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Persistent mind: The effects of information provision on policy preferences☆

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Abstract

In constitutional states, representatives exchange information, discuss the budget direction and agree on a budget. The agreement is assumed to hold for one fiscal year. To test the validity of this constitutional assumption, we implemented an online panel survey with a randomized conjoint design three times over one year in Japan to track the direction of respondents' preferences within a multidimensional public policy space. The policy space consisted of spending on education, infrastructure, health insurance, pensions, and poverty relief programs, as well as fiscal retrenchment. Providing information on the poverty rate in the first wave directed respondents' preferences toward support for poverty relief programs by either increasing or reallocating the budget. The effects persisted in the second wave 5 months later across a diverse range of respondent backgrounds and political positions. By the third wave one year later, the effects had diminished. Once placed in a multidimensional space, information exchange might have a more extended scope than unidimensional approaches have shown. This finding, we believe, can broaden our capability to implement policies for poverty reduction.
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Keywords: Persistent information treatment effects; Multidimensional policy space; Income redistribution; Randomized conjoint experiment; Machine learning

1. Introduction

In a constitutional state, representatives of the legislature debate the allocation of the budget to each dimension of expenditure every year. Raising spending in one category requires reducing spending in others. If citizens update their preferences for the next year, their representatives can discuss the policy direction again and update the budget legislation accordingly for the next year. The first prerequisite in this process of budget legislation is that citizens affect each other's preferred policy direction by providing each other with information. The second is that the effects of information provision persist for a certain period of time. Fiscal constitutionalism thus assumes that information provision affects policy direction preferences and that the effects persist for a certain period.

Although persistent information treatment effects are a critical assumption of the regime in any constitutional democracy, the validity of the assumption has not yet been empirically established. Research based on data from the US, UK, France, Italy, and Sweden (Alesina et al., 2018) showed that information on pessimistic prospects for social mobility raised support for income redistribution among left-wing people and that the effect was consistent one week later. Using a US dataset, however, Kuziemko et al. (2015) showed that information provision led to substantial updates of perceptions of income inequality hardly affected support for income transfers except through estate taxes, which are paid by only a small minority. Meanwhile, using data from the same US survey service, Becker (2020) showed that a focus on inequality of opportunity rather than outcomes raised support for redistribution among Americans and that the effects persisted.

Related interests and motivations are shared in the field of environmental and energy economics, considering the possibility of coordinating the behaviors of the public toward better usages of energy and other resources. Allcott and Rogers (2014) described the attenuation of the intervention effects in energy saving as "action and backsliding." In turn, Ferraro et al. (2011), Ferraro and Miranda (2013), Ferraro and Price (2013), and Bernedo and Price (2013) found that "norm-based" messages had persistent impacts on water saving from 4 months to even two years.

We suspect that one factor that produces seemingly mixed results regarding issues of broad concern, such as poverty or environmental problems, is that the previous works measured the effects of information treatment on support for a specific policy unidimensionally. For instance, income redistribution can be financed either through reallocation of spending on other public policies or through tax increases. While the former alternative does not change the size of government, the latter does. Unidimensional measurement cannot identify which scenario respondents are considering.

Therefore, our interest is in evaluating information treatment effects under a framework that allows us to measure the effects of information provision on preferences in a multidimensional policy space. To this end, we use a fully randomized conjoint design (Hainmueller et al., 2015; Bansak et al., 2018) to estimate the effects of an information treatment on the preferred public policy direction. In our design, the tax increase is assumed to be given. The public policy space consists of the dimensions of education, national health insurance, the national basic pension plan, infrastructure, poverty relief programs, and fiscal retrenchment. We provide information

about the poverty rate in Japan to a randomly chosen half of the respondents. Then, the respondents are asked about the relative importance of each element as the preferred policy focus.

The experimental design allows us to identify the preference for a given policy expenditure in comparison to alternative public policies and fiscal retrenchment. By allowing the respondent to reallocate expenses across alternative public policies and to shrink or enlarge the size of government, we can identify the information treatment effects on the policy direction in a multidimensional policy space.

Moreover, this framing closely resembles the practices employed in functional democracies. The policy direction is often discussed and set through the democratic process, with the tax revenue for each fiscal year taken as given instead of a discussion of tax rate rise being required every year.

We find that provision of information about the poverty rate in Japan in the first wave raises relative support for poverty relief programs; the rise in support for poverty relief programs persists to the second wave 5 months later but attenuates by the third wave one year later. Information exchanges affect the preferred direction of policy, and the effects persist for a certain period of time.

The remainder of the paper is organized as follows. Section 2 describes the Japanese context of income inequality and fiscal conditions. Section 3 describes our fully randomized conjoint experimental design. Section 4 presents our estimation strategy. Section 5 reports our results. Section 6 concludes the paper.

2. Poverty as an urgent issue in Japan

Rapid aging during the last three decades has pushed Japan to extend its public medical and pension insurance. Increases in social security contributions have mainly financed this spending.[1] This rise has led Japan to join a group of large welfare states along with continental European nations.

However, to cope with the aging of its population, Japan has also financed medical and pension insurance through debt. Its general government debt over gross domestic product has soared to from 100% in the mid-1990s to more than 250% in the 2010s.[2] Given these circumstances, Japan raised the consumption tax (value-added tax) rate from 8% to 10% in October 2019.

Another issue is poverty. Japan's self-impression as an egalitarian society has been questioned, particularly since the 1990s (Chiavacci, 2008; Hommerich & Kikkawa, 2019; Kanbayashi, 2019). Japan's relative poverty rate as of the mid-2010s was second only to that of the US among seven major advanced economies.[3]

[1] According to the OECD, "Social security contributions are compulsory payments paid to general government that confer entitlement to receive a (contingent) future social benefit. They include: unemployment insurance benefits and supplements, accident, injury and sickness benefits, old-age, disability and survivors' pensions, family allowances, reimbursements for medical and hospital expenses or provision of hospital or medical services. Contributions may be levied on both employees and employers. Such payments are usually earmarked to finance social benefits and are often paid to those institutions of general government that provide such benefits." OECD (2021) doi: https://doi.org/10.1787/3ebfe901-en (Accessed on September 10, 2021).

[2] OECD (2021), General government debt. doi: https://doi.org/10.1787/a0528cc2-en (Accessed on September 10, 2021).

[3] OECD (2021), Poverty rate (indicator). doi: https://doi.org/10.1787/0fe1315d-en (Accessed on September 10, 2021).

Poverty is a reality in Japan. Mandatory spending for medical and pension insurance to cope with demographic changes has already financially pressured Japan.

3. Experimental design

3.1. Survey respondents

We sent our questionnaire to 15,000 respondents through a survey company, Rakuten Insight, in each wave. If a participant did not respond to our survey in two consecutive waves, we replaced her or him with a new participant. Detailed information about the respondents treated by Rakuten Insight is available on its website.[4]

3.2. Background characteristics of respondents

In our panel surveys, we collected respondents' background information. Additionally, we asked about the poverty rates perceived by respondents before the information treatment. The question aimed to identify whether an update of perception affects policy preferences. The questions on demography covered gender, age, prefecture of residence, marital status, number of children, and whether the respondent lives with parents, parents-in-law, or neither. Questions on socioeconomic status included the highest degree of education, whether the respondent works, whether she or he is employed or self-employed, and, if she or he is employed, whether it is full- or part-time and regular or nonregular work, the job title, size of the employer, the respondent's own annual income, and the respondent's household's annual income. We also asked about party support, satisfaction with politics, subjective political position, and subjective perception of own social class.

3.3. Randomized conjoint design

In a fully randomized conjoint design, respondents were presented with alternative packages, whose attributes were randomly assigned and hence statistically independent of one another.

Our design assumed that the consumption tax rate is raised from 8% to 10%. Then, our conjoint experiment randomly assigned 0, 5, 10, 15, or 20% of the revenue from an increase in consumption tax to (*a*) "poverty relief (minimum wages, unemployment benefits, public housing for low-income earners, etc.)," (*b*) "pensions," (*c*) "health insurance," (*d*) investment in "infrastructure (roads, running water, airports, etc.)," and (*e*) "education (subsidies for tuition, expansion of nursery schools, etc.)," as described in Table 1. If any residual exists after all the percentages are summed, it is allocated to redemption of government bonds.

Note that under the Japanese social security system, the national pension plan and national health insurance are universal insurance policies covering all adult residents in Japan. The beneficiaries of the increased subsidies to pensions and health insurance from the raised consumption tax are thus not limited to poor individuals but include all residents. Therefore, only spending for (*a*) poverty relief directly aims to transfer income from rich to poor individuals.

[4] https://insight.rakuten.co.jp/download/PanelProfile_EN.pdf and https://insight.rakuten.co.jp/download/PanelCharacteristicSurveyEN.pdf (Accessed on September 10, 2021).

Table 1
Attributes and attribute levels for hypothetical public policies.

Policy attributes	Level	Policy attributes	Level	Policy attributes	Level
Poverty relief	0%	Pensions	0%	Health insurance	0%
	5%		5%		5%
	10%		10%		10%
	15%		15%		15%
	20%		20%		20%
Infrastructure	0%	Education	0%	Residual is to redeem debt	
	5%		5%		
	10%		10%		
	15%		15%		
	20%		20%		

Let $\boldsymbol{A}_j$ denote a five-dimensional policy package vector that includes (*a*) to (*e*) described above, and let $\boldsymbol{A}_{-j}$ denote an alternative policy package vector. In each round of the conjoint experiment, each respondent was asked to choose her or his preference between the randomly generated $\boldsymbol{A}_j$ and $\boldsymbol{A}_{-j}$ packages. Any residual was allocated to government debt redemption. We requested completion of these 5 rounds ($r = 1, \ ...,5$) of each respondent i in each period (wave) t. Let $\boldsymbol{A}_j^{i,r,t}$ and $\boldsymbol{A}_{-j}^{i,r,t}$ be packages j and $-j$ that were shown to respondent i in round r in period t. For instance, for respondent i in round r in period t, package $\boldsymbol{A}_j^{i,r,t}$ might assign 10% to (*a*) poverty relief, 5% to (*b*) pensions, 5% to (*c*) health insurance, 20% to (*d*) infrastructure, 20% to (*e*) education, and 40% to redemption of government bonds. Another package $\boldsymbol{A}_{-j}^{i,r,t}$ might assign 5% to (*a*), 10% to (*b*), 15% to (*c*), 0% to (*d*), 10% to (*e*), and 60% to redemption of government bonds.

The timing of our survey was $t=1$ (November 2018), $t=2$ (March 2019), and $t=3$ (October 2019). As described above, Japan raised its consumption tax in the third period, October 2019.

3.4. Information treatment

In each wave, to measure respondents' expectations about Japan's poverty rate, we asked them to estimate how many of Japan's total households are in poverty and how many single-parent households are in poverty. The same questions were asked in all three waves from November 2018 to October 2019.

In the first wave, in November 2018, we provided half the respondents, who were randomly chosen, with information about Japan's poverty rate based on the National Livelihood Survey by the Ministry of Health, Labour and Welfare of the government of Japan in 2015. The ministry survey indicated that 16% of total households and 51% of single-parent households were in poverty as of 2015.[5] We had the treatment group view the information in two graphs (Figs. 1 and 2). The control group did not view them.

[5] The estimate of the relative poverty rate followed the standard defined by the Organisation of Economic Co-operation and Development (https://www.mhlw.go.jp/toukei/list/dl/20-21-h28_rev2.pdf (Accessed on September 10, 2021).

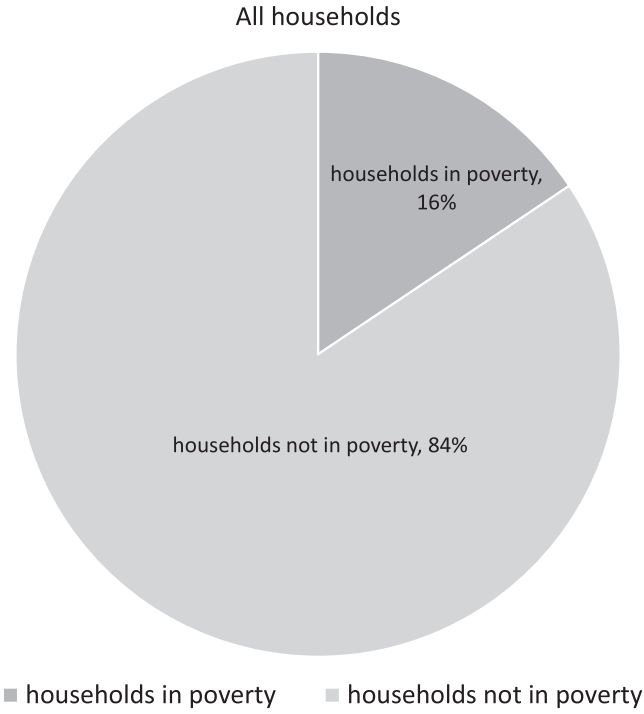

Fig. 1. Provided information: All households.

In the second wave, in March 2019, and the third wave, in October 2019, we did not provide any information. Therefore, our experiment was designed to identify whether the intervention effects through information provision on policy preference persisted throughout the second and third waves.

4. Estimation strategy

4.1. Individual information treatment effect

We estimate heterogeneous treatment effects in the potential outcomes framework (Imbens & Rubin, 2015; Wager & Athey, 2018; Athey & Imbens, 2019). Thus, by comparing one potential outcome with the information treatment and one without it, we identify the difference as the causal effect of the information treatment.

In our design, half of the respondents received the information treatment and the other half the control treatment in period $t = 1$. Let W_i denote the information treatment indicator, which takes a value of one if respondent i received the information treatment and zero if i received the control treatment. After receiving the information or control treatment, respondents were asked about their preferences on hypothetical policy packages randomly generated by our randomized conjoint design.

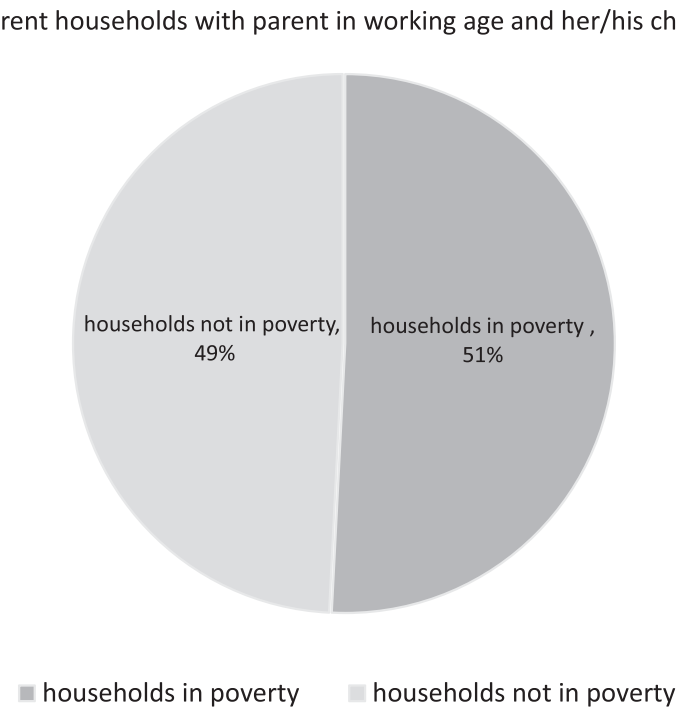

Fig. 2. Provided information: Single-parent households.

Policy package $\boldsymbol{A}_i$ is characterized by its attributes such that $\boldsymbol{A}_i = [a_{j,1}, ..., a_{j,L}]$, where $a_{j,l}$ is a level of the lth attribute of policy j. In our case, $a_{j,l}$ takes 0, 5, 10, 15, or 20% for $l = 1, ..., 5$, which refer to policy (a) to policy (e) described above, and the residual is assumed to be spent on government debt repayment.

Consider a potential outcome, given information treatment status W_i, for respondent i, in round r of period t, $Y_{i,r,t}(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t} | W_i)$ such that

$$Y_{i,r,t}\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t} \middle| W_i\right) = \begin{cases} 1 \text{ if } \boldsymbol{A}_j^{i,r,t} \succ_i \boldsymbol{A}_{-j}^{i,r,t} \\ 1 \text{ if } \boldsymbol{A}_j^{i,r,t} \prec_i \boldsymbol{A}_{-j}^{i,r,t} \end{cases}. \tag{1}$$

We randomly assign the information treatment and hence satisfy the unconfoundedness assumption,

$$W_i \perp\!\!\!\perp \left[Y_{i,r,t}\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t} \middle| W_i = 0\right), Y_{i,r,t}\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t} \middle| W_i = 1\right) \right] | \boldsymbol{X}_i$$

where $\boldsymbol{X}_i$ denotes the vector of background characteristics of surveyed respondent i described in section 3.2. Then, we define the individual information treatment effect for respondent i in period t as

$$\tau_{i,r,t}\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}\right) = Y_{i,r,t}\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t} | W_i = 1\right) - Y_{i,r,t}\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t} | W_i = 0\right), \tag{2}$$

which captures the information treatment effects on respondent i's preference over policy packages $\boldsymbol{A}_j^{i,r,t}$ and $\boldsymbol{A}_{-j}^{i,r,t}$ in round r of period t.

4.2. Average marginal component effects and treatment effects

We first review the policy preference by estimating the average marginal component effect for each policy (Hainmueller et al., 2014) such that

$$E\left[Y_{i,r,t} | a_{j,l} = a_1, W_i = w\right] - E\left[Y_{i,r,t} | a_{j,l} = a_0, W_i = w\right], \tag{3}$$

where $a_{j,l}$ is the lth attribute of policy j, $w \in \{0, 1\}$. Then, we assess the overall information treatment effect by estimating the distribution of conditional information effects in period t, defined as

$$E\left[\tau(W_i)\right] = E\left[Y_{i,r,t} | W_i = 1\right] - E\left[Y_{i,r,t} | W_i = 0\right]. \tag{4}$$

4.3. Group average treatment effects

Information treatment effects might be potentially heterogeneous across background characteristics $\boldsymbol{X}_i$. However, since respondent i receives either the information or the control treatment and we observe only one outcome, we cannot directly estimate the individual treatment effect $\tau_{i,r,t}$. Thus, we first predict individual treatment effects $\tilde{\tau}_i$ as a function of $\boldsymbol{X}_i$, $\boldsymbol{A}_j^{i,r,t}$ and $\boldsymbol{A}_{-j}^{i,r,t}$, employing the causal forest algorithm (Wager & Athey, 2018; Athey & Tibshirani, & Wager, 2019).

Next, we estimate group average marginal treatment effects (GATES). Suppose that $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}\} \subset \chi$, and consider subset $S \subset \chi$. We consider group average marginal treatment effects in region S such that

$$E\left[\tau\left(\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}\right) | S\right] = E\left[Y_{i,r,t} | W_i = 1, S\right] - E\left[Y_{i,r,t} | W_i = 0, S\right]. \tag{5}$$

Suppose that the individual treatment effect predicted by the original entire sample is $\tilde{\tau}_i$, $i = 1, 2, 3, ..., N$. Then, according to the regression tree approach (Breiman et al., 1984), we partition the original sample $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\}$ into two regions S_1 and S_2 such that

$$\min_{S_1, S_2}\left[\sum_{i=1}^{l}\left(\tilde{\tau}_i - \overline{\tau}_{S_1}\right)^2 + \sum_{i=l+1}^{N}\left(\tilde{\tau}_i - \overline{\tau}_{S_2}\right)^2\right], \tag{6}$$

where $l \leq N$, $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\} \in S_1$ for $i = 1, 2, ..., l$, $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\} \in S_2$ for $i = l+1, l+2, ..., N$, $S_1 \bigcup S_2 = \chi$, S_1 and S_2 are mutually disjoint, and

$$\begin{aligned}\bar{\tau}_{S_p} = & E\left[Y_{i,r,t} \middle| \left\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i\right\} \in S_p, W_i = 1\right] \\ & - E\left[Y_{i,r,t} \middle| \left\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i\right\} \in S_p, W_i = 0\right].\end{aligned} \tag{7}$$

Furthermore, we again partition each of regions S_1 and S_2 into two regions S_{1_1}, S_{1_2}, S_{2_1}, and S_{2_2} such that

$$\min_{S_{1_2}, S_{1_2}} \left[\sum_{i=1}^{k} \left(\tilde{\tau}_i - \bar{\tau}_{S_{1_1}}\right)^2 + \sum_{i=l+1}^{l} \left(\tilde{\tau}_i - \bar{\tau}_{S_{1_2}}\right)^2 \right], \tag{8}$$

and

$$\min_{S_{2_1}, S_{2_{12}}} \left[\sum_{i=l+1}^{m} \left(\tilde{\tau}_i - \bar{\tau}_{S_{2_1}}\right)^2 + \sum_{i=m+1}^{N} \left(\tilde{\tau}_i - \bar{\tau}_{S_{2_2}}\right)^2 \right], \tag{9}$$

where $k \leq l \leq m \leq N$, $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\} \in S_{1_1}$ for $i = 1, 2, ..., k$, $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\} \in S_{1_2}$ for $i = k+1, k+2, ..., l$, $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\} \in S_{2_1}$ for $i = l+1, l+2, ..., m$, $\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i, Y_{i,r,t}\} \in S_{2_2}$ for $i = m+1, m+2, ..., N$, $S_{1_1} \cup S_{1_2} \cup S_{2_1} \cup S_{2_2} \subset \chi$, and S_{1_1}, S_{1_2}, S_{2_1}, $S_{2_2}$are mutually disjoint. Then, we obtain surrogate GATES as

$$\begin{aligned}\bar{\tau}_{S_{p_q}} & E\left[Y_{i,r,t} \middle| \left\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i\right\} \in S_{p_q}, W_i = 1\right] \\ & - E\left[Y_{i,r,t} \middle| \left\{\boldsymbol{A}_j^{i,r,t}, \boldsymbol{A}_{-j}^{i,r,t}, \boldsymbol{X}_i\right\} \in S_{p_q}, W_i = 0\right].\end{aligned} \tag{10}$$

for $S_{p_q} = S_{1_1}, S_{1_2}, S_{2_1}, S_{2_2}$.

4.4. Estimation steps

In summary, we evaluate the impacts of information treatment according to the following steps:

1. We evaluate overall information treatment effects as the average marginal component effects.
2. Next, we evaluate GATES.
   (a) We predict the individual information treatment effects on preferences over policy packages as a function of policy attributes $\boldsymbol{A}_j^{i,r,t}$, $\boldsymbol{A}_{-j}^{i,r,t}$and background characteristics $\boldsymbol{X}_i$. The causal forest algorithm (Tibshirani et al., 2022) is employed for prediction.
   (b) Using the regression tree approach, we partition the combination of policy package $\boldsymbol{A}_j^{i,r,t}$, $\boldsymbol{A}_{-j}^{i,r,t}$, the background characteristics of respondent i, $\boldsymbol{X}_i$, and the outcome of choice of respondent i, $Y_{i,r,t}$, of the sample into two regions such that the within-region variance of the predicted individual treatment effects are minimized.
   (c) We partition each region into two regions again such that the within-region variance of the predicted individual treatment effects is minimized.
   (d) We calculate the average treatment effects for the four regions.

5. Results

5.1. Descriptive statistics of the background survey

Of the 15,000 respondents that we surveyed, 9,000 responded to the survey in all three periods. We analyze their choices of hypothetical public policy packages generated by our randomized conjoint experiment.

Among the background characteristics indicated in Table 3.2, the descriptive statistics of our 9,000 respondents' background characteristics are presented in Table A1 in the appendix. While our sample has a slightly denser distribution of high household income (10 million Japanese yen or higher) than that of the sample of the National Livelihood Survey 2019 by the Ministry of Health, Labour and Welfare (Table A2 in the appendix), the samples do not differ substantially.

5.2. Average marginal component effect

We first report the average marginal component effect without the information treatment, which is a case where $w = 0$ in Eq. (3), on the horizontal axis of Fig. 3 by simple regressions. In this and all following result figures, 95% confidence intervals are shown. The dependent variable is the probability that policy package $\boldsymbol{A}_j^{i,r,t}$ is preferred to policy package $\boldsymbol{A}_{-j}^{i,r,t}$. The independent variables denote spending of 0–20% of the raised consumption tax revenue on each policy instead of austerity, as described in section 3.3. We take the weighted average over the background characteristics vector $\boldsymbol{X}_i$ surveyed in Period 1 ($t = 1$). Periods 1 ($t = 1$), 2 ($t = 2$), and 3 ($t = 3$) denote our survey timings of November 2018, March 2019, and October 2019, respectively.

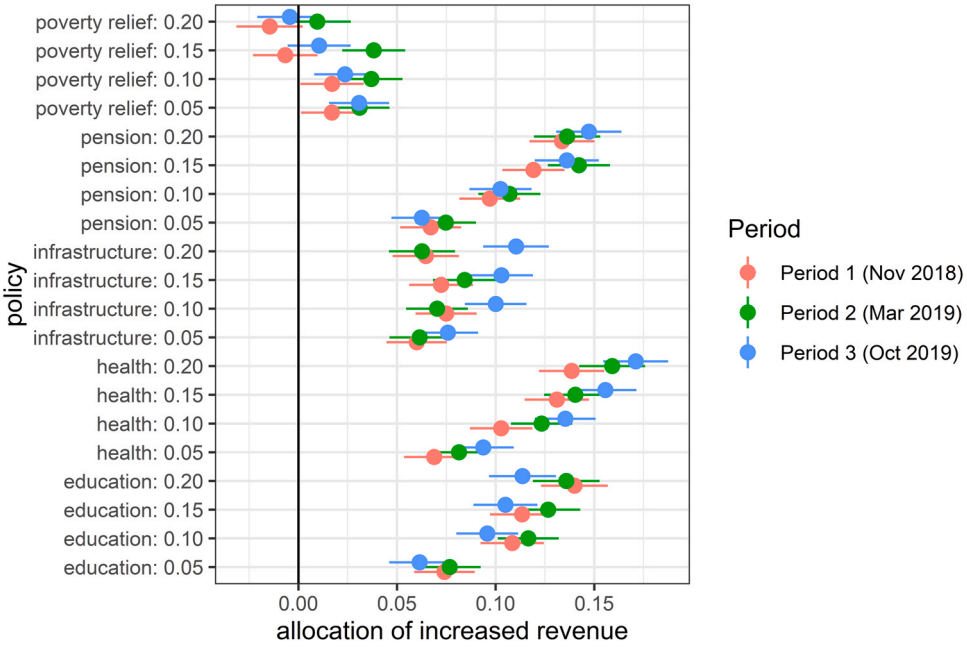

Fig. 3. Average marginal component effects of policy attributes.

First, respondents preferred spending on public health insurance and the national basic pension over spending on education and poverty relief programs in all three periods. The priority given to national health insurance and the national pension plan was widely shared.

Second, the probabilities of support for pension spending, public health insurance spending, and education spending monotonically increased with the share of expenditure in all three periods. For these three categories, respondents' attitude toward spending was "the more, the better."

Third, in all three periods, support for poverty relief was nonmonotonic. Support was increasing in spending shares from 0% to 5% but was mixed for shares between 5% and 15% and decreasing in shares beyond 15%.

5.3. *Overall information treatment effects*

First, we evaluate the overall information treatment effects on the average marginal effects in Fig. 4. As the panels for Periods 1 and 2 show, respondents who received information about the poverty rate lowered their support for policy packages allocating 0% or 5% to poverty relief and raised their support for those allocating 15–20% in Period 1 and in Period 2, 5 months later. While those who received the information lowered their support for the packages allocating 20% to infrastructure, the effect almost vanished in Period 2. Therefore, respondents who raised their support for poverty relief because of the information treatment considered financing the policy partially by reducing expenditure on infrastructure but primarily by relaxing fiscal retrenchment.

In Period 3, however, the information treatment effects mostly evaporated, such that we do not find a statistically significant difference between the treatment group and control group.

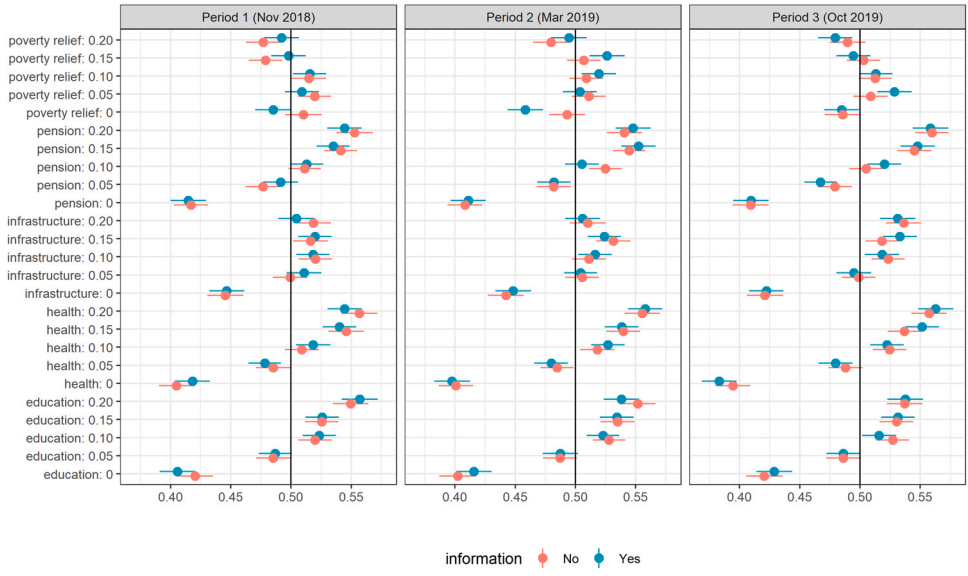

Fig. 4. Overall information treatment effects in average marginal component effects.

5.4. Group average marginal effects

Next, we estimate the GATES defined by Eq. (5) for four groups. According to Eq. (6), the first condition for partitioning the sample was whether the respondents preferred to allocate 10% or higher (10%, 15% or 20%) or less (0% or 5%) to poverty relief. According to Eqs. (8) and (9), whether respondents preferred to allocate 10% or higher (10%, 15%, or 20%) or less (0% or 5%) to education was chosen as the second condition for partitioning the sample. The results are presented in Fig. 5. The horizontal axis denotes the value of the GATES calculated by Eq. (5) for observations partitioned into the region. Background characteristics such as age, gender, education, income, prior perceptions of poverty, and party support were not chosen as Eqs (6), (8), and (9).

Support for policy packages allocating 10% or higher to poverty relief with spending 10% or higher on education rose by 1.7 percentage points in period 1 due to the information treatment, particularly in the first period. Although the information treatment effects weakened to 0.9 percentage points in the second period, policy packages allocating 10% or higher to poverty relief gained higher support due to the information treatment than in the control treatment. Packages allocating 10% or higher with allocating 5 or 0% to education also received higher support by 2.1 percentage points because of the information treatment. However, the rise in support for high spending for poverty relief due to the information treatment vanished in the third period.

In addition, support for policy packages allocating 5% or 0% to poverty relief decreased by 1.7 percentage points with spending of 10% or higher for education and by 1.5 percentage points with spending of 5% or 0% for education in the first period. The information treatment substantially decreased support for policy packages allocating just 5% or nothing to poverty relief. The effects were persistent in the second period, such that support for policy packages

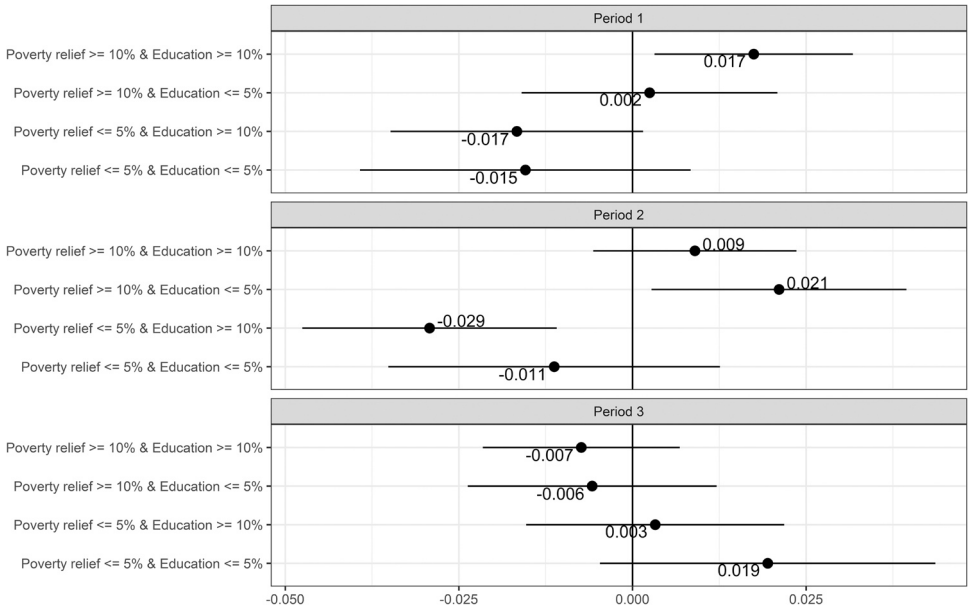

Fig. 5. Group average treatment effects.

allocating 5% or 0% to poverty relief decreased by 2.9 percentage points with education spending of 10% or higher and by 1.1 percentage points with education spending of 5% or 0%. However, again, this penalty on minimal or no expenditure for poverty relief due to the information treatment vanished in the third period.

Finally, we evaluate the magnitude of the information treatment effects. For comparison, we calculate the difference in support for policy packages allocating 10% or higher for poverty relief and education by background characteristics in the first period. As Balafoutas et al. (2012), Almås et al. (2020), and Kerschbamer and Müller (2020), among others, reported a difference in distributional preferences between genders, we consider gender first. Female respondents who did not receive the information treatment were more likely to support the policy packages allocating 10% or higher to poverty relief and education to a greater extent than male respondents who did not receive the information treatment by 2.1 percentage points (standard error: 0.007; confidence interval: 0.007 to 0.035). Moreover, Cappelen et al. (2013) and Alesina et al. (2018), among others, reported differences in policy preferences due to political positions. Therefore, we next examine partisanship. Supporters of the ruling Liberal Democratic Party and the Clean Government Party were less likely to support the policy packages allocating 10% or higher to poverty relief and education than independents by 1.8 percentage points (standard error: 0.009; confidence interval: −0.035 to −0.001). In summary, the impacts of the information treatment on policy preferences are comparable to the differences in policy preferences due to gender and partisanship. Note, however, that the effects of the information treatment on its own were not strongly associated with gender or political position, as the partitioning above shows.

6. Conclusion

We proposed a more generalized design to measure the effects of information provision on public policy preferences than unidimensional approaches such as those of Kuziemko et al. (2015), Alesina et al. (2018), and Becker (2020). The setting enabled us to find that the effects of an information treatment on policy direction preferences were not sensitive to respondents' background characteristics such as age, gender, education, income, partisanship, and prior perception of poverty and that the effects persisted for 5 months. The impact of the information treatment was comparable in magnitude to the differences in policy preferences attributable to gender and partisanship. Furthermore, we revealed how respondents supported financing the increase in relative support for poverty relief programs: they preferred to finance poverty relief programs by cutting spending partially on infrastructure and mainly on government debt repayment. This change in policy direction was only detectable because of our multidimensional approach.

In summary, our findings demonstrate that simple exchanges of information affect people's preferences and that the updates persist for several months. Once a simple information exchange is placed in a multidimensional space, it might have a longer scope than unidimensional approaches have previously shown. This finding could extend the possibility for a budget allocation policy to reduce poverty. In real political processes, such information exchange occurs among policymakers in the legislature when they discuss and approve of/consent to annual budgets and in their interactions with voters during elections. The provision of new information might affect the preferences of voters and hence their votes. This is why candidates discuss facts and issues and their attitudes toward them. Our results indicate that the availability of both choices to reallocate existing tax revenue to a specific purpose and an increase in tax

revenue for the purpose could broaden our capability to financially relieve poverty once relevant information is shared.

A crucial policy implication of our results is that information exchange between different people helps alleviate political divides. Income redistribution for poverty relief is an almost always controversial issue in advanced democracies. However, an information treatment notifying respondents of a simple fact about poverty in the nation drove a favorable change in support for poverty relief. Furthermore, the treatment effects lasted for several months.

Regarding the interpretation of this core finding, one limitation of our research is that it is restricted to one country. Aristei and Perugini (2010) presented that inequality tolerance substantially differs between nations even within Europe. Additionally, the quality of the space where people communicate matters. Using West African data, Kouadio and Gakpa (2022) presented that improvements to democratic institutions contribute to poverty reduction. If the quality of democratic institutions is different, the power of democratic communication must also differ. Furthermore, Fambeu and Yomi (2023) posed the question of whether democracy contributes to poverty reduction and, using sub-Saharan African data, determined that the answer can be yes if the growth rate and human capital are high. We identified the power of communication to reduce poverty, but the power is likely to depend on institutional quality, human capital, and the economic growth of Japan. If so, the degree of such complementarity between the power of democratic communication and factors such as institutional quality, human capital, and economic growth should be studied by cross-country comparisons.

Disclosure statement

The authors declare that they have no relevant or material financial interests that relate to the research described in this paper.

Ethical review

The Ethical Review Board, Institute of Social Science, The University of Tokyo approved this study (Approval number: 26).

Informed consent

The authors, Keisuke Kawata and Masaki Nakabayashi, obtained informed consent from all participants of the survey by designing the survey such that on the opening page, the survey described itself, requested informed consent, and did not allow the respondent to participate in the survey unless the respondent gave informed consent.

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Data availability

The datasets generated by the survey research and analyzed during the current study will be publicly available after cleaning at the Center for Social Research and Data Archives, Institute

of Social Science, The University of Tokyo (https://csrda.iss.u-tokyo.ac.jp/english/). Before they become publicly available at the institution, they are available from the corresponding author, Masaki Nakabayashi, upon request.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.jpolmod.2023.05.002.

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