_i)\cdot\partial h_i/\partial w_{i|dF=0}$ is the uncompensated (Marshallian) wage elasticity of labor supply when virtual income is held constant, and $\xi_i \equiv (F_i/h_i)\cdot\partial h_i/\partial F_i$ is the income elasticity of labor supply. Depending on the value of the income effect, the uncompensated elasticity can be either positive or negative, as seen from the following Slutsky equation

$$\eta_i=\eta_i^c+p_i\cdot\frac{Y_i}{F_i}\cdot\xi_i, \tag{8}$$

where $\eta_i^c \equiv (w_i/h_i)\cdot\partial h_i/\partial w_{i|dU=0,dF=0}$ is the compensated (Hicksian) wage elasticity of labor supply, which is always positive if we assume the standard preferences. The income effect is negative if we assume that leisure is a normal good. With Eq. (8), Eq. (7) is now:

$$\frac{dh_i}{dp_i}\cdot\frac{p_i}{h_i}=\eta_i^c+p_i\cdot\frac{Y_i-Y_L}{F_i}\cdot\xi_i. \tag{9}$$

Therefore, the response to a change in the net-of-tax share can also be either negative or positive.

Since the information on hours worked is not available in our dataset, we re-express the above maximization problem in terms of earnings Y_i, which is the standard formulation in the ETI literature (e.g., Gruber and Saez 2002). We rewrite Eq. (4) as:

$$U_i=V(c_i,Y_i;W_i,\boldsymbol{a}_i)\equiv U\left(c_i,H-\frac{Y_i}{W_i};\boldsymbol{a}_i\right). \tag{10}$$

If we plot indifference curves over (c_i,Y_i), the pre-tax wage W_i is now another indifference-curve shifter. The budget constraint can be rewritten as Eq. (5a). **Figure 3** over (c_i,Y_i) exemplifies the indifference curves from Eq. (10) and the budget line from Eq. (5a). **Figure 3** simplifies the budge line with two kinks at $Y_i=Y_L$ and $Y_i=Y_H$. The line has slopes of 1, $1-\tau'$, and $1-\tau''$ for each segment with $\tau'<\tau''$. The actual (approximated) budget line is the deduction schedules in **Figure 2** that are shifted upward to the amount of BCL M_i.

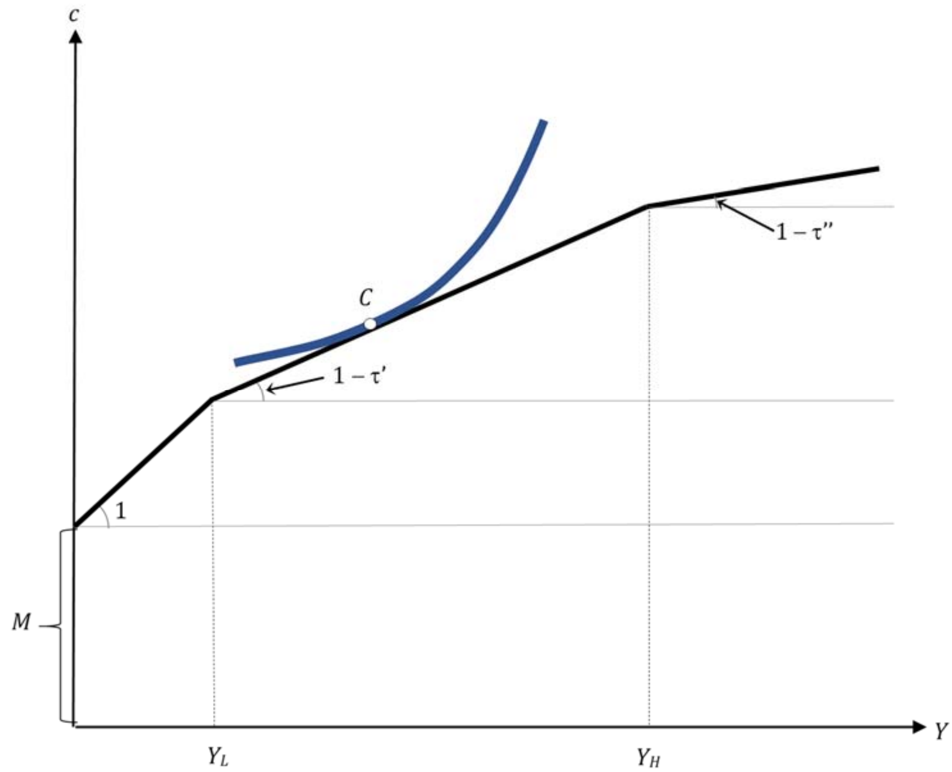
For a given value of W_i, the maximum of Eq. (4) with Eq. (5b) over (c_i,h_i) is equivalent to the maximum of Eq. (8) with Eq. (5a) over (c_i,Y_i). Since $Y_i\equiv W_i\cdot h(w_i,F_i)$ or $\ln Y_i=\ln W_i+\ln h(p_i\cdot W_i,F_i)$, we obtain that $d\ln Y_i=d\ln h(w_i,F_i)$ if we can assume that the pre-tax wage rate W_i is held constant ($d\ln W_i=0$). Accordingly, the following two equations hold:

$$\frac{p_i}{Y_i}\cdot\frac{\partial Y_i}{\partial p_{i|dW=0,dF=0}}=\frac{\partial\ln Y_i}{\partial\ln p_i}=\frac{\partial\ln h(w_i,F_i)}{\partial\ln p_i}=\eta_i. \tag{9}$$

$$\frac{F_i}{Y_i}\cdot\frac{\partial Y_i}{\partial F_{i|dW=0}}=\frac{\partial\ln Y_i}{\partial\ln F_i}=\frac{\partial\ln h(w_i,F_i)}{\partial\ln F_i}=\xi_i. \tag{10}$$

If the wage rate is held constant, therefore, we obtain the two elasticities, η_i and ξ_i, by regressing the logarithm of earnings on the logarithms of net-of-tax share and virtual income.

Figure 3. Indifference curve.



3. Empirical implementation

3.1 Regression model

According to the literature on ETI, we assume the following log-log specification

$$\ln Y_{it} = \eta \cdot \ln p_{it} + \xi \cdot \ln F_{it} + \sum_{l=1}^{L} \beta_l \cdot x_{it} + \alpha_i + u_{it}, \tag{11}$$

where i and t index PA recipients and year, respectively, and $x_{l,it}$'s are covariates, α_i is unobserved heterogeneity, and u_{it} is an error term. Since Eq. (11) is in a log-log form, coefficients η and ξ are interpreted as the price elasticity of earnings with virtual income held constant (i.e., Eq. [8]) and income elasticity of earnings. When it is appropriate to assume that gross wage rates are constant, they are also interpreted to be equal to the respective elasticities of labor supply. By including virtual income as another key explanatory variable, we construe the coefficient on the logged price as the uncompensated (Marshallian) price elasticity of labor supply, which is given by Eqs. (8) and (9). The uncompensated elasticity can be either positive or negative.

We take first difference of Eq. (11) to eliminate unobserved heterogeneity α_i. Our dataset is only available for July of 2012, 2013, 2014, 2015, and 2016, where there are two periods before the reform (August 2013), and three periods after it.[6] Since we examine changes between 2003 and 2003 + k, we estimate the model with cross-section observations of differenced variables

$$\begin{aligned} &\ln Y_{it_0+k} - \ln Y_{it_0} \\ &= \eta \cdot \left(\ln p_{it_0+k} - \ln p_{it_0}\right) + \xi \cdot \left(\ln F_{it_0+k} - \ln F_{it_0}\right) \\ &+ \sum_{l=1}^{L} \beta_l \cdot \left(x_{it_0+k} - x_{it_0}\right) + u_{it_0+k} - u_{it_0}, \end{aligned} \tag{12}$$

where $t_0 = 2013$. With the data provided, we examine three intervals with k = 1, 2, and 3, that is, 2013-2014, 2013-2015, and 2013-2016. We reserve the observations from 2012 for the instruments by Weber (2014).

3.2 Sample selection

We construct our sample as follows. First, we only use the observations of recipients aged between 20 and 64 years old. The latter limit is used because caseworkers are thought to act differently once the recipients are regarded as "old," which is considered to be aged 65 and older. Second, we only include those who are considered to be "employed" and "unemployed," and exclude others (e.g., "self-employed"). This criterion reflects the economic framework utilized in Section 2. Third, we exclude observations from households having more than one earner to avoid possible theoretical complications caused by the existence of multiple earners. Fourth, we only include those recipients who continuously remained in the PA system from July 2012 to July 2016, excluding others.[7]

Since our dependent variable is the logarithm of earnings, observations with zero earnings are automatically excluded from the sample, possibly causing the sample selection problem.[8]

[6] The SPAR annually collects information from PA recipients at the end of or during July, depending on whether the data are stock or flow.

[7] This exclusion criterion makes our sample comprise those who are aged between 20 and 60 in July 2012.

[8] Additionally, there are observations with zero values for p. Such zero prices occur with observations that face 100% marginal tax rate, which was the case if their monthly earnings exceed JP¥156,000 in Class 3 regions (R10), JP¥196,000 in Class 2 regions (R11), and JP¥240,000 in Class 1 regions (R9) before the reform (i.e., 2012 and 2013). These observations may cause additional issues since those observations with zero drop out and the price (marginal tax rate) depends on the earnings. However, we do not address such an issue here. First, the number

The analogous issue applies to the studies on ETI, since they typically truncate observations with earnings less than a certain threshold value. However, the literature is largely silent on this issue except Burns and Ziliak (2015) who allow for selection in the base year. However, taxpayers that pocketed more than some threshold in the base year might earn less than that threshold in the following years. Similarly, PA recipients earning more than zero in the base year (2013) may reduce their earnings to zero in the latter years (i.e., 2013 + k). We evade the issue of sample selectivity in the base year by specifying our population of interest as a subset of the set of all PA recipients. In particular, we declare we are interested in the behavior of PA recipients who attained positive earnings in July 2013. We then allow for sample attrition that depends on earnings in 2013 + k. This procedure follows Wooldridge (2010, pp. 837–840) and requires the inclusion of the inverse Mills ratio as an additional regressor in Eq. (12). We derive the ratio by performing a Probit estimation for labor participation (i.e., positive earnings) in the end year with covariates from Eq. (11) in addition to the binary variables that indicate the type of public health insurance covering a recipient. We also include the values of the time-variant covariates observed in the base year.

3.3 Endogeneity and instruments

The two key variables, p_i and F_{it}, are endogenous.[9] To allow for the endogeneity, we follow the ETI literature to use instruments that exploit institutional change. While we limit our discussion to instrumenting price, the analogous argument applies to instrumenting virtual income. We use the reform in August 2013 and apply the following three types of instruments.

First, Gruber and Saez (2002) proposed an instrument, which is given as

$$\Delta \ln p_i^{GS} \equiv \ln p_{t_0+k}\left(Y_{it_0}\right) - \ln p_{t_0}\left(Y_{it_0}\right), \tag{13}$$

where $p_{t_0}(\cdot)$ and $p_{t_0+k}(\cdot)$ correspond to different step functions that reflect the pre - and post-reform schedules, respectively. While $p_{t_0}\left(Y_{it_0}\right)$ is the actual price recipient *i* faced in period t_0, $p_{t_0+k}\left(Y_{it_0}\right)$ is the price recipient *i* would face if the pre-reform earnings are retained in period $t_0 + k$. Gruber and Saez (2002) claim that as Eq. (13) solely reflects the change in legislation, it should be exogenous. Still, Eq. (13) is invalid. While the original discussion by Weber (2014) is

of such observations is really small relative to the whole sample size (i.e., 0.04% = 1,296/3,204,196). Second, since theory does not predict such observations, we could argue that such rare choices are not endogenous (i.e., not by the recipients' choice) but due to some exogeneous factors. As such, we regard their occurrence as random and do not address the issue in our estimation.

9 We construct the price data as $p_i = 1 - \tau_i$ by referring to the earnings of PA recipients in our sample in **Table 1**. The data for virtual income F_{it} are constructed analogously. We refer the earnings in the sample to **Table 2** to obtain net-of-BCL virtual income $F_{it} - M_{it}$, and then add the BCL, M_{it}, to it to obtain F_{it}.

contrived, the essence is simple. It is apparent from Eq. (11) that $\ln p_{t_0}(Y_{it_0})$ correlates with u_{it_0} as u_{it_0} affects Y_{it_0}. Thus, $\ln p_{t_0}(Y_{it_0})$ is also correlated with the error term $u_{it_0+k} - u_{it_0}$ in Eq. (12) as the term contains u_{it_0}. Likewise, $\ln p_{t_0+k}(Y_{it_0})$ is correlated with u_{it_0}, and the error term in Eq (12), as it also depends on Y_{it_0}. Therefore, $\Delta \ln p_i^{GS}$, defined in Eq. (13), is also correlated with u_{it_0} and with the error term in Eq. (12).

Weber (2014) proposed an alternative instrument that uses Y_{it_0-s} instead of Y_{it_0} in Eq. (13) for some positive value of $s > 0$,

$$\Delta \ln p_i^W \equiv \ln p_{t_0+1}(Y_{it_0-s}) - \ln p_{t_0}(Y_{it_0-s}). \tag{14}$$

Generally, $s = \pi + 1$ suffices if u_{it} follows the π-th order serial correlation. For example, if u_{it} is serially independent ($\pi = 0$), only lagging once ($s = 1$) is sufficient as $\ln p_{t_0+1}(Y_{it_0-1}) - \ln p_{t_0}(Y_{it_0-1})$ is uncorrelated with $u_{it_0+k} - u_{it_0}$.[10] Since our data source starts in 2012, we have only two periods before the reform and have no choice but to proceed with $s = 1$, hoping that the serial correlation would not substantially harm our Weber instruments.

Lastly, Burns and Ziliak (2017) proposed an instrument that takes the mean value of Eq. (13) for a specific set of cross-sectional units C

$$\Delta \ln p_i^{BZ} \equiv \frac{1}{n_C} \sum_{i \in C} \Delta \ln p_i^{GS}, \tag{15}$$

where n_C is the number of cross-sectional units in set C. The validity of this instrument is based on the premise that the correlation between the group mean and individual error is negligible. Bruns and Ziliak (2017) defined the group with the date of birth (cohort), education attainment, region (US state), and year. Since our data are a cross-section of differenced variables, the last item (year) is irrelevant for our case. Besides, the SPAR does not survey information on the educational attainment of PA recipients. Nonetheless, we may substitute it with a binary variable that specifies the type of public social insurance of the recipients before they were admitted to the PA program since these types supposedly correlate with their ability to earn income.[11] Therefore, we obtained the Burns-Ziliak instrument using cohort (i.e., year of birth), educational attainment (i.e., the type of social insurance program), and region (i.e., prefecture).

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[10] The idea is akin to the instrument of Anderson and Hsiao (1983) for the first-differenced model. See Woodridge (2010, pp. 371 – 374).

[11] The SPAR identifies six types: (1) no public health insurance (i.e., uninsured), (2) National Health Insurance, (3) employees' associations programs (i.e., subscribers), (4) employees' association programs (i.e., dependents of subscribers), (5) programs for those aged older than 74, and (6) others. For more information on the system of public medical insurance in Japan, see Hayashi (2010, 2018).

3.4 Other issues

The ETI literature highlights estimation issues caused by the mean reversion of income and secular changes in income inequality. Studies in the literature has attempted to mitigate these two issues through inclusion of polynomials or splines of base-year income Y_{it_0}, $f(Y_{it_0})$, in their regression models. Yet, as seen from our discussion of the Gruber-Saez instrument, the inclusion of $f(Y_{it_0})$ constitutes another source of endogeneity because the error term in Eq. (13) correlates Y_{it_0}. In other words, if $f(Y_{it_0})$ is to be included, it must also be instrumented. As discussed previously, the two issues may be of less concerns in our case as our sample is drawn from a single income group located at the bottom of the distribution. The issue here should be more related to the inclusion of covariates x_{it}'s that adjusts for potential confounders in the regression model. With exceptions of Kleven and Schultz (2014) and Burns and Ziliak (2015), studies in the ETI literature typically use a small number of covariates due to the limited information obtained from tax return data. Meanwhile, our dataset offers a large number of recipients' characteristics. We use the following variables as covariates in our estimation:

- Seven types of physical and mental conditions
- Nine types of nationalities
- Five types of relationships with household heads
- Seven types of households (i.e., depending on the characteristics of household heads)
- Six types of dwellings
- Six types of institutionalization (including "not institutionalized")
- Regions of residence (47 prefecture dummies)
- The number of household members (12 dummies for households with family sizes ranging from 1 to 12), and
- Ages (78 dummies for those aged 20–97 and older).

The variables listed above are all binary, some of which are excluded because of collinearity when regression is performed. While they are all differenced as in Eq. (12), the prefecture dummies are not differenced. Such dummies adjust for the region (prefecture) specific drifts with regards to the earnings of PA recipients. A reason for this association is that we are concerned about different trends in earning variations across regions within the country, since local labor markets for low-ability individuals may plausibly be different among localities.

4. Results

4.1 Baseline estimation

We alternatively estimate Eq. (12) for k = 1, 2, and 3 using one of the three types of instruments, Eqs. (13)−(15).[12] The IV estimation is just identified with two instruments for two endogenous regressors. **Table 4** shows the results that use all the data in our sample, as described in the previous section. The coefficient estimates on the covariates are suppressed. The table also lists the results without the instruments (i.e., OLS estimates) as a reference. For each of the combinations of the three lengths of interval (i.e., k = 1, 2, and 3) and the four types of estimation (i.e., OLS and three IV estimations), the table contains the cases with and without the estimated inverse Mill's ratio that allows for attrition in the second period. Except the case with Gruber and Saez instruments for k = 3, all the coefficients of the inverse Mill's ratio are statistically significant. Yet, allowing for attrition does not noticeably change the estimates. The table also lists the P-values for the test of instrument relevance (i.e., weak IV), showing that all three types of instruments pass the test.

The three sets of OLS estimation conflict with theoretical expectations, as they all have a combination of a negative price effect and a positive income effect which together implies a negative compensated effect. In the other cases with IVs, the results are theoretically consistent in terms of the combination of the signs of the two effects. The income effects are negative in all the IV cases. Meanwhile, the price effects are positive with the Gruber-Saez and Burns-Ziliak instruments, while they are negative with the Weber instruments. We prefer the results with the Burns-Ziliak instruments to those with the other two instruments. We highly suspect the endogeneity of the Gruber-Saez instruments as Weber (2014) shows. We also speculate that our version of the Weber instruments is not free from endogeneity. As we mentioned in the previous section, we have no choice but to proceed with $s = 1$ since we have only two periods before the 2013 reform with the data starting in 2012. If there is any serial correlation among error terms, the Weber instruments with one lag are not exogenous.

The results with the Burns-Ziliak instruments that allow for attrition reveal that the estimates of price and income elasticities are in the ranges of (0.084, 0.113) and (−0.597, −0.377), respectively. Specifically, the former increases with an increase in the time span, while the latter shows the lowest value in absolute value with the shortest time span (i.e., k = 1).

[12] The statement in this section is conditioned on the restricted population of PA recipients of interest. See Section 3.4.

Table 4. Results with all observations.

OLS	$k = 1$		$k = 2$		$k = 3$	
$\Delta \ln p_{it} : \eta$	−0.134*** (0.006)	−0.138*** (0.006)	−0.167*** (0.007)	−0.173*** (0.007)	−0.198*** (0.007)	−0.205*** (0.008)
$\Delta \ln F_{it} : \xi$	0.106*** (0.048)	0.086* (0.048)	0.191*** (0.052)	0.167*** (0.052)	0.354*** (0.062)	0.328*** (0.061)
Inverse Mill's ratio		0.410*** (0.059)		0.427*** (0.054)		0.481*** (0.053)
Sample size	21,214	21,202	19,940	19,930	18,973	18,962
Gruber & Saez	$k = 1$		$k = 2$		$k = 3$	
$\Delta \ln p_{it} : \eta$	0.103*** (0.006)	0.102*** (0.006)	0.143*** (0.007)	0.141*** (0.007)	0.179*** (0.008)	0.177*** (0.009)
$\Delta \ln F_{it} : \xi$	−0.471*** (0.060)	−0.480*** (0.061)	−0.619*** (0.066)	−0.624*** (0.066)	−0.415*** (0.080)	−0.418*** (0.080)
Inverse Mill's ratio		0.128** (0.065)		0.108* (0.062)		0.067 (0.063)
Sample size	21,213	21,201	19,939	19,929	18,973	18,962
Weak IV	0.000	0.000	0.000	0.000	0.000	0.000
Weber	$k = 1$		$k = 2$		$k = 3$	
$\Delta \ln p_{it} : \eta$	−0.181*** (0.033)	-0.164*** (0.031)	−0.236*** (0.033)	−0.206*** (0.030)	−0.245*** (0.032)	−0.218*** (0.030)
$\Delta \ln F_{it} : \xi$	−0.427*** (0.073)	−0.402*** (0.071)	−0.514*** (0.074)	−0.467*** (0.070)	−0.308*** (0.081)	−0.270*** (0.079)
Inverse Mill's ratio		0.470*** (0.075)		0.476*** (0.063)		0.501*** (0.064)
Sample size	21,214	21,202	19,940	19,930	18,793	18,962
Weak IV	0.000	0.000	0.000	0.000	0.000	0.000
Burns & Ziliak	$k = 1$		$k = 2$		$k = 3$	
$\Delta \ln p_{it} : \eta$	0.087*** (0.015)	0.084*** (0.016)	0.096*** (0.016)	0.089*** (0.017)	0.120*** (0.018)	0.113*** (0.019)
$\Delta \ln F_{it} : \xi$	-0.365** (0.145)	-0.377*** (0.147)	-0.580*** (0.153)	-0.597*** (0.153)	-0.442** (0.182)	-0.446** (0.183)
Inverse Mill's ratio		0.146** (0.069)		0.163*** (0.062)		0.138** (0.064)
Sample size	21,214	21,202	19,940	19,930	18,793	18,962
Weak IV	0.000	0.000	0.000	0.000	0.000	0.000

Notes: (1) "***", "**", and "*" respectively indicate $p \le 0.01$, $0.01 < p \le 0.05$, and $0.05 < p \le 0.1$ where p is the P-value based on two-tailed tests. (2) Robust standard errors are shown in parentheses. (3) The last row shows the P values for weak IV which are obtained with the STATA command `zhdnly`. The null hypothesis is that all weakly identified coefficients are zero. The P-values are based on the Anderson-Rubin test statistic. The conditional likelihood ratio test, the Lagrange multiplier K test, the overidentifications test, and a combination of the K and overidentifications tests are not available because the models are all just identified.

4.2 Subsample estimates

The estimates in **Table 4** are based on a sample that contains various types of PA recipients. While we adjust for such variations by including covariates that represent a variety of household characteristics, the estimation for **Table 4** still assumes that the price and income elasticities are constant for all observations. Therefore, we split the sample into subsamples to examine how the estimates change across them. We consider three subgroups of PA recipients consisting of two groups of single-member households (male and female) and single mother households. **Table 5** lists the results with these subsamples which are estimated with the inverse-Mill's ratio and the Burns-Ziliak instruments. The coefficient estimates on the covariates and the inverse-Mill's ratio are again suppressed.

Table 5. Results with subsamples.

Single households (Male)	$k = 1$	$k = 2$	$k = 3$
$\Delta \ln p_{it} : \eta$	0.074*** (0.025)	0.091*** (0.086)	0.100*** (0.031)
$\Delta \ln F_{it} : \xi$	−0.328*** (0.194)	−0.964*** (0.025)	−0.836*** (0.241)
Sample size	7,839	7,234	6,751
Weak IV	0.000	0.000	0.000
Single households (Female)	$k = 1$	$k = 2$	$k = 3$
$\Delta \ln p_{it} : \eta$	0.097*** (0.020)	0.129*** (0.021)	0.138*** (0.025)
$\Delta \ln F_{it} : \xi$	−0.655*** (0.170)	−0.270* (0.160)	−0.291 (0.234)
Sample size	10,407	9,910	9,534
Weak IV	0.000	0.000	0.000
Single mother households	$k = 1$	$k = 2$	$k = 3$
$\Delta \ln p_{it} : \eta$	0.153*** (0.025)	0.173*** (0.046)	0.210*** (0.034)
$\Delta \ln F_{it} : \xi$	−0.630*** (0.236)	−1.022 (0.777)	−0.279 (0.311)
Sample size	5,319	4,991	4,631
Weak IV	0.000	0.000	0.847

Notes: (1) "***", "**", and "*" respectively indicate $p \leq 0.01$, $0.01 < p \leq 0.05$, and $0.05 < p \leq 0.1$ where p is the P-value based on two-tailed tests. (2) Robust standard errors are shown in parentheses. (3) The last row shows the P values for weak IV which are obtained with the STATA command `zhdnly`. The null hypothesis is that all weakly identified coefficients are zero. The P-values are based on the Anderson-Rubin test statistic. The conditional likelihood ratio test, the Lagrange multiplier K test, the overidentifications test, and a combination of the K and overidentifications tests are not available because the models are all just identified.

The first and second sets of the estimates are for male single-member households and female single-member households, respectively. For both groups, the signs are positive for the price effect and negative for the income effects. In addition, as time span k gets larger, the price effects become larger both for males (from 0.074 to 0.100) and females (from 0.097 to 0.138). These values are larger for females for a given time span. Meanwhile, when the time span is the shortest (i.e., $k = 1$), the income effects in absolute value are the smallest for males (−0.328) and the largest for females (−0.655). Notably, the income elasticity of males is quite large with −0.964 for $k = 2$ and −0.836 for $k = 3$. In contrast, the income response of females gets smaller with $k = 2$ and statistically insignificant with $k = 3$. The third set is for single-mother households whose estimates follow a pattern that is similar to those of single female households. As time span k gets larger, the price elasticities of single mothers become larger from 0.157 to 0.210. Meanwhile, when the span is the shortest (i.e., $k = 1$), the income effects in absolute value are the largest (−0.630). The value gets statistically insignificant with $k \geq 2$.

Given that female single-member households and single-mother households exhibits similar patterns, we may summarize the results above as follows. First, positive price responses became larger as time span gets longer for all subsamples, suggesting that adjustments to changes in net-of-tax share require time. Second, while their short-term responses to changes in virtual income are twice as large as male responses are, female recipients get less responsive in the longer term. Particularly, their responses become statistically insignificant as the time span gets longer. Perhaps, in longer term, the circumstances surrounding female recipients may be forcing them to work at fixed hours even when PA benefits change. Lastly, in contrast to these female responses, male recipients are more responsive to changes in virtual income in the longer term and less so in the short term. Notably, their longer-term responses are quite large with elasticities that are close to unity, suggesting that, while it may take a few years, male recipients will reduce their earnings sharply if they receive more of PA benefits.

Using different subsamples, we identify patterns of estimates that are different between males and females as above. However, it is beyond the scope of this study to find out what factors cause such differences. As this question certainly merits further inquiry, it will be an important topic of our future research.

5. Concluding remarks

In this study, we examined the effects of transfer programs on the earnings of their recipients by taking advantage of the SPAR dataset that contains the observations of all PA recipients with rich information on their characteristics. We estimate with instruments their responses to changes in net-of-tax share and virtual income, allowing for sample attrition. We utilized three types of instruments proposed in the literature, taking advantage of the 2013 reform of the Japanese PA system. We based our discussion on the estimates obtained with the Burns-Ziliak instruments. Results were consistent with theory with positive estimates for the price effects and negative estimates for the income effects. We also found patterns that are different between male and female recipients. Notably, except for the longer-term estimates for female recipients, the income elasticities are all statistically significant and quantitatively large with their values ranging from −0.964 to −0.446. In other words, the earnings of PA recipients are found to be responsive to a lump-sum change in PA benefits. This result highlights the importance of income effect when estimating labor responses for the lower tail of the income distribution.

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