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OKUDAIRA Hiroko
Okayama University

OHTAKE Fumio
Osaka University

KUME Koichi
Nagoya University of Commerce and Business

TSURU Kotaro
RIETI

The Research Institute of Economy, Trade and Industry
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What Does a Temporary Help Service Job Offer? Empirical suggestions from a Japanese survey*

OKUDAIRA Hiroko†
OHTAKE Fumio‡
KUME Koichi§
TSURU Kotaro**

Abstract

The aim of this paper is to test whether or not a temporary help service (THS) job benefits workers in Japan. By applying the average treatment effect on the treated estimation and its sensitivity tests to the Japanese survey data, we obtained the following findings. First, we observed evidence that THS work negatively impacts the probability of permanent employment in subsequent waves, when compared to directly hired part-time employment. Second, THS workers earn a significantly higher hourly wage than those originally unemployed. For those seeking permanent employment in particular, THS work provides a quick way to make a living for up to two years. We conclude that THS work in Japan has provided a means to obtaining quick earnings but has not offered a stepping-stone to permanent employment.

Keywords: Temporary help service job, ATT Estimation, Stepping-stone.
JEL classification: J08, K31

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† Correspondence: Department of Economics, Okayama University, 3-1-1, Tsushima-naka, Kita-ku, Okayama City, Okayama 700-8530, Japan. Email: okudaira@e.okayama-u.ac.jp. Tel& fax: +81-86-251-7542.
‡ Institute of Social and Economic Research, Osaka University
§ Department of Economics, Nagoya University of Commerce and Business
** Research Institute of Economy, Trade and Industry

1. Introduction

In the list of new items in the 2010 edition of *Fundamental Knowledge of Japanese in Current Use*, we can find a newly added word that hints at a harsh policy debate on temporary help service (THS) workers in Japan: *THS workers' cutbacks* (*hakengiri* in Japanese). After the Lehman shock, the fact that many THS workers lost their jobs was repeatedly reported by the media and was portrayed as THS-worker cutbacks, whereas permanent employees maintained relatively stable employment.[6] Some THS workers even lost their homes and formed hobo camps. Their serious situation gave the general impression that "THS work is in an extremely unstable employment system that makes workers suffer from low wages" (Labour Lawyers Association of Japan 2010) and led the Japanese Cabinet to submit an amendment to the Act for Dispatched Workers to the ordinary Diet session in April 2010. The submitted bill is expected to change the current provision for worker dispatching significantly if enacted. For example, registered THS work would be banned except for 26 fields of work that require specialized skills, and worker dispatching to manufacturing firms would also be prohibited. In principle, the amendment would tighten regulations for worker dispatching to "protect workers," a demonstrated objective of the bill.

However, there is no direct evidence to support the idea that the amendment would benefit workers in Japan. In fact, some THS workers disagree that the option for THS work should be discontinued. According to the Temporary Worker Survey conducted in January 2010, only 9% of THS workers agree with the amendment while more than one-third of them disagree. Economic theories also provide ambiguous pictures of THS work: some show that THS work can offer a stepping-stone to permanent employment and others portray it as a trap of endless

[6] "Several hundred of these unfortunates congregated in Hibiya Park in the center of Tokyo, forming a "hobo camp" that the media named 派遣村 (*haken-mura*, temp-workers village)" (*The Japan Times*, January 27, 2010).

precariousness. Many studies have tested the effect of dispatched work in Europe and the United States, but not in Japan, where direct evidence is urgently needed for the policy debate.[7]

This paper aims to fill this void, by applying the ATT (Average Treatment effect on the Treated) method to data from the Japanese survey that was conducted from 2008 through 2010. In particular, we consider THS work as a treatment, and compare THS workers' employment status and hourly wage in subsequent waves to those of the comparable control groups. A key to identifying the true treatment effect in the ATT method is the CIA (Conditional Independence Assumption), which ensures that subjects do not self-select into the treatment. In order to justify that our estimates are robust to the specific failures of the CIA, we take an approach proposed by Ichino et al. (2008): we simulate the unobservable confounders that can induce self-selection and check the sensitivity of the baseline ATT results against including the simulated confounders in the propensity score estimations.

Our results are in line with those from previous research. First, we observe weak evidence that a THS job has a negative impact on the probability to be permanently employed in later waves, relative to direct-hire part-time jobs. Second, THS workers earn a significantly higher hourly wage than those who were originally unemployed do. Especially for those who are seeking permanent employment, THS work provides a quick way to make a living at least for two years. Our results are robust to possible attrition and selection bias since we never observe any positive effects of THS work on future employment prospects in some robustness tests.

We note that our results are weak in the sense that we assume that our simulated confounders represent the true unobserved factors that could, if any can, induce a selection into the treatment. Inasmuch as we have any valid reasons to believe that this assumption is satisfied, we can draw the following policy implications from our results: first, THS work offers quick

[7] Section 2-2 explains the findings in the previous works.

earnings to those who would otherwise be unemployed, at least in the short run. Thus, immediate removal of registered THS work, as was proposed in the amendment plan of April 2010, may hinder the workers' welfare, especially of those who are not willing to work as permanent employees.[8] Second, THS work has not offered a stepping-stone to permanent employment. Rather, it has offered a trap of endless precariousness to those who are seeking a permanent job, if anything. When taken together, these findings do not support the idea of removing THS work; rather, they suggest the need to design a fixed-term contract that directs those seeking permanent jobs to stepping-stone jobs.

The remainder of this paper is organized as follows. Section 2 provides the institutional framework for THS work in Japan, and presents previous findings in the literature. Section 3 introduces the estimation method and data source, and defines a treatment and its comparison group. Section 4 summarizes estimation results, and section 5 provides our conclusions.

2. Background

2-1. THS workers in Japan

Japan experienced major regulation changes in worker dispatching from the late 1990s to the early 2000s. Until 1999, the Japanese government permitted worker dispatching only in very limited cases—i.e., for jobs that require such specialized skills as market research, translation, electric calculator programming, and design. This restriction was removed in the 1999 revision of the Worker Dispatching Act, whereby worker-dispatching activities were permitted except for foreign harbor transport, construction, policing, medical-related, and manufacturing occupations.

[8] About 57% of THS workers were not seeking permanent jobs as of wave 1 of the Temporary Worker Survey, December 2008. This paper presents the estimation results, separately for a whole sample and a sample of those who are seeking permanent jobs.

However, the 1999 revision of the Act still limited the dispatching period to one year, aside from the 26 fields of occupations that require specialized skills.[9] A 2003 revision of the Act replaced this one-year limitation for the dispatching period with a three-year limitation under some procedural conditions (Art. 40-2, Par.2-4).[10] In addition to extending the dispatching period, the 2003 revision also allowed worker dispatching in services for manufacturing products, which had been illegal until then. The dispatching period for manufacturing services was also extended from one year to three years in 2007. See Sugeno (2002, pp.206-217, in English) for more information about the development of the Act.

In line with the legal changes, the presence of THS workers in the Japanese labor force has grown significantly, although the total number of THS workers is still not very large. According to the Employment Status Survey, conducted by the Japanese Ministry of Internal Affairs and Communications, the proportion of THS workers among the employed was only 0.47% in 1997, more than doubling to 1.32% in 2002 and more than quintupling to 2.81% in 2007. The number of THS workers in manufacturing jobs also increased, especially after 2007, when the legitimate dispatching period was extended from one year to three years, from about 80 thousand registered THS workers in 2006 to 200 thousand in 2008 (THS Establishment Reports, conducted by the Japanese Ministry of Health, Labour and Welfare). Relaxations of the regulation are also reflected in the Employment Protection Legislature (EPL) index published

[9] The 26 specialized fields have no limitation of contract period in worker dispatching. These 26 fields include (1) electric calculator programming and design, etc., (2) machinery design and drafting, (3) machinery operation for producing sounds and images for broadcast programs, (4) performance in broadcast programs, (5) operation of office machinery, (6)interpretation, translation and shorthand, (7) secretarial work, (8) filing, (9) market research, (10) management of financial affairs, (11) the drafting of foreign and domestic exchange documents, (12) the presentation and explanation of manufactured goods, (13) travel guides and tour conducting, (14) cleaning of building materials, (15) driving, inspection and maintenance of construction equipment, (16) building receptionist and guide, (17) research and development, (18) planning and developing enterprise systems, (19) creating and editing publications, (20) designing goods and advertisements, (21) interior coordinator, (22) announcer, (23) Office Automation instruction, (24) telemarketing, (25) sales and engineering business, (26) installation of high-end and low-end devices in broadcast programs (Work. Disp. Law Ord., Art.4).

[10] Employers are required to hear opinions from a representative of the majority of workers, etc. (Art. 40-2, Par.4)

by OECD. The EPL index for temporary employment in Japan was reduced from 1.8 in the late 1980s to 1.3 in 2003, while the same index for the United States remained at 0.3 for the same period (OECD 2004). According to the 2003 version of the index, Japan has the 11th most lenient EPL for temporary workers among 28 OECD countries.

This trend of relaxation was reversed around the late 2000s, however. Especially after the two major THS agencies in Japan, Fullcast Co. and Goodwill Inc., suspended their operations due to illegal worker dispatching, the media paid lots of attention to the problematic working conditions in day-to-day worker dispatching. One prominent example is a series of sensational reports about daily-dispatched workers living in a small booth at an Internet café.[11] Debate over the regulations for worker dispatching became even harsher after the Lehman shock in 2008, when many THS workers were fired. Because of these events, the Japanese Cabinet submitted the amendment bill to the Worker Dispatching Act, whereby they are seeking to restrict worker dispatching in some situations. For instance, the April 2010 version of the submitted bill proposes that registered dispatched work be banned except for 26 fields of work (see footnote 9), and that worker dispatching to manufacturing firms and daily hiring dispatching also be prohibited. As of October 2011, the submitted bill remains under Diet deliberation. We might see a major turning point for the regulation of worker dispatching in Japan if this bill were to be passed.

2-2. Previous Literature

Temporary jobs generally offer less to workers than permanent employment, although they can

[11] "Many Net café inhabitants rely on their cell phones to arrange day jobs that don't require a fixed address" (*The Japan Times*, August 29, 2007). About 31% of such Internet refugees are more than 50 years old, and nearly half of them engage in daily hiring jobs (General Survey on Daily Dispatched Workers, 2007, conducted by the Ministry of Health, Labour and Welfare).

be better than nothing. Using a British Household Panel Survey from the 1990s, Booth et al. (2002) showed that temporary jobs pay lower wages and provide less job training, but they also confirmed that more than 30% of workers originally employed in fixed-term contracts obtained permanent employment within the survey period. Similarly, Graaf-Zijl et al. (2011) showed from labor supply panel data in the Netherlands that temporary work shortens the duration of unemployment, although it has no significant larger effect than being unemployed on the hazard of finding regular employment. One of the important explanations behind this "better-than-nothing" effect is that part-time employment provides an opportunity to acquire general training. A growing number of studies have recently paid more attention to whether a temporary job provides any extra economic effects if it is specifically offered by THS agencies.

Ex ante, it is not possible to predict whether the extra effect would lead workers to permanent employment. For example, having a THS job can send either bad or good signals. By engaging in THS jobs, some workers may present themselves that they are ready for general training (Autor 2003), while for others it may imply that they have no better options. THS agencies may also induce efficient matching by exchanging information between workers and firms. However, it is also hard to conclude that THS agencies provide better matching than an intermediate job exchange office, which can be used for direct-hire jobs. They may also offer occasions to accumulate general skills to those who otherwise would have been without employment, but again direct-hire temporary jobs can offer the same opportunities.

Autor and Houseman (2010) tested the empirical effect of agency jobs in the United States and found no evidence that THS jobs have a positive effect. Among many empirical studies, their study is the first and only one that identified the THS effect through quasi-experiment. They used data from low-skilled workers in a welfare-to-work program in Detroit, in which participants are rotationally assigned to contractors with differing job

placement rates to temp agencies, and showed that temporary help placement may hinder subsequent employment and earnings. Autor et al. (2011) further investigated the same placement effect among the distribution of participants' earnings, rather than using an average, and showed that the negative effect of temporary help placement is concentrated in the upper tail.[12]

In contrast to their findings, the rest of the empirical studies point in different directions.[13] Lane et al. (2003) showed in the U.S. dataset from the Survey of Income and Program Participation, 1990-93, that working at THS jobs increases the probability of having employment compared to being without employment, thereby supporting the use of THS placements in welfare programs. Heinrich et al. (2005) reported that welfare recipients in Missouri and North Carolina experience high wage growth if they work in THS jobs. With an extended dataset, Heinrich et al. (2009) also showed that working in THS jobs has very little long-term negative impact on workers' earnings and employment. Anderson et al. (2009) found evidence implying that THS jobs provide better access to higher wage firms among prime-aged adults in five U.S. states. For European evidence, using specifically Italian data, Ichino et al. (2008) developed a simulation-based ATT sensitivity test and showed that THS workers have a significantly higher probability of obtaining permanent employment than do other non-permanent workers and the unemployed. On the other hand, from a dataset in Germany, Kvasnicka (2009) found that THS work does not increase the probability of obtaining non-THS jobs but that THS work reduces the monthly risk of unemployment. Thus, the evidence is quite mixed in both the United States and Europe, although discrepancies found in the United States

[12] Due to their causal findings that both the negative effect of temporary help placement and the positive effect of direct-hire placement are concentrated on the upper tail of conditional earnings of participants, Autor et al (2011) cast doubt on the idea that the welfare program in Detroit has indeed helped the least advantaged.

[13] One exception is Amuedo-Dorantes et al. (2008), who found a negative effect from THS jobs, using Spanish data.

may be attributable to differences in estimation methods (Autor and Houseman, 2010).[14]

Despite a harsh recent policy debate over revising the Worker Dispatching Act, no studies have directly quantified the empirical effect of THS jobs in Japan. Esteban-Pretel et al. (2011) structurally estimated a job search model by using the Employment Status Survey to test the stepping-stone effect of all types of non-regular jobs, including both directly hired part-time workers and THS workers. They conclude that non-regular jobs are neither stepping-stones nor a dead end with regard to permanent employment, but they did not identify the effects specific to THS jobs. Several other studies use samples of THS workers, but none has focused on identifying its causal effect on future employment prospects.[15] This paper attempts to provide the first causal evidence in Japan, by relying on a recent development in the ATT method and in the Japanese questionnaire survey that fits this purpose.

3. Methodology and Data

3-1. Estimation Strategy

This paper takes the ATT (Average Treatment effect on the Treated) estimation approach to identify the effects of THS work. To be precise, we estimate the propensity score to indicate the probability of being treated (i.e., working as a THS worker), and match each treated observation

[14] "Substantial differences between the marginal treatment effects of temporary-help placements, recovered by our instrumental variables estimates and the average treatment effects recovered by estimators in other studies could account for these disparate findings" (Autor and Houseman, 2010, 99).

[15] Genda (2008, in Japanese) used a large sample from the Employment Status Survey and examined the significance of previous experience at the same company in transitioning to permanent employment. Nakamura (2010, in Japanese) used employment records from one THS agency and descriptively analyzed the re-employment rate of THS workers into the same agency. Nagase and Mizuochi (2009, in Japanese) analyzed a Labour Force Survey and found significantly positive effects of THS work on the probability of being permanently employed, although they did not focus on the selection bias between the choice of THS jobs and other unstable jobs. A previous version of this paper (Okudaira et al., 2011, available only in Japanese) presented baseline results similar to those in the current paper, but did not show any sensitivity results.

to a non-treated observation that has similar propensity scores. A key assumption underlying this method is the CIA (Conditional Independence Assumption), which ensures that selection into the treatment depends only on the observables. As will be discussed, our research design does not employ a quasi-experiment scheme to satisfy the CIA, nor do we use instrumental variable estimations. Instead, we take a completely different approach, which was proposed by Ichino et al. (2008): we simulate the unobservable confounders and check the sensitivity of the baseline ATT results against including the simulated confounders in the propensity score estimations. This section briefly summarizes the baseline ATT method and presents the idea of the sensitivity test proposed by Ichino et al. (2008).[16]

Let T be the binary variable to indicate the treatment status: T=1 if the treatment is assigned at wave 1 (i.e., the person is employed as a THS worker), and T=0 if the treatment is not assigned and the person is in a control group at wave 1 (i.e., the person is unemployed or directly hired as a part-time worker).[17] Let Y_0 and Y_1 be the potential outcomes that take a value if one if the individual is permanently employed at a later wave and zero otherwise. Subscripts in Y_0 and Y_1 indicate the treatment status. Thus, Y_1 indicates the potential outcomes, as if the individual had been treated, and Y_0 indicates the potential outcomes, as if the individual had not been treated, regardless of the actual treatment status. Only one of them is observed. We are interested in estimating the ATT (Average Treatment effect on the Treated), defined as follows:

$$E(Y_1 - Y_0 \mid T = 1) \qquad (1)$$

[16] This section follows largely from the discussion in Cameron and Trivedi (2005) and Ichino et al. (2008).

[17] The next subsection discusses details about the definition of treatment and controls.

Let W be the observable characteristics that can affect the treatment status and the potential outcomes. By assuming the following, we can identify the ATT effect from the observed outcomes (Rosenbaum and Rubin, 1983):

$$Y_0 \perp T \mid W \qquad (2)$$

$$0 < \Pr(T = 1 \mid W) < 1 \qquad (3)$$

The first assumption is called CIA (Conditional Independence Assumption), which assumes that potential outcome Y_0 is independent from treatment status T, conditional on observed characteristics W. In other words, the selection into the treatment depends only on W. The second assumption is called common support, which assures that each treatment has a comparable control that has the same observable characteristics W. Under the two assumptions, we have

$$E(Y_1 - Y_0 \mid T = 1) = E(E(Y_1 \mid T = 1, W) - E(Y_0 \mid T = 0, W) \mid T = 1). \quad (4)$$

That is, under the CIA, we can replace the potential outcome, Y_0, with the actual outcome of the non-treated to estimate the ATT effects in equation (1), conditional on W.

It is desirable that we always have a comparable control unit with exactly the same values for the observed characteristics, W, for each treatment unit. However, in general, it is hardly possible to condition on all the observable characteristics. To avoid this dimensionality problem, we estimate the probability of being treated, or the propensity score $p(W) = p(T = 1 \mid W)$, and make the condition on $p(W)$ instead of on W in estimating the ATT effect:

$$\tau \equiv E(Y_1 - Y_0 \mid T = 1) = E(E(Y_1 - Y_0 \mid p(W), T = 1)) \\ = E(E(Y_1 \mid p(W), T = 1) - E(Y_0 \mid p(W), T = 0) \mid T = 1) \quad (5)$$

In order to construct comparable combinations of treatment and control pairs, we employ a nearest-neighbor matching algorithm, where a treatment unit is matched with a control unit that has the closest value of the propensity score to that of a treatment unit.

Identification of true ATT effects relies entirely on the fact that the research design satisfies the CIA. Unfortunately, we do not have any explicit reason to believe that the CIA holds in our case. In fact, CIA is not a testable assumption (Ichino et al., 2008). To obtain reliable ATT estimates, we apply an approach suggested by Ichino et al. (2008). In particular, we simulate a binary confounder, U, which induces the CIA to hold:

$$\Pr(T = 1 \mid Y_0, Y_1, W) \neq \Pr(T = 1 \mid W) \quad (6)$$

$$\Pr(T = 1 \mid Y_0, Y_1, W, U) = \Pr(T = 1 \mid W, U) \quad (7)$$

The binary confounder can be considered as a “skill” (Ichino et al., 2008) that is usually unobserved but that selects individuals into a treatment. Assuming that equations (6) and (7) are satisfied, the unobserved binary confounder is characterized by specifying the parameters

$$\Pr(U = 1 \mid T = i, Y = j, W) = \Pr(U = 1 \mid T = i, Y = j) \equiv p_{ij} \quad (8)$$

with $i, j \in \{0,1\}$. If our baseline ATT estimation satisfies the CIA, the ATT estimates should be robust to including a binary confounder with the observable characteristics, W. In this vein, we can test the sensitivity of our baseline results to the inclusion of binary confounders, assuming

that the binary confounder replicates true unobserved characteristics that, if any can, could induce a violation of the CIA.[18]

This paper simulates the confounder by using the available information about the actual observed characteristics. Appendix Table 2 presents examples of confounders that have been characterized by four parameters, p_{ij}, where the parameters are replicated from the actual distribution of observed characteristics. For instance, parameter p_{11} for a confounder like "married" is 0.20, as presented in the last row of Appendix Table 2. This value is calculated by taking an average of male dummies for the sample, who had been treated at wave 1 (i =1) and had obtained a permanent job at wave 5 (j=1).

To obtain the results with unobserved binary confounders, we first attribute a value of U to each subject, according to his or her treatment and outcome status: $i, j \in \{0,1\}$. In an example of the binary confounder, such as "married," one is assigned a value of one with the probability of 0.20 if he or she had been treated at wave 1 (i =1), and had obtained a permanent job at wave 5 (j=1). Then we include a value of U as one of the explanatory variables in the propensity score specifications and match a treatment unit to a control unit using this new propensity score to obtain the ATT estimate. We repeat this procedure 100 times to obtain the ATT estimate, which is an average of ATT estimates over the distribution of the simulated U.[19]

One of the advantages of this method is that there is no risk of mis-specifying the selection process, but that we can still obtain the point estimates. Moreover, parameters can be chosen to make the distribution of U similar to the empirical distribution of observable binary characteristics. Although we never know which U represents a true unobserved confounder, this approach can still examine the validity of baseline estimates by repeatedly simulating many

[18] The sensitivity test proposed by Ichino et al. (2008) has been applied in some empirical studies, such as Cornelissen et al. (2010) and Maertens et al. (2008).
[19] Sensitivity estimations are conducted by Stata command, sensatt, written by Nannicini (2007).

kinds of confounders, thereby narrowing down the possible candidates that could contaminate the baseline result. Finally, and more important, this method allows us to avoid "ad-hoc" identification strategies such as finding appropriate instrumental variables or exogenous variations in programs. This feature is especially appealing to countries where social experiment is not common (e.g., Japan).

3-2. Data

In order to have comparable treatment and control groups, this paper draws on the *Questionnaire Survey on the Life of Temporary Workers and Their Job Search Behavior* (hereafter referred to as the Temporary Worker Survey). The Temporary Worker Survey is an Internet survey conducted by the Research Institute of Economy, Trade, and Industry (RIETI), which is an affiliated research agency of the Japanese Ministry of Economy, Trade, and Industry (METI).[20] This survey was aimed at contingent workers and the unemployed in Japan, including part-time workers directly hired by firms or THS workers indirectly hired through the temporary help agencies. The main survey was conducted in January 2009 (wave 1), and succeeded by four follow-up surveys in July 2009 (wave 2), January 2010 (wave 3), July 2010 (wave 4), and January 2011 (wave 5).[21] All surveys ask questions pertaining to changes in individual attributes, such as employment status. In addition, the main survey (wave 1) included numerous detailed items for ascertaining factors such as preference parameters, including time discounting rates, household factors, and degree of economic deprivation.[22] The response rate

[20] Information about RIETI is available in the following website (in English): http://www.rieti.go.jp/en/. The survey was conducted through a Japanese survey company, Intage, Inc.: http://www.intage.co.jp/english/

[21] We found no statistically significant differences in the group means for the attrition rate by treatment status, which will be defined in the next section.

[22] The summaries and reports of the survey results can be found on the following websites of the

ranges from 55% to 73%, with approximately 1,000 to 2,000 respondents on each wave. All respondents are 18 years of age or older, and are not students, homemakers, or retirees as of wave 1.

The advantages of using the Temporary Worker Survey come from its wide coverage among non-regular workers, along with the unemployed. Given the low proportion of THS workers (i.e., 2.81% in 2007, according to the Employment Status Survey), the Temporary Worker Survey efficiently extracted many types of contingent workers by using the information from a screening survey sent to a large respondent pool.[23] In addition, former surveys aimed at THS workers used firms and temporary work agencies as the means of survey distribution to actual temporary workers.[24] In contrast, the Temporary Worker Survey was conducted through direct communication with individual temporary workers through the Internet. Given the fact that this survey has no extraction based on the attributes of the firms that were supplied with questionnaire, it is possible to infer that the samples covered a wide range of temporary workers. Finally, but most important, the Temporary Worker Survey was also conducted in a way that enabled us to construct the comparable treatment and control. As will be discussed in the next section, both the treatment and controls are extracted from the survey, and thus, the same questionnaire is used for both groups. Heckman et al. (1997) suggest using the same questionnaire for both treatment and controls to reduce the bias in matching estimates. That we did so is an advantage of this paper since some of the previous studies with ATT estimations

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Research Institute of Economy, Trade, and Industry (available only in Japanese):
Wave 1, January 2009 http://www.rieti.go.jp/jp/projects/research_activity/temporary-worker/01.html
Wave 2, July 2009 http://www.rieti.go.jp/jp/projects/research_activity/temporary-worker/02.html
Wave 3, January 2010 http://www.rieti.go.jp/jp/projects/research_activity/temporary-worker/03.html
Wave 4, July 2010 http://www.rieti.go.jp/jp/projects/research_activity/temporary-worker/04.html

[23] The survey population consists of those who registered their bank account information in Yahoo! Research.

[24] Examples include Survey Concerning the Actual Conditions of Day-to-day Employment for Temporary Workers (Ministry of Health, Labour and Welfare) and Survey on Actual Conditions for Temporary Workers (Ministry of Health, Labour and Welfare).

extracted their treatment and control groups from separate surveys or datasets.[25]

Despite these advantages, a word of caution is in order regarding use of the Temporary Worker Survey, because the sample does not necessarily represent the whole working population in Japan. Table 1 compares summary statistics for the Temporary Worker Survey with some government statistics. The overall comparison indicates that the respondents in the Temporary Worker Survey, at least on average, have (1) a relatively high educational background and (2) a relatively high female ratio—and, in the case of temporary workers employed for less than a month (i.e., short-term THS workers), (3) a relatively high ratio of older workers. The following ATT estimates, therefore, indicate the effects of THS work that accrue mainly to these groups.

3-3. Treatment and Control

Our analysis begins by setting the treatment and control groups with information about the main employment status reported at wave 1. In particular, the treatment is assigned to respondents whose main employment status in the month before wave 1 (December 2008) is "temporary help service worker with a fixed-term contract of *more than one month* (hereafter, long-term THS worker)."[26] We compare the outcome of the treatment group against the outcome of two kinds of control groups:

C1: the unemployed

[25] For example, Ichino et al.'s (2008) data collection was based on the data that was being collected. The data on temporary workers—i.e., the treatment group—were collected via submissions from the temporary agency, whereas data on people not seeking permanent employment—i.e., the control group—were collected through a telephone survey.

[26] Note that THS workers are those temporarily hired by the temporary work agency (i.e., registered THS or *Touroku Gata Haken*), and those who are regularly employed by the temporary work agency (i.e., regular THS or *Joyo Gata Haken*) are excluded from our sample.

C2: directly hired part-time worker

C1 denotes the respondents who replied that their main employment in December 2008 was "not in an employment but seeking a job." C2 denotes the respondents who replied that their main employment in December 2008 was "directly hired part-time worker with a fixed-term contract of more than one month," or "directly hired part-time worker with a contract of no specified period." In order to satisfy the common support assumption in equation (3), we make sure to exclude the control units that worked in occupations in which worker dispatching is prohibited. In other words, we exclude the occupations in which $\Pr(T=1|W)$ equals zero.[27]

For all the treatment and control groups, we compare the outcomes in the subsequent four waves of the Temporary Worker Survey: June 2009 (wave 2); December 2009 (wave 3); June 2010 (wave 4); and December 2010 (wave 5). As an outcome variable Y, we employ two variables to examine the economic effect of THS work: (1) a dummy variable, which takes one if a respondent works on a permanent contract (*seisyain* in Japanese) at a subsequent wave, and zero otherwise; and (2) the hourly wage.[28] For those who do not hold any jobs, hourly wages are set to zero. Table 2 shows the descriptive statistics for the outcome variables by treatment status. About 9% of respondents who had been originally working in THS jobs obtained permanent jobs by two years from the start of the survey. Note that our sample size is relatively small but still comparable to the one in Ichino et al. (2008).[29]

It should be noted that our research design contain two kinds of possible bias by defining the treatment and control groups in this way. The first possibility is selection bias, which could

[27] Okudaira (2011, in Japanese), the previous and preliminary version of this paper, did not exclude this possibility, although it does not change most of our results.

[28] The wage rate per hour is calculated as follows: hourly wage ratio = monthly earnings /(work days in a month × labor hours per day). Extreme values are treated as missing values, although we have only a few of such cases.

[29] The numbers of treated units and matched controls units are 281 and 133, respectively in Tuscany; 230 and 131, respectively in Sicily (Ichino, Mealli, and Nannicini 2008).

arise due to the possible violation of the CIA. Unfortunately, our research design does not necessarily ensure the random assignments of THS jobs to each respondent, and this is the reason we conduct the sensitivity analysis, as already explained in section 3.1.[30] A second possible bias could arise because we defined respondents' treatment statuses by their main employment statuses in December 2008. The observations in our sample start working at different points in time, and those who had started working at similar points in time, but who obtained permanent jobs before December 2008, are excluded from the sample. Thus, our observations have a lower propensity to be permanently hired, and if this propensity differs systematically between treatment and control groups, the baseline estimates would contain attrition bias. After conducting the sensitivity analysis, we will come back to this issue and conduct some robustness tests, although we obtain the similar conclusion.

4. Results

4-1. Baseline ATT estimation

One important key for obtaining plausible ATT estimates is to specify the propensity score models that take out the comparable treatment-control pairs from a sample. In specifying a plausible model for propensity scores, we make sure that our models (1) satisfy the balancing property, (2) use the predetermined characteristics for W in section 3-1, and (3) have sufficient explanatory power.[31] Table 3 reports the averages for the predetermined variables, W, used in the propensity score estimations, separately for treatment status. Averages are also shown for the observations in control groups that are actually matched to the treatment in ATT estimations.

[30] Selection into the treatment is based not only on workers' characteristics, but also on firms' attributes. Unfortunately, we cannot overcome this problem in the current estimations strategy. In fact, the literature has failed to account for this possibility. Even in Autor and Houseman (2010), who used almost random assignments of agency jobs, firms' decisions to use agency jobs may not be necessarily random.

[31] We evaluate the explanatory power of our models by McFadden's adjusted R-squared.

The difference between control and treatment groups became smaller in terms of most variables (not all) when compared to the matched controls, indicating that our method picks up appropriate comparable units, at least in terms of averages.

As an example of the propensity score estimates, Appendix Table 1 shows the propensity score estimation results that are used in the estimation of ATT for the probability of obtaining a permanent job at wave 2, where long-term THS workers are compared to directly hired part-time workers (C2). It is important to note that our propensity score specification does not provide consistent estimates since we use choice-based samples.[32] According to Heckman and Todd (2009), even if the sampling weight is unknown in choice-based samples, it has been shown that the odds ratio of the propensity score in a miss-specified model is still monotonically related to the odds ratio of the true propensity score. Since we use a nearest-neighbor algorithm, the usual propensity score is used to match a treatment to the control.[33] Appendix Figure 1 presents histograms for estimated propensity scores, whereas Appendix Figure 2 shows scatter plots for the hourly wage at wave 2 and estimated propensity scores. Dashed lines indicate the common support region. Only observations within the common support are used in the nearest-neighbor matching. As can be seen, we have sufficient overlap in propensity scores for both groups. The following ATT results are based on the similar propensity score estimations specified separately for each of the four waves and the two kinds of outcome variables. All of them pass the balancing test.[34]

[32] The Temporary Worker Survey employed a stratified random sampling scheme by types of employment contract (i.e., THS workers or directly hired part-time workers) to ensure that a given number of respondents would be included in the survey. Thus, propensity scores are not estimated consistently, because the dependent variable is determined in accordance with the treatment-to-control ratio imposed by the survey design.

[33] In the case of the logit propensity score, matching on the log odds ratio gives identical estimates to matching on the (unknown) true propensity score, because the odds ratio preserves the ranking of the neighbors (Heckman and Todd, 2009, S233).

[34] Note that in some specifications we excluded a variable to indicate the number of child or dummy variables for the fathers' education in order to satisfy the balancing property.

Table 4 shows the baseline ATT estimates with *long-term* THS work as a treatment. Analytical standard errors are reported following Abadie and Imbens (2006). When we compare the probability of being permanently employed for long-term THS workers to the one for the unemployed (C1), we find no significant differences in all waves. On the other hand, long-term THS workers earn significantly higher hourly wages than the unemployed do. Even two years after the start of the survey, the THS workers still earned 645 yen (i.e., about US$8.3 as of September 15, 2011) more in hourly wages than did those who were unemployed at wave 1. A different picture emerges when we compare the treatment to directly hired part-time workers (C2): long-term THS workers are significantly less likely to obtain permanent employment than are the directly hired workers at waves 4 and 5. We observe no statistically significant differences in their hourly wages, except at wave 4, where THS workers earn slightly, but statistically significantly, higher hourly wages than do the directly hired part-time workers. To summarize, *assuming that the CIA holds*, our baseline ATT estimates indicate that THS work provides a means to earn a living for at least two years when compared to staying without employment, and even to earn a higher hourly wage than in directly hired part-time work, while we find some evidences that THS work deters attainment of permanent employment.

4-2. Sensitivity Analysis

The CIA is a fundamental assumption for identifying the true treatment effect in the ATT estimation framework. Unfortunately, our research design does not entail any explicit grounds to believe that the CIA holds in the baseline case presented in the last section. This paper addresses the possibility of violating the CIA by taking an approach suggested by Ichino et al. (2008), as already discussed in section 3-1. To be precise, we simulate the unobserved confounders, based

on the distribution of the predetermined characteristics, W; include it as one of the explanatory variables in the propensity score estimation; and repeat this process 100 times to obtain the averaged ATT.

Table 5 reports the results from the sensitivity analysis with directly hired part-time worker as a control group. The first row replicates the baseline ATT estimates presented in Table 4, and each cell shows estimation results from separate sensitivity analyses where confounder U is replicated from a listed predetermined variable. Note that our inference provides conservative conclusions, because we calculate variance as a weighted sum of both between-imputation and within-imputation variances (Nannicini 2007).[35] Our sensitivity analysis reveals that the baseline conclusion remains in most of the cases but not all the time. Columns (1) to (4) of Table 5 show that a long-term THS job has a negative impact on the probability of being permanently employed in waves 4 and 5, although some of the estimated effects are only weakly significant. Our conservative estimates at wave 5 indicate that, given the observed covariates and a simulated confounder U fixed, the impact of originally working at THS jobs ranges from zero to -19%. Because we never know which confounder resembles a true unobservable, we cannot conclude whether the true effect would be negative or zero. However, we can conclude that it is unlikely that long-term THS work provides a stepping-stone to a

[35] Let *m* be the number of imputations, and let $\hat{ATT}_k$ and se_k^2 be the point estimate and the estimated variance of the ATT estimator at the *k*-th imputed data set (*k*=1,2,..,*m*). $\hat{ATT}$ is obtained by averaging $\hat{ATT}_k$ over *m* replications. The variance of our estimate is given by

$$se_T^2 = se_W^2 + (1+\frac{1}{m})se_B^2$$

where the within-imputation variance is equal to

$$se_W^2 = \frac{1}{m}\sum_{k=1}^{m} se_k^2$$

and the between-imputation variance is equal to

$$se_B^2 = \frac{1}{m-1}\sum_{k=1}^{m}(\hat{ATT}_k - \hat{ATT})^2$$

permanent job, at least within two years, because we never observed a significant *and* positive impact on the probability of permanent employment, even after repeatedly simulating several types of confounders. In a similar vein, columns (5) to (8) of Table 5 show that a long-term THS worker could possibly earn higher hourly wages at wave 4 than directly hired part-time workers do. No significant wage effect is observed at the other waves.[36]

Columns (1) to (8) of Table 6 show similar estimation results but with the unemployed as a control. Again, our baseline results are moderately robust to possible violation of the CIA: we observe no significant impact on the probability of being permanently employed, but THS work offers a higher hourly wage than staying without employment in some cases of simulated confounders for waves 3 through 5. Because the hourly wage is set to zero for those who do not hold any jobs at each wave, the positive wage effect could reflect both possibilities: that THS agencies offer access to higher paying jobs than if prospective employees were searching on their own and that THS jobs, at most, provide the employment opportunities. To separate out these effects, we restrict our sample to those who hold any jobs at each wave. In other words, we conduct a similar analysis as in columns (5) to (8) of Table 6 but exclude those without employment in each wave.[37] The results are shown in columns (9) to (12). Although we obtained significantly positive effects for the baseline estimates of wave 3, we no longer observe any significant wage differences across sensitivity estimates. Therefore, temp agencies in Japan do offer access to some jobs, but these jobs do not necessarily pay higher wages than jobs that an unemployed person would find on his or her own.

[36] We observed heterogeneous effects across waves, although we do not know the reason. Since the survey was conducted every six months, seasonality may be one reason. Another reason may be related to the days of the week on which the survey was conducted; wave 2 was conducted over the weekend, while other waves were conducted during weekdays.

[37] Due to our small sample size, we cannot restrict our sample to those who hold any *permanent* jobs at each wave.

4-3. Further Robustness Tests

This section presents results from two kinds of robustness tests. First, we restrict our sample only to those who are looking for permanent jobs to take into account the fact that many female workers in Japan voluntarily choose non-permanent jobs. This is partly because the Japanese tax system has maintained disproportionate financial incentives for household dependents who do not work full time (Houseman and Osawa, 2003).[38] Another reason relates to the Japanese case law. Since the Japanese courts have established that "a labor relationship in which there has [sic] been repeated renewals of a short term contract is converted into one with a contract without a definite term" (Sugeno, 2002, p. 193), some directly hired part-time workers consider themselves to be employed "permanently," even if they are employed on a temporary basis. Indeed, according to the survey question, less than half of the respondents in our sample are willing to be permanent workers (see footnote 41). We have to exclude those respondents from our estimation; otherwise, our negative baseline estimates on permanent employment status may contain biases because workers do not want permanent jobs in the first place.

For this purpose, Tables 7 and 8 present the estimation results when we restrict our sample only to those who are looking for permanent jobs as of wave 1.[39] While the effect on permanent employment status is no longer significant at waves 4 and 5 in Table 7, the signs of the estimates are still negative in most cases. Even after the sensitivity analysis, no significant *positive* effect is observed on the probability of being permanently employed. In contrast, wage differences compared to the unemployed are statistically significant, and became even larger in Table 8, indicating the possibility that temp agencies offer quick access to the higher paying

[38] "…workers earning up to 1,030,000 yen per year do not pay taxes on their income, and, if they are married, their spouse [sic] may claim a dependent deduction from his or her [sic] income taxes"(Houseman and Osawa, 2003, p.199). Some part-time workers also receive health insurance and pension benefits. See Houseman and Osawa (2003) for details.

[39] Respondents who selected "yes" to the question at wave 1, "Would you like to work as a permanent worker in the future?" A dummy to indicate the willingness to obtain a permanent job is added to the propensity score specifications for panel A.

jobs.

A second robustness test examines the magnitude of the attrition bias in our baseline estimates. As explained in section 3.3, we defined respondents' treatment status by their *main* employment status in December 2008, so our observations start working at different points in time. This situation may cause a bias to our estimates if our respondents have a lower propensity to be permanently employed and if this propensity differs systematically between treatment and control groups. In fact, it is not sufficient to have observations with the same start date of work, as was done in most of the previous studies, because workers may change their jobs by registering at different temp agencies. It is ideal here to test treatment effects among new entrants to the labor market since they have no previous experience, either in THS or in directly hired part-time jobs; thus, no attrition occurs in terms of the potential prospect of obtaining a permanent job.[40]

We address this issue by using the wave 2 survey questions about respondents' past working experience at THS jobs. In particular, we restrict our sample to those who had never worked in THS jobs as of June 2002, 2004, and 2007, two to seven years before wave 2 of the survey. Tables 9 and 10 show the estimation results. Again, most of our estimates in Table 9 on the probability of being permanently employed are not always significantly negative after the sensitivity test, although the estimated signs are never significantly positive. In Table 10, we can observe significantly positive effects on the hourly wage of wave 3. The baseline estimates at waves 4 and 5 are negative, but very sensitive against including simulated confounders.

To summarize, since our conservative estimates never show any significant and positive effects on permanent employment status at subsequent waves, we confirmed that long-term THS

[40] We can identify whether the respondents are re-entrants to the labor market after childbirth and child-bearing. Unfortunately, however, we do not have a sufficient number of observations to obtain reliable estimates.

jobs do not offer a stepping-stone to permanent employment. While we observed a non-positive effect on the future prospect of permanent employment, we still observed some positive effects of long-term THS work on hourly wages when compared to those who otherwise had been without employment. Thus, even to those who are willing to obtain a permanent job, long-term THS work offered a quick way to make money, but did not offer better access to permanent jobs.

5. Conclusion

Despite recent and rapid changes in the Japanese legal environment for temporary help service jobs, no studies have directly identified whether these jobs benefit Japanese workers. This paper examined the empirical effects of THS jobs, by applying the ATT method to data from Japanese surveys conducted from 2008 through 2011. We considered THS work as a treatment, and compared the respondents' employment status and hourly wage in subsequent waves to those of the control groups, such as the unemployed or directly hired part-time workers. Instead of ensuring that the CIA is necessarily satisfied in our research design, we identified the treatment effect by relying on a sensitivity analysis proposed by Ichino et al. (2008): we simulate the unobservable confounders that can induce self-selection, and check the sensitivity of the baseline ATT results against including the simulated confounders in the propensity score estimations.

Our results are similar to the ones found in some previous works, such as that of Autor and Houseman (2010). First, we observe some evidence that THS jobs have a negative impact on the probability of an individual's being permanently employed in later waves, relative to directly hired part-time jobs. This effect is not always significant after the sensitivity analysis,

but we never found any significant positive effect. Second, THS workers earn significantly higher hourly wages than the unemployed do. This positive effect is mostly significant even after the sensitivity analysis, especially when we restrict our sample to those who are seeking permanent employment. Thus, this paper adds further evidence to the literature to show that a THS job is better than no job but that it does not provide any positive extra effect on future prospects for obtaining permanent employment.

It is important to note that our results are weak, in the sense that we assume that our simulated confounders represent the true unobserved factors that could, if any can, induce a selection into the treatment. Because we never know which confounder resembles a true unobservable, we cannot conclude whether the true effect would be negative or zero. However, we have narrowed down the possible empirical impact that THS jobs would have on workers' employment prospects: a long-term THS job is unlikely to provide a stepping-stone to a permanent job, since we never observed any significant and positive impact on the probability of permanent employment, even after repeatedly simulating several types of confounders. We must keep in mind that our results should be further scrutinized, and that our analysis does not provide estimates for the removal of the Act, per se. Nonetheless, we can draw the following policy implications from our results: first, dispatched work has offered a means of getting quick earnings to those who otherwise would have been unemployed, at least in the short run. Thus, immediate removal of registered dispatched work, as was proposed in the amendment plan of April 2010, may hinder the welfare of many workers, especially those who are not willing to work as permanent employees.[41] Second, dispatched work is not a stepping-stone to permanent employment. Rather, if anything, it becomes a trap of endless precariousness to those who are

[41] About 57% of THS workers were not seeking permanent jobs at wave 1 of the Temporary Worker Survey, December 2008. This paper presents the estimation results, separately for a whole sample and for a sample of those who are seeking permanent jobs.

seeking a permanent job. When taken together, these findings do not lend support to the amendment plan of removing dispatched works; rather, they suggest the need to design a fixed-term contract that directs permanent-job seekers to stepping-stone jobs.

Appendix. Short-term THS job as a treatment

As a special case of our treatment, we also examined the effect of short-term THS workers, respondents whose main employment status was "THS worker with a fixed-term contract of *less than one month*" in December 2008. Because a job of short duration indicates that workers have few opportunities to accumulate human capital, we do not expect that short-term THS jobs improve workers' future employment status. This speculation is confirmed in Appendix Tables 3 through 6. In essence, we obtained very similar results to the case of long-term THS workers as treatment: short-term THS work does offer a means of quick earnings (columns 5 through 8 of Appendix Table 6), but at the same time, significant evidence demonstrates that they do not offer stepping-stones to permanent jobs (columns 1, 3, and 4 of Appendix Table 3, and column 4 of Appendix Table 5). Note that the estimated effects of short-term THS work are not directly comparable to those of long-term THS work because the observation characteristics for the two treatment groups are different, and their control groups are not comparable between the short-term and long-term THS cases.

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Table 1. Characteristics of the Survey; Comparison to the Government Statistics

	Temporary Worker Survey (used in this paper)		*General Survey on Dispatched Workers*	*General Survey on Daily Dispatched Workers*
	THS Worker short term	THS Worker long term	THS Worker	THS Worker short term
Looking for a permanent job? (Yes =1)	0.43	0.42	0.41	0.30
Reasons to choose the current job (multiple choice)				
To earn a living	0.21	0.13		0.37
To adjust onw's own time schedule	0.53	0.26		0.48
Until obtaining a permanent job	0.25	0.23		0.25
Age				
Total	38.13	35.96		
Under 19	0.00	0.00	0.01	0.07
20～24	0.05	0.03	0.08	0.25
25～29	0.12	0.17	0.19	0.21
30～34	0.19	0.25	0.21	0.16
35～39	0.22	0.28	0.17	0.14
40～49	0.33	0.23	0.20	0.12
50～59	0.06	0.04	0.09	0.03
60 and over	0.03	0.01	0.06	0.01
Male	0.26	0.15	0.44	0.58
Education				
Junior high school	0.03	0.00	0.05	
High school	0.29	0.32	0.45	
Junior college or vocational school	0.39	0.34	0.26	
University or graduate school	0.30	0.33	0.21	
No. of observations	381	439	8339	698
conducted by	RIETI		MHLW	MHLW
survey period	Dec 2008（wave 1)		Oct 2008	Jun-Jul 2007

Note. RIETI stands for Research Institute of Economy, Trade, and Industry. RIETI is an affiliated organization of Ministry of Economy, Trade, and Industry. MHLW stands for the Japanese Ministry of Health, Labor, and Welfare.

Table 2. Summary Statistics for the Outcome Variables (Whole Sample)

	THS Worker long term T		Unemployed C1		Directly hired (Part-time) C2	
	mean	N	mean	N	mean	N
Permanently hired at						
Wave 2 (Jun. 2009)	0.040	274	0.064	109	0.048	168
Wave 3 (Dec. 2009)	0.064	235	0.101	99	0.039	152
Wave 4 (Jun. 2010)	0.065	200	0.103	78	0.051	137
Wave 5 (Dec. 2010)	0.087	172	0.120	75	0.065	123
Hourly wage (yen) at						
Wave 2 (Jun. 2009)	1064.416	256	386.993	95	879.665	163
Wave 3 (Dec. 2009)	1026.908	202	369.041	82	969.018	138
Wave 4 (Jun. 2010)	985.975	228	407.326	94	880.026	154
Wave 5 (Dec. 2010)	962.780	199	410.233	88	814.661	134

Table 3. Summary Statistics for the Pre-treatment Variables

	THS workers (*long term*)	VS. directly hired		VS. unemployed	
		Matched Controls	All Controls	Matched Controls	All Controls
Age	36.14	37.45	39.83	36.29	39.03
Age squared	1349.08	1467.33	1668.54	1393.41	1624.11
Male*married	0.04	0.02	0.02	0.10	0.10
Male	0.14	0.17	0.14	0.37	0.57
Married	0.40	0.62	0.71	0.12	0.11
No. of Children	0.28	0.62	1.02	0.15	0.20
Permanently employed right after graduation	0.75	0.74	0.71	0.61	0.63
Father graduated from high school	0.48	0.44	0.40	0.46	0.51
Father graduated from university	0.29	0.36	0.28	0.25	0.26
Graduated from junior high school/ high school	0.28	0.30	0.38	0.36	0.32
Graduated from junior college or vocational school	0.34	0.37	0.38	0.27	0.28
Graduated from the univ. or grad. school	0.38	0.33	0.24	0.37	0.39
Looking for a permanent job	0.42	0.29	0.24	0.68	0.72
No. of observations	274	87	168	59	109

Note. This table shows pre-treatment characteristics for the sample in ATT estimation for the probability of finding a permanent job at wave 2. "Matched Controls" are individuals who belong to the control sample and are used in the nearest-neighbor propensity score matching. Matching is restricted to the sample in a common support.

Table 4. Baseline ATT Estimates: Treatment is *Long-term* THS Workers.

A. Whole sample	VS. directly hired (part-time)			VS. unemployed		
	ATT	N(treated)	N(matched controls)	ATT	N(treated)	N(matched controls)
Permanently employed at						
Wave 2 (Jun. 2009)	-0.055 (0.046)	274	87	0.007 (0.105)	274	59
Wave 3 (Dec. 2009)	0.021 (0.046)	235	84	-0.094 (0.152)	235	50
Wave 4 (Jun. 2010)	-0.100 (0.048)	200	67	0.025 (0.117)	200	48
Wave 5 (Dec. 2010)	-0.198 (0.058)	172	59	0.023 (0.132)	172	38
Hourly wages at						
Wave 2 (Jun. 2009)	129.559 (86.489)	256	84	508.355 (284.222)	256	56
Wave 3 (Dec. 2009)	-50.918 (180.628)	202	69	466.685 (208.591)	202	46
Wave 4 (Jun. 2010)	155.157 (89.696)	228	85	505.321 (236.834)	228	54
Wave 5 (Dec. 2010)	109.011 (99.043)	199	73	645.478 (340.324)	199	47

Note: Analytical standard errors in parentheses.

Table 5. ATT Estimates with Simulated Confounders (Long-term THS vs. Directly Hired)

A. Whole sample	Permanently employed at				Hourly wages at			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Wave 2	Wave 3	Wave 4	Wave 5	Wave 2	Wave 3	Wave 4	Wave 5
No confounder (baseline)	-0.06	0.02	-0.10	-0.20	129.56	-50.92	155.16	109.01
	(0.05)	0.05	(0.05)	(0.06)	(86.49)	(180.63)	(89.70)	(99.04)
Confounder such as								
Looking for a permanent job	-0.12	0.01	-0.09	-0.17	142.26	-41.88	143.03	82.35
	(0.08)	(0.06)	(0.07)	(0.10)	(114.04)	(252.50)	(131.16)	(118.92)
Young	-0.06	0.02	-0.08	-0.14	123.46	-100.46	162.25	75.29
	(0.07)	(0.05)	(0.01)	(0.10)	(121.59)	(293.15)	(121.52)	(109.60)
Male	-0.07	0.03	-0.10	-0.13	117.93	9.15	153.74	106.33
	(0.06)	(0.05)	(0.07)	(0.11)	(101.48)	(189.09)	(104.00)	(106.98)
Married	-0.12	0.02	-0.09	-0.19	112.33	-100.78	151.63	30.61
	(0.09)	(0.06)	(0.08)	(0.10)	(132.76)	(305.15)	(144.77)	(121.21)
Permanently employed right after graduation	-0.07	0.03	-0.09	-0.17	117.42	10.15	164.95	93.60
	(0.06)	(0.04)	(0.07)	(0.07)	(103.51)	(235.50)	(104.44)	(105.67)
Graduated from high school	-0.08	0.03	-0.11	-0.17	109.49	-23.46	169.99	98.27
	(0.06)	(0.05)	(0.06)	(0.08)	(106.90)	(224.02)	(104.28)	(108.23)
Graduated from university or grad school	-0.08	0.03	-0.10	-0.16	118.54	-10.89	164.55	91.98
	(0.06)	(0.05)	(0.07)	(0.08)	(110.50)	(259.46)	(104.08)	(105.55)
Hyperbolic	-0.07	0.03	-0.10	-0.17	120.48	1.19	159.45	99.00
	(0.06)	(0.05)	(0.07)	(0.07)	(101.47)	(199.50)	(102.97)	(101.95)

Note: Analytical standard errors in parentheses if baseline estimates; imputed standard errors in parentheses otherwise. Each cell presents an estimate from separate estimations.

Table 6. ATT Estimates with Simulated Confounders (Long-term THS vs. Unemployed)

A. Whole sample	Permanently employed at				Hourly wages at				Hourly wages only for the employed at			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Wave 2	Wave 3	Wave 4	Wave 5	Wave 2	Wave 3	Wave 4	Wave 5	Wave 2	Wave 3	Wave 4	Wave 5
No confounder (baseline)	0.01	-0.09	0.03	0.02	508.36	466.69	505.32	645.48	308.13	690.13	217.65	140.37
	(0.11)	(0.15)	(0.12)	(0.13)	(284.22)	(208.59)	(236.83)	(340.32)	(411.30)	(213.20)	(228.28)	(381.72)
Confounder such as												
Looking for a permanent job	-0.02	-0.05	-0.04	0.01	442.45	552.36	609.07	427.42	202.65	328.60	50.69	-52.40
	(0.11)	(0.17)	(0.17)	(0.15)	(452.24)	(296.13)	(284.81)	(530.31)	(457.97)	(391.68)	(403.88)	(586.02)
Young	-0.03	-0.11	-0.10	-0.02	477.74	518.22	592.28	590.39	356.08	493.42	233.04	111.00
	(0.11)	(0.20)	(0.19)	(0.16)	(402.02)	(279.75)	(273.56)	(336.02)	(301.93)	(345.85)	(282.38)	(501.34)
Male	-0.04	-0.10	0.00	-0.04	443.94	533.38	620.88	546.46	312.18	383.61	241.76	44.33
	(0.12)	(0.20)	(0.15)	(0.18)	(450.32)	(299.10)	(283.69)	(450.76)	(286.60)	(389.87)	(311.45)	(693.47)
Married	-0.04	-0.16	-0.10	-0.06	404.90	492.20	312.66	350.44	265.56	-21.88	168.20	94.15
	(0.12)	(0.25)	(0.23)	(0.22)	(560.55)	(388.29)	(426.95)	(524.51)	(304.84)	(610.40)	(331.21)	(594.71)
Permanently employed right after graduation	-0.02	-0.08	-0.10	0.01	468.80	538.16	567.48	607.93	219.59	438.53	211.47	63.64
	(0.10)	(0.18)	(0.19)	(0.13)	(393.76)	(264.44)	(252.87)	(327.42)	(426.31)	(361.88)	(277.16)	(536.28)
Graduated from high school	-0.01	-0.07	-0.09	0.01	459.06	533.75	580.85	556.05	337.91	438.53	234.48	119.34
	(0.10)	(0.16)	(0.19)	(0.13)	(380.47)	(263.70)	(252.10)	(408.58)	(274.37)	(361.88)	(266.88)	(486.63)
Graduated from university or grad school	-0.01	-0.09	-0.09	0.00	424.06	524.64	582.30	579.12	246.52	468.01	227.60	83.65
	(0.10)	(0.18)	(0.19)	(0.15)	(366.94)	(249.53)	(249.47)	(376.26)	(397.95)	(354.61)	(264.41)	(557.66)
Hyperbolic	-0.01	-0.07	-0.05	0.00	455.76	515.68	553.51	567.68	243.21	474.46	17.55	147.92
	(0.10)	(0.16)	(0.18)	(0.14)	(369.67)	(254.55)	(268.90)	(408.68)	(392.35)	(388.40)	(364.55)	(458.16)

Note: Analytical standard errors in parentheses if baseline estimates; imputed standard errors in parentheses otherwise. Each cell presents an estimate from separate estimations.

Table 7. ATT Estimates with Simulated Confounders (Long-term THS vs. Directly Hired)

B. Only those who are looking for permanent jobs	Permanently employed at				Hourly wages at			
	(1) Wave 2	(2) Wave 3	(3) Wave 4	(4) Wave 5	(5) Wave 2	(6) Wave 3	(7) Wave 4	(8) Wave 5
No confounder (baseline)	-0.21	0.06	-0.07	-0.11	9.94	301.69	146.19	240.09
	(0.09)	(0.11)	(0.11)	(0.12)	(153.97)	(328.99)	(147.80)	(152.03)
Confounder such as								
Young	-0.20	0.06	-0.14	-0.13	-52.29	236.60	196.00	198.42
	(0.12)	(0.12)	(0.16)	(0.15)	(186.25)	(374.91)	(175.00)	(168.23)
Male	-0.15	0.06	-0.11	-0.10	-40.20	306.32	205.87	215.36
	(0.13)	(0.12)	(0.16)	(0.17)	(186.60)	(363.18)	(165.10)	(162.90)
Married	-0.19	0.05	-0.15	-0.13	-66.30	210.42	192.94	203.46
	(0.11)	(0.12)	(0.16)	(0.16)	(217.73)	(410.16)	(186.90)	(172.68)
Permanently employed right after graduation	-0.20	0.06	-0.15	-0.12	-46.70	284.19	214.28	202.85
	(0.11)	(0.11)	(0.16)	(0.16)	(180.84)	(371.09)	(170.27)	(167.08)
Graduated from high school	-0.19	0.06	-0.16	-0.15	-94.88	243.63	182.28	189.00
	(0.12)	(0.11)	(0.15)	(0.16)	(200.86)	(375.76)	(177.58)	(167.14)
Graduated from university or grad school	-0.18	0.04	-0.18	-0.12	-45.04	141.68	178.65	196.72
	(0.12)	(0.13)	(0.15)	(0.15)	(211.22)	(467.99)	(172.10)	(176.56)
Hyperbolic	-0.19	0.07	-0.14	-0.12	-57.51	248.41	167.35	183.41
	(0.12)	(0.11)	(0.15)	(0.15)	(188.25)	(374.59)	(185.21)	(167.63)

Note: Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (128:32) at wave 2, (96:25) at wave 3, (95:25) at wave 4, and (83:21) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (127:33) at wave 2, (95:27) at wave 3, (94:25) at wave 4, and (83:21) at wave 5.

Table 8. ATT Estimates with Simulated Confounders (Long-term THS vs. Unemployed)

B. Only those who are looking for permanent jobs	Permanently employed at				Hourly wages at			
	(1) Wave 2	(2) Wave 3	(3) Wave 4	(4) Wave 5	(5) Wave 2	(6) Wave 3	(7) Wave 4	(8) Wave 5
No confounder (baseline)	-0.02	>0.00	0.03	0.06	730.21	692.61	233.18	811.60
	(0.08)	(0.13)	(0.08)	(0.10)	(132.58)	(328.47)	(174.10)	(148.02)
Confounder such as								
Young	-0.03	-0.06	0.02	-0.01	645.67	623.11	283.42	764.52
	(0.09)	(0.16)	(0.12)	(0.15)	(218.40)	(305.00)	(208.85)	(191.04)
Male	-0.06	-0.11	0.06	-0.03	584.81	588.70	417.91	729.91
	(0.13)	(0.20)	(0.11)	(0.17)	(246.86)	(365.42)	(266.05)	(218.72)
Married	-0.10	-0.19	-0.03	-0.03	525.44	515.54	357.32	692.80
	(0.17)	(0.21)	(0.18)	(0.18)	(265.83)	(400.03)	(223.95)	(226.70)
Permanently employed right after graduation	-0.02	-0.05	0.03	0.01	667.05	671.77	340.88	798.52
	(0.09)	(0.15)	(0.11)	(0.13)	(200.10)	(294.30)	(219.57)	(176.53)
Graduated from high school	-0.03	-0.04	0.04	0.03	627.72	626.81	416.63	747.29
	(0.10)	(0.15)	(0.11)	(0.13)	(218.85)	(319.09)	(234.42)	(211.90)
Graduated from university or grad school	-0.02	-0.05	0.04	0.02	661.18	668.72	383.12	787.22
	(0.10)	(0.15)	(0.11)	(0.12)	(199.79)	(289.08)	(238.94)	(175.10)
Hyperbolic	-0.03	-0.05	0.03	0.01	638.25	668.43	341.28	775.12
	(0.09)	(0.14)	(0.11)	(0.13)	(218.17)	(313.73)	(220.35)	(186.02)

Note. Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (129:46) at wave 2, (97:30) at wave 3, (95:32) at wave 4, and (83:30) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (128:50) at wave 2, (96:30) at wave 3, (94:34) at wave 4, and (83:30) at wave 5.

Table 9. ATT Estimates with Simulated Confounders (Long-term THS vs. Directly Hired)

C. Only those who had never worked in THS jobs in 2002, 2004, and 2007	Permanently employed at				Hourly wages at			
	(1) Wave 2	(2) Wave 3	(3) Wave 4	(4) Wave 5	(5) Wave 2	(6) Wave 3	(7) Wave 4	(8) Wave 5
No confounder (baseline)	-0.09	0.06	-0.14	-0.17	56.30	-243.06	-50.71	63.54
	(0.07)	(0.06)	(0.08)	(0.09)	(104.12)	(170.47)	(100.29)	(125.18)
Confounder such as								
Looking for a permanent job	-0.07	0.04	-0.11	-0.16	146.89	-169.10	-19.52	70.37
	(0.09)	(0.08)	(0.10)	(0.11)	(137.18)	(300.50)	(195.22)	(160.32)
Young	-0.02	0.05	-0.08	-0.19	125.31	-154.97	-14.01	62.66
	(0.09)	(0.08)	(0.09)	(0.12)	(150.14)	(286.79)	(177.52)	(160.95)
Male	-0.06	0.06	-0.06	-0.13	138.88	-242.65	-51.05	66.26
	(0.08)	(0.06)	(0.07)	(0.11)	(132.83)	(218.54)	(146.14)	(148.93)
Married	-0.08	0.04	-0.11	-0.19	116.33	-198.12	-39.73	29.83
	(0.10)	(0.08)	(0.11)	(0.13)	(154.73)	(311.82)	(205.16)	(161.30)
Permanently employed right after graduation	-0.05	0.07	-0.06	-0.12	161.35	-234.47	-39.97	80.84
	(0.08)	(0.06)	(0.07)	(0.10)	(124.79)	(211.78)	(141.77)	(144.82)
Graduated from high school	-0.06	0.06	-0.05	-0.12	136.81	-215.00	-47.84	78.84
	(0.08)	(0.06)	(0.07)	(0.10)	(135.25)	(203.68)	(149.13)	(146.90)
Graduated from university or grad school	-0.04	0.06	-0.07	-0.12	121.12	-219.18	-49.99	80.17
	(0.09)	(0.07)	(0.08)	(0.11)	(146.17)	(254.79)	(170.07)	(147.52)
Hyperbolic	-0.04	0.07	-0.05	-0.11	153.71	-202.37	-52.56	87.98
	(0.08)	(0.06)	(0.07)	(0.10)	(124.64)	(205.04)	(135.97)	(145.31)

Note: Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (87:43) at wave 2, (83:43) at wave 3, (76:35) at wave 4, and (70:39) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (87:39) at wave 2, (82:42) at wave 3, (75:43) at wave 4, and (70:39) at wave 5.

Table 10. ATT Estimates with Simulated Confounders (Long-term THS vs. Unemployed)

C. Only those who had never worked in THS jobs in 2002, 2004, and 2007	Permanently employed at				Hourly wages at			
	(1) Wave 2	(2) Wave 3	(3) Wave 4	(4) Wave 5	(5) Wave 2	(6) Wave 3	(7) Wave 4	(8) Wave 5
No confounder (baseline)	0.06	0.02	0.05	0.01	359.75	695.58	-486.71	-950.42
	(0.10)	(0.14)	(0.07)	(0.10)	(249.55)	(361.73)	(265.71)	(353.53)
Confounder such as								
Looking for a permanent job	0.01	-0.01	-0.01	-0.01	336.16	485.18	328.13	-16.83
	(0.12)	(0.18)	(0.15)	(0.15)	(415.21)	(374.10)	(441.06)	(798.77)
Young	0.00	0.00	0.00	-0.04	339.51	573.09	68.22	-478.15
	(0.12)	(0.17)	(0.13)	(0.17)	(369.10)	(356.82)	(537.50)	(790.75)
Male	0.01	-0.03	-0.01	-0.03	310.05	621.12	279.04	59.83
	(0.13)	(0.18)	(0.16)	(0.16)	(468.36)	(375.70)	(504.74)	(793.84)
Married	-0.01	-0.15	-0.11	-0.04	350.34	579.67	102.47	262.15
	(0.13)	(0.25)	(0.21)	(0.18)	(497.19)	(406.74)	(441.42)	(645.75)
Permanently employed right after graduation	0.02	0.02	0.01	0.00	321.69	689.13	81.85	-678.01
	(0.11)	(0.14)	(0.12)	(0.12)	(347.46)	(332.86)	(588.11)	(717.15)
Graduated from high school	0.02	0.03	0.00	0.00	319.17	682.87	-37.80	-509.76
	(0.11)	(0.14)	(0.13)	(0.12)	(364.67)	(341.09)	(579.66)	(819.92)
Graduated from university or grad school	0.01	0.02	-0.01	0.00	328.63	697.92	-66.07	-486.30
	(0.12)	(0.14)	(0.15)	(0.12)	(353.63)	(331.73)	(590.36)	(812.99)
Hyperbolic	0.02	0.03	0.01	-0.01	316.50	713.35	33.15	-656.55
	(0.11)	(0.14)	(0.12)	(0.13)	(362.78)	(313.99)	(587.98)	(735.25)

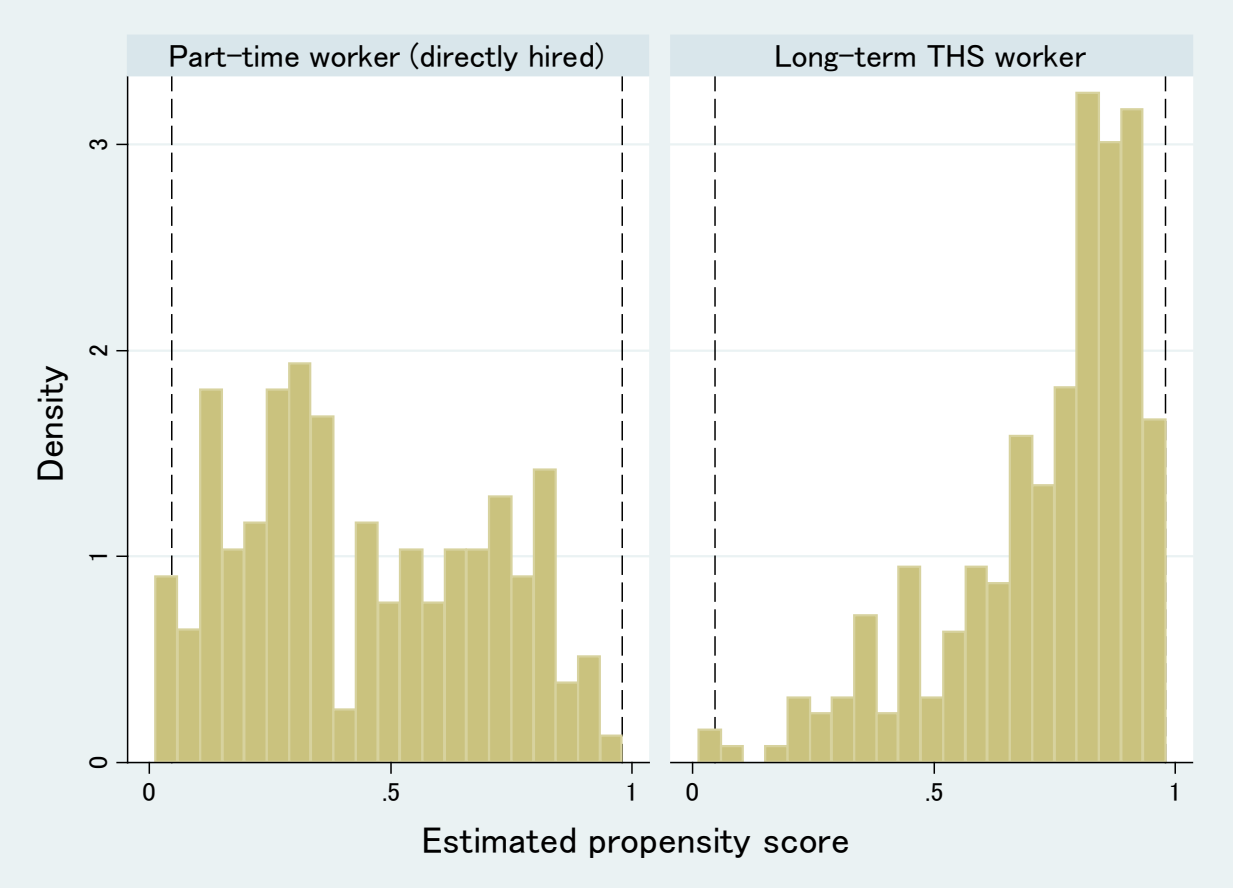
Note. Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (87:32) at wave 2, (96:32) at wave 3, (76:31) at wave 4, and (79:33) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (100:39) at wave 2, (95:32) at wave 3, (75:43) at wave 4, and

Appendix Table 1. Estimated coefficients in the propensity score estimation

Dependent variable: dummy variable which takes one if long-term THS worker, zero if directly-hired part time worker	
Age	0.5083
	(0.1313)
Age squared	-0.0069
	(0.0017)
Male*Married	2.9898
	(1.1014)
Male	-1.2433
	(0.3939)
Married	-1.1701
	(0.3026)
No. of children	-0.7852
	(0.1561)
Permanently employed right after graduation	0.6729
	(0.2893)
Father graduated from high school	0.3917
	(0.2971)
Father graduated from university	0.1024
	(0.3337)
Graduated from junior college or vocational school	-0.0591
	(0.2951)
Graduated from the univ. or grad. school	0.6054
	(0.3126)
Looking for a permanent job	0.5528
	(0.2670)
Constant	-8.3103
	(2.4926)
Log likelihood	-223.53
McFadden's Adjusted R-squared	0.194
No. of observations	442

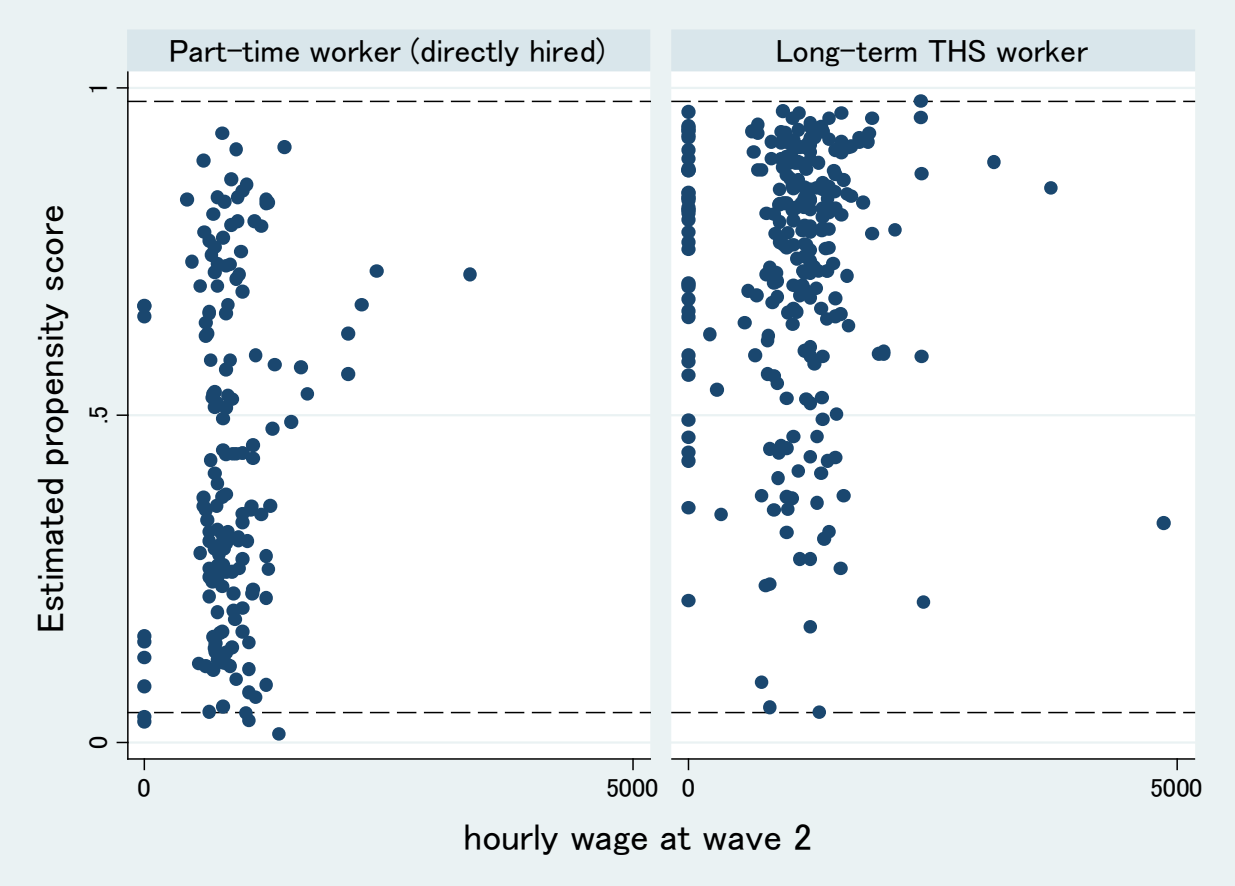
Note: Standard errors in parentheses.
This table shows the results from the propensity score estimation for the ATT estimation of the probability of finding a permanent job at wave 2
Propensity score is estimated by logit model.

Appendix Figure 1. Histograms for propensity scores by treatment status



Note : Propensity scores are calculated from the results in Appendix Table 1.
Dashed lines indicate the region of common support [0.04586273, 0.97978].

Appendix Figure 2. Propensity score and hourly wage at wave 2



Note : Propensity scores are calculated from the results in Appendix Table 1.
Dashed lines indicate the region of common support [0.04586273, 0.97978].

Appendix Table 2. Simulating Confounders (vs. Directly Hired Part-time Workers)

Simulated ATT on the probablility that a worker is permanently hired at Wave 5

	Fraction U=1 by treatment/outcome						ATT	SE	%baseline
	p_{11}	p_{10}	p_{01}	p_{00}	$p_{1.}$	$p_{0.}$			
No confounder (baseline)	0.00	0.00	0.00	0.00			-0.20	0.06	100
Confounder such as									
Looking for a permanent job	0.60	0.43	0.63	0.17	0.44	0.20	-0.17	0.10	85
Young	0.60	0.54	0.50	0.32	0.54	0.33	-0.14	0.10	73
Male	0.07	0.18	0.63	0.11	0.17	0.15	-0.13	0.11	63
Maried	0.20	0.40	0.25	0.76	0.38	0.72	-0.19	0.10	93
Permanently employed right after graduation	0.67	0.74	0.75	0.72	0.73	0.72	-0.17	0.07	85
Graduated from high school	0.27	0.32	0.13	0.42	0.32	0.40	-0.17	0.08	85
Graduated from university or grad school	0.40	0.32	0.50	0.27	0.33	0.28	-0.16	0.08	82
Hyperbolic	0.33	0.31	0.38	0.27	0.31	0.28	-0.17	0.07	85

Note : ATT estimates with simulated confounders are replicated from column (4) of Table 5.

Appendix Table 3. ATT Estimates with Simulated Confounders (Short-term THS vs. Directly Hired)

A. Whole sample	Permanently employed at				Hourly wages at			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Wave 2	Wave 3	Wave 4	Wave 5	Wave 2	Wave 3	Wave 4	Wave 5
No confounder (baseline)	-0.06	0.01	-0.13	-0.13	27.60	-561.41	-118.80	-64.76
	(0.04)	(0.04)	(0.06)	(0.07)	(82.69)	(152.96)	(113.15)	(99.05)
Confounder such as								
Looking for a permanent job	-0.11	-0.02	-0.12	-0.14	27.63	-199.38	-79.19	-23.40
	(0.07)	(0.07)	(0.07)	(0.10)	(110.63)	(269.52)	(156.72)	(128.10)
Young	-0.08	0.01	-0.10	-0.11	13.00	-460.84	-113.29	-56.65
	(0.05)	(0.05)	(0.07)	(0.08)	(97.07)	(245.35)	(124.13)	(103.92)
Male	-0.08	0.01	-0.11	-0.13	7.52	-478.47	-134.12	-68.37
	(0.06)	(0.05)	(0.07)	(0.08)	(100.39)	(253.86)	(146.13)	(109.95)
Married	-0.12	0.00	-0.11	-0.14	-9.09	-281.05	-93.16	-74.44
	(0.07)	(0.06)	(0.07)	(0.09)	(127.76)	(274.71)	(155.27)	(125.05)
Permanently employed right after graduation	-0.07	-0.01	-0.11	-0.12	19.57	-468.75	-127.32	-63.77
	(0.05)	(0.05)	(0.06)	(0.08)	(92.63)	(255.15)	(117.91)	(107.29)
Graduated from high school	-0.08	0.01	-0.10	-0.12	5.52	-464.41	-124.61	-47.10
	(0.05)	(0.05)	(0.06)	(0.08)	(101.97)	(253.39)	(138.23)	(113.72)
Graduated from university or grad school	-0.08	0.01	-0.09	-0.11	10.12	-497.16	-119.80	-58.66
	(0.05)	(0.05)	(0.07)	(0.07)	(99.19)	(260.05)	(139.28)	(107.59)
Hyperbolic	-0.07	0.01	-0.10	-0.08	21.31	-509.40	-139.92	-53.91
	(0.05)	(0.05)	(0.07)	(0.08)	(92.39)	(234.12)	(127.44)	(108.00)

Note: Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (239:89) at wave 2, (182:66) at wave 3, (183:60) at wave 4, and (150:57) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (235:84) at wave 2, (182:75) at wave 3, (180:68) at wave 4, and (148:59) at wave 5.

Appendix Table 4. ATT Estimates with Simulated Confounders (Short-term THS vs. Unemployed)

A. Whole sample	Permanently employed at				Hourly wages at			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Wave 2	Wave 3	Wave 4	Wave 5	Wave 2	Wave 3	Wave 4	Wave 5
No confounder (baseline)	-0.07	-0.05	-0.38	0.03	318.53	411.86	19.45	639.59
	(0.10)	(0.12)	(0.11)	(0.11)	(338.74)	(295.96)	(212.69)	(204.80)
Confounder such as								
Looking for a permanent job	-0.03	-0.14	-0.12	0.00	385.71	427.83	253.06	471.04
	(0.10)	(0.20)	(0.19)	(0.13)	(350.16)	(313.28)	(311.52)	(327.73)
Young	-0.04	-0.13	-0.28	0.01	402.67	451.98	91.43	573.63
	(0.10)	(0.19)	(0.20)	(0.13)	(332.09)	(279.09)	(281.79)	(276.20)
Male	-0.05	-0.24	-0.11	-0.02	423.31	484.02	250.85	486.27
	(0.10)	(0.21)	(0.19)	(0.16)	(342.47)	(265.47)	(307.62)	(299.54)
Married	-0.06	-0.27	-0.21	-0.04	361.36	415.26	149.67	423.29
	(0.11)	(0.20)	(0.20)	(0.18)	(474.87)	(336.36)	(282.71)	(342.03)
Permanently employed right after graduation	-0.03	-0.15	-0.32	0.01	361.36	431.60	204.72	569.57
	(0.10)	(0.20)	(0.18)	(0.13)	(414.96)	(293.69)	(325.76)	(268.91)
Graduated from high school	-0.04	-0.12	-0.32	0.01	435.44	446.70	48.91	574.48
	(0.10)	(0.19)	(0.18)	0.13	(319.83)	(271.06)	(255.62)	(265.07)
Graduated from university or grad school	-0.04	-0.16	-0.25	0.00	448.16	463.74	158.27	568.71
	(0.10)	(0.20)	(0.21)	(0.13)	(325.69)	(282.20)	(319.29)	(288.72)
Hyperbolic	-0.04	-0.12	-0.26	0.01	406.13	437.82	135.77	511.93
	(0.10)	(0.19)	(0.21)	(0.13)	(324.04)	(282.93)	(306.32)	(323.30)

Note: Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (239:69) at wave 2, (182:55) at wave 3, (183:53) at wave 4, and (154:48) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (235:60) at wave 2, (182:51) at wave 3, (180:46) at wave 4, and (152:46) at wave 5.

Appendix Table 5. ATT Estimates with Simulated Confounders (Short-term THS vs. Directly Hired)

B. Only those who are looking for permanent jobs	Permanently employed at				Hourly wages at			
	(1) Wave 2	(2) Wave 3	(3) Wave 4	(4) Wave 5	(5) Wave 2	(6) Wave 3	(7) Wave 4	(8) Wave 5
No confounder (baseline)	-0.09	-0.04	-0.13	-0.25	160.73	60.13	-16.63	-105.42
	(0.09)	(0.08)	(0.12)	(0.13)	(146.05)	(322.04)	(152.11)	(138.34)
Confounder such as								
Young	-0.11	-0.01	-0.19	-0.28	42.57	74.61	-0.10	-77.02
	(0.11)	(0.10)	(0.15)	(0.16)	(198.89)	(362.17)	(181.23)	(170.59)
Male	-0.14	-0.02	-0.19	-0.30	-9.06	13.13	-35.01	-78.58
	(0.12)	(0.11)	(0.15)	(0.17)	(212.87)	(375.19)	(183.76)	(168.24)
Married	-0.14	-0.04	-0.17	-0.28	47.10	23.43	-23.30	-106.63
	(0.12)	(0.14)	(0.16)	(0.19)	(205.25)	(427.72)	(196.74)	(184.71)
Permanently employed right after graduation	-0.10	-0.01	-0.17	-0.26	61.10	59.12	8.65	-75.97
	(0.10)	(0.10)	(0.15)	(0.16)	(190.12)	(338.28)	(179.28)	(168.78)
Graduated from high school	-0.14	-0.03	-0.21	-0.25	27.34	47.86	-46.48	-95.42
	(0.12)	(0.11)	(0.15)	(0.17)	(203.44)	(367.00)	(191.91)	(175.19)
Graduated from university or grad school	-0.16	-0.01	-0.19	-0.28	-2.37	44.69	-19.46	-83.42
	(0.12)	(0.10)	(0.15)	(0.17)	(220.06)	(366.90)	(181.34)	(165.85)
Hyperbolic	-0.11	0.00	-0.18	-0.27	50.48	67.86	-51.41	-78.39
	(0.10)	(0.09)	(0.15)	(0.18)	(193.42)	(338.55)	(189.65)	(171.87)

Note: Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (118:34) at wave 2, (90:23) at wave 3, (93:28) at wave 4, and (79:23) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (112:26) at wave 2, (88:24) at wave 3, (91:30) at wave 4, and (77:22) at wave 5.

Appendix Table 6. ATT Estimates with Simulated Confounders (Short-term THS vs. Unemployed)

B. Only those who are looking for permanent jobs	Permanently employed at				Hourly wages at			
	(1) Wave 2	(2) Wave 3	(3) Wave 4	(4) Wave 5	(5) Wave 2	(6) Wave 3	(7) Wave 4	(8) Wave 5
No confounder (baseline)	-0.03	-0.18	0.02	-0.01	718.45	795.68	324.27	644.31
	(0.07)	(0.08)	(0.08)	(0.10)	(196.71)	(203.52)	(137.78)	(112.61)
Confounder such as								
Young	-0.02	-0.11	0.01	0.00	675.24	744.36	384.64	599.11
	(0.07)	(0.09)	(0.10)	(0.10)	(183.58)	(206.99)	(165.78)	(136.98)
Male	-0.04	-0.09	0.02	-0.01	624.85	757.03	406.77	579.14
	(0.08)	(0.10)	(0.10)	(0.10)	(204.63)	(201.76)	(179.05)	(165.11)
Married	-0.05	-0.10	-0.04	0.00	633.57	724.87	416.26	608.60
	(0.09)	(0.10)	(0.12)	(0.11)	(206.56)	(214.54)	(170.60)	(139.33)
Permanently employed right after graduation	-0.01	-0.09	0.00	0.00	664.98	749.14	402.70	615.49
	(0.06)	(0.10)	(0.10)	(0.10)	(194.25)	(200.26)	(165.40)	(131.68)
Graduated from high school	-0.02	-0.07	0.00	-0.01	655.65	738.59	424.75	619.32
	(0.07)	(0.10)	(0.11)	(0.10)	(197.10)	(199.92)	(171.83)	(138.44)
Graduated from university or grad school	-0.02	-0.10	0.01	0.01	662.18	741.93	401.30	629.92
	(0.07)	(0.10)	(0.10)	(0.10)	(192.43)	(208.83)	(171.32)	(133.49)
Hyperbolic	-0.01	-0.07	0.00	0.00	666.61	763.01	406.18	568.26
	(0.06)	(0.10)	(0.11)	(0.11)	(187.76)	(202.46)	(165.97)	(143.29)

Note: Analytical standard errors are in parentheses. Number of observations (Treatment: Control) in the estimations of the probability of being permanently employed is (118:53) at wave 2, (90:44) at wave 3, (93:41) at wave 4, and (79:33) at wave 5. Number of observations (treatment: control) in the estimations of hourly wages is (116:53) at wave 2, (90:42) at wave 3, (91:38) at wave 4, and (77:32) at wave 5.