サービス産業における価格と生産性の計測

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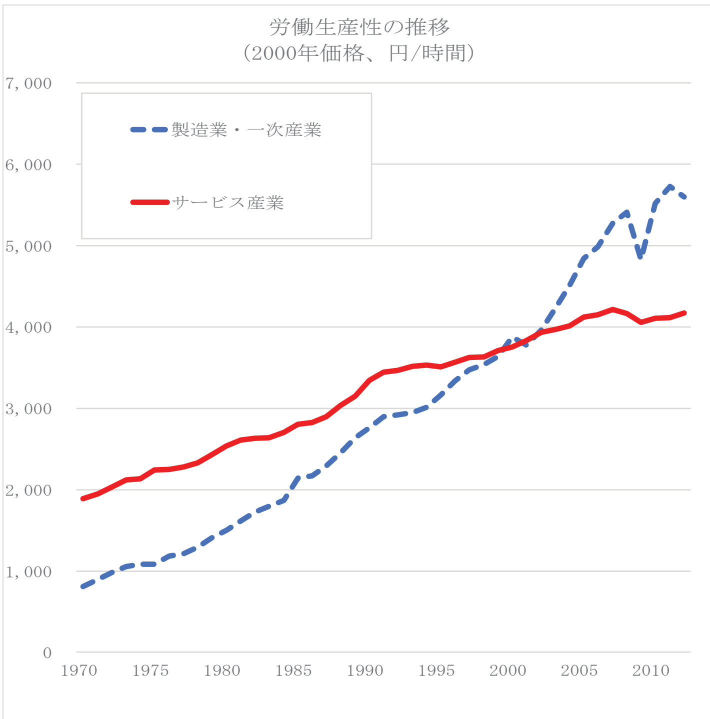
1. はじめに

- 多くの先行研究は，日本のサービス産業の生産性上昇率が遅く，また欧米諸国と比較して，その生産性水準は半分程度であるとの指摘をしてきた（Inklaar and Timmer 2008, Fukao 2013, 経済産業省 2013, Jorgenson, Nomura and Samuels 2016）．
- 生産性の時間を通じた変化に関する推計は，物価統計やGDP統計に依存する．



出所：深尾（2017）．原データは一橋大学・経済産業研究所のJIPデータベース2015.

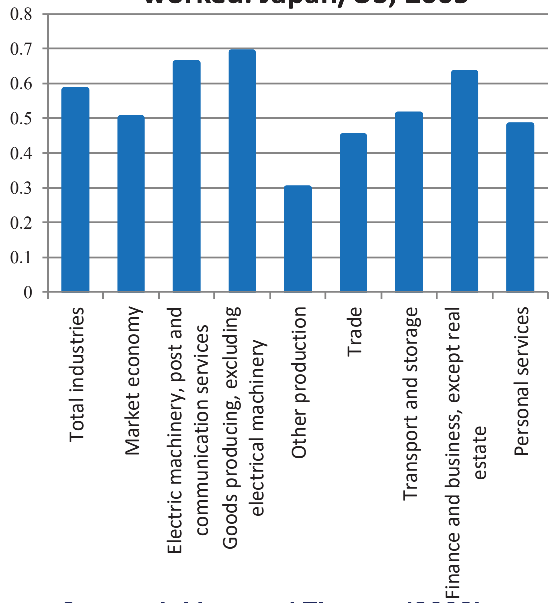
ICP-PPPs as a basis for International Comparisons of Productivity

- Previous studies report lower service sector productivity in Japan compared to the US.

(Inklaar and Timmer 2008, Ministry of Economy, Trade and Industries 2013, Jorgenson, Nomura and Samuels 2016)

- These studies heavily rely on PPPs from the International Comparison Program (ICP) to compare sectoral gross output and input between Japan and the US.

PPP based value added per hour worked: Japan/US, 2005



Source: Inklaar and Timmer (2008)

1. はじめに

サービス産業の生産性は本当に低いのか（サービス品質を反映した生産性計測、他産業・国際比較における課題）

広義のサービス産業と建設業（経済全体のうち一次産業と製造業以外）は、GDPの8割を占めるが、そのうち半分（GDPの4割）は、実質生産や質の計測に深刻な問題がある。

	Gross value added share in GDP			Man-hour input share in the total economy		
	US (2010)	UK (2010)	Japan (2012)	US (2010)	UK (2009)	Japan (2012)
Construction	3.6%	7.0%	6.1%	5.9%	8.2%	9.0%
Wholesale and retail	9.7%	10.8%	13.4%	13.5%	14.4%	14.4%
Education	5.9%	6.8%	3.5%	8.2%	6.9%	3.2%
Health care and social work	12.6%	8.1%	6.0%	17.9%	10.9%	10.6%
Public administration and defense, compulsory social security	4.2%	5.3%	9.3%	3.4%	5.7%	5.5%
Total	36.0%	37.9%	38.2%	48.9%	46.1%	42.7%

Source: Fukao, et al. (2016)

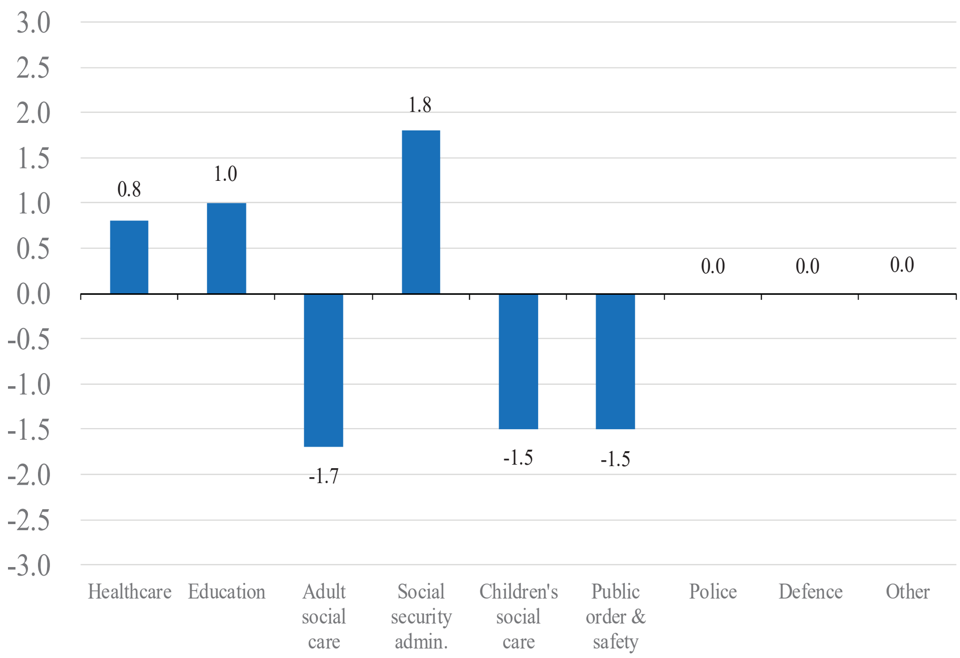
1. はじめに

- 産業・企業レベルで生産性の上昇を計測するには、実質生産量変化のデータが必要である。その基礎となる日本の国民経済計算統計において、公務、教育の大部分、建設、社会福祉など、GDPの2割強を占める活動については、名目生産額を実質化するための適切な価格データ作成が困難であるとして、名目生産コストを、投入生産要素の価格指数で割った生産要素投入指数が実質生産指数の代わりに使われている。つまり、労働や資本の投入量で生産量を測っている（インプット＝アウトプット　アプローチ）。このため、これらの産業では生産性の上昇が定義によってほぼゼロになる。

←多くの先進諸国では、価格データの新規作成（例：建築・土木）、アウトプットの数量指数（例：卒業者数）とサービスの質指数（例：学力テストの平均点）の組み合わせによる実質生産の把握、等が進められている（Atkinson 2005、OECDによる計測方法に関するマニュアル作成、欧州委員会におけるSPINTANプロジェクト等）。 日本ではこの分野の研究・政府の取組が立ち後れている。

2. サービス産業の生産性計測の課題（続）

英国公的サービス業におけるTFP上昇：1997－2012年、年率％



Source: ONS (2015)

1. はじめに

報告の構成

第2節　日本政府による物価と実質生産計測における課題

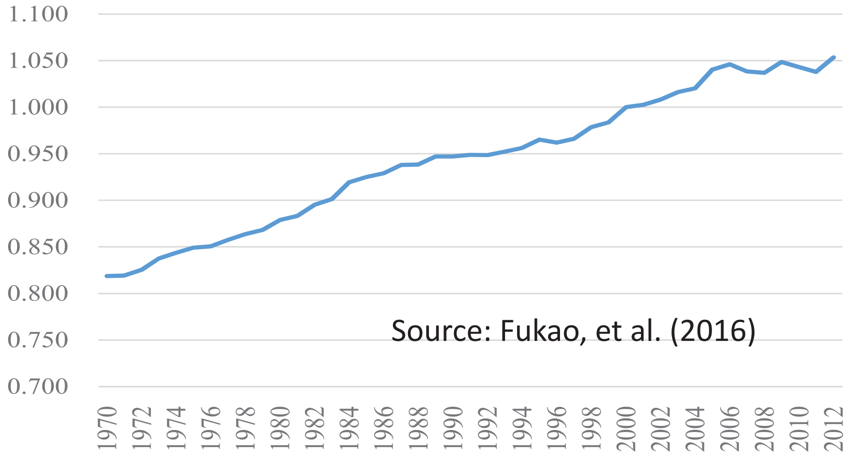
第3節　サービス産業における生産性水準の国際比較における課題

2. 日本政府による物価と実質生産計測における課題

建設業

日本のGDP統計では、建設業や私立大学の教育等については、コストアプローチを採用しているのに、労働の質向上を考慮してない。建設業におけるこの無視により、政府・JIPデータベースは1973-2012年における日本全体のGDP成長とTFP上昇を1.7%ポイント過小に推計していると考えられる。

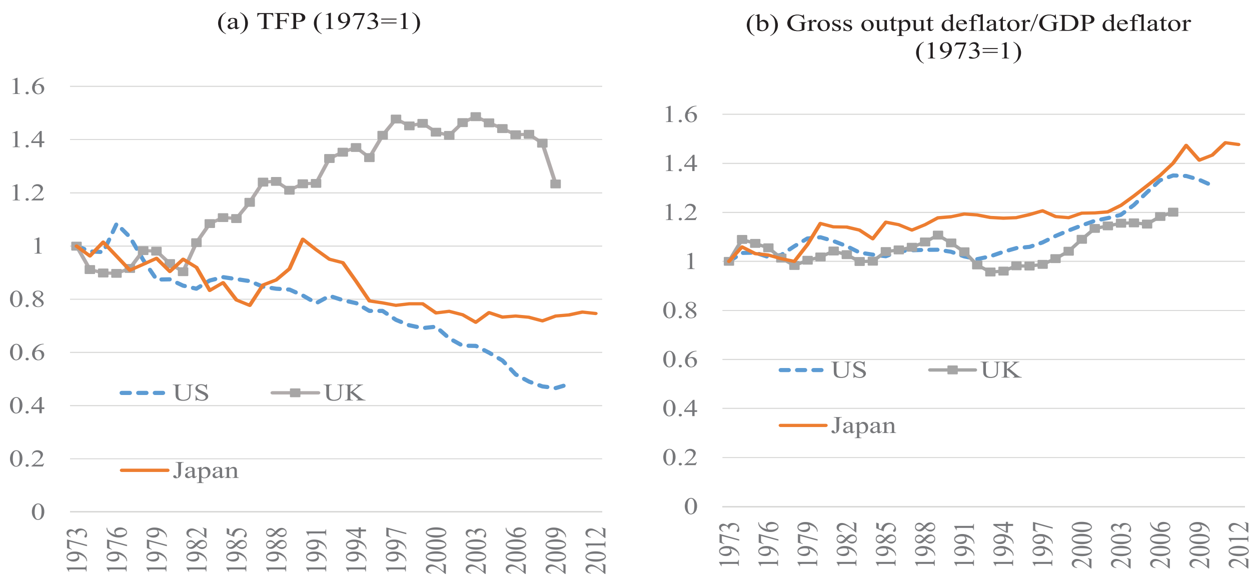
建設業における労働の質の推移：2000年=1、JIPデータベース



2.　日本政府による物価と実質生産計測における課題

建設物の物価統計がある米・英のうち、米国については、日本より観察されるTFP上昇が特に高いわけではない。

建設業：日米英比較



Source: Fukao, et al. (2016)

2.　日本政府による物価と実質生産計測における課題

- 卸・小売、医療などGDPの約2割を占める活動についても、生産額を実質化するための物価統計に深刻な問題がある。

商業：サービスの質（消費者への近接性、営業時間、取引形態等）変化の計測が必要（日本銀行調査統計局の企業向けサービス価格指数における新しい試み）。

医療：質の変化（疾患毎の死亡率の低下や生活の質の向上)を調整した生産量指標に基づいて生産性を計測する必要。

2.　サービス産業の生産性計測の課題（続）

卸・小売業

　OECD/Eurostat (2014)は、扱っている商品1単位あたりの商業サービスの質が不変なら、

商品1単位あたりのマージン価格＝
商品1単位あたりの販売価格－商品1単位あたりの仕入価格

を商業サービスの価格とするのが適当と推奨。米国とカナダのGDP統計は、この方式に移行済み。

　日本をはじめ他の多くの国は、商品価格を商業サービスの価格としている（日銀は最近、一部商品の卸売業について、上記推計を試行）。マージン率（（販売価格－仕入価格）／仕入価格）が変化すると、米加と日本の結果は大きく異なりうる。

2.　サービス産業の生産性計測の課題（続）

卸・小売業：2つのアプローチの違い

　仕入価格不変の下で、同じ取引形態における商業のTFPが2倍となり、商品1単位取引のための生産要素投入量が半分になり、完全競争の下で、商業マージン率も半分になると、

　米国ではデフレーターが半分になるため、商業の実質アウトプット（商業マージン／商業デフレーター）は不変、TFPは2倍になる。

　日本方式では、デフレーターは不変、実質アウトプット（商業マージン／商業デフレーター）は半分となるため、TFPは不変となる。

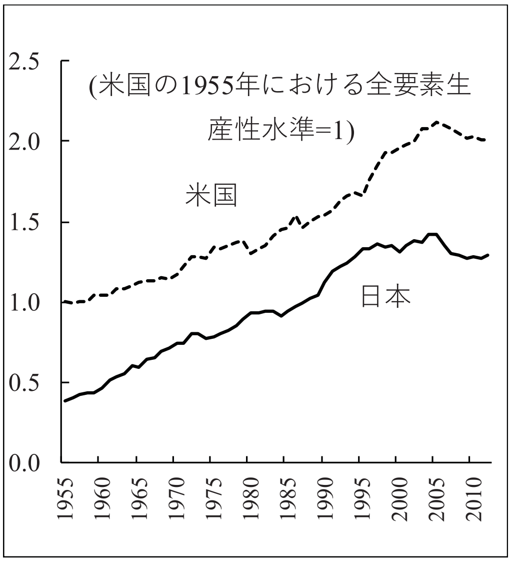
　なお、マージン率が高い取引形態へのシフトは、日本でも米国でも実質生産の拡大として認識される。

　米国では、ヘドニック・アプローチによる質の計測も試行されている。

2.　サービス産業の生産性計測の課題（続）

米国では、TFP上昇が1990年代末より加速。新方式がいつまで遡及されているか、要確認。

卸・小売業：日米比較



出所：Jorgenson, Nomura and Samuels (2016).

ICP-PPPs as a basis for International Comparisons of Productivity - continued

- **As part of the ICP, the OECD requests governments of the participating countries to conduct price surveys to collect prices of specified items (specifications for each good and service are prescribed). Based on these reports, the OECD compiles the PPP data of the ICP.**

For example, in the case of railway transportation in urban areas, the item for pricing is specified as

"an area ticket that allows changing to another mode of transport (such as a bus or tram) with a validity of 60 to 120 minutes for one ride, weekdays at 5pm"

- **As this example shows, specifications of items are mainly based on European experience. Moreover, quality differences in the provison of services, such as the frequency of trains, delays, crimes, accidents, the cleanliness of trains, etc., are not taken into account (Tsukada 2017).**

How can we account for quality differences in price comparisons?

- **There can be many factors that determine "quality".**
- **Such factors are usually very hard to identify and measure.**
- **Service is a package of various unobservable factors.**

Ask consumers directly

Conduct surveys on consumer preferences

Objective of the paper

- **We would like to quantify the welfare differences between US and Japan caused by differences in quality of services in US and Japan using quality adjusted quantity index numbers**

$$Q^{QA}_{US,Japan} = Q_{US,Japan}\big|_{quality\_unadjusted} \times \frac{Quality_{Japan}}{Quality_{US}}$$

- The quality un-adjusted quantity index is derivied using PPP from ICP

$$Q_{US,Japan}\big|_{quality\_unadjusted} = \frac{\sum_{i=1}^{N} p_{iJapan} q_{iJapan} \Big/ PPP_{US,Japan}}{\sum_{i=1}^{N} p_{iUS} q_{iUS}}$$

The objective is to estimate the *quality adjustment factor* using consumer surveys

Identification Procedure (1)

Suppose services of the average Japanese quality were offered in the US in English.

If the Japanese service was better in quality than the corresponding US service, how much more would you be willing to pay for the Japanese service?

Relative MWP (or MRS) for Japanese Service Evaluated at the US Price by the US (price premium for higher quality of Japanese Services)

$$\text{Relative MWP for JPN service by the US people} = (1 + b_{US})$$

Q3 Suppose services of average Japanese quality were offered in the US in English.
If the Japanese service was better in quality than the corresponding US service, how much more would you be willing to pay for the Japanese service?
Conversely, if the Japanese service was worse in quality, how much cheaper would it have to be for you to choose it over the corresponding US service?
* Please note that the numbers in the list below do not necessarily match the numbers in the explanation of service categories.
*

	Japanese quality is worse and so I feel a discount is necessary			←How much cheaper would it have to be for you choose the Japanese service? How much more would you be willing to pay for the Japanese service? →							Japanese quality is better and so I would be willing to pay more		
	60% or even more of a discount is necessary/will absolutely not use	-50%	-40%	-30%	-20%	-10%	0	+10%	+20%	+30%	+40%	+50%	Would be willing to pay 60% or even more
1. Taxi Response to previous question: → (【Q2S1の選択内容】)	○	○	○	○	○	○	○	○	○	○	○	○	○
2. Rental car Response to previous question: → (【Q2S2の選択内容】)	○	○	○	○	○	○	○	○	○	○	○	○	○
3. Automobile repair Response to previous question: → (【Q2S3の選択内容】)	○	○	○	○	○	○	○	○	○	○	○	○	○
4. Subway/urban commuter train Response to previous question: → (【Q2S4の選択内容】)	○	○	○	○	○	○	○	○	○	○	○	○	○
5. Long-distance train Response to previous question: → (【Q2S5の選択内容】)	○	○	○	○	○	○	○	○	○	○	○	○	○
6. Air travel Response to previous question: → (【Q2S6の選択内容】)	○	○	○	○	○	○	○	○	○	○	○	○	○

US-Japan Survey: Quality differences and willingness to pay

- **Survey was supported by funding from Japan Productivity Center**
- **Internet surveys were conducted both in Japan and the United States in February-April, 2017.**
- **Sampling 20-60s, reflecting the age-gender distribution in the census.**
- **Japanese Sample: From individuals who stayed in the United States at least for a period of three months since April, 2012.**

Sample size: 519 (480 valid responses – eliminated extreme answers)

- **US Sample: Individuals who stayed in Japan for at least one month since April, 2012. (initially imposing staying for three months or longer, but it was very hard to collect enough sample size)**

Sample size: 528 (412 valid responses)

Service Industry

1	Taxi	15	Hotel (mid-range)
2	Rental car	16	Hotel (economy)
3	Automobile repair	17	ATM, money wiring service
4	Subway	18	Real-estate agent
5	Long-distance train	19	Hospital
6	Air travel	20	Postal mail
7	Parcel delivery service	21	Provider with a mobile phone line
8	Convenience store	22	TV reception service using cable, satellite, Wi-Fi, etc.
9	General merchandise store	23	Hair dressing/beauty services (including beauty salons)
10	Department store	24	Laundry
11	Coffee shop	25	Travel services
12	Hamburger restaurant	26	Electricity, gas, heat supply, sewerage and water distribution/pipe repairs & management
13	Casual dining restaurant	27	Museum/art gallery
14	Hotel (luxury)	28	University education

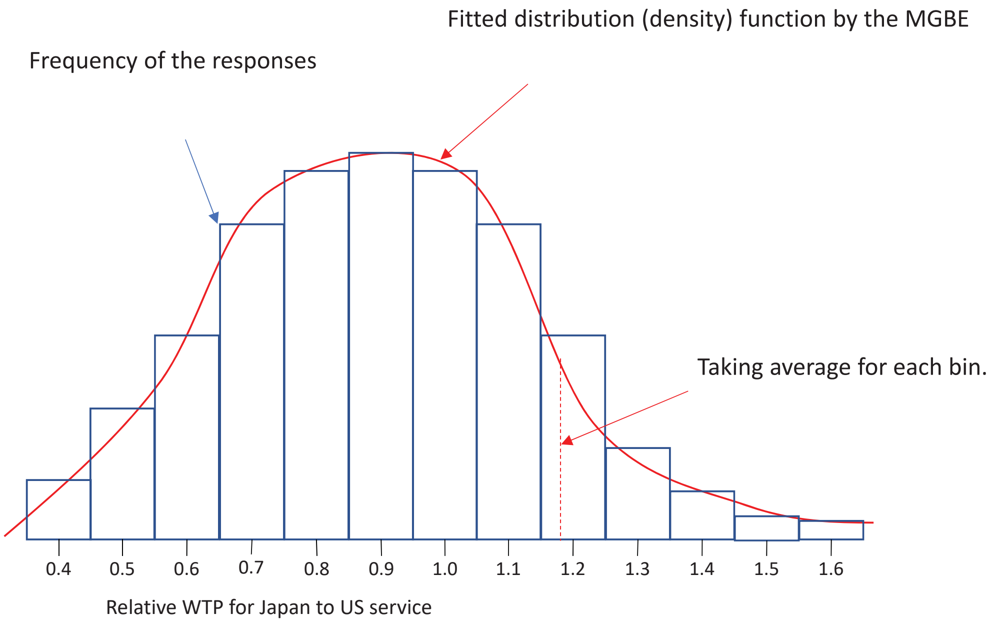
Estimation of Quality Difference and Willingness to Pay

Two Issues:

1) Questionnaire has responses in intervals – converting them to single values
   - Use average of the intervals but poses problems with the final open-ended interval
2) Differences between population and sample characteristics indicates that self-selection could induce biases into estimates

We deal with both of these econometrically.

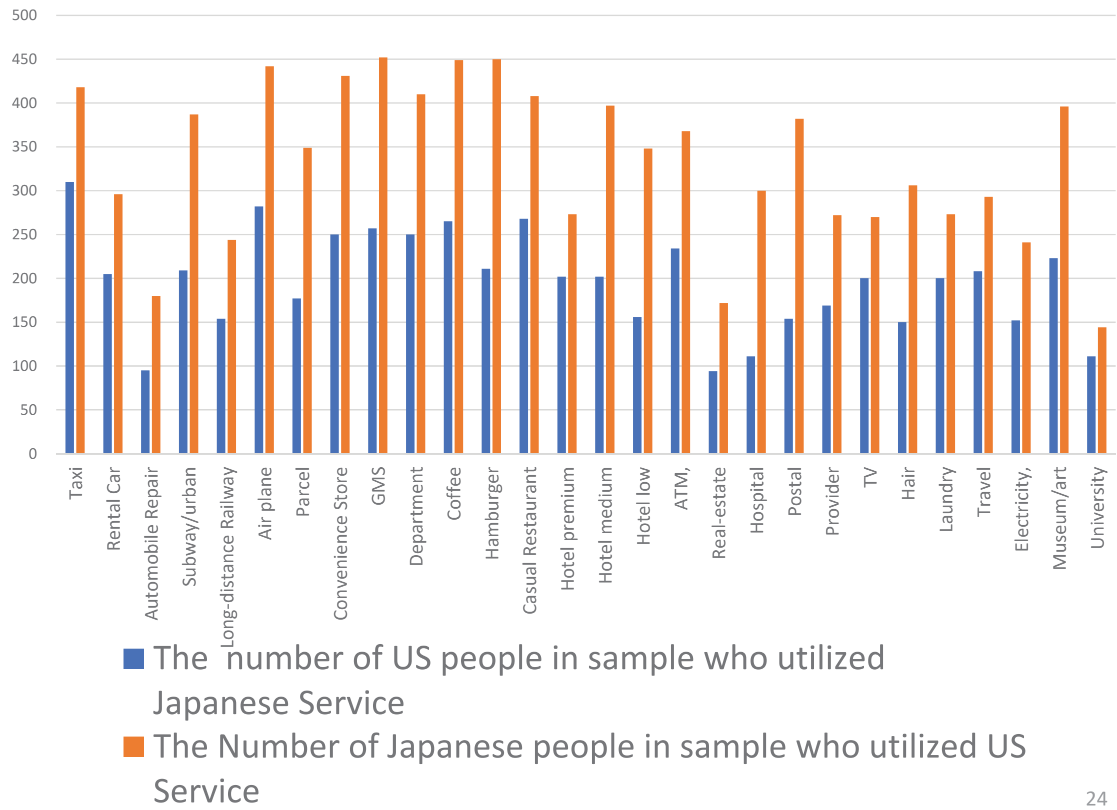
Curve Fitting to Interval and Open Ended Data



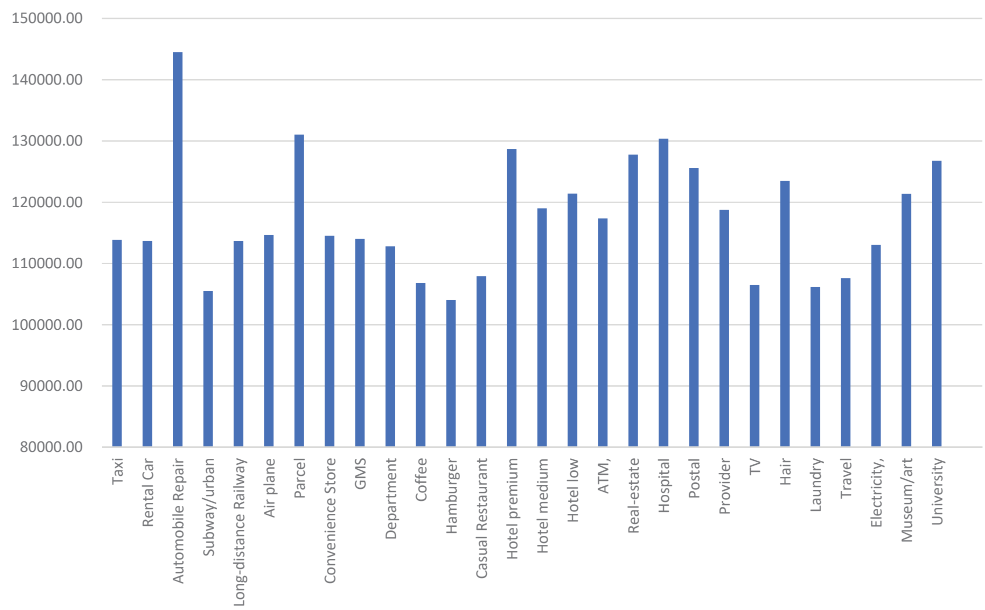
Sample versus Population characteristics

	US		Japan	
	Survey	Nationwide	Survey	Nationwide
household income	$107,902	$53,889	¥9,772,578	¥5,458,000
age	35.522	37.6	44.315	46.4
female	0.473	0.508	0.500	0.514
famsize	3.145	2.64	3.042	2.38
univ graduate	0.553	0.205	0.702	0.299
marriage	0.326	0.524	0.704	0.589
Total Population		321419		127110

Service Utilization



Mean Income Differences (US)



The Average Income of US people who experienced Japanese services.

Selection Biases

- **The sample averages of income and other variables are different from those in the census and other survey.**
- **Two types of selection biases might exist**
  - **1) Selection to visiting US or Japan**
  - **2) Selection to utilizing particular service**
- **Unfortunately, 1) is very hard to deal with because of very tiny fraction of US people visit and stay in Japan for more than one month.**
- **We control for the second bias using Heckman's selection model.**

Estimation of Quality Difference and Willingness to Pay

1) Estimate both OLS and Heckman for each country and sector
2) Use the national average values for the covariates, construct the predicted values
3) If the inverse Mills ratio is significantly different from zero, use the predicted values from Heckman's model, otherwise, use estimates of OLS.

Identification Procedure (2)

- From our survey, we can obtain two estimates of the relative price premium for Japanese Service
- Relative MWP for Japanese services by US people $= (1 + b_{US})$
- Relative MWP for US services by Japanese people $= \left(1 + b_{JPN}\right)$
- Relative MWP for Japanese services by Japanese people $= \frac{\mathbf{1}}{(1+b_{JPN})}$

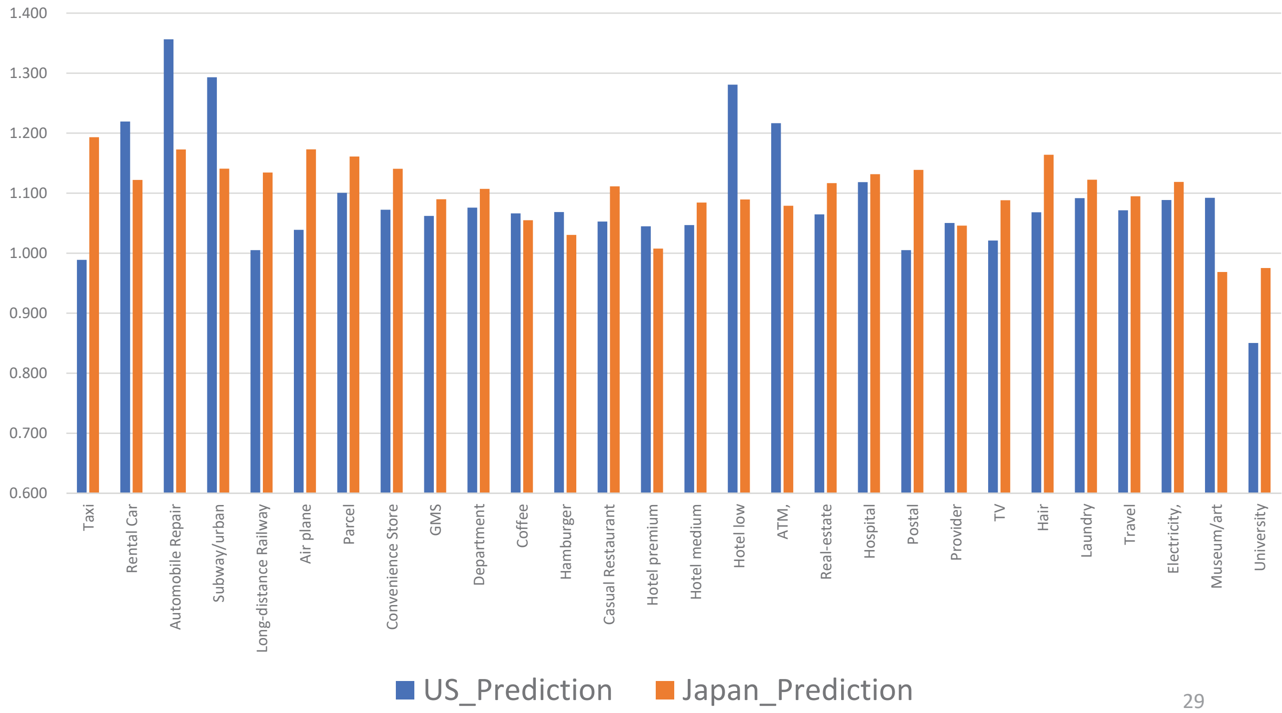
If preferences in both countries are identical, $\frac{\mathbf{1}}{(1+b_{JPN})} = (1 + b_{US})$

- As the quality adjustment ratio, we take the Geometric Mean of the two estimates:

$$\frac{(\mathbf{1}+\boldsymbol{a_{JPN}})}{(\mathbf{1}+\boldsymbol{a_{US}})} = \sqrt{\frac{(1+b_{JPN})}{(1+b_{US})}}$$

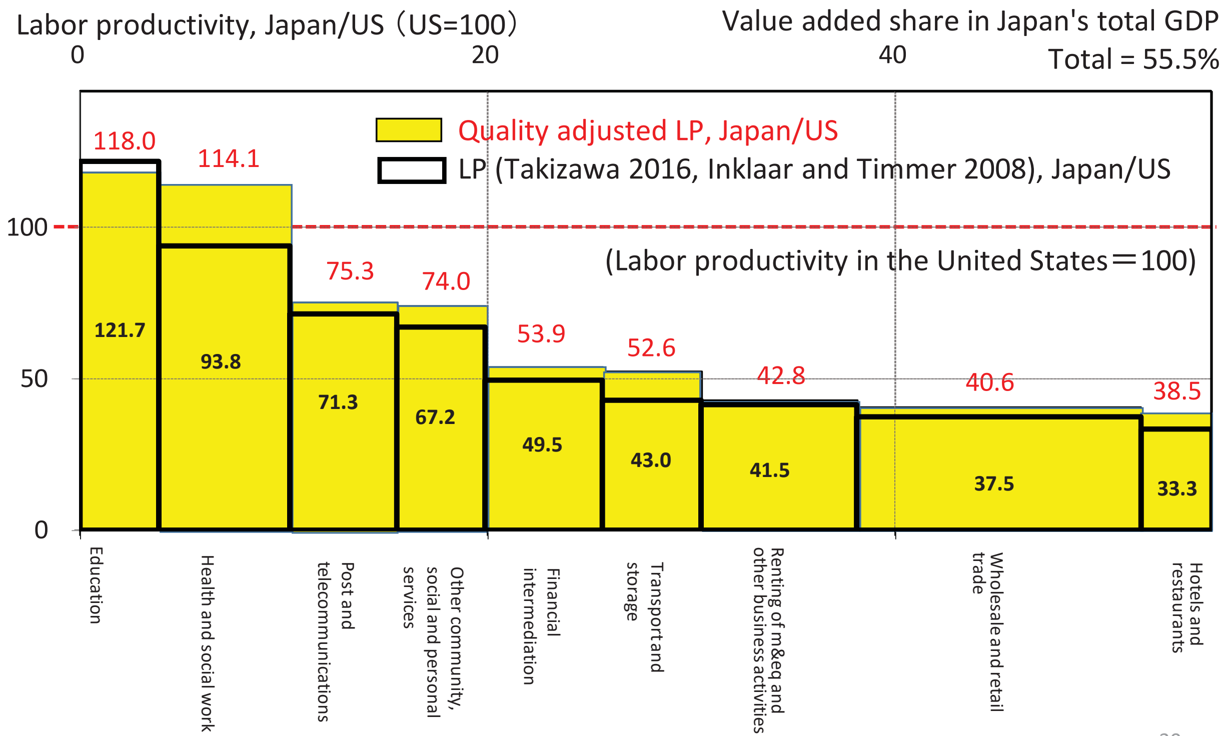
Predicted National Average MWP service sectors - Japan/US

$$\left(1 + b_{JPN}\right) \text{ and } \left(\frac{1}{1+b_{US}}\right)$$



Implication for Japan-US Labor Productivity Gap

Quality Adjusted Labor productivity gap between Japan and the United States and value added share (2010-2012)



We did not make quality adjustment of intermediate inputs.

Conclusions

- PPPs from ICP are used to convert service sector expenditures as well as outputs for international comparisons.
- While ICP uses Structured Product Descriptions to specify items for price surveys, these surveys do not adequately account for quality differences.
- To the extent quality differences are not captured, PPPs reflect both price as well as quality differences in the items priced.
- Quality differences are likely to be significant in service sector products (transport etc.) – at least anecdotal evidence suggests this.
- This paper represents first ever attempt to estimate PPPs for the services sector after adjustment for quality differences.

Conclusions

- Conducted a special survey of consumers in Japan and USA who have spent a reasonable length of time in visitor countries
- The survey is facilitated by funding from Japan Productivity Center.
- Based on the analysis of data on differences in quality as perceived by consumers, a quality adjusted PPP is constructed.
- Econometric analysis is used for correcting sample selection bias.
- We make use of Sato-Vartia index as it allows for a simple multiplicative decomposition of quality effects.
- Our estimated effect of quality difference is about 10%.
- We are currently in the process of estimating the effect of quality differences in services sector on overall Household Consumption PPP.

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Estimating the Impacts of Program Benefits: Using Instrumental Variables with Underreported and Imputed Data*

Melvin Stephens Jr.
University of Michigan and NBER

Takashi Unayama
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Estimating the Impacts of Program Benefits:

Using Instrumental Variables with Underreported and Imputed Data

Abstract

Survey non-response has risen in recent years which has increased the share of imputed and underreported values found on commonly used datasets. While this trend has been well-documented for earnings, the growth in non-response to government transfers questions has received far less attention. We demonstrate analytically that the underreporting and imputation of transfer benefits can lead to program impact estimates that are substantially overstated when using instrumental variables methods to correct for endogeneity and/or measurement error in benefit amounts. We document the importance of failing to account for these issues using two empirical examples.

1 Introduction

Vast economic literatures estimate the impacts of government benefits, typically using instrumental variables (IV) methods that treat benefit amounts as an endogenous regressor since program participation is often a choice (e.g., see surveys by Krueger and Meyer 2002; Currie 2004). Benefits reported on household surveys are typically measured with error and these errors are not likely to be classical as it is quite common for benefit amounts to be underreported (understated) or contain imputed values. We demonstrate analytically and via two empirical examples that IV estimation in such cases tends to overstate, sometimes substantially, the causal effect of program benefits.

Benefits are routinely imputed when households acknowledge receiving a benefit but do not recall the amount.[1] The top left corner of Figure **??** shows the well-known substantial increase in earnings imputations in the CPS (Lillard, Smith, and Welch 1986; Hirsch and Schumacher 2004; Bollinger and Hirsch 2006; Heckman and LaFontaine 2006).[2] The Figure also shows the far less appreciated fact that benefit imputations have increased just as dramatically over this period.[3]

A related issue is the underreporting of benefits in surveys. For example, the Consumer Expenditure Survey requires a single valid non-zero report from a major income source to deem a household as a "complete income reporter," potentially ignoring many other income sources (Paulin and Ferraro 1994). Meyer, Mok, and Sullivan (2009) find that total benefits received, computed by aggregating and appropriately weighting survey responses, fall short of administrative records of total benefit disbursements even when including imputations.

These types of measurement error yield important inconsistencies in empirical analysis. E.g.,

[1] Many researchers fail to acknowledge imputed data. Surveying articles that use the Current Population Survey (CPS) as the primary data source and were published in 2004 and 2013, inclusive, we find only 19 percent (16 out of 86 articles) mention imputed values. Even when acknowledged, some studies still treat imputed values as actual data. The eight journals surveyed are the *American Economic Review* (except *Papers and Proceedings* issues), *Industrial and Labor Relations Review*, the *Journal of Human Resources*, the *Journal of Labor Economics*, the *Journal of Public Economics*, *Labour Economics*, the *Quarterly Journal of Economics*, and the *Review of Economics and Statistics*.

[2] A sharp rise in earnings imputations is also found in the 1990s in the CPS Outgoing Rotation Group (Bollinger and Hirsch 2006). Determining which earnings values are imputed in the March CPS becomes less transparent beginning in 1988. See http://www.psc.isr.umich.edu/dis/data/kb/answer/1349.

[3] Meyer, Mok, and Sullivan (2009) present a related set of results in terms of dollars imputed rather than individuals. Prior to 1988, unemployment insurance and worker's compensation benefits are combined with other benefits. Prior to 1982, the CPS imputation codes for AFDC/TANF do not match the codebook values. Figure **??** imputation rates account for item non-response and whole supplement non-response, the roughly 10% of households that do not provide sufficient data for the March supplement. The variable FL-665, a flag for whole supplement non-response, does not appear on the public-use CPS data until 1991 although it does appear on the Unicon CPS files beginning in 1988. We thank Jay Stewart of the Bureau of Labor Statistics for directing us to these pre-1991 data.

since CPS earnings imputations do not account for union status, imputed earnings are uncorrelated with union status. As a result, Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006) find that OLS estimates of the union wage gap are substantially understated (attenuated) when including imputed earnings as compared to only using non-imputed earnings observations.

We show that underreporting and imputation can lead IV estimates to dramatically overstate the impacts of transfer programs. For example, if the instrument is based on program rules that vary across states and over time, imputed benefit values are not correlated with the instrumental variable if the imputation procedure does not condition on state of residence. The first stage estimated impact of the instrument on benefit amounts generally will be attenuated when using imputed benefits and, since the IV estimate is the ratio of the reduced form to the first stage coefficients on the instrument, the IV estimate will exceed is true value. When the instrument is uncorrelated with the imputed values and missing observations are randomly assigned, we show that the probability limit of the IV estimator exceeds the true IV parameter by a factor of $1/p$ where p is the fraction of households correctly reporting benefits. Since only two-thirds of recent CPS households correctly report benefits, IV estimates generated using imputed benefits are biased upwards by 50 percent. Benefit underreporting has a similar impact on the IV estimator.

If the non-reporting is randomly assigned, a straightforward empirical solution is to only use the non-imputed sub-sample. If values instead are missing at random (i.e., random after conditioning on covariates), methods which account for selection on observable characteristics such as inverse propensity score weighting can be applied. With selection on unobservables, estimates using the non-imputed sample are also inconsistent. We briefly discuss possible solutions in such instances.

We present two examples to demonstrate the empirical importance of these estimation issues. The first example uses the U.S. Social Security "notch" which Englehardt, Gruber, and Perry (2005) exploit to examine the impact of Social Security income on the propensity of the elderly to live independently. Since Social Security benefit imputations in the CPS use broad age categories rather than exact age, we find that the IV estimates are biased upwards by 20 to 30 percent. Our second example is a test for "excess sensitivity" among Japanese households in which monthly consumption changes are regressed on monthly income changes using the predictable pattern of child benefit payments as an instrument. Since only one quarter of eligible households report receiving these payments, the IV estimate is overstated by more than a factor of three.

The measurement error induced by underreporting and imputation is akin to "mean reverting" measurement error (Bound and Krueger 1991; Bound, Brown, Duncan, and Rodgers 1994).[4] Berger, Black, and Scott (2000) analyze the inconsistency of the IV estimator when using a noisy measure to instrument for another noisy measure when both suffer from mean reverting measurement error.[5] In our analysis, this inconsistency arises even when the instrument is correctly measured as is typical when benefit rules vary by well-measured characteristics such as age and state of residence. Our results easily extend to situations where the outcome of interest is underreported or imputed.

2 Econometric Framework

2.1 Model Setup

We focus on the population regression model for a continuous outcome y

$$y = \beta_0 + \beta_1 x + u \tag{1}$$

where x is an endogenous, continuous regressor such that $Cov(x, u) \neq 0$.[6]

Suppose that z is a valid, continuous instrumental variable for x such that $Cov(x, z) \neq 0$ and $Cov(z, u) = 0$. The first stage and reduced form equations are, respectively,

$$x = \pi_0 + \pi_1 z + \epsilon \tag{2}$$

$$y = \delta_0 + \delta_1 z + \varepsilon \tag{3}$$

Since z is assumed to be exogenous and free from measurement error, the OLS estimators for the coefficients on z in equations (2) and (3) are consistent as long the left-hand side variables in each equation are free of measurement error or suffer from classical measurement error. In addition, under these conditions, the IV estimator for β_1, which can be written as $\hat{\delta}_1/\hat{\pi}_1$, is also consistent.

[4] Gibson and Kim (2010) discuss a related issue for errors from using long-term retrospective recall data.
[5] See Card (1996) and Kane, Rouse, and Staiger (1999) for related analyses.
[6] It is straightforward to extend our analysis to include exogenous covariates and to account for binary variables.

Suppose the data contain an indicator, s_i, for whether the endogenous regressor, x, is an actual report, $s_i = 1$, or an underreport/imputed value, $s_i = 0$.[7] Writing $\hat{\pi}_1$ as a weighted average of the OLS estimators for each sub-group defined by s_i yields

$$\begin{aligned}
\hat{\pi}_1 &= \frac{\sum_i (z_i - \bar{z})(x_i - \bar{x})}{\sum_i (z_i - \bar{z})^2} \\
&= \frac{\sum_i s_i (z_i - \bar{z})(x_i - \bar{x}) + \sum_i (1 - s_i)(z_i - \bar{z})(x_i - \bar{x})}{\sum_i (z_i - \bar{z})^2} \\
&= \frac{\sum_i s_i (z_i - \bar{z})^2}{\sum_i (z_i - \bar{z})^2} \cdot \frac{\sum_i s_i (z_i - \bar{z})(x_i - \bar{x})}{\sum_i s_i (z_i - \bar{z})^2} + \frac{\sum_i (1 - s_i)(z_i - \bar{z})^2}{\sum_i (z_i - \bar{z})^2} \cdot \frac{\sum_i (1 - s_i)(z_i - \bar{z})(x_i - \bar{x})}{\sum_i (1 - s_i)(z_i - \bar{z})^2} \\
&= \frac{SS_{z,s=1}}{SS_z} \cdot \hat{\pi}_{1,s=1} + \frac{SS_{z,s=0}}{SS_z} \cdot \hat{\pi}_{1,s=0}
\end{aligned} \tag{4}$$

Thus, the OLS estimator for the first stage slope coefficient, $\hat{\pi}_1$, is a weighted average of the corresponding estimators when the model is estimated separately for each group, $\hat{\pi}_{1,s=1}$ and $\hat{\pi}_{1,s=0}$, where the weights are the share of the variation in the instrument, SS_z, belonging to each group.[8]

2.2 Interpreting the IV estimator

Suppose that s_i is randomly assigned and $p = P[s = 1]$ is the probability of providing an actual report. The first stage slope estimator using the sample of actual reporters, $\hat{\pi}_{1,s=1}$, is a consistent estimator of π_1. In addition, the weights in the final line of (4), $\frac{SS_{z,s=1}}{SS_z}$ and $\frac{SS_{z,s=0}}{SS_z}$, are consistent estimators of p and $1-p$, respectively. However, the corresponding estimator for the under/imputed reporters, $\hat{\pi}_{1,s=0}$, depends upon the corresponding underreporting or imputation process.

One common imputation procedure, the "hot deck," selects a replacement amount from a "donor" with the same values for a small set of characteristics. Hirsch and Schumacher (2004) and Bollinger and Hirsch (2006) note that this procedure does not preserve the covariance between the

[7] Most datasets contain flags to indicate which observations are imputed and which are not. This set-up is also useful for understanding the impact of underreporting even though this behavior typically is not explicitly flagged.

[8] The analysis focuses on imputed/underreported values of x but can be extended to either y or z. Typically, the instruments for benefits depend on well-measured demographic characteristics. E.g., Medicaid eligibility may depend on a child's age and the earned income tax credit (EITC) depends upon the family's number of children. Thus, it is likely that the endogenous regressor will be underreported or imputed while the instrumental variable is not.

allocated variable and the characteristics in the data that are left out of the imputation procedure. If the imputed value of x does not depend upon z, the correlation between x and z among the imputed observations will be quite small, if not zero.[9] Thus, $\hat{\pi}_{1,s=0} \approx 0$ and, per equation (4), the probability limit of $\hat{\pi}_1$ will equal $p\pi_1 + (1-p) \cdot 0 = p\pi_1$.[10]

For underreporting, suppose that observed x is a constant fraction, θ, of actual x. It is straightforward to show that $plim\,(\hat{\pi}_1)$ for underreporters is $\theta \cdot \pi_1$ and, thus, for the full sample $plim\,(\hat{\pi_1}) = p\pi_1 + (1-p)\,\theta\pi_1 < \pi_1$. Alternatively, when failing to report benefits (i.e., $\theta = 0$), perhaps when payments are small or received infrequently, the probability limit of $\hat{\pi}_1$ falls to $p\pi_1$.

The impact of underreported or imputed values of x on the IV estimator can be seen by substituting (4) and an analogous expression for the reduced form estimator into the IV estimator

$$\hat{\beta}_1^{IV} = \frac{\hat{\delta}_1}{\hat{\pi}_1} = \frac{\frac{SS_{z,s=1}}{SS_z} \cdot \hat{\delta}_{1,s=1} + \frac{SS_{z,s=0}}{SS_z} \cdot \hat{\delta}_{1,s=0}}{\frac{SS_{z,s=1}}{SS_z} \cdot \hat{\pi}_{1,s=1} + \frac{SS_{z,s=0}}{SS_z} \cdot \hat{\pi}_{1,s=0}} \tag{5}$$

As discussed above, the denominator converges to values smaller than π_1 when the endogenous regressor is underreported or imputed. The reduced form slope estimates, $\hat{\delta}_{1,s=1}$ and $\hat{\delta}_{1,s=0}$, for the actual and under/imputed reporters, respectively, are consistent if y is neither imputed nor underreported. Thus, the probability limit of the IV estimator exceeds β_1 and equals $\beta_1/p > \beta_1$ if underreporters all report no benefits or if the imputations are uncorrelated with the instrument.[11]

If the non-reporting of values is randomly assigned across observations (i.e., missing completely at random), then a practical solution to generate consistent IV estimates is to simply restrict the analysis to only non-imputed/non-underreported observations.[12] Alternatively, the availability of administrative data can provide a straightforward re-scaling of the first stage estimate when non-

[9] Bollinger and Hirsch (2006) note that some correlation between the imputed x's and z will occur if the covariates used in the imputation process for x are correlated with instrumental variable.

[10] In general, whether or not the probability limit of $\hat{\pi}$ exceeds π depends upon the imputation procedure. Bollinger and Hirsch (2006) and Heckman and LaFontaine (2006) show that CPS earnings imputations pool GED recipients with high school graduates and those attending, but not graduating from, a post secondary institution. Regressions yield larger GED returns among those with imputed wages relative to those who provide wage information ($plim\,(\hat{\pi_1}) > \pi_1$.)

[11] Extending the analysis to include exogenous regressors, $\mathbf{w}$, is straightforward using the Frisch-Waugh-Lovell Theorem. The OLS estimate for the coefficient on x when regressing y on x plus a vector of covariates $\mathbf{w}$ is numerically equivalent to first separately regressing y and x on $\mathbf{w}$ and then using the resulting residuals in a simple regression. As analogous procedure is available for 2SLS, we can again apply (5) but must compute the weights using the shares of the variation in the residualized values of the instrument z between the actual and underreported/imputed sub-samples.

[12] As a referee noted, Two Sample IV (Angrist and Krueger 1992) using the full sample for the reduced form and the non-imputed sample for the first stage may provide an efficiency gain over 2SLS with only non-imputed data.

reporting is random. If the non-reporting of values follows the selection on observables assumption, a number of straightforward methods are applicable: apply inverse propensity score weighting to the non-imputed sample (Bollinger and Hirsch 2006), construct imputations using the instruments in the imputation process (Hirsch and Schumacher 2004; Heckman and Lafontaine 2006), and implement the "general correction" formula of Bollinger and Hirsch (2006) to adjust the estimates for observable differences between the groups defined by s_i. When implementing these methods, the studies listed above do not find substantively different results between using the non-imputed sample only and correcting for selection on observables.

Additional methods may prove useful when confronted with non-random non-response. Recent estimation methods have focused on providing consistent point estimates when data are missing as a function of the outcomes only (Tang, Little, and Raghunathan 2003; Ramalho and Smith 2013) rather than as a function of the regressors as with the selection on observables assumption.[13] Another option is to construct bounds for π_1 which, since using the full sample yields consistent estimates of δ_1, will help produce bounds on β_1. Since most government benefits have a natural set of bounds due to programmatic rules, it may be possible to adapt methods developed by Manski (1997) and Kline and Santos (2013) to generate bounds on π_1 or use the approach of Manski and Pepper (2000) to derive bounds on β_1.[14] We do not pursue these approaches in the current paper.

3 Empirical Examples

3.1 The Impact of the Social Security Notch Using Imputed Benefits

The U.S. Social Security "notch" generated a sizable change in Social Security (hereafter S.S.) benefits for the affected birth cohorts (Krueger and Pischke 1992).[15] Englehardt, Gruber, and Perry (2005) (hereafter EGP), using data from the 1980-1999 March CPS supplements, investigate the impact of S.S. income on the probability that elderly-headed families live independently.[16] As OLS estimates of this relationship are likely inconsistent because S.S. benefits are a function

[13] These methods are related to those used in the literature on choice based sampling.

[14] See Kreider et al (2012) for a recent application of bounding when SNAP (food stamp) benefits are mis-reported.

[15] The Social Security Administration provides details of the notch at http://www.ssa.gov/history/pdf/notch.pdf

[16] EGP limit their analysis to families containing a S.S. recipient who is a male or never married female age 65 and up or is a widowed or divorced female age 62 and up. Their paper provides details of sample construction.

of lifetime earnings (e.g., wealthier individuals have higher benefits and are more likely to live independently), EGP use the variation across birth cohorts driven by the notch to instrument for S.S. benefits. Our analysis is likely important in this case since the share of S.S. benefit recipients with imputed benefits in the CPS rises from 20% to nearly 30% during this period.

To create their instrument, EGP construct a lifetime earnings profile based on the median male earner in the 1916 birth cohort. They use this profile to compute the S.S. benefit for every birth cohort from 1900-1933, using the Consumer Price Index (CPI) to deflate earnings across time. By fixing the earnings profile, the instrumental variable only reflects changes in the programmatic rules across birth cohorts. The solid line in Figure **??** shows the instrument by birth cohort.[17]

Imputations in the March CPS arise from two types of non-response. Item non-response arises when the respondent reports receiving a benefit but does not provide an amount. Whole supplement non-response occurs when households finish the basic CPS interview but do not participate in the March Supplement. As whole supplement non-response has remained constant at roughly ten percent, the recent increase in non-reporting is driven by item non-response.[18]

The CPS uses the hot deck imputation method to allocate missing values by taking a value from a donor observation with the same values for a subset of observable characteristics. For the March CPS supplement, all donors are drawn from the same year. To broaden the scope of potential matches, continuous match characteristics are collapsed into categorical values (e.g., age) while some values of a single categorical characteristic are combined (e.g., race/ethnicity).[19]

Age is used in the hot deck procedure to impute missing S.S. benefits. For item non-response, the imputation procedure uses seven age categories for selecting a donor: less than 35, 35 to 54, 55 to 61, 62 to 64, 65 to 69, 70 to 74, and 75+. For whole supplement non-response, the procedure always groups those ages 65 and up. The long and short dashed lines in Figure **??** show average S.S. income by birth cohort for non-imputed and imputed values, respectively. Actual S.S. income reports exhibit strong evidence of the notch while the imputed values do not.

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[17]We thank Gary Englehardt for sharing the values of the instrument by birth cohort.

[18]Prior to 1988, information on whole supplement imputation was contained in the data allocation flag for each income measure. In subsequent years, there is a single flag indicating whole supplement non-response.

[19]We thank Ed Welniak for providing us with the internal Census Bureau documents detailing the hot deck procedure beginning with the March 1989 CPS and retroactively applied to the March 1988 CPS. These documents are available from the authors. We have not been able to determine the imputation procedures used in earlier years.

EGP estimate the equation

$$P_{i,t} = \theta SSIncome_{i,t} + \boldsymbol{\beta}\mathbf{X}_{i,t} + \boldsymbol{\gamma}_i + \boldsymbol{\alpha}_t + \boldsymbol{\phi}_i + u_{i,t} \tag{6}$$

where $P_{i,t}$ is an indicator for having a shared living arrangement; $SSIncome_{i,t}$ is family S.S. income in thousands of dollars; $\mathbf{X}_{i,t}$ includes indicators the head's and spouse's (if present) education, spouse's age (if present), marital status (married, widowed, and divorced), white, and female; $\boldsymbol{\gamma}_i$ is a full set of indicators for the age (age+3 for widowed and divorced women) from ages 65 to 90; $\boldsymbol{\alpha}_t$ is a set of survey year indicators, and $\boldsymbol{\phi}_i$ is a set of indicators for the nine Census divisions.[20]

Table 1 presents our results.[21] Applying OLS to equation (6) shows that the probability of living in a shared arrangement falls as S.S. income increases. The impact is over twice as large in the non-imputed sample (column (2)) than in the imputed sample (column (3)), consistent with an attenuation bias due to including those with allocated benefits.

The first stage estimates vary as predicted by our analytical results. The estimated effect of the instrument on S.S. income is nearly 20% larger in the non-imputed sample than in the full sample, consistent with the share of S.S. benefit imputations during this period. Relatedly, the first stage relationship is more than three times larger for the non-imputed sample than the imputed sample.

As the shared living arrangements measure is based on the household roster, there is no reason to expect the reduced form estimate to depend upon whether S.S. income is imputed. While the reduced form estimate in the imputed sample is larger than the non-imputed sample estimate, the standard errors are sufficiently large that these differences are not statistically meaningful.

The final row of Table 1 presents the 2SLS estimates. The 2SLS estimate of -0.023 when using the full sample is over 25% larger than the estimate of -0.018 using the non-imputed sample only. Assuming that S.S. benefits are missing at random, the results from the full sample substantially overstate the efficacy of S.S. benefits in reducing shared living arrangements.[22]

[20] Our analysis differs from EGP's in two ways. First, we use the 1900-1930 birth cohorts rather than the 1900-1933 cohorts. Second, whereas EGP use age-by-year of birth cells, we use individual-level data to match our analytical results. Cell-level results are quite similar to our findings shown here (see Stephens and Unayama 2015a).

[21] Our estimates are weighted by the individual sampling weight for the S.S. recipient. The standard errors are clustered at the year of birth level. As equation (6) includes a number of exogenous covariates, we apply the Frisch-Waugh-Lovell theorem as described earlier and use the resulting residuals to estimate the first stage and reduced form models in order to be consistent with the decomposition shown in equation (5).

[22] When converted to elasticities, following EGP, we find a full sample elasticity of -0.53 which is 25% larger than our

Finally, we use inverse propensity score weighting (IPW) to correct for selection on observable characteristics (Bollinger and Hirsch 2006). We estimate a probit using an indicator for reporting an actual S.S. value as the outcome and use the same regressors as in equation (6). The IPW estimates (column (4)) are nearly identical to the non-imputed sample only estimates (column (2)).

3.2 Excess Sensitivity and the Underreporting of Japan's Child Benefit

A number of developed countries provide transfers based the age and number of children in a family (OECD 2011). In Japan, child benefits are paid three times a year, in equal amounts, in February, June, and October. While the Life-Cycle/Permanent Income Hypothesis (LCPIH) predicts that households will smooth consumption in response to predictable changes in income, a number of papers find that consumption is sensitive to the timing of income receipt including various types of government transfer payments (Stephens 2003; Shapiro 2005; Mastrobouni and Weinberg 2009; Stephens and Unayama 2011) and paychecks (Stephens 2006).

Japan introduced its child benefit system in 1972 by providing benefits to households with three or more children, extending to two child families in 1986, and to one child families in 1992.[23] Child benefits were means tested until 2009. Benefits initially continued until the child was fifteen but this age limit was lowered to three in 1986 before being incrementally raised over multiple years and again reached age fifteen in 2009. Benefits were relatively stable in real terms in the 1970s and 1980s, increased in 1992 and again in the mid and late 2000s, before subsequently decreasing. These benefits constitute over three percent of family income (Stephens and Unayama 2015b).

We test whether consumption exhibits "excess sensitivity" using monthly panel data from the Japanese Family Income and Expenditure Survey (JFIES) during 1992-2009 when all families with children are eligible for benefits and benefits are means tested. Families are surveyed in the JFIES for six consecutive months and are instructed to enter all expenditures and income into a daily diary. Our data contains detailed expenditure and income categories at a monthly frequency.[24]

Child benefits are recorded as part of an "other social security" variable which contains all social welfare benefits except public pension payments. In benefit distribution months, only 24% of

elasticity of -0.41 for the non-imputed sample. Stephens and Unayama (2015a) provide details of these calculations.

[23] Stephens and Unayama (2015b) provide a more detailed discussion of Japan's child benefit system.

[24] Additional details regarding the JFIES are given in Stephens and Unayama (2011).

eligible households report positive benefits amounts with 70% of positive reports exactly matching the child benefit value predicted by programmatic rules (i.e., based on age and number of children) and 20% of positive reports being too high, likely due to receiving additional transfer benefits. Only 4% of households report benefit receipt in non-benefit distribution months.

We regress monthly non-durable consumption changes on monthly income changes and, since income changes may reflect unexpected information (e.g., job loss), instrument for income changes using the monthly child benefit disbursement pattern.[25] Specifically, we estimate the equation

$$\Delta C_{i,t} = \alpha_0 + \alpha \Delta HHincome_{i,t} + \boldsymbol{\gamma} \mathbf{X}_{i,t} + u_{i,t} \tag{7}$$

where $\Delta C_{i,t}$ is the change in non-durable consumption from month $t-1$ to month t, $\Delta HHincome_{i,t}$ is the change in household income between adjacent months, and $\mathbf{X}_{i,t}$ are additional controls for monthly consumption growth including calendar year and month indicators, survey month indicators, the change in the number of household members, and the age of the household head and its square. The substantial amount of child benefit underreporting reduces the endogenous variable, $\Delta HHincome_{i,t}$, which makes our analytical results relevant for this analysis.[26]

Table 2 reports the tests of excess sensitivity.[27] Using our full sample (column (1)), the OLS estimate of the marginal propensity of consume out of income is 0.086. After instrumenting for income changes, we find a relatively large and significant estimate of 0.171. A finding of this magnitude typically is considered to be evidence of a substantial violation of the LCPIH.

The full sample first stage estimate is 0.283 although, in the absence of underreporting, we would expect this coefficient to equal one, i.e., income increasing one for one with benefits.[28] Thus, the large degree of underreporting severely attenuates the first stage estimate. Furthermore, since the IV estimate is the ratio of the reduced form estimate to the first stage estimate, in this example

[25] Non-durable expenditure is the outcome commonly used in the literature (e.g., Stephens and Unayama 2011). Upon entry into the JFIES, households report total household income for the twelve months prior to the survey period. We use this measure to determine whether households are above or below the means test threshold.

[26] $HHincome_{i,t}$ includes all monthly household income sources except bonus income. Bonuses are typically received in June (a child benefit month) and/or December. The first stage estimates are sensitive to including bonuses although they remain substantially less than one and the corresponding IV estimates are still biased upwards.

[27] The standard errors are clustered at the household level.

[28] One possibility is that child benefits crowd out other sources of income, e.g., earnings are reduced as a behavioral response to child benefits. Even if benefits lead households to work less, it seems very unlikely that these work effort reductions would exactly coincide with the months of benefit receipt rather than be spread throughout the year.

we would expect the IV estimate to simply equal the reduced form estimate in the absence of underreporting. A comparison of the IV and reduced form estimates indicates that underreporting inflates the causal estimate by more than a factor of three and yields a quite different substantive interpretation of the deviation of behavior from the standard model.[29]

One theoretical mechanism for excess sensitivity is that liquidity constrained households respond to anticipated income changes. Following Zeldes (1989), we split the sample based upon whether the household is above (unconstrained) or below (constrained) current year median sample income. For both types of households we find evidence of excess sensitivity in the reduced form estimates in Table 2. However, we find similar attenuated first stage estimates and dramatically overstated 2SLS estimates for both groups due to benefit underreporting.

Assuming that underreporting occurs randomly, we examine a "correct reports" sample defined as observations where the "other social security" amount matches the amount computed for the instrument. We only lose roughly one-third of the sample as benefit changes are zero for the majority of months. The OLS estimate for the correct reports sample (column (4)) is similar to the full sample estimate. Moreover, consistent with the prediction that one yen of child benefits raises family income by one yen, we cannot reject the null that the first stage estimate is one.

However, the remaining estimates in column (4) suggest that the correct reports sample estimates are subject to selection on unobservables. The reduced form estimate for this sample is twice as large as the corresponding full sample estimate. Adjusting for selection on observables (column (5)) yields similar results. One possibility is households that are most likely to report child benefits are more likely to change their spending due to benefits. Clearly, only using non-missing observations is not a universal panacea for addressing underreported and imputed data.

4 Discussion

The continuing rise in survey non-response has increased the share of observations with imputed and underreported values for government benefits. We demonstrate analytically that the underreporting

[29] One concern is that we may misclassify ineligible households as being eligible due to mis-measured household income. However, we split the sample between high and low income households below in order to examine whether the response can be attributed to liquidity constraints, we find nearly identical first stage estimates for both samples.

and imputation of government transfers can lead to a substantial overstatement of the causal effect of government transfers when applying instrumental variables methods to correct for the endogeneity and/or measurement error. Our empirical findings confirm these concerns.

We conclude with some observations for empirical research. First, researchers should pay close attention to the magnitude of the first stage estimates in addition to the strength of the instruments. Second, when non-reporting is not random, caution needs to be used when dropping non-responders as illustrated by our child benefit example. Third, researchers should take care to construct correct variance estimates when using imputed data, possibly through adjusted variance formulas (Abadie and Imbens 2012) or bootstrap methods (Shao and Sitter 1996). Finally, it is important to understand the imputation procedures a data provider uses. For example, for the four benefit items in the March CPS that use state of residence for imputations, this information is collapsed into five broad groupings which do not reflect geographic location, are constant over time and, thus, are unlikely to be correlated with the state-year variation used in many IV applications.

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Table 1 - Social Security Notch Regressions

Sample:	Pooled	Non-Imputed S.S. Income	Imputed S.S. Income	Non-Imputed S.S. Income IPW
	(1)	(2)	(3)	(4)
OLS	-0.010	-0.012	-0.005	-0.011
	(0.0005)	(0.0005)	(0.0008)	(0.0005)
First Stage	0.227	0.267	0.070	0.270
(Residuals)	(0.050)	(0.056)	(0.040)	(0.059)
Reduced Form	-0.0052	-0.0048	-0.0071	-0.0049
(Residuals)	(0.0026)	(0.0023)	(0.0045)	(0.0022)
2SLS	-0.023	-0.018	-0.097	-0.018
	(0.014)	(0.010)	(0.068)	(0.010)
N	256,710	203,983	52,727	203,983

Notes: Each estimate in the Table is from a separate regression. The dependent variable is an indicator whether the family is living in a shared arrangement. The OLS and 2SLS estimates are the coefficients on family S.S. income and also include controls listed in the text. The first stage and reduced form estimates are the coefficient on the S.S. instrument based on the Frisch-Waugh-Lovell decomposition. Standard errors are clustered at the year of birth.

Table 2 - Excess Sensitivity Regressions

Sample:	Full	Below Median Income	Above Median Income	Correct Reports	Correct Reports IPW
	(1)	(2)	(3)	(4)	(5)
OLS	0.086	0.079	0.090	0.087	0.090
	(0.009)	(0.010)	(0.013)	(0.011)	(0.011)
2SLS	0.171	0.135	0.220	0.086	0.094
	(0.083)	(0.080)	(0.144)	(0.038)	(0.039)
First Stage	0.283	0.312	0.253	1.07	1.10
	(0.032)	(0.047)	(0.043)	(0.045)	(0.046)
Reduced Form	0.048	0.042	0.056	0.093	0.103
	(0.024)	(0.025)	(0.037)	(0.041)	(0.044)
N	186,383	93,063	93,320	122,217	122,217

Notes: Each estimate in the Table is from a separate regression. The dependent variable is the change in in non-durable consumption from month $t-1$ to t. The OLS and 2SLS estimates are the coefficients on the change in reported other social security income from month $t-1$ to t. The first stage and reduced form estimates are the coefficient on the programmatic child benefit change from month $t-1$ to t. Additional controls are listed in the text.

Figure 1: March CPS Share Imputed Among Those With Positive Amounts 1988-2013

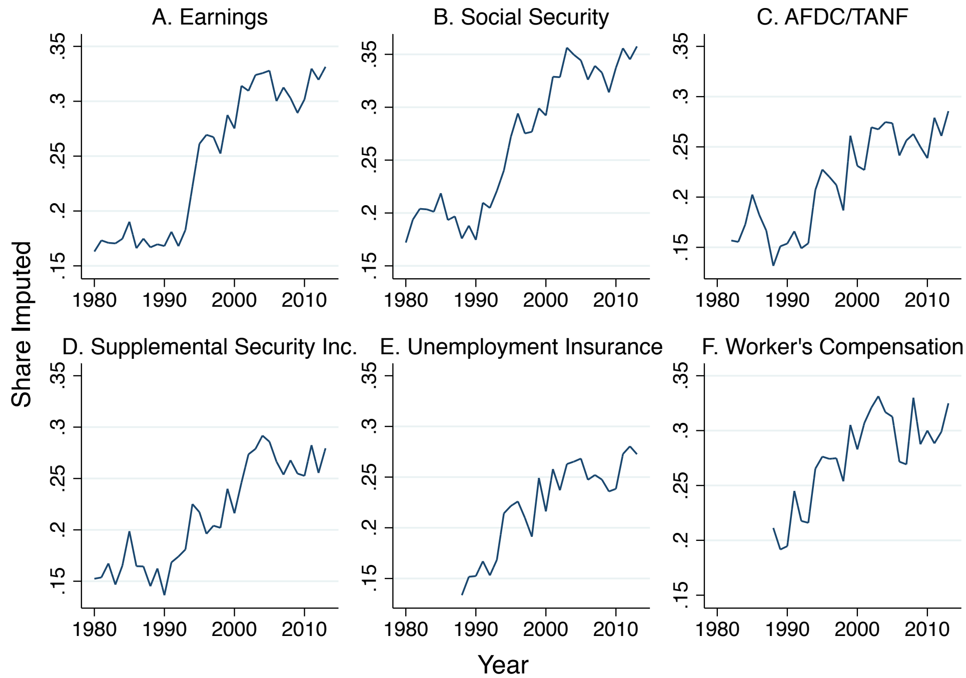
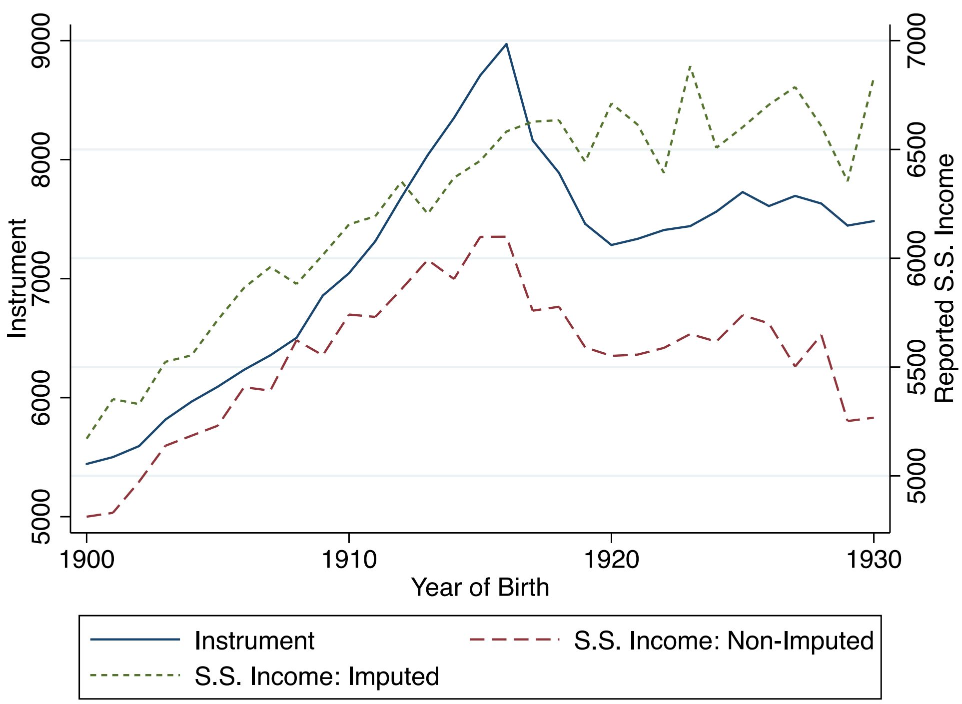


Figure 2: CPS Social Security Income and the Instrument by Year of Birth



世代間資産移転と相続税

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家計資産統計

- 家計の保有する資産については、国民経済計算（ＳＮＡ統計）の貸借対照表の中で、国民資産・負債残高が報告されている。
- わが国では、日本銀行が1905年に「国富調査」を始め、1970年の経済企画庁の調査まで12回行われてきたが、1970年の調査以来、大規模な国富調査は行われていない。
- 現在、遺産相続が行われた土地について相続登記されていない所有者不明の土地が私有地の20％にも上っていることが社会問題化している（吉原（2017））。

家計資産の推移

- 国民資産・負債についての概観

  2014年度のSNA統計によれば、国民資産残高は9684.4兆円（内、非金融資産2741.7兆円、金融資産6942.7兆円）。負債は6575.9兆円である。

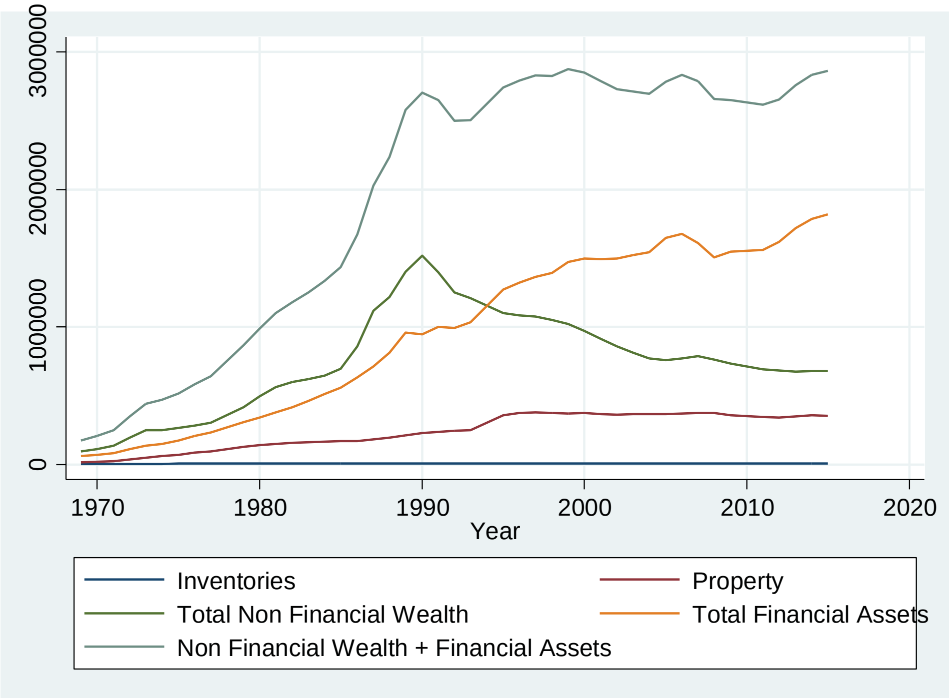
- 部門別正味資産

  2014年度のSNA統計によれば、資産から負債を差し引いた正味資産を部門別に見ると、家計は2359.4兆円、金融機関は139.7兆円、非金融法人は548.2兆円、一般政府は－13.5兆円となっている。

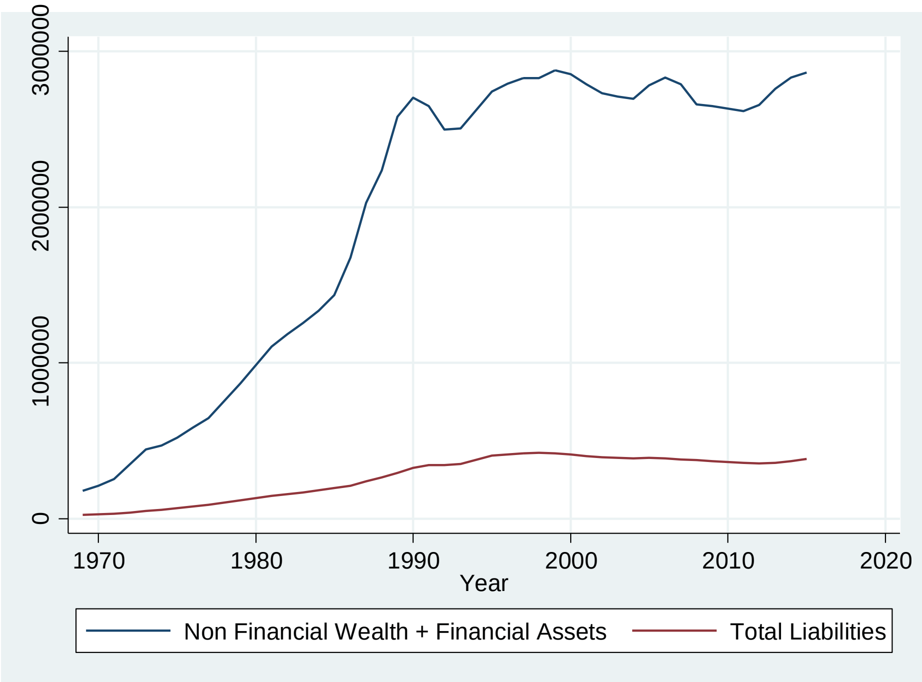
- 家計の資産・負債残高

  家計の資産残高は2727.1兆円（内、非金融資産1031.6兆円、金融資産1695.5兆円）。負債残高は367.7兆円で正味資産2359.4兆円。

図表1．家計資産名目残高の推移



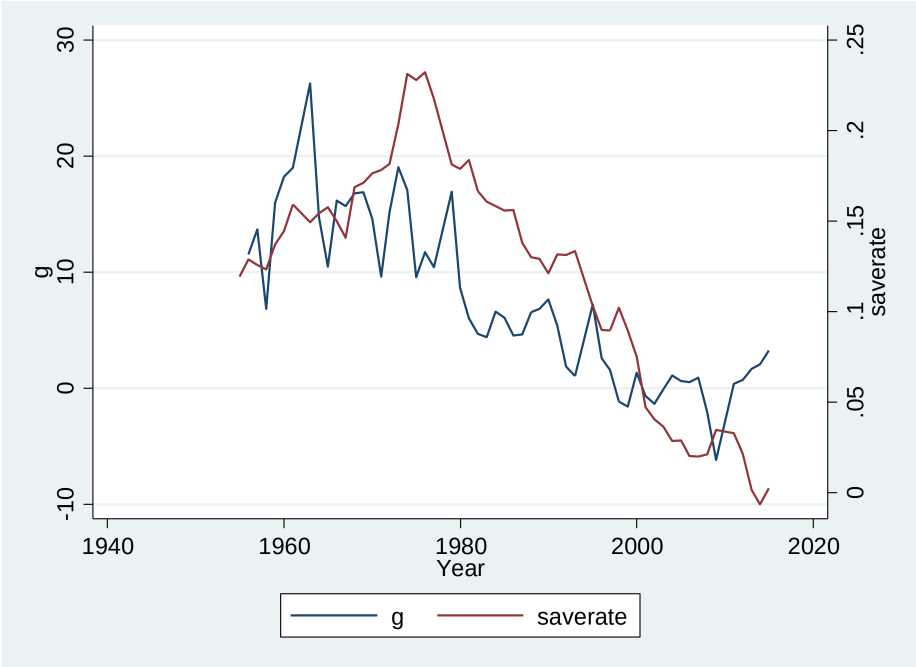
図表2. 家計名目総資産と負債の推移



家計貯蓄モデルとの関係

- 1950年代より現在に至るまで、家計の消費・貯蓄モデルの主流はライフサイクル仮説（恒常所得仮説）、遺産動機仮説の3つ、特にライフサイクル仮説と恒常所得仮説を1モデルと考えると、2つのモデルについて論じられてきた。
- 家計部門の資産残高は1969年度末で176.85兆円、非金融資産112.6兆円、金融資産64.26兆円、負債残高24兆円、正味資産152.87兆円であったことを思えば、日本家計は高度成長期を通して、戦争で資産を失い、ほぼゼロの状態から資本蓄積を行ってきたことは疑いない。
- 現在進行中の少子高齢化の下でも、家計部門の資産残高は減っていないとすれば、資産は世代間、家計間で移転されていると考えられる。

図表3. 家計貯蓄率と経済成長率



遺産相続および相続税・贈与税への関心の高まり

なぜ相続に関心を払うのか（1）

- 相続という行動は何を目的としているのか？家族内での世代間生活水準の確保を通した家系の維持や家族経営事業（農業を含む）の承継。
- 経済が定常状態に入った時には、フローでの貯蓄は低下し、資産蓄積は進まないが、資産移転が行われることで、ライフサイクルのステージが異なる経済主体に資産の所有権が移る。経済社会的に何が起こるのか？
- 高度成長期（1950－80年代）から低（ゼロ）成長期（1990－2010年代）を経験することで、経済環境の変化が貯蓄や資産蓄積行動に及ぼす影響を識別できるようになった。高度成長期の資産蓄積に対する解釈としての遺産動機を統計的に検証するチャンス。

なぜ相続に関心を払うのか（2）

- 貯蓄モデルとして見直した場合、恒常所得・ライフサイクル仮説と遺産動機仮説の大きな違いは、前者が主としてフロー概念の分析であるのに対して、後者は主としてストック概念の分析であるということにある。
- 会計学の概念を援用すれば、前者が損益計算書内での議論であるのに対して、後者は貸借対照表と損益計算書を合わせて用いている複式簿記のような考え方をしている。
- 家計行動は、土地や家屋と言った実物資産、その他の耐久消費財を保有して基本的な生活水準を維持しながら、金融資産によっていざという時の支出に備え、日常の支出はフロー所得の中で収支尻を合わせていると考えるのが一般的。
- 相続はその資産が世代間で移転される生涯に数度の機会。

相続の概要（１）

- 近年高齢化の影響で年間死亡者数が１２０万人を超えるようになり、相続件数も増えている（2013年で126.8万人）。
- 死亡者のうち相続税課税対象になる件数の比率は2013年で4.3%と極めて低い。この数字は、バブル経済のピーク期に当たる1987年に7.9%の最高値を記録して以来低下し、2000年代に入ってからも低迷している。
- 納税者の73％が2億円以下の遺産額であり、10億円を超える遺産額を受け取った人は全体の0.4％程度。
- 相続税課税対象になった人の一人当たりの相続税額の最高額はバブル経済のピークが過ぎた1991年に7011.2万円で、実効税率22.2%を記録している。2013年で20％を超える実効税率に該当するのは7億円超の相続額受け取る者。

相続の概要（２）

- 死亡した人の内、相続税の対象とならない残り95％の相続資産額はどのぐらいあるのだろうか？後で説明するように、2015年1月より相続税制度が改正され、相続人が1人の最もシンプルなケースで課税控除額は3600万円まで免税となり、それ以下の遺産額を受け取った家族は相続税は払っていない。
- 税務当局もそれらの無税者の相続に関するデータは公表していないし、基本的に税務データとして収集していない（ただ、死亡が確認された後、税務署から納税が必要かどうかの確認はあるので、ある程度の情報は蓄積しているはずである）。
- 少額であれ、遺産相続を受け取っているとすれば、これを推計する必要はある。誰も墓場まで資産を持っていけないので、必ず資産の移転は発生している。
- 対応：相続を受けた人にアンケート調査をして平均遺産相続額を聞くか実際の家計資産データから推計する。

相続の概要（3）

- アンケート調査（家計経済研究所『消費生活に関するパネル調査』(第11回調査（2004年）以後)、内閣府経済社会総合研究所（2010）、フィデリティ退職・投資教育研究所レポート（2012）、第一生命『中高年者の遺産相続に関する調査』（2007）等））
- 例えば、2012年に5500人の相続人に対してアンケートを行ったフィデリティ退職・投資教育研究所レポートによると、相続額の平均は3172万円、中央値が862万円である。そのうち、親子間の相続額の中央値は1002万円、配偶者間の相続額の中央値は1999万円であり、これに配偶者相続人数53.2万人、親子間相続人数278万人をそれぞれ掛けて足し合わせて年間相続総額は38.5兆円程度であると推計している。

相続の概要（4）

- Piketty (2010)はフランスの相続に関する長期データ（1820－2008）と追加的推計を用いて、次のような指標を計算。

- $B_t/Y_t = \mu_t\, m_t\, W_t/Y_t \Leftrightarrow b_{yt} = \mu_t m_t \beta_t$

  ここで、B_t=年間遺産相続額、Y_t＝国民所得、W_t=家計総資産、m_t=死亡確率、μ_t＝被相続人平均資産/相続人平均資産、$b_{yt}=B_t/Y_t$=年間遺産相続額/国民所得、β_t＝W_t/Y_t=家計総資産/国民所得

- $b_{wt} = \frac{B_t}{W_t} = \mu_t m_t$

  ここで、b_{wt}＝年間遺産相続資産比率、$e_t=W_t/B_t$=資産回転率（乗数）

データ

- Piketty（2010）の指標を計算するためには、年間遺産相続額を推計する必要があるが、他の指標は国民経済計算や人口統計、金融統計から使える。
- 年間遺産相続額の計算においては、遺産相続だけではなく、贈与も含める。また相続は配偶者間、親子間、親族間でも行われるため、その実態についている程度の情報が必要になる。
- 全国消費実態調査（総務省）1984-2014年（７回分　30年間）
- 人口動態統計（厚生労働省）
- 国民経済計算（内閣府）毎年

遺産相続額の推計方法

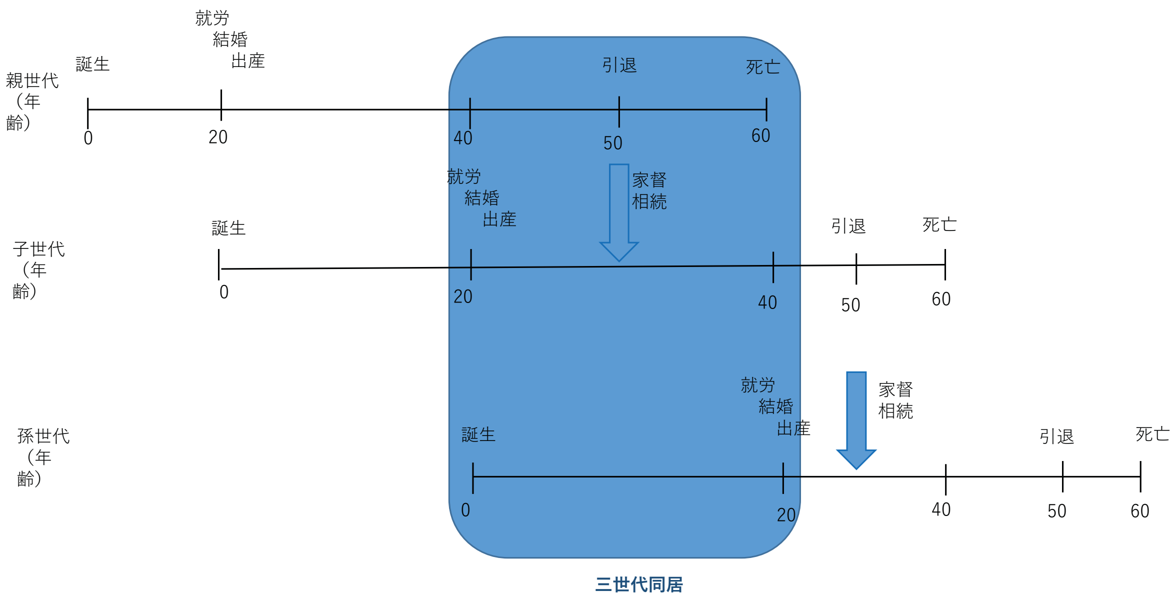
- Piketty (2010)の定義によれば、年間遺産相続・贈与額は被相続者の死亡時点の正味総資産（実物資産に金融資産から負債を引いた正味金融資産を加えたもの）、あるいは他の被相続者が存命中に年間に行った贈与の合計を指す。
- 被相続者の一人当たり正味総資産保有額は『全国消費実態調査』より年齢階層毎に計算でき、かつ年齢階層別の死亡者数は『人口動態統計』に記載されている。一人あたりの正味純資産に年齢階層別死亡数を掛け合わせて、集計すれば、年間遺産相続額を一応求められる。
- 年間での贈与総額の推計をしてそれを年間遺産相続額と加えれば求める年間遺産相続・贈与額が得られる簡易な計算のよれば70兆－８0兆円ぐらいの規模と推計されている。

相続の推計戦略（１）

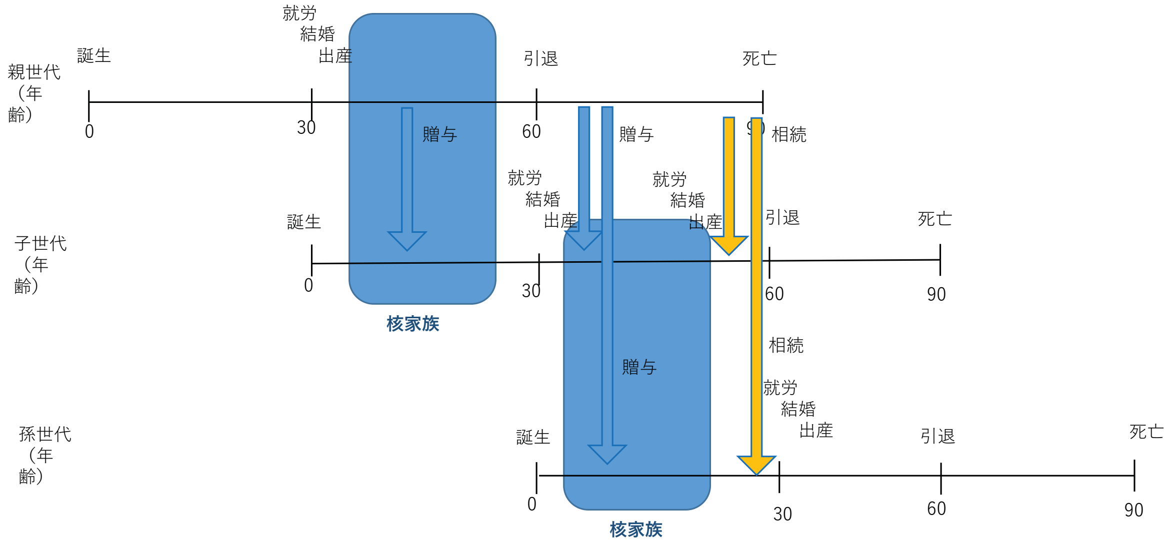
- Piketty (2010)の手法

　　$e_t=W_t/B_t$=資産回転率（乗数）を世代交代の１期間の長さであると解釈すると、e_t=平均出産年齢として、 $B_t=W_t/e_t$ として求める。日本の平均出産年齢は過去２０年ばかりは２９－３０歳程度である。

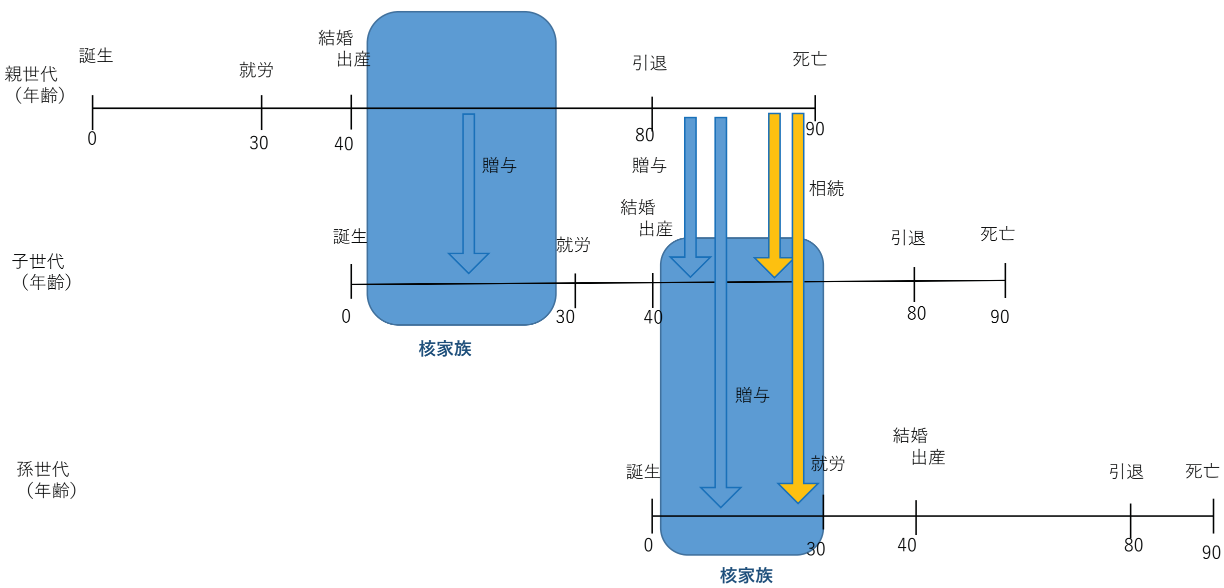
図表4.　古典的な結婚・出産タイミング下での重複世代モデル（1世代20年、生涯60年時代）



図表5．現代的な結婚・出産タイミング下での重複世代モデル（1世代30年、生涯90年時代）



図表6．結婚タイミング先延ばし下での重複世代モデル（1世代40年、生涯90年時代）



遺産相続の指標: フランス

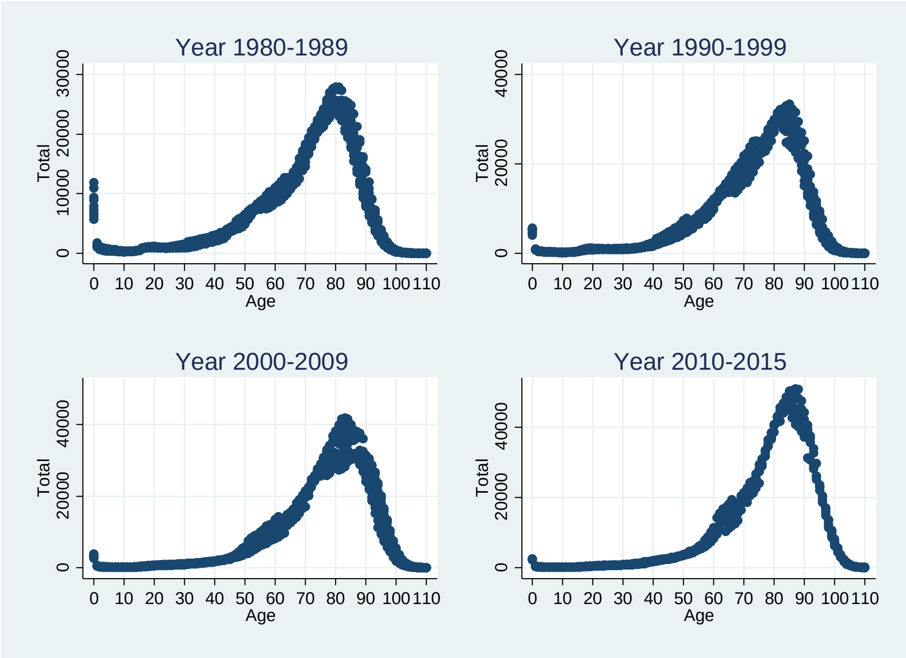
- Piketty(2010)によれば、フランスの数値は次のようになっている。
- 年間遺産相続額/国民所得$b_{yt}=B_t/Y_t$は20.3%（1820）、22.7%（1910-13）、14.5%（2008）であり、年間遺産相続資産比率$b_{wt}=B_t/W_t$は3.7%（1820）、3.5%（1910-13）、2.6%（2008）である。資産回転率e_tは27year (1820)、28.9year(1910-13)、38.7 year (2008)である。
- この間、フランスでは遺産相続の占める割合が20世紀半ばにかけて低下し、その後上昇しつつあるというのがPikettyの推論である。

相続の推計戦略（2）

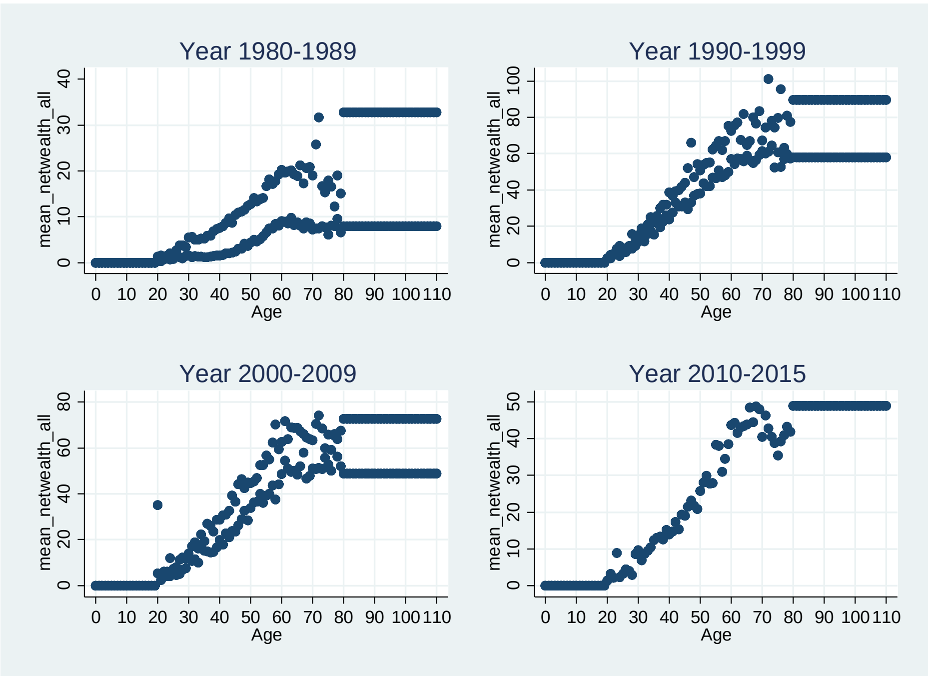
- ミクロデータからの積み上げ法

　全国消費実態調査の年齢階層別正味資産保有額の平均値を求め、それに年間年齢階層別死亡者数を掛け合わせて、年間遺産相続額を推計する。ここには、表面的には贈与は含まれていない。贈与部分はPikettyの手法との階差を取ることで、推計する。

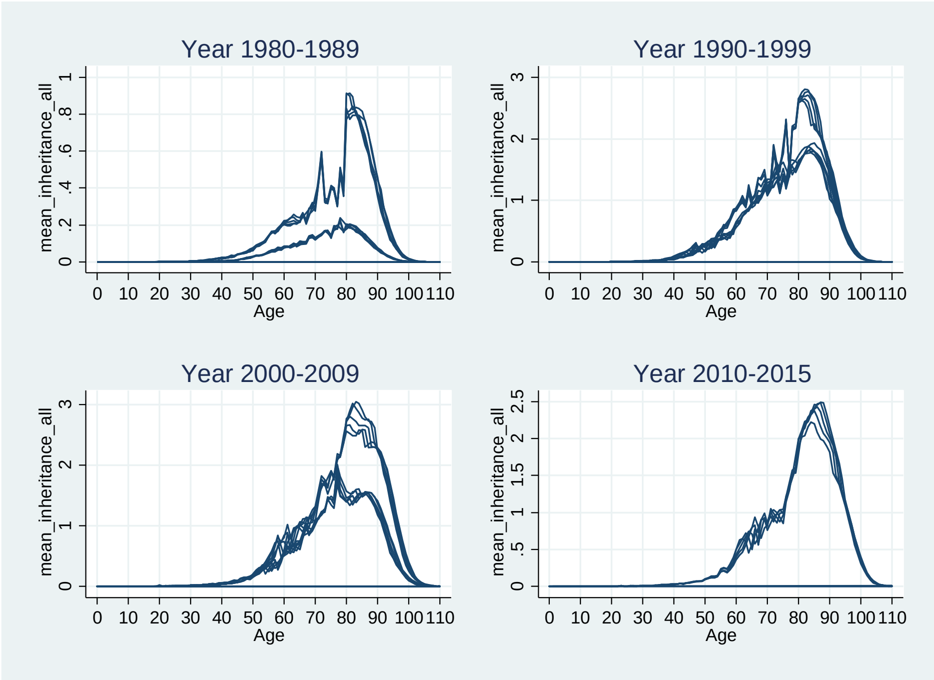
図表7．年齢別階層別死亡者数



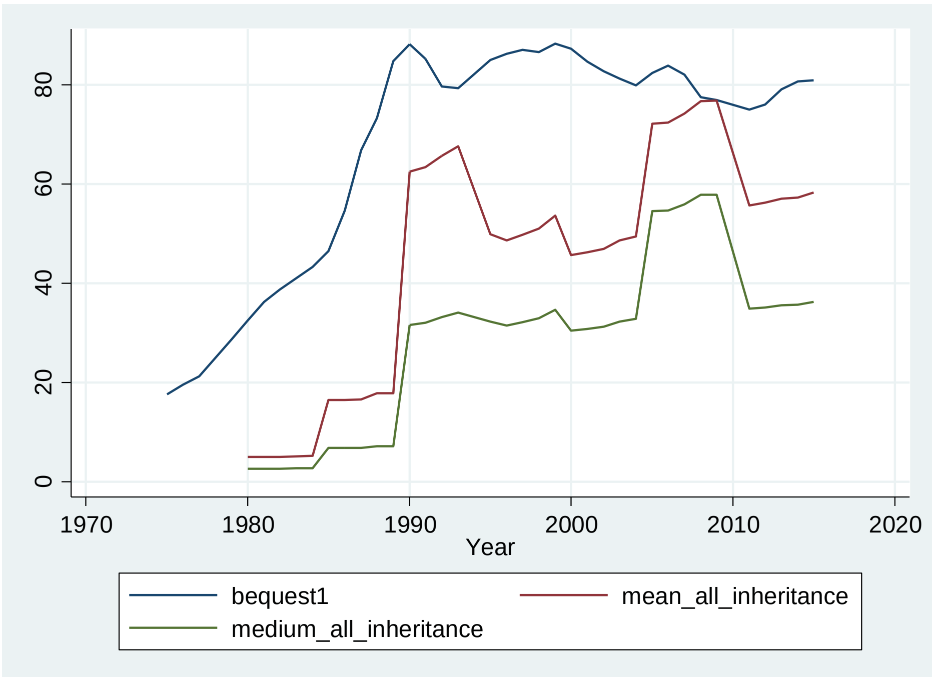
図表8．年齢階層別正味資産保有額



図表9. 年齢階層別遺産相続推計額



図表10. 年間世代間資産移転の推計



図表11. 世代間資産移転の推計

(trillion yen 兆円)

Year年	Bequest 世代間移転	Inheritance 遺産相続	Gifts 生前贈与
1980-1984	38.34	5.09	33.26
1985-1989	65.16	17.06	48.10
1990-1994	83.57	65.34	18.23
1995-1999	86.60	50.57	36.02
2000-2004	83.10	47.76	35.74
2005-2009	80.48	74.41	6.06
2010-2015	78.30	56.90	21.41
Average 平均	73.73	45.47	28.26

遺産相続の指標：日本

- 日本では、Piketty (2010)の手法で、１９６９－２０１４年まで毎年推計すると年間相続額が６５兆円から８７兆円程度であることがわかった（図表10参照）。
- ミクロデータに基づく積み重ね（ミクロデータに基づく推計には改善の余地があり、今後の課題として残っている）では、同期間に４７兆円から７４兆円程度である（図表１1参照）。
- 図表10から明らかなように、両者の動きはかなり連動でぃており、従来指摘されてきた年間相続額よりかなり大きい。３５－５０兆円は課税対象から外れた移転である（図表１1参照）
- 贈与として移転されているものは、高齢者の消費として扱われることがほとんどで、税の対象としては扱われていない。贈与税についての扱いが大きな課題として残っている。

相続税の考え方（1）

- 相続税には国際的に統一した課税方式がある訳ではない。それぞれの国の相続の複雑さや民法などの歴史的経緯を反映している。大別すると遺産を残す人（被相続人）が納税義務を負うと考える遺産税方式と遺産を受取る人（相続人）が納税義務を負うと考える取得税方式に分かれる。
- 遺産税方式では遺産額に応じて課税され、残りを遺産分割するというもので、アメリカ、イギリス、オーストラリア、ニュージーランドなど英米法諸国で採用されている。税収中立的であり、かつ税務行政上の負担は軽い。
- 取得税方式は先に遺産を分割し、その後、相続人の取得分に応じて課税される。ドイツ、フランスなどで採用。遺産分割によって納税額を減らすことができる税収の下方バイアスがある。

相続税の考え方（2）

- 我が国の方法は法定相続分方式と呼ばれている。この方式では、法定相続人に対して法定相続応じて仮分割し、それに累進税率を乗じて税額を決め、その後、遺産分割をして、分配額に応じて、先に決まった税額が割り振られるという仕組みである。
- 法定相続分方式は、相続人を納税義務者としながらも、取得税方式には徹しておらず、実際の納税額と取得相続額が対応していないということで相続人間の水平的公平性が保たれていないという指摘がある。
- この方式の利点は税額を遺産額と法定相続人という恣意性の少ない要因で比較的短期間に決められるという税務行政上の簡便さにある。

相続税の考え方（３）

- 税制改革を巡る議論の中で、2008年1月には政府税調は現行の法定相続分方式から取得税方式に改めることを検討するとしたが、同年12月には自民党税調が現行制度からの改革に消極的であることを表明した。
- 民主党は2008年12月の党税制改革案で遺産課税方式への転換を主張している。
- 学界ではマーリーズ・レビュー（2011）などを中心に「機会の平等」という観点から取得税方式への支持が多い。
- ただ、個人の財産権を尊重する英米法では遺産を自らの意思（遺言）によって決めている被相続人が納税するという遺産税方式の考え方が歴史的には自然であるとされてきた。

相続税の考え方（４）

- 相続に遺族の扶養や社会扶助の役割を重視したフランスやドイツでは取得税方式が導入されている。
- 日本は戦前の旧民法下では、長男の一括家督相続という制度であり、相続人間での不平等は著しかった。戦後の民法では、これを改革し、相続人間での扱いは極めて平等なものとなった。むしろ遺言書を残して被相続人が指定した遺産分配に対する異議申し立て（遺留分減殺請求）ができる。
- 自民党は一度は政府税調で取得税方式への移行を表明しながら、最近では法定相続分方式を強化するような税制改革を行っている。
- グローバル化の現在、相続税に関する国際的な共通認識、制度設計が求められている。

相続税の考え方（5）

- Piketty(2010)の議論にあったように、相続総額は生前贈与も含めて考えるべきであるが、この贈与額を正確に把握することは難しい。既に見たように一般に意識されているより多くの贈与が行われている可能性が高い。
- 日本の場合、教育費は大学院まで親が出すことが普通であり、その他関連の塾や海外での語学研修などについても同同様に親が出している場合が多い。これらは税法上、贈与とは見なされず、親の消費と見なされている。
- 現行の贈与税は、（1）相続時精算課税制度と（2）暦年課税制度に分かれている。
- （1）2500万円までの贈与は特別控除額として贈与財産額から控除し、被相続人が亡くなった時に、特別控除額を超えた贈与額を加算して相続税額を計算する。

相続税の考え方（6）

- （2）贈与額から毎年110万円まで基礎控除を受けることができ、それを超える場合には、申告して超過累進税率に従って納税する。被相続人が亡くなった場合、この制度を適用した贈与は相続財産として加算する必要はない。

税務統計によれば、（2）の制度を利用するケースが（1）の6倍程度ある。いずれにしても贈与税として把握されている贈与額は贈与全体からするとかなり少ない。

- 2015年の改革では（1）の制度を用いて20歳以上の孫への2500万円までの教育費贈与が認められた。

相続税の考え方（7）

- 高齢化、晩婚化の帰結として、遺産相続のタイミングが人生後半にずれ込んでいる。
- 親が80歳後半まで生きると、相続が起こるのは、子供が若くて50歳代後半から60歳代前半である。子供のライフサイクルの中で、一番資金・資産需要が高いのが40歳代後半から50歳代前半だとすると、死後相続から生前贈与へのシフトを促すような仕組みを考えた方が望ましい。
- このような資産再分配を税制によって促進させることはできるだろうか？住宅の親子間スワップなどは考えてもいいのではないだろうか。
- 自営業（農業）は税制が有効な事業継承を阻害しているのではないか？

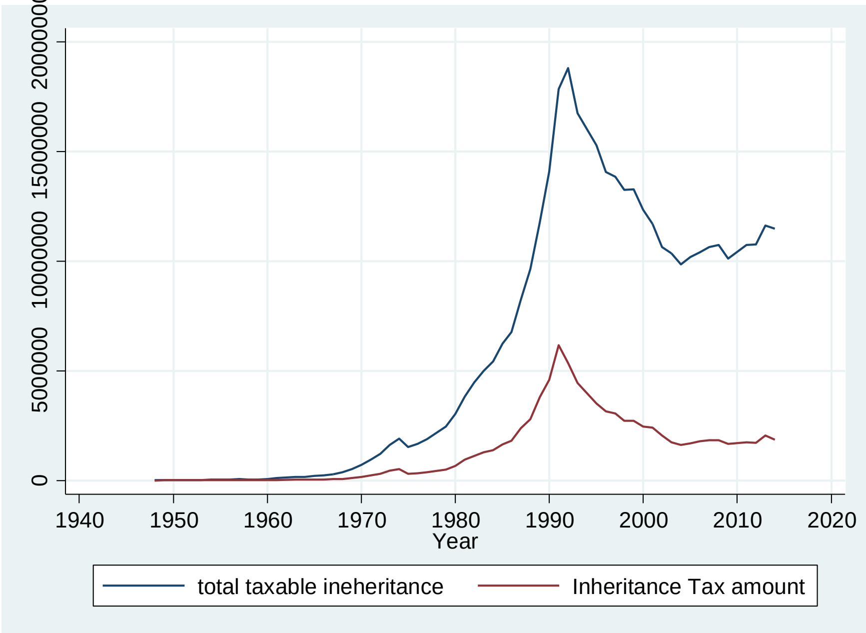
図表12．相続税の課税状況の推移

区分	相続税								
	死亡者数・課税件数等				課税価格		相続税額		
年分	死亡者数（a）	課税件数（b）	(b)／(a)	被相続人1人当たり法定相続人数	合計額（c）	被相続人1人当たり金額	納付税額（d）	被相続人1人当たり金額	(d)／(c)
昭和	人	件	%	人	億円	万円	億円	万円	%
58	740,038	39,534	5.3	4.10	50,021	12,653	7,153	1,809.0	14.3
59	740,247	43,012	5.8	4.00	54,287	12,621	7,769	1,806.0	14.3
60	752,283	48,111	6.4	4.00	62,463	12,983	9,261	1,925.0	14.8
61	750,620	51,847	6.9	3.99	67,637	13,045.6	10,443	2,014.2	15.4
62	751,172	59,008	7.9	3.93	82,509	13,982.6	14,343	2,430.7	17.4
63	793,014	36,468	4.6	3.68	96,380	26,428.6	15,629	4,285.5	16.0
平成元	788,594	41,655	5.3	3.90	117,686	28,252.5	23,930	5,744.9	20.3
2	820,305	48,287	5.9	3.86	141,058	29,212.4	29,527	6,114.8	20.9
3	829,797	56,554	6.8	3.81	178,417	31,548.0	39,651	7,011.2	22.2
4	856,643	54,449	6.4	3.85	188,201	34,564.7	34,099	6,262.5	18.1
5	878,532	52,877	6.0	3.81	167,545	31,685.9	27,768	5,251.5	16.6
6	875,933	45,335	5.2	3.79	145,454	32,084.4	21,058	4,644.9	14.5
7	922,139	50,729	5.5	3.72	152,998	30,159.9	21,730	4,283.5	14.2
8	896,211	48,476	5.4	3.71	140,774	29,039.9	19,376	3,997.0	13.8
9	913,402	48,605	5.3	3.68	138,635	28,522.8	19,339	3,978.8	13.9
10	936,484	49,526	5.3	3.61	132,468	26,747.1	16,826	3,397.4	12.7
11	982,031	50,731	5.2	3.59	132,699	26,157.3	16,876	3,326.5	12.7
12	961,653	48,463	5.0	3.55	123,409	25,464.7	15,213	3,139.0	12.3
13	970,331	46,012	4.7	3.52	117,035	25,435.7	14,771	3,210.2	12.6
14	982,379	44,370	4.5	3.46	106,397	23,979.4	12,863	2,899.0	12.1
15	1,014,951	44,438	4.4	3.40	103,582	23,309.4	11,263	2,534.6	10.9
16	1,028,602	43,488	4.2	3.35	98,618	22,677.0	10,651	2,449.1	10.8
17	1,083,796	45,152	4.2	3.33	101,953	22,579.9	11,567	2,561.8	11.3
18	1,084,450	45,177	4.2	3.26	104,056	23,032.9	12,234	2,708.1	11.8
19	1,108,334	46,820	4.2	3.20	106,557	22,758.9	12,666	2,705.3	11.9
20	1,142,407	48,016	4.2	3.17	107,482	22,384.7	12,517	2,606.8	11.6
21	1,141,865	46,439	4.1	3.13	101,230	21,798.6	11,632	2,504.7	11.5
22	1,197,012	49,891	4.2	3.08	104,630	20,971.7	11,753	2,355.7	11.2
23	1,253,066	51,559	4.1	3.03	107,468	20,843.7	12,516	2,427.5	11.6
24	1,256,359	52,572	4.2	3.00	107,718	20,489.6	12,446	2,367.4	11.6
25	1,268,436	54,421	4.3	2.97	116,381	21,385.3	15,366	2,823.5	13.2

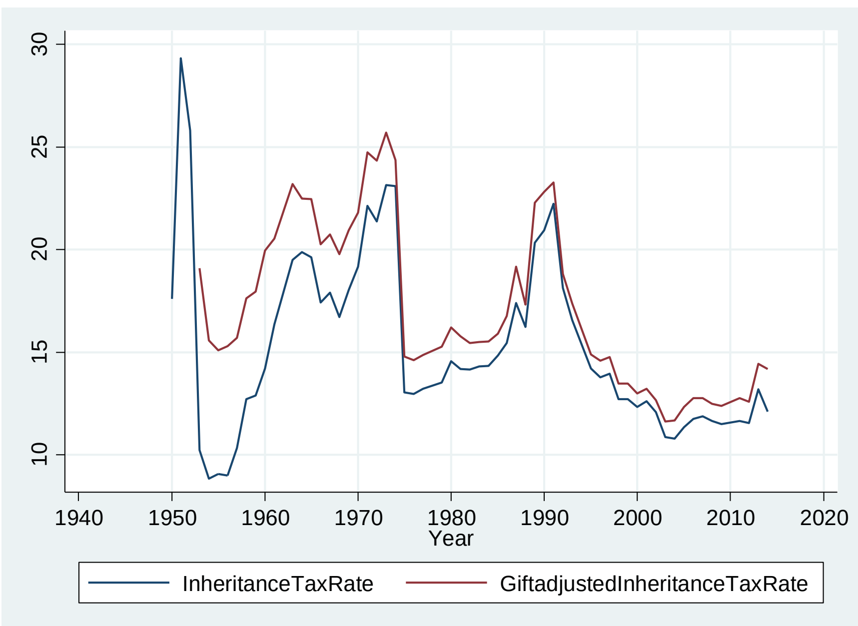
（備考）
1．”死亡者数(a)”は「人口動態統計」(厚生労働省)により、その他の係数は「国税庁統計年報書」による。
2．”被相続人1人当たりの法定相続人数”は、当初申告ベースの係数である（修正申告を含まない）。ただし、昭和63年分には、更正の請求により納付税額がゼロとなったものの係数が含まれている。
3．”課税件数(b)”は、相続税の課税があった被相続人の数である。
4．”課税価格(c)”および”納付税額(d)”には更正・決定分を含む。また、”納付税額(d)”には納税猶予額を含まない。

（出典）財務省（ http://www.mof.go.jp/tax_policy/summary/property/137.htm）

図表13．課税対象相続額と相続納税額



図表14．実効相続税率



図表15．相続税の合計課税価格階級別の課税状況等
（平成25年分）

合計課税価格階級区分	件数		納付税額		平均課税価格(a)	平均納付税額(b)	負担割合
	件数	累積割合	税額	累積割合			(b)/(a)
	件	%	億円	%	万円	万円	%
～1億円	13,843	25.4	181	1.2	8,374	131	1.6
～2億円	25,959	73.1	1,680	12.1	13,830	647	4.7
～3億円	7,286	86.5	1,677	23.0	24,159	2,302	9.5
～5億円	4,310	94.4	2,371	38.5	37,847	5,501	14.5
～7億円	1,397	97.0	1,532	48.4	58,418	10,963	18.8
～10億円	822	98.5	1,505	58.2	82,253	18,308	22.3
～20億円	612	99.6	2,126	72.1	134,499	34,746	25.8
～100億円	173	99.9	1,718	83.2	325,505	99,308	30.5
100億円超	19	100.0	2,577	100.0	3,189,053	1,356,142	42.5
合計	54,421		15,367		21,362	2,824	13.2

（備考）　1.「国税庁統計年報書」による。
　　　　　2. 当初申告ベースの係数である(修正申告を含まない)。

（出典）　財務省（http://www.mof.go.jp/tax_policy/summary/property/138.htm）

2015年相続税改革（１）

- 2015年1月より相続税制が変わった。具体的には基礎控除がそれまでの「5000万円＋法定相続人数×1000万円」であったものが、「3000万円＋法定相続人数×600万円」に引き下げられ、最高税率は50％から55％（6億円超に対して）に引き上げられた。
- その結果、国税庁によれば、2015年中に亡くなった人（約129万人）のうち、相続税の課税対象となる遺産を残した人（10万3千人）の割合は、前年の4.4％から8.0％に急増した。
- 　地域別に見ると、東京（15.7％）、神奈川（12.4％）、千葉（8.3％）と首都圏での増加が大きいことがわかる。相続税制で把握された遺産総額は14兆5554億円（26.8％増）で、相続税額は約1兆8116億円（30.3％増）なので、実効平均税率は12.45％となる。

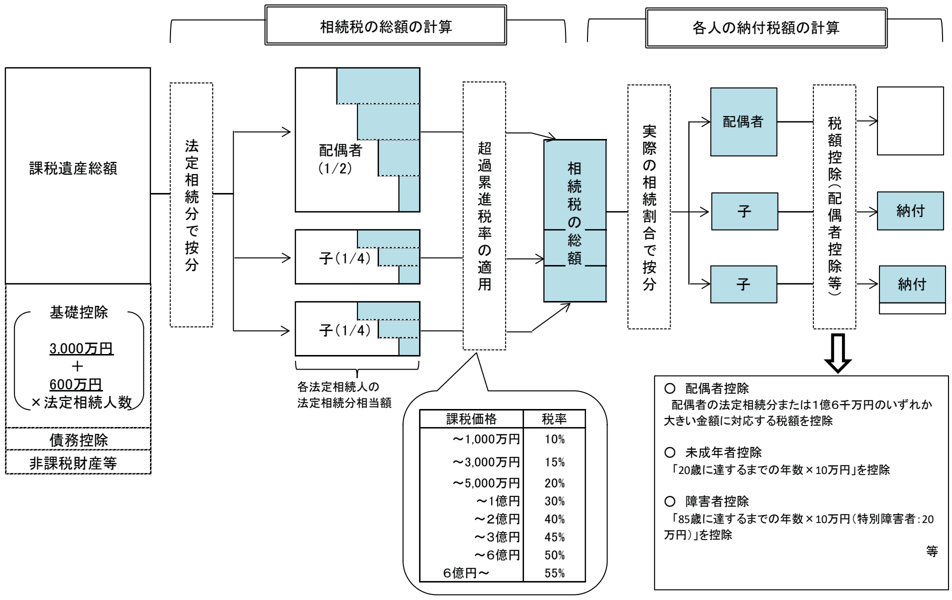
2015年相続税改革（２）

- 2015年の相続税改革は一般的には基礎控除が引き下げられ、最高税率が引き上げられるなど、増税の方向に舵を切ったとされるが、未成年者の税額控除を1年につき6万円から10万円に引き上げたり、小規模宅地特例により、土地評価の減額割合80％適用の限度面積が240平米から330平米に拡大された。
- 事業承継税制として、非上場株式等についての相続税及び贈与税の納税執行猶予及び免除の特例の適用要件の緩和や手続きの簡素化が認められた。
- 世界的な相続税改革の方向性については、70年代にカナダ、オーストラリア、90年代にニュージーランド、2000年代にスェーデン、ポルトガル、オーストリア、マレーシア、インド、香港、シンガポールが相続税を廃止した。一方、アメリカ、イギリス、フィンランド、フランス、アイスランド、日本では税率を引き上げ、控除を引き下げたりして課税強化を行っている（図表15．参照）。

図表16．相続税の主な改正内容

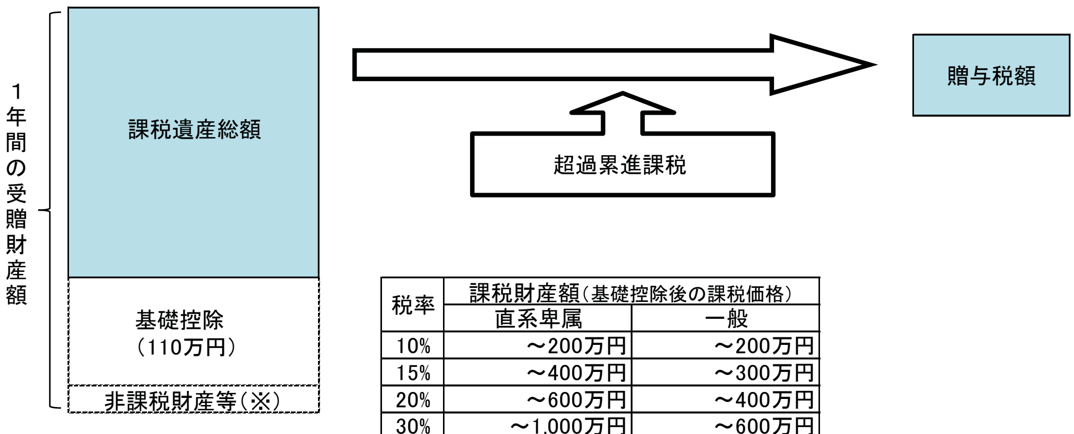
区分	抜本改正前	抜本改正 （1988年12月） （1988年1月1日以降適用）	1992年度改正 （1992年1月1日以降適用）	1994年度改正 （1994年1月1日以降適用）	2003年度改正 （2003年1月1日以降適用）	2010年度改正 （2010年4月1日以降適用）	2013年度改正 （2015年1月1日以降適用）
（1）遺産に係る基礎控除 定額控除 法定相続人数比例控除	2,000万円 400万円X法定相続人の数	4,000万円 800万円x法定相続人の数	4,800万円 950万円×法定相続人の数	5,000万円 1,000万円x法定相続人の数	同左	同左	3,000万円 600万円x法定相続人の数
（2）税率	10%　200万円以下 15〃　500万円　〃 20〃　900万円　〃 25〃　1,500万円　〃 30〃　2,300万円　〃 35〃　3,300万円　〃 40〃　4,800万円　〃 45〃　7,000万円　〃 50〃　1億円　〃 55〃　1億4,000万円　〃 60〃　1億8,000万円　〃 65〃　2億5,000万円　〃 70〃　5億円　〃 75〃　5億円　超 （14段階）	10%　400万円以下 15〃　800万円　〃 20〃　1,400万円　〃 25〃　2,300万円　〃 30〃　3,500万円　〃 35〃　5,000万円　〃 40〃　7,000万円　〃 45〃　1億円　〃 50〃　1億5,000万円　〃 55〃　2億円　〃 60〃　2億5,000万円　〃 65〃　5億円　〃 70〃　5億円　超 （13段階）	10%　700万円以下 15〃　1,400万円　〃 20〃　2,500万円　〃 25〃　4,000万円　〃 30〃　6,500万円　〃 35〃　1億円　〃 40〃　1億5,000万円　〃 45〃　2億円　〃 50〃　2億7,000万円　〃 55〃　3億5,000万円　〃 60〃　4億5,000万円　〃 65〃　10億円　〃 70〃　10億円　超 （13段階）	10%　800万円以下 15〃　1,600万円　〃 20〃　3,000万円　〃 25〃　5,000万円　〃 30〃　1億円　〃 40〃　2億円　〃 50〃　4億円　〃 60〃　20億円　〃 70〃　20億円　超 （9段階）	10%　1,000万円以下 15〃　3,000万円　〃 20〃　5,000万円　〃 30〃　1億円　〃 40〃　2億円　〃 50〃　3億円　超 （6段階）	同左	10%　1,000万円以下 15〃　3,000万円　〃 20〃　5,000万円　〃 30〃　1億円　〃 40〃　2億円　〃 45〃　3億円　〃 50〃　6億円　〃 55〃　6億円　超 （8段階）
（3）配偶者に対する相続税額の軽減	遺産の2分の1または4,000万円のいずれか大きい金額に対応する税額まで控除	配偶者の法定相続分または8,000万円のいずれか大きい金額に対応する税額まで控除	同左	配偶者の法定相続分または1億6,000万円のいずれか大きい金額に対応する税額まで控除	同左	同左	同左
（4）死亡保険金の非課税限度額	250万円x法定相続人の数	500万円x法定相続人の数	同左	同左	同左	同左	同左
（5）死亡退職金の非課税限度額	200万円x法定相続人の数	500万円x法定相続人の数	同左	同左	同左	同左	同左
（6）税額控除							
未成年者控除	20歳までの1年につき3万円	20歳までの1年につき6万円				同左	20歳までの1年につき10万円
障害者控除	70歳までの1年につき3万円	70歳までの1年につき6万円	同左	同左	同左	85歳までの1年につき6万円	85歳までの1年につき10万円
特別障害者控除	70歳までの1年につき6万円	70歳までの1年につき12万円				85歳までの1年につき12万円	85歳までの1年につき20万円

図表17. 現行の相続税の仕組み



(出典) 財務省(http://www.mof.go.jp/tax_policy/summary/property/135.htm)

図表18. 贈与税の暦年課税制度の仕組み



税率	課税財産額(基礎控除後の課税価格)	
	直系卑属	一般
10%	~200万円	~200万円
15%	~400万円	~300万円
20%	~600万円	~400万円
30%	~1,000万円	~600万円
40%	~1,500万円	~1,000万円
45%	~3,000万円	~1,500万円
50%	~4,500万円	~3,000万円
55%	4,500万円~	3,000万円~

※ 扶養義務者相互間の生活費または教育費に充てるための受贈財産

等

(出典) 財務省(http://www.mof.go.jp/tax_policy/summary/property/153.htm)

最適相続税の理論（1）

- 各国の相続税制度はかなりばらついており、所得税や消費税のように、国際的にほぼ統一した制度的な枠組みがあるわけではない。
- その中で、Piketty and Saez (2013a)は最適所得税の枠組みとFarhi and Werning (2010)を拡張して、最適相続税の理論を提案している。
- 簡便な仮定の下で、最適税率は正であり、もし相続の税率弾力性が低く、相続の分布が集中しており、かつ社会が相続を受けられないような貧者への再分配を望むのであれば、最高税率は50－60％であってもおかしくないという結果を得ている。

最適相続税の理論（2）

- 有名なChamley (1986)やJudd(1985)の「資本（相続）課税は長期的には税率0であるべきだ」という結果と対照的である。
- モデルの中で税率0でないためには、（1）所得に経済主体個別のショックが起こる、（2）想定外相続、（3）被相続人が税引き後遺産額にこだわる、（4）長期的な厚生最大化、（5）固定的な税率、（6）政府のコミットメント不足、などの条件が加えられている。
- 実際に、ある国家は相続税を廃止しており、他の国家は相続税を強化しており、理論的あるいは実証的に議論されているというより、きわめて政治的判断に基づいて相続税制は決められている。

最適相続税の理論（3）

- 近年では、政策をエビデンスに基づいて立案しようという機運が高まってきており、統計データの充実が望まれている。
- 国税庁の保有している相続関連の税務データが利用可能になれば、遺産相続という行動に対して、現状よりも深い理解に達することは可能であろう。
- しかし、よりディープなパラメータである、相続の税率弾力性などは概念としては理解できるが、実証的に検証することはほぼ不可能に近いように思われる。贈与は長期にわたって調整可能であり、税率弾力性を推計することも可能。
- 現状では、財政専門家の知見を集めて、経済社会のあり方に関して合意を得ながら、個別の税制の改革を行っていくしかないだろう。

世代間移転再考（1）

- 資産の再分配の考え方

相続は家族内で異時点間の資源分配をすることで、保険で対応できないような個別ショックに備えるという意味合いがある反面、社会的な資源分配からすると非効率な分配になっている可能性が高い。相続という機会をとらえて、多少の社会的再分配を行うことは望ましいのではないだろうか。

- 家族内での世代間移転と社会への再分配（寄付）をどのようにバランスさせるか？

寄付行為を強制化するのではなく、むしろ自由化して所得控除の対象にするなどの工夫が必要。また寄付受入れ機関の条件緩和も望まれる。

世代間移転再考（２）

- 教育投資を社会で行おうという政策をどう考えたらいいのか？

家族でサポートしていた教育投資はどうするのか？増税して、政府の教育投資に回すのか、親は、自分の子供を行かせたい学校に寄付することを好むのではないか？教育の品質の確保と、学校間競争による質の格差の発生はどう考えればいいのだろうか？

- 人口減少は世代間移転にどのような影響を与えるだろうか？

遺産相続資産のシェアが土地資産から金融資産に移ってきている。親と子の居住地が離れている場合、親の土地住宅資産や事業は維持管理が難しくなる（空き家問題、事業承継問題）。日経新聞によれば、法定相続人がいない遺産の国庫納付が年４００億円となり１０年間で２．５倍になっている（２０１７年４月１６日付）。

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