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Does Temporary Help Work Provide a Stepping Stone to Regular Employment?

Michael Kvasnicka

10.1 Introduction

Temporary help work has expanded rapidly across Europe over the last decade. While concerns have been raised about this trend expansion, because of the perceived inferior quality of jobs created in this submarket, growing attention is being paid to the potential longer-term effects of temporary help service (THS) employment on the labor market prospects of workers. Labor turnover in this industry is exceedingly high, and THS employment spells generally constitute but a short transitory period in the labor market histories of workers. Policymakers throughout Europe, in fact, have taken an increasingly active stance over the last years in further promoting THS work by dismantling existing national regulations on temporary help work that circumscribed the operation of temporary help agencies and the use of THS workers by client firms.

The German case constitutes a prime example of this trend. Because of the strong employment record of the THS industry in Germany and the acclaimed stepping-stone function of THS work to regular employment for the jobless, restrictive provisions of the Law on Placement Activity (LoPA),

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which governs the operation of the German THS submarket since 1972, were increasingly relaxed during the 1990s, a process that culminated in the large-scale labor market reform legislated in late 2002.[1] Among other measures, the latter initiated a near complete dismantling of hitherto existing regulations imposed on temporary help work in Germany (for details, see Burda and Kvasnicka 2006). With stubbornly high rates of unemployment putting a drain on public resources and the efficiency of the federal employment service increasingly being questioned, policymakers in Germany saw THS work as a cost-effective and complementary means to get the unemployed back into work. Apart from the deregulation of the Law on Placement Activity (LoPA), this is evinced by the large-scale creation following the 2002 reform of subsidized temporary help agencies, or personnel-service-agencies (PSA), in all of Germany's 181 employment office districts. These PSA operate as ordinary THS agencies for the sole purpose of providing unemployed workers ports of entry to the labor market and, above all, subsequent springboards to social security employment ("temp-to-perm") by way of temporary work assignments with different firms. What is surprising about these initiatives is that solid empirical evidence for the existence of such a stepping-stone function of THS work for unemployed job seekers was in fact lacking for Germany, and internationally sparse at best.

Using statistical matching techniques, this chapter investigates the validity of the stepping-stone hypothesis of THS work in Germany. We confine the empirical analysis to an investigation of the stepping-stone function of THS employment for unemployed job-seekers only. This restriction in focus is inspired by the fact that the most recent reform of the law on placement activity in Germany has been enacted largely for the acclaimed bridging function of THS work for this particular group of workers. With about every second worker entering THS work in Germany from unemployment, the population of interest chosen does, however, represent a significant share of all inflows into THS work. Conditioning the analysis on prior unemployment experience has the additional advantage of providing some sort of initial condition among the sample chosen—excluding, for instance, students and pupils who only perform vacation work in a THS agency, without seeking any steady employment. Focusing on the first unemployment spell of individuals who register as unemployed in 1994 to 1996, we are able to follow these workers for up to a minimum of five years and hence are in a position to study both the short-term and long-term effects of THS work experience on their subsequent employment trajectories. The matching approach employed in this study is based on the methodology applied by Barbara Sianesi in her studies on the short- and long-term effects of worker participation in Swedish labor market programs (Sianesi 2001, 2004).

Our results show that unemployed workers who enter THS employment

1. Details of the LoPA are provided in section 10.5.2 (subsection 2).

within twelve months of unemployment registration benefit from both higher monthly employment (THS or non-THS) and THS employment chances, as well as from significantly reduced monthly risks of unemployment throughout the four-year period these workers are followed post-entry compared to similar workers who did not join THS work at the same elapsed unemployment duration. Temporary help service workers, however, do not appear to enjoy greater chances of future non-THS employment. While our results, therefore, do not lend empirical support to the stepping-stone hypothesis of THS work for unemployed job-seekers, they neither confirm the existence of adverse effects of agency work on the future chances of workers to find employment outside agency work nor to return to unemployment. If anything, THS work seems to provide an access-to-work function for unemployed workers that leaves them with a higher probability of employment and a lower probability of unemployment for the entire four years their subsequent labor market states are followed.

In the remainder of this chapter, we will, as is commonly done, refer to social-security employment outside the THS industry as "regular employment." This is understood, at least in part, as a terminological convention. For apart from the irregular triangular setup of the THS submarket, workers in the German THS industry do in fact enjoy the same employment protection and worker rights as other workers under the provisions of general labor and social security law (Klös 2000). Temporary help service workers are regular employees of their agencies, for which the two bodies of law regulate and provide minimum standards regarding health and safety in the workplace, worktime, paid annual leave, sick pay, and periods of notice and dismissal protection more generally. Temporary help service workers are covered by the public pension and unemployment insurance system and must have health insurance. To all three of these, the agency and THS worker contribute equally. The distinction between regular and THS employment is hence not grounded in a generally inferior legal position of THS workers. In practice, of course, working conditions encountered may differ, sometimes considerably so. Until recently, wages and working conditions of most THS workers in Germany were not determined by collective bargaining to the effect that workers in agency work tended to be paid less and enjoyed less fringe benefits, such as extra holiday pay or on-the-job training, than workers employed outside agency work (a large fraction of such wage differences, however, as shown by Kvasnicka and Werwatz [2002], can be attributed to earnings-related productivity differences between agency and nonagency workers). Furthermore, employment spells in THS work often fall short of probabitionary periods granted by law in which layoffs are permitted at significantly shorter notice.

The chapter is structured as follows. Section 10.2 surveys arguments for and against the existence of a stepping-stone function of THS work, section 10.3 reviews the existing literature on the subject, and section 10.4 describes the data. Section 10.5 addresses the evaluation problem encountered in esti-

mating the stepping-stone function of THS work and proposes an appropriate framework for empirical evaluation. Section 10.6 contains the empirical results, and section 10.7 concludes.

10.2 Preliminary Considerations

A number of reasons have been cited in the literature as to why THS work may provide a bridge to regular employment for the unemployed. First, THS workers are frequently recruited among the un- or nonemployed and are hence given access to paid work or entry-level jobs (see, for example, Mangum, Mayall, and Nelson [1985]), otherwise potentially denied to them on the general labor market. Surveys of THS workers show that one of the main reasons for taking up a job in the THS industry is the inability to find a regular job (see, for example, CIETT [2000] for Europe, Cohany [1998] for the United States, or IWG [1995] for Germany). Second, unemployed workers may acquire skills and gain work experience in THS jobs that increase their productivity and hence improve their future labor market prospects (Autor, Levy, and Murnane 1999; Paoli and Merrlié 2001; Kvasnicka and Werwatz 2003), which puts to a halt the depreciation of human capital that would take place in continued unemployment. Third, the search for regular employment may be more effective on a THS job than in unemployment, as work assignments with client firms provide opportunities for workers to get to know different potential employers (Storrie 2002), and to signal their ability (Ichino, Mealli, and Nannicini 2005). Fourth, employers may, in turn, deliberately utilize temporary help work as a riskless screening device to prospect and recruit workers for permanent positions (Segal and Sullivan 1997a; Houseman 1997; Abraham and Taylor 1996; Autor 2001). As client firms are in no way contractually bound to THS workers during a work assignment, on-the-job screening is possible without subjecting oneself to any firing restrictions or direct monetary firing costs. Finally, THS workers are prescreened by the agency, both in terms of general marketable skills when recruiting the worker, and in terms of the specific requirements of client firms for a particular work assignment (Autor 2001; Burda and Kvasnicka 2006). Increased screening may in turn also lead to better and therefore more stable employment relationships, as match quality is improved by prior extensive on-the-job screening (Katz and Krueger 1999).

However, there have also been dissenting voices, pointing out that THS workers often cycle between short employment spells in the industry and extended periods of unemployment (Bronstein 1991), leading to potential labor market segmentation into low wage, less stable THS jobs with little opportunities for career advancement and highly paid permanent jobs (Mangum, Mayall, and Nelson 1985; Segal and Sullivan 1997a). In particular, THS agencies are likely to provide less formal training on the job (Ferber and Waldfogel 1998), as investment in general and therefore

marketable skills, by definition the only skills traded on this submarket, increase the risk of the worker being poached before the agency can recoup its outlays through temporary work assignments. Temporary help service employment, especially when full time, may also crowd out productive direct-hire job search. Finally, THS employment may stigmatize workers in the eyes of potential employers under incomplete information, as their inability to obtain regular work may be perceived by the latter as a signal of low productivity.

10.3 Previous Research

Lack of adequate longitudinal data on the individual employment histories of temporary help workers has tended to circumscribe empirical research on the stepping-stone function of THS work. However, a number of studies exist for different countries that have investigated the effect of agency work on the future labor market prospects of workers. These studies, as will be seen, differ markedly in their respective methodologies employed, institutional settings investigated, and populations of workers considered, a heterogeneity that makes it difficult to draw general conclusions. With the notable exception of the quasi-experimental study by Autor and Houseman (2005), however, work in this area has tended to find that THS employment improves rather than harms the subsequent labor market outcomes for workers.

While existing studies for Europe are in the majority descriptive in nature (see Storrie [2002] for a recent survey), they are exclusively so for Germany. Based on administrative data from the German federal employment service, Rudolph and Schröder (1997), for instance, calculate that a third of all THS jobs that were dissolved between 1980 and 1990 in Germany led to subsequent transitions of workers into non-THS employment within one month of job termination. Similarly, using retrospectively collected survey data on THS workers who left a major THS company in the second half of 1986, Brose, Schulze-Böing, and Mayer (1990) find that after their THS employment spell, more workers are employed outside agency work and less are unemployed than before their engagement in agency work. Lacking a comparison group of workers in their respective analyses, however, both studies do not permit any causal interpretation of their findings.[2]

Different types of flexible employment forms, such as fixed-term contract, casual, THS, or part-time employment, have also been frequently subsumed under the ambiguous catch-all term "temporary employment" to then estimate their impact on the future labor market prospects of those holding

2. The same applies to other studies that have documentated transitions out of agency work without any reference to a suitably chosen control group of workers who did not join agency work. Examples include the studies by Finegold, Levenson, and van Buren (2003), and Segal and Sullivan (1997b) for the United States.

these jobs. Marked differences in their respective contractual arrangements, employment compositions, and economic roles, however, raise the question to what extent results obtained from such analyses do in fact apply to each and every of these heterogeneous employment forms considered. For the United Kingdom, for instance, Booth, Francesconi, and Frank (2002) study the effects on subsequent employment of temporary work, which in their study includes agency and fixed-term contract work. Similarly, Zijl, Heyma, and van den Berg (2004) subsume workers on fixed-term employment contracts, in THS work, on on-call contracts, and in subsidised temporary jobs. They estimate a multistage duration model using longitudinal survey data for the Netherlands. Their findings support a stepping-stone function for these contingent employment forms, as the latter tend to shorten unemployment durations and significantly increase the future chances of workers to be in standard employment.

Positive employment effects have also been found in the majority of studies that focus exclusively on the consequences of THS employment. Using Spanish social security administrative data for 1995 to 2000, García-Pérez and Muñoz-Bullón (2005) estimate a switching regression model to control for self-selection into agency work so as to quantify the effects of THS work spells on occupational upgrading and the chances of workers to find permanent employment (defined as holding an open-ended work contract). The results show that especially high-skilled workers benefit in their subsequent chances of obtaining such employment from a work spell in THS employment. Almus et al. (1999), in turn, examined whether workers unemployed in late 1996 in the German state of Rhineland-Palatinate benefited in terms of their postagency work employment chances from working in nonprofit THS firms that received special subsidies under a program of the federal German government designed to assist the reintegration of unemployed job-seekers. Using data for three employment office districts, they find former agency workers to exhibit significantly higher chances of employment outside agency work than the control group of unemployed workers not previously working in nonprofit agency work. Ichino, Mealli, and Nannicini (2005), also applying propensity-score matching, estimate the effect of a THS work assignment with Manpower in two regions of Italy in early 2001 on the probability to find a permanent job after eighteen months. They find THS work to increase the chances of permanent employment for workers by as much as 19 percentage points in Tuscany and by 11 percentage points in Sicily, compared to baseline probabilities of matched controls of 31 and 23 percent, respectively. This estimated treatment effect, however, is barely significant for Sicily. Furthermore, it is highly heterogeneous with respect to observable characteristics such as age, education, and firm's sector. Finally, Autor and Houseman (2005) exploit the random assignment of welfare-to-work clients in 1999 to 2003 (Work First program) across several welfare service providers with substantially different placement rates at temporary

help jobs in a major metropolitan area in the U.S. state of Michigan to study the effect of holding a THS job on the labor market advancement for low-skilled workers. They find THS jobs to boost the short-term earnings of welfare clients, but to reduce their earnings and employment chances one to two years later, and to increase their welfare recidivism over this period. Temporary help service jobs, the authors conclude, appear overall no more effective than providing no job placements at all for low-skilled workers.

As this literature review illustrates, there is great heterogeneity in the methodologies used by different studies (e.g., mere descriptive statistics, statistical matching, quasi-experiments), their respective settings investigated (e.g., entire countries, regions, or subpopulations such as welfare clients), and definitions of treatment used (e.g., THS work, or contingent work more generally, such as fixed-term contract employment and agency work). While the majority of studies tends to find positive effects of THS employment on the subsequent labor market outcomes of workers, the only quasi-experimental that exists does not, which makes it hard to draw any general conclusions regarding the existence and quantitative importance of the stepping-stone function of THS work. With this study on the German THS submarket, we hope to contribute to this actively researched area.

10.4 The Data

The analysis is based on an extended version of the public-use IAB Employment Sample (IABS) of the Institute for Employment Research (IAB) at the German Federal Employment Agency, a 2 percent random sample of all employees registered in the period 1975 to 2001 by the social security system in Germany (data on East German workers is included from 1992 onward). Employment information in the IABS is based on statutory notifications of employers on their workforces to the institutions of the social security system. Containing a host of worker, firm, and job-specific attributes, and with information on unemployment periods involving benefit payments added from the federal unemployment register, the IABS provides exact daily information on the employment and unemployment trajectories of more than one million individuals in the twenty-seven years sampled. Large sample sizes and detailed flow information are indispensable for analyses of the THS industry, as the latter still accounts for only a small employment share in the German economy and is characterized by very high rates of labor turnover. As administrative data, typical problems besetting longitudinal survey data, such as panel mortality due to nonresponses, or memory gaps in retrospective questions, are not encountered in the IABS (Bender, Haas, and Klose 2000).

However, the IABS also has a number of potential shortcomings for the present analysis. First, as THS employment is identified by the industry affiliation of an employer in the IABS, THS workers cannot be differentiated

from the administrative staffing personnel of THS firms in the data set. This shortcoming, encountered also in other data sets that have been used for analyses of THS employment, such as the U.S. Current Population Survey, is, however, unlikely to be of major practical importance for our analysis, as the workforce share of staffing personnel is generally very small. Second, as the THS firm alone issues the statutory employment notifications, the IABS neither contains information on client firms nor on work assignments of THS workers (this shortcoming is also shared with all public-use administrative data sources, which by design are tailored to the standard bilateral employment relationship). Lack of information on client firms implies that we are unable to tell whether a successful transition to regular employment occurred to a former client firm or not. As a consequence, we may not directly test the relative importance of the screening and signalling hypotheses for the stepping-stone function of THS work. Our analysis, by necessity, will hence be a reduced form in kind, seeking to uncover a causal effect of THS work without explicitly analyzing its potential causal pathways (see section 10.5.1, Treatment and Nontreatment Status, for further discussion of this point). Complementary future research could fruitfully analyze these pathways and assess their respective quantitative importance for any stepping-stone function of THS work. Finally, covering only employment relationships that are subject to social security contributions, civil servants, the self-employed, and those in marginal dependent employment (until 1999) are not included in the IABS. We may therefore only investigate the employment trajectories of workers in such dependent employment.

In the next section, we discuss in detail the peculiar features of the present evaluation problem of the stepping-stone function of THS employment for the unemployed in comparison to an archetypical-administered social experiment. In doing so, we define key terms, such as treatment (THS employment) and nontreatment status (the counterfactual for the treated), as well as various outcome measures that describe workers' future employment prospects, so as to formulate testable causal questions about the stepping-stone function of THS work, subject to the restrictions imposed by the nature of the phenomenon under investigation and the data available. Sample selection issues will be addressed in the course of this discussion, so that a presentation of summary statistics on major variables recorded in the data is deferred until then.

10.5 The Evaluation Problem

10.5.1 Evaluating the Stepping-Stone Function of THS Work

The archetypical-administered social experiment is conducted only once, with a specific starting and ending date, clearly circumscribed in the nature and scope of the treatment provided therein (e.g., a particular one-time

training program), and linked to specific formal eligibility requirements for participation (e.g., a certain skill level of workers, or a minimum elapsed unemployment duration). None of these features, however, applies to the present evaluation problem of the stepping-stone function of THS employment for the unemployed. As an ongoing program, unemployed workers may join THS work both at different calendar dates and at different individual-elapsed unemployment durations. In addition, employment spells in the THS industry vary endogenously in length. Temporary help service workers may also be assigned to different numbers of client firms for different durations and for different tasks, rendering THS employment heterogeneous across workers in several respects that are endogenously determined but post-entry. An unemployed worker may furthermore hold a THS job more than once, and thus be subject to multiple treatments with interspersed repeated spells of unemployment or regular employment. One and the same worker may therefore be counted as treated at one point in time (when in THS work) and as nontreated at another. Finally, formal requirements for participation are absent. General profitable employability, a function of both individual characteristics of the unemployed job-seeker and general labor market conditions encountered, is alone decisive for temporary help agencies in the recruitment process. Likewise, unemployed workers decide on whether to seek employment in THS work, based on factors that determine job search behavior in general, such as the likelihood of finding alternative employment opportunities, reservation wages, and the like.

The definition of outcomes is equally beset with difficulties. Above all, the question to be addressed is when one should start to measure outcomes, both for those treated and for those not treated. For the former, the more obvious choice is between the start of a THS employment spell and its end, depending on how THS employment is valued relative to regular employment or the specific causal question asked. For those workers not treated, the case is even more ambiguous, as neither entry date to nor exit date from THS work are observed. These specific features inevitably require choices to be made with respect to the timing, as well as the definition of potential treatment and control groups. This we do in the remainder of this section, beginning with the units (workers) to be analyzed—that is, the sample selected.

Sample Selection

For the ensuing analysis, we select all individuals who in 1994 to 1996 register as unemployed and consider only their first unemployment spell in this period.[3] This allows us to observe the subsequent employment histories of those workers for an extended period of time in the IABS (up to a minimum total of five years). Entries into unemployment are sampled over a three-

3. An unemployment spell is defined as consecutive unemployment notifications for an individual in which the time between these notifications does not exceed one week.

year period to increase the absolute number of subsequent transitions to THS work observed in the data. The years of entry chosen have the advantage to sufficiently predate the 1997 reform of the LoPA, which, among other things, introduced a one-time exemption to the general rehiring ban in the THS industry. We further restrict this inflow sample to individuals who are between eighteen and fifty-five years of age at the time of unemployment registration. The upper age limit is imposed to reduce the likelihood of sampling older workers who may be entitled to some form of early retirement scheme that permits them to exit unemployment straight into inactivity without having to search for a job or accept job offers by the public employment service while drawing benefits. Furthermore, we exclude workers who lack some prior employment experience. This measure is imperative, given the data collection process, for important worker attributes, in particular the educational-vocational qualifications obtained, are recorded in employment notifications issued by employers, but not in the information collected in and contributed to the IABS from the federal unemployment register.

These restrictions leave us with a raw total of 106,383 workers in the sample selected who enter unemployment between 1994 and 1996. Summary statistics on major variables for this sample recorded at the time of inflow into unemployment are provided in appendix table 10A.1. As documented in table 10.1 following, 0.4 percent of these unemployment spells are right-censored at the end of 2001, and 7.1 percent end with no subsequent transition recorded within the sampling period 1994 to 2001. Of all unemployed workers, 68.1 percent enter a regular job (non-THS employment), almost eight out of ten within one month of exiting unemployment. Another 2.3 percent of the unemployed leave for a THS job, the great majority (80.8 percent) again within one month.

It is noteworthy that a much larger fraction, or 8.0 percent of all entries into unemployment at some point until December 2001, do in fact take

Table 10.1 **Transitions to THS and regular employment of unemployment inflows in 1994–1996**

Group of workers	Absolute number	Share (%)
Total entries into unemployment	106,383	100
With right-censored unemployment spells in 2001	429	0.4
With no subsequent record in sampling period	7,531	7.1
Who subsequently enter THS work		
Within $<$ 1 month of exiting unemployment	2,006	1.9
After $\geq$ 1 month of exiting unemployment	477	0.4
At some point within sampling period	8,529	8.0
Who subsequently enter regular work		
Within $<$ 1 month of exiting unemployment	59,070	55.5
After $\geq$ 1 month of exiting unemployment	13,441	12.6

Source: IABS.

up a job in the THS sector. With close to one in ten unemployed workers joining THS work over this period, THS employment appears to be more dispersed in the working population than its still small employment share in the economy suggests. In addition, but not shown in table 10.1, 85.5 percent of all direct entries into THS work (those who enter within one month of exiting from unemployment), do eventually find regular employment within the sampling period. The latter statistic is especially important in the present context, for a high "frequency of transitions from temporary to permanent employment suggests that the size of any permanent 'underclass' of temporary workers must be small" (Segal and Sullivan 1997a, 123). No subsequent transitions out of THS work are observed for only 2.2 percent of direct entries into THS employment. Moreover, only four out of ten of these 2.2 percent are accounted for by right-censored THS employment spells at the end of the sampling period. The median duration of THS job spells is four months (124 days). Sixty percent of them last less than half a year, 79 percent less than one year, and 92 percent end within two years.

However, past work experience in the THS sector seems to affect the probability of renewed entry into THS work. As table 10.2 shows, 13.2 percent of workers with some prior THS work experience exit unemployment for a THS job, compared to only 1.7 percent of workers who never worked in the THS sector. An even larger fraction (24.0 percent) of workers who enter unemployment directly from THS work again take up a THS job within one month of exiting from unemployment, but only one in eight of these return to their previous THS agency. Thus a sizeable fraction of THS workers, at least in the short-to-medium run, indeed appears to cycle between unemployment and temporary help service work spells before eventually finding regular employment. The vast majority of THS workers, however, do not. Additional explorations, not shown in table 10.2, underscore the importance

Table 10.2 **Subsequent transitions to THS and regular employment of unemployment inflows in 1994–1996 by prior THS work experience**

	Subsequent direct transition to			
		THS work		
THS work experience prior to entry into unemployment	Regular work (%)	Any agency (%)	Same as before (%)	No subsequent transition observed in the data (%)
Anytime in the past	56.2	13.2		4.9
Entered unemployment from THS work	47.6	24.0	3.2	4.4
None	68.8	1.7		7.6

Source: IABS.

Note: A direct transition is defined as the taking up of employment within thirty days of exiting from unemployment. Deviations of row totals from 100 percent are comprised of transitions to employment occurring later than this threshold period and of workers who reenter unemployment.

of the THS submarket for labor market flows, and of past THS work experience for the likelihood of unemployed workers to enter THS employment. Workers with prior THS work experience and workers who enter unemployment directly from a THS agency, respectively, account for 5.1 percent and 1.5 percent of all entries into unemployment and for 29.0 percent and 11.5 percent of all observed subsequent direct transitions from unemployment to temporary help work. Rehirings within the THS industry, however, occur far less frequently than on the general labor market. Six percent of all workers last employed at a THS agency, in fact, return to the same agency when leaving unemployment. In contrast, 11.8 percent of workers entering unemployment from a regular job again return to the same employer when leaving unemployment. The rehiring ban imposed by the LoPA at the time is likely to be in the main accountable for this discrepancy, for a significant share of former THS workers, as we have seen, do in fact return to THS work, albeit not to the same employer. An additional reason is that THS workers are likely to accumulate less firm-specific human capital when in temporary help work, which reduces their attachment to former THS employers.

In the following, we restrict the analysis to transitions of individual workers to other labor market states (regular or non-THS employment, and THS work) that occur within one month of leaving unemployment (direct transitions). Apart from workers with some prior unemployment experience, we also retain workers in the analysis who have been employed in the THS sector—that is, "treated," before entering unemployment in 1994 to 1996, because of the scale of reentry into THS work documented previously. Exclusion of either of these two groups of workers from the analysis would likely result in above-average productivity individuals being sampled. It would also restrict the treatment effect investigated to a significantly reduced subsample of THS inflows from unemployment, which, at least from a policy perspective, does not represent the group of unemployed workers mostly concerned with in the context of the stepping-stone function of THS work. For completeness, however, we consider the case of unemployed workers with no prior work experience in THS employment in section 10.6.2, where we investigate potential heterogeneities in the treatment effects of THS work on the future regular employment chances of individuals for different subgroups of workers.

Treatment and Nontreatment Status

With respect to the definition and the timing of the treatment, we consider the first entry of workers into THS employment after having registered as unemployed in 1994 to 1996. Any subsequent treatments are therefore viewed as outcomes of the initial treatment. More specifically, we define treatment as entry into THS employment. Assuming the causal effects of THS work to set in upon entry into the sector, we disregard differences in

THS employment experience across workers (e.g., in terms of employment duration, number of work assignments), and focus on the overall or average effects of joining THS work on the subsequent labor market prospects of workers. As noted, practical restrictions imposed by the data, in particular the complete lack of information on client assignments, in part dictate this approach. However, one may argue for the definition of treatment adopted also on purely methodological grounds. For, in contrast to the archetypical program discussed previously, virtually all aspects of individual THS employment relationships formed are ultimately determined endogenously, depending on the post-entry decisions of both the temporary help agency and the THS worker. Moreover, the available, albeit limited evidence for Germany on the distribution of client assignments across THS workers and on the transitions of THS workers to regular employment suggests that the ability of THS workers to sample many potential employers during temporary work assignments and the ability of client firms to screen THS workers during such work assignments for permanent positions may not, in fact, be of primary importance for the acclaimed stepping-stone function of THS work. As the case study by Kvasnicka (2003) has shown, most THS workers have but a singular client assignment, while first evidence on the recently created PSA in Germany reveals that, in fact, less than a fourth of all transitions out of THS work into regular employment occurred to a former client firm (Jahn and Windsheimer 2004).

Outcomes, yet to be defined, are consequently measured from the month of entry into THS work for those actually receiving treatment. This raises the question as to when one should start to measure outcomes for those not observed to enter THS work. Theory suggests that unemployed workers conduct their job search sequentially, accepting or declining a particular job offer depending on the respective net payoffs associated with either decision. There is, in addition, ample evidence that THS workers in the majority prefer regular employment to holding a job in the THS industry,[4] largely because of the higher pay and superior working conditions expected to accrue in the former. Moreover, surveys reveal that unemployed workers frequently enter THS work after a period of unsuccessful search for a regular job, and because they hope to thereby improve their chances to find regular employment (see, for example, IWG [1995]; CIETT [2000]; Cohany [1998]). In other words, unemployed workers are likely to decide sequentially whether to enter THS work in a given month of unemployment. This implies that for the construction of an adequate control group for those actually observed to enter THS work in a given month of elapsed unemployment duration (u^1), only those unemployed workers should be chosen as potential controls who have

4. See, for example, Storrie (2002) for the European evidence, Finegold, Levenson, and van Buren (2003) for the United States, Hegewish (2002) for the United Kingdom, or IWG (1995) for Germany.

been unemployed for at least $u^0 \geq u^1$ and are not treated in u^1. Note that these workers may well enter THS work and therefore be treated at a later month of elapsed unemployment duration. Thus while potential controls can be treated at a later stage, treated workers may never subsequently become controls for workers who enter THS work at longer unemployment durations.

As individual months of entry into THS work (treatment) differ across workers, we adopt a relative timescale in measuring subsequent outcomes (the effects of the treatment) for the treated. For a matched control person, outcomes are measured from the observed u^1 of the treated worker. However, as u^1, that is, elapsed unemployment duration before entry into THS work, is an unobserved counterfactual for nontreated unemployed workers, it cannot be included as a regressor in the estimation of the propensity score. We nevertheless condition the construction of matches on elapsed unemployment duration, by estimating separate propensity scores for every month (u), where each estimation is based on those treated in a particular u and those not treated in the same u. This approach is equivalent to estimating a discrete hazard-rate model, where all estimated parameters are allowed to be duration specific (Sianesi 2004, 140).

Choosing potential controls from such a duration-based flow sample has an inherent advantage in the present context over the primary alternative comparison group design employed in the evaluation literature—that is, the exclusive selection of potential controls from among those workers never observed to enter the particular program investigated. For in the latter case, the construction of a comparison group is, in fact, conditioning on the future, and hence the outcomes when the program starts are not restricted to a particular period (see on this point, for example, Fredriksson and Johansson [2003]). In the current application, such restriction would, in all likelihood, introduce a downward bias in the estimated treatment effects of THS work on the future regular employment probabilities of individuals, as unemployed workers who act as controls are likely to never be observed to enter THS work simply because they have instead made a successful transition to regular employment.

We next formalize these ideas in the form of the average treatment effect on the treated (ATT) to be estimated in the present context, deferring a discussion of the different outcome measures employed in this study to the subsequent subsection.

Formal Specification of the Evaluation Problem

We formalize the evaluation problem based on Sianesi's (2001/2004) exposition in her application of statistical matching techniques to the evaluation of the effectiveness of active labor market program in Sweden. The outcomes of interest are various labor market states of individuals over time, that is, $(Y_{jt}^{(u)})_{t=u+1}^{T}$, where j denotes the type of outcome, that is, the particular labor market status considered, and $t = u + 1, \ldots, T$ are the months these

outcomes are measured post elapsed unemployment duration of at least u months. At time u, the population of interest comprises workers with elapsed unemployment duration of at least u months. Treatment assignment is denoted by $D^u = (1, 0)$, with $D^u = 1$ for unemployed workers who join THS work in u, and $D^u = 0$ for those unemployed job seekers who have elapsed unemployment duration of at least u and do not join THS work in u. We further denote the potential labor market states of an individual at time t, where $t > u$, which joins THS work in his or her u'th month of unemployment with $Y_{jt}^{1(u)}$, and with $Y_{jt}^{0(u)}$ if an individual has not joined THS work up to that month, respectively.

The average treatment effects on the treated (ATTs), Δ_{jt}^u, in the present application then correspond to the average effects of joining THS work in month u of elapsed unemployment duration ($Y_{jt}^{1(u)}$) compared to not joining THS work in that month ($Y_{jt}^{0(u)}$) for those unemployed workers who actually take up a THS job in that same month ($D^u = 1$), that is,

$$\begin{aligned}(1)\qquad \Delta_{jt}^u &\equiv E(Y_{jt}^{1(u)} - Y_{jt}^{0(u)} \mid D^u = 1) \\ &= E(Y_{jt}^{1(u)} \mid D^u = 1) - E(Y_{jt}^{0(u)} \mid D^u = 1) \text{ for } t = u + 1, \ldots, T.\end{aligned}$$

To identify the second term in equation (1), that is, the unobserved counterfactual, we have to assume stable unit treatment value and conditional independence. The conditional independence assumption (CIA) in formal terms requires that:

$$(2)\qquad Y_{jt}^{0(u)} \perp D^u \mid X = x \quad \text{for } t = u + 1, \ldots, T,$$

that is, for observably similar individuals ($X = x$) having reached the same elapsed unemployment duration (u), the distribution of potential nonparticipation outcomes ($Y_{jt}^{0(u)}$) is the same for unemployed workers entering THS work ($D^u = 1$) and unemployed workers not entering THS work ($D^{(u)} = 0$) in month u. Common support in the present context amounts to the condition that:

$$(3)\qquad 0 < \Pr(D^u = 1 \mid X) < 1.$$

In other words, conditional on elapsed unemployment duration and individual worker characteristics of interest, a potential control has to exist for each treated individual. In the empirical analysis, we set $U = 12$; that is, we investigate the effect of taking up a THS job within one year of registering as unemployed in the period 1994 to 1996. The period workers are followed while still in unemployment is restricted for two reasons. First, to have a sufficiently long period at one's disposal in which the subsequent labor market outcomes of these workers can be studied in the IABS: with data until December 2001, this restriction provides us with at least forty-eight months for each individual worker, irrespective of the particular calendar months he or she entered and exited his or her unemployment spell. Second, as

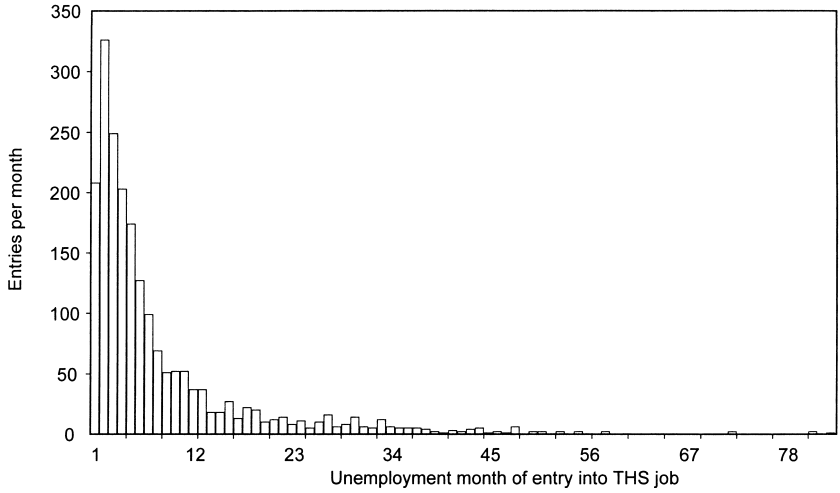

Fig. 10.1 Treated unemployed workers by month of entry into THS work
Source: IABS.
Note: Sample comprises inflows to unemployment in 1994–1996.

Table 10.3 Definition of outcome measures used in the empirical analysis

Outcomes for each month up to four years post-treatment	
Outcome 1	Monthly probability of regular employment
Outcome 2	Monthly probability of THS employment
Outcome 3	Monthly probability of employment (regular or THS)
Outcome 4	Monthly probability of unemployment

shown in figure 10.1, the total number of transitions from unemployment to THS work declines rapidly with elapsed months of unemployment duration. More than eight out of ten (82.1 percent), or 1,647 out of the 2,006 transitions to THS employment recorded in the sampling period 1994 to 2001 take place within the first year of unemployment.

Outcomes

To gain a comprehensive view of how the future labor market prospects of unemployed workers in Germany are affected by taking up a job in the THS industry, we employ a set of four different outcomes measures ($Y_{jt}^{(u)}$), described in table 10.3. These (respectively) forty-eight monthly post-treatment probabilities of regular employment (non-THS employment), THS employment, any type of employment (regular or THS), and unemployment allow us to study the dynamics of the effects that taking up THS employment exert on the individual likelihoods of observing these states over time.

It is important to note that these monthly outcome measures refer to the respective probabilities of observing workers in a particular labor market state at any point in time during a particular month. As workers may naturally spend time in more than one of these labor market states in a given month, outcomes 1, 2, and 4 are not mutually exclusive, and therefore, do not necessarily add up to 1 for a particular group of workers. Moreover, workers who return to education, or general inactivity—that is, states that are not recorded in the IABS, are retained in the respective monthly base groups from which the four outcome measures are calculated. For by virtue of the data generation process, we have complete information on the employment (regular and THS) and unemployment trajectories of workers over time who comprise our individual outcome measures of interest; that is, employment subject to social security contributions and unemployment periods that involve some kind of entitlement to financial support from the public authorities.

While Outcome 1 is of primary interest for the empirical assessment of the stepping-stone function of THS work, the remainder does provide important supportive evidence in this context. Outcome 2 provides information on the degree to which workers remain or tend to get stuck in the THS sector over time, whereas Outcome 3 conveys information on overall employment probabilities. The latter is of interest in its own right, for even if treated workers turn out not to benefit in their likelihood of obtaining regular work, or to suffer from increased risk of future unemployment (Outcome 4), they might still prove to enjoy relatively higher chances of employment in general.

10.5.2 Implementation of Propensity-Score Matching

Nearest-Neighbor Matching

We apply nearest-neighbor propensity score matching without replacement, but within caliper (Cochran and Rubin 1973).[5] In other words, conditional on elapsed unemployment duration u, each treated individual i in month u is matched to that nontreated individual z with the closest estimated propensity score $p(X)$ and used as a control C_i for individual i, subject to the condition that the absolute difference in the two estimated propensity scores, that is, the degree of residual mismatch, does not exceed a certain maximum Ψ, or caliper (see, for example, Heckman, LaLonde, and Smith [1999, 1954]):

$$C_i = z \,|\, \Psi > \min_{z \in \{1,\ldots,N^0\}} \| p_i(X) - p_z(X) \| . \tag{4}$$

In the empirical analysis, we set the caliper to $\Psi = 0.03$. From these pairs of treated and control individuals, the nearest-neighbor matching estimator

5. The matching estimator "psmatch2" by Leuven and Sianesi (2003) for STATA is used and adapted to the specific features of the present evaluation problem.

estimates the j times t ATTs (Δ^u_{jt}) for each entry month into THS work—that is, u, as the difference in mean outcomes between the treated and their matched controls:

$$\Delta^u_{jt} = \frac{1}{N^{u1}} \sum_{i=1}^{N^{u1}} (y^{1(u)}_{jt} - y^{0(u)}_{jt}), \tag{5}$$

where N^{u1} is the number of matched treated workers with completed unemployment duration u. Assuming independent observations, homoskedasticity of the outcome variables within the treatment and control groups, and nondependence of the variance of the outcome on the propensity score (Lechner 2001), the variances of the ATTs, Δ^u_{jt} can then be calculated as (see Sianesi 2001, 28):[6]

$$\mathrm{Var}(\Delta^u_{jt}) = \frac{1}{N^{u1}} \mathrm{Var}(Y^{1(u)}_{jt} \mid D^u = 1) + \frac{\sum_{z=1}^{N^{u0}} \omega_z^2}{(N^{u1})^2} \mathrm{Var}(Y^{0(u)}_{jt} \mid D^u = 0), \tag{6}$$

where $D^u = 1$ and $D^u = 0$ denote matched treated and nontreated workers at time u, respectively, and ω_z is the number of times individual z is being used as a control, with $\sum_{z=1}^{N^{u0}} \omega_z = N^{u1}$. As matching is conducted without replacement to reduce the standard errors of the estimated effects, however, $\omega_z = 1$ for all controls, so that $\sum_{z=1}^{N^{u0}} \omega_z^2 = N^{u1}$, too. As the true propensity score is unknown, its estimate has to be used, which leads to reduced estimated variances of the ATTs. Standard errors may be obtained by bootstrapping, which, however, is not pursued here for the amount of computing time required.

Estimating the Propensity Score

The plausibility of the CIA in equation (2) depends on the richness of the available data with respect to the underlying mechanism that determines treatment assignment and future outcomes—that is, the ability to control for all factors that both determine selection into THS work and affect potential outcomes in the two participation states. We discuss these factors in relation to the two principal actors involved; that is, the THS and the unemployed job-seeker, the potential restrictions imposed on their conduct by the LoPA, as well as the general labor market conditions they are confronted with.

The Temporary Help Agency As pointed out before, few formal requirements besides general "profitable employability," a function of both individual characteristics of the unemployed job-seeker and general labor market conditions, are relevant for temporary help agencies in the recruitment process. Deferring a discussion of the latter for the time being, the former

6. Note, however, that unlike Sianesi (2001), we do not have to condition on treated workers being observed at individual outcome months, as we do not have any measurement error in the labor market states of interest that underlie our outcome measures (see section 10.5.1 [subsection 4]).

necessitates the consideration of attributes related to the productivity of individuals in the estimation of the propensity score. Besides personal characteristics—that is, age, sex, foreign nationality, marital status, presence of children, as well as the highest educational and vocational attainment recorded for the worker, we control for the previous (recent and more distant) labor market history of individuals in the estimation of the propensity score. With respect to the last employment relationship, we control for employment tenure, real earnings, real average earnings in the last establishment, type of occupation held, part-time status, industrial sector, and whether the last job was a THS job. The latter in particular appeared quite significant in the descriptive explorations of observed transitions from unemployment to THS work in section 10.5.1 (sample selection). Information on the last sector the worker was employed, in turn, is likely to capture human capital and work experience that might be of use in the mainly manual, industrial tasks THS workers are usually assigned to at client firms. In addition, and by virtue of the data set, key summary statistics with respect to individuals' more distant labor market history are constructed. Attributes that are controlled for include whether the individual has ever worked in the THS sector before and whether the worker has ever been unemployed before. The latter acts as a proxy for past instability of employment, and possibly for the degree of labor market attachment of the worker, which itself may be related to unobserved individual characteristics related to worker productivity. Furthermore, a dummy for unemployment registration in the new German Lander is included, where THS agencies have only been able to operate from 1990 onward, and annual as well as seasonal indicators are used to capture cyclical and seasonal variations in the demand of THS agencies for manpower. By virtue of conditioning on elapsed unemployment durations in the estimation of the propensity score, we also implicitly control for unobservables correlated with the duration of unemployment, such as average time-invariant and time-variant differences in individual worker productivity not captured by our other covariates measured only at entry into unemployment.

The Unemployed Job-Seeker The aforementioned factors are also likely to affect the participation decision and future labor market outcomes of unemployed job-seekers at a given time. Elapsed unemployment duration is of primary importance in this context. First, surveys, as noted, regularly find unsuccessful search for a regular job to be one of the most important motives for taking up work in the THS sector, thereby lending support to the notion of sequential decision-taking on the part of unemployed job-seekers of whether to join THS work. Second, benefit entitlement levels, and thus the reservation wage, decline with elapsed unemployed duration. As remuneration in the THS sector generally falls short of levels attainable in other industries, workers with prolonged unemployment spells, and hence

a lower reservation wage, should be more likely to take up a THS job than workers who have just entered unemployment. Third, elapsed unemployment duration is likely to be correlated with individual unobserved ability, as more productive workers are, on average, more likely to exit unemployment quickly. And finally, job search activity and more generally "drive" are likely to decline with prolonged unemployment, as workers become discouraged. The latter raises the attractiveness of turning to THS agencies, who each manage a whole portfolio of potential job opportunities. Registering in the new German Lander and the local unemployment rate at entry are likely to have an effect on the employment opportunities of individual job-seekers, both in the THS sector and in other industries. We also control for the real gross daily earnings workers received at their last employer before entering into unemployment. These proxy individual worker productivity, and affect benefit entitlement levels as well as potential aspiration wages when searching for a new job in unemployment. As Kvasnicka and Werwatz (2002) have shown, relative earnings of workers who enter THS employment in Germany, on average, fall short of those of otherwise comparable workers even two to three years before actually entering temporary work. We in addition control for the type of entitlements received by a worker in a particular month of elapsed unemployment duration; that is, unemployment benefits, unemployment assistance, or unemployment support. The first is limited in duration and generally exceeds the latter two in financial terms. Eligibility for benefit entitlements is conditional on past employment, and its level depends on the last income earned.

Restrictions Imposed by the Law on Placement Activity (LoPA) Before its large-scale deregulation in 2002, the LoPA contained a number of provisions that circumscribed the terms and conditions under which agencies could employ workers and place them with client firms for temporary work assignments (a detailed discussion of the LoPA is provided in Jahn and Rudolph [2002] and Burda and Kvasnicka [2006]). The most important of these provisions limited the maximum permissible duration of work assignments with client firms, banned the use of fixed-term employment contracts between agency and worker (special ban on fixed-term contracts), prohibited agencies to confine the term of an employment contract with a THS worker to the duration of his or her first client assignment (synchronization ban), and disallowed agencies to rehire a previously laid-off worker within three months of employment termination (rehiring ban).[7] All of these restrictive provisions served the same purpose of ensuring that agencies

7. As we have seen in section 10.5.1 (subsection 1), rehirings among our unemployment inflow sample in 1994 to 1996 indeed occurred much less frequently in the THS sector than in the economy at large.

indeed assumed employer responsibilities for their workforces. They prevented agencies from simply adjusting their stock of THS workers in line with the often volatile demand for staffing services by client firms. Initially set to three months, the maximum permissible duration of work assignments was raised by quarter of a year in 1985, 1994, and 1997, and was further extended to twenty-four months in 2002 before it was dropped altogether in the latest reform, which took effect in 2004. As to the three bans, agencies were permitted a respective one-time exemption for each worker from April 1997, before they, too, were dropped altogether in 2004. In the case of the rehiring ban—that is, the only restriction imposed by the LoPA in the observation period on the conduct of THS agencies in the recruitment process, this exemption implied that agencies were henceforth allowed to once dismiss a worker and rehire her or him again with three months. By virtue of sampling only inflows into unemployment between 1994 and 1996, however, the April 1997 reform of the rehiring ban is, in fact, immaterial for subsequent transitions of workers out of their spell of unemployment.

In our empirical analysis, we cannot directly account for the rehiring ban in its effect on the recruitment behavior of THS agencies in the estimation of the propensity scores by way of a dummy variable that takes the value 1 if less than three months have elapsed since a worker has been laid off by a THS, and zero otherwise. We run separate probit regressions for each elapsed month of unemployment. As a consequence, in months of unemployment greater than three, this indicator will always take the value zero—that is, we will have no variation in the data, as the rehiring ban ceases to be binding for all workers still unemployed after three months. In the context of our matching algorithm, however, we would expect immediately preceding employment in the THS sector to have less of a positive effect on the probability to reenter THS work in the first three months of unemployment than in the fourth, if the rehiring ban does indeed exert a material influence. We do, in fact, find such evidence (see appendix table A10.2).

General Labor Market Conditions General labor market conditions influence both the search behavior and potential employment chances of unemployed job-seekers, as well as the recruitment decisions of THS agencies. Labor demand of THS firms is known to be both highly procyclical and subject to strong seasonal variations. We control for general labor market conditions along three dimensions: cyclical, seasonal, and regional. Cyclical and seasonal factors are controlled for by annual and quarterly indicator variables, measured at entry into unemployment. These indicator variables also account for differences in the inflow composition of workers into unemployment. The average annual unemployment rate in the employment office district where the worker registers as unemployed, in turn, is used to capture differences in local labor market imbalances. Finally, recorded unemploy-

ment registration in the new German Lander proxies for persistent structural differences between East and West Germany.

A note is in order on a problem that is commonly encountered in evaluation studies; that is, the presence of anticipatory effects of future treatment on the pretreatment behavior of workers and its likely pervasiveness in the current application. Anticipatory effects of unemployed job-seekers, leading to reduced job search prior to entry into THS work (akin to Ashenfelter's dip), are unlikely to be a major problem in the present evaluation problem of the stepping-stone function of THS work. Temporary help service agencies in Germany tend to hire workers predominantly on call, in line with current realizations of client demand (see Kvasnicka 2003), which is unlikely to be predictable with certainty even one or two weeks in advance. In addition, as already discussed, worker rehirings on the THS submarket at the time were prohibited within three months of prior employment termination, which effectively circumscribes the problem of anticipatory effects related to potential rehiring among workers who entered unemployment from THS work.

Matching Quality

The regression output of the probit estimations of the propensity scores for a number of treatment months are provided in appendix table 10A.2. All covariates except current entitlement status are measured at entry into unemployment. In particular, previous THS work experience and direct entry into unemployment from a THS job have a sizeable and statistically significant positive effect on the probability of transition to a THS job. Previous real earnings and the local unemployment rate, in contrast, surprisingly never exert any statistically significant effect on the likelihood of treatment assignment. The latter finding may be the product of two countervailing effects of local labor market conditions on the probability of treatment. While unemployed job-seekers may be more willing to accept a THS job, when other employment opportunities are scarce, THS agencies may only be inclined to recruit more workers when client demand for their services is high—that is, local labor market conditions are tight. Workers who spent less than one year in their last job, which proxies for past instability of employment, turn out to be at times more likely to enter THS work, potentially for the otherwise reduced chances to find employment and lack of sufficient occupation-specific skills acquired in their last job. Workers entering unemployment from training also appear to be more inclined to take up a THS job in the first months of their unemployment spells. Temporary help service work thus indeed appears to provide an access-to-work function for recent labor market entrants. It is important to keep in mind that all estimated probit regressions are conditional on treated and nontreated workers in the respective subsamples to have reached the same

elapsed duration of unemployment. As the latter is likely to be correlated both with observable and unobservable worker characteristics, the respective monthly subsamples should already be more homogeneous than the full groups of treated and nontreated workers sampled for the entire first twelve months of elapsed unemployment duration. As a consequence, the estimated coefficients of the observable attributes controlled for in the individual probit regressions measure only the impact of these covariates on the probability of treatment assignment conditional on elapsed unemployment duration. Following Sianesi (2004), table 10.4 provides various summary statistics on covariate balancing and hence matching quality for all twelve probit regressions.

Given the very large groups of potential controls available for each unemployment month (column [3]), finding a suitable match partner for treated individuals is not a problem. Only one out of the 1,647 workers leaving unemployment for a THS job, as shown in column (10), are excluded for lack of common support.[8] The pseudo-R^2 from the individual probit regressions before matching (column [4]) indicate the extent to which the covariates explain the probability of treatment in a particular month of unemployment. The respective pseudo-R^2 from monthly probit regressions after matching (column [5]) show that, on average, over the twelve probits run, the covariates continue to explain only 8 percent of the variance in treatment assignment across the matched subsamples, and thus only about half the average respective figure obtained from the original samples of treated and nontreated workers. Associated probability values of likelihood ratio tests before and after matching are reported in columns (6) and (7). Whereas before matching, the null hypothesis of joint insignificance of the covariates is always rejected for any of our twelve probit regressions, it is always accepted after matching. Matching on the estimated propensity scores leads to significant improvements in the balancing of attributes between treated and (potential) control workers in the matched subsamples for each unemployment month u, as shown by the respective median absolute standardized biases before and after matching (columns [8] and [9]).[9]

8. In the treated/control group samples, the mean absolute distance in propensity scores between treated and control (neighbor) is very small: 0.00003 (standard deviation of 0.0003; max of 0.005). In terms of percentage differences in propensity scores between treated and controls, the mean difference is 0.04 percent (standard deviation of 0.17 percent; max 4.5 percent). So overall, distributions show very close overlap.

9. The median is taken over all regressors and calculated for each unemployment month u, following Rosenbaum and Rubin (1985) as: $Bias_{Before}(X) = (\overline{X}_1 - \overline{X}_0)/\sqrt{\{[V_1(X) + V_1(X)]/2\}} \times 100$ before matching and as $Bias_{After}(X) = (\overline{X}_1^M - \overline{X}_0^M)/\sqrt{\{[V_1(X) + V_1(X)]/2\}} \times 100$ after matching, where $\overline{X}_1$ and $\overline{X}_0$ are the respective sample means in the entire subsamples of treated and nontreated workers, $V_1(X)$ and $V_1(X)$ their associated variances, and $\overline{X}_1^M$ and $\overline{X}_0^M$ the respective sample means in the group of matched treated individuals within the common support and nontreated individuals—that is, controls (see Sianesi 2004, 154)

Table 10.4 **Indicators of covariate balancing, before and after matching, by month**

Month (u) (1)	Treated workers before (2)	Nontreated workers before (3)	Probit pseudo-R^2 before (4)	Probit pseudo-R^2 after (5)	Pr > χ^2 before (6)	Pr > χ^2 after (7)	Median bias before (8)	Median bias after (9)	Treated workers lost to CS after (10)
1	208	95,265	0.113	0.029	0.000	0.991	14.3	3.9	0
2	326	80,905	0.138	0.015	0.000	0.999	16.5	3.7	0
3	249	69,270	0.124	0.017	0.000	1.000	16.3	3.6	1
4	203	59,768	0.166	0.030	0.000	0.995	18.8	2.9	0
5	174	52,997	0.159	0.023	0.000	1.000	16.4	4.5	0
6	127	47,184	0.125	0.055	0.000	0.973	14.4	6.5	0
7	99	42,280	0.153	0.065	0.000	0.986	21.7	6.5	0
8	69	38,302	0.127	0.093	0.000	0.973	21.0	6.5	0
9	51	35,008	0.149	0.177	0.000	0.805	16.6	8.1	0
10	52	31,897	0.171	0.138	0.000	0.957	16.8	8.7	0
11	52	29,250	0.152	0.123	0.000	0.975	27.1	7.2	0
12	37	25,674	0.193	0.192	0.000	0.943	28.1	11.6	0

Source: IABS.

Note: Before = before matching, after = after matching; CS = common support. (1): Elapsed month u in unemployment; (2): Number of treated (i.e., joining THS work in month u of unemployment); (3): Number of potential controls (i.e., still unemployed in month u and not joining in u); (4), (5): Pseudo-R^2 from probit regressions for the monthly conditional treatment probability; (6), (7): P-value of likelihood ratio tests for the joint significance of regressors; (8), (9): Median absolute standardized biases taken over all regressors; (10): Number of treated workers outside the common support (using a caliper of 3 percent).

10.6 Empirical Findings

10.6.1 Summarizing Outcomes over Time

We begin with a graphical summary of the average time pattern of the different treatment effects, before presenting the results for the respective Δ^u_{jt} by month of entry into THS work in section 10.6.2; that is, the causal effects identified under the CIA in equation (2). Following Sianesi (2004, 140), an average effect on each outcome measure j in outcome month t may be derived for the entire group of workers treated in their first twelve months of unemployment as:

$$(7) \quad E_U(\Delta^u_{jt} \mid D = 1) = \sum_{u=1}^{U=12} [E(Y^{1(u)}_{jt} - Y^{0(u)}_{jt} \mid D^u = 1)\, P(D^u = 1 \mid D = 1)],$$

where $E(Y^{1(u)}_{jt} - Y^{0(u)}_{jt} \mid D^u = 1) = \Delta^u_{jt}$, which are weighted in the summation by the monthly entry distribution into THS work for those actually leaving unemployment for THS work—that is, $P(D^u = 1 \mid D = 1)$. The following subsections graph estimates of these average monthly effects on our four outcome measures together with 95 percent confidence intervals calculated on the basis of equation (6) for the entire population of individuals treated in their first twelve months of unemployment. These graphs summarize how unemployed job-seekers who take up THS work, on average, fared in their subsequent employment and unemployment trajectories by joining THS work relative to the counterfactual situation in which they would have continued their job search in registered unemployment.

Outcome 1: Probability of Regular Employment

Figure 10.2 shows that entering a THS job has no statistically significant effect for most of our four-year period that outcomes are measured on the monthly probabilities of regular employment. In other words, for the majority of months post treatment, neither a stepping-stone effect of THS employment nor an adverse effect on the future probabilities of regular employment is discernable.

In the first months, however, THS work appears to reduce the relative chances of being in a regular job, a differential effect that subsequently turns slightly positive, and then seems to increase in the fourth year post treatment. The estimated reduced probabilities of regular employment are in all likelihood the result of a lock-in effect of program participation, as THS employment spells of treated workers have a median duration of four months (and a mean duration of eight-and-a-half months), as pointed out before. The positive differentials observable in the fourth year post treatment, in turn, suggest that the potential advantages in terms of regular employment chances of taking up a THS job from unemployment tend to materialize rather late than early. It remains to be seen whether these posi-

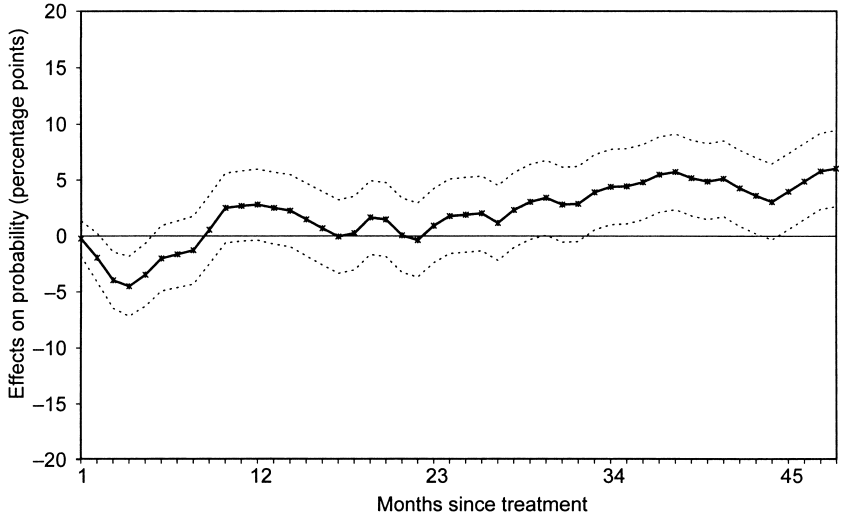

Fig. 10.2 Treatment effects over time on the probability of regular employment
Source: IABS.
Note: Dashed lines denote 95 percent confidence intervals.

tive differentials in our descriptive graphical analysis for all workers who enter within their first twelve months of unemployment registration remain when we explore the causal effects of THS work for each entry month into unemployment—that is, the treatment effects identified under the conditional independence assumption (equation [2]).

Some unemployed workers entering THS work might still be employed in their job at later outcome months, while others may also cycle between different THS jobs. While inspection of figure 10.2 provides little support for a stepping-stone function of THS work for most of the four-year post-treatment period, it does not give an answer as to whether, and if so, to what extent unemployed workers benefit in their overall future probability of employment, be it regular or temporary help work employment, from entering a THS job from unemployment. Having explored the former constituent part of this outcome measure in figure 10.2, we next turn to the latter component (Outcome 2), before considering both parts in combination—that is, Outcome 3.

Outcome 2: Probability of THS Employment

As is evident from figure 10.3, individuals who leave unemployment for THS work are significantly more likely throughout the four-year period that follows to be employed in the THS sector.

While the positive probability differential declines rapidly over the first eight to nine months, its declines become subsequently less marked and the differential roughly stabilizes at around 11 percent toward the end of

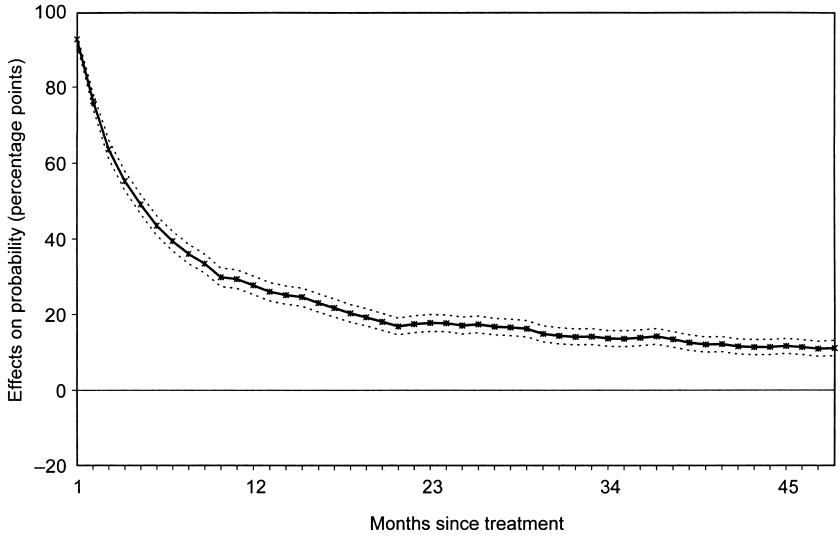

Fig. 10.3 Treatment effects over time on the probability of THS employment
Source: IABS.
Note: Dashed lines denote 95 percent confidence intervals.

the observation period. Prolonged program duration and repeated program participation may, in general, be a matter of concern if it keeps workers from obtaining regular work. This does not seem, however, to be the case in the present context. For, as we have seen in figure 10.2, unemployed workers entering THS work, on average, do not exhibit statistically significant lower monthly probabilities of regular employment than unemployed workers who chose not to join THS work as yet.

Outcome 3: Overall Probability of Employment

With respect to any social-security employment (THS or regular), figure 10.4 reveals that unemployed workers who take up a THS job exhibit a higher employment probability than those unemployed workers who do not join THS work in the same month of elapsed individual unemployment duration in each month following entry into the THS sector for the entire four-year period under investigation.

With Outcome 3 being a composite of outcomes 1 and 2, and the general time pattern of treatment effects discernable in figures 10.3 and 10.4, it is clear that the overall monthly employment probabilities quite closely resemble the levels and the trend of the increased likelihoods of THS employment for workers treated upon exit from unemployment. It remains to be seen how entry into THS work affects the risks of future unemployment over time. As noted, our outcome measures are neither mutually exclusive nor all

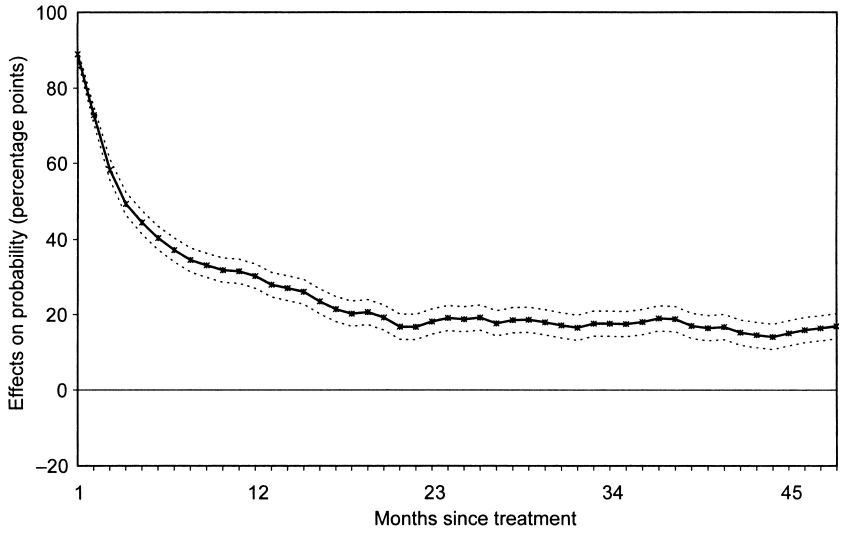

Fig. 10.4 Treatment effects over time on the probability of employment
Source: IABS.
Note: Dashed lines denote 95 percent confidence intervals.

inclusive (e.g., inactivity is not considered as an outcome), so that we cannot infer the treatment effects of THS work on a particular outcome from the treatment effects estimated for the other outcomes.

Outcome 4: Probability of Unemployment

Figure 10.5 documents that monthly probabilities of unemployment are significantly reduced for treated workers throughout the four-year observation period post-entry into THS work, but tend to converge to those experienced by workers who were not treated as of yet toward the end of the four-year observation period. Summarizing the four figures considered, it appears that unemployed workers seem to substantially improve (reduce) their overall future employment chances (risks of unemployment), while only benefiting potentially in terms of their future regular employment probabilities from their engagement in THS work toward the latter quarter of the four-year period that their subsequent labor market states are followed.

10.6.2 Treatment Effects by Month of Entry into THS Work

Having so far explored the average dynamics of the different treatment effects, table 10.5 reports the respective causal effects averaged over the forty-eight outcome months for different entry months into THS work, as well as for the entire population of workers entering THS work within their first twelve months of unemployment. The former only correspond to the causal effects identified under the CIA, equation (2), whereas the latter summarize

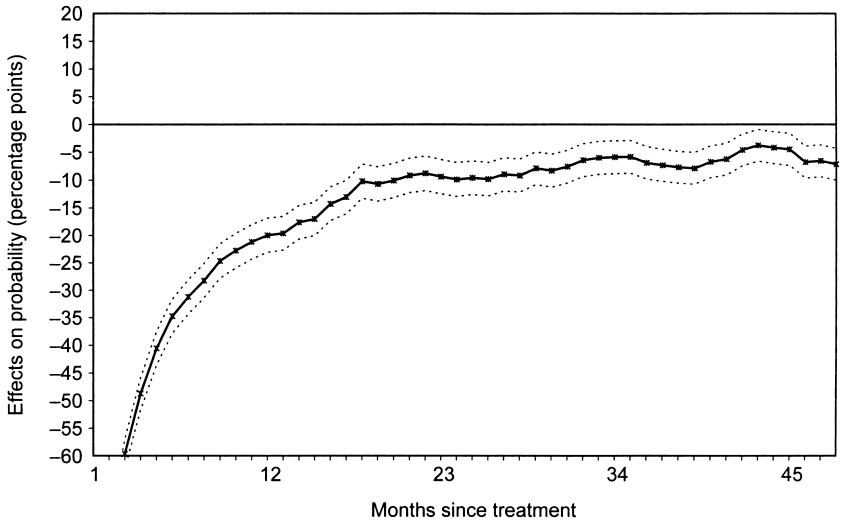

Fig. 10.5 Treatment effects over time on the probability of unemployment

Source: IABS.

Note: Dashed lines denote 95 percent confidence intervals.

Table 10.5 Average treatment effects of THS work by unemployment month of entry into THS work

	Effect (percentage points)[a]					
Outcomes	u = 1–12	u = 1	u = 3	u = 6	u = 9	u = 12
Reg. empl.	2.0	5.3	3.2	0.7	7.4	3.2
	(–1.3;5.3)	(–4.1;14.7)	(–5.3;11.8)	(–7.8;9.2)	(–11.0;25.7)	(–19.0;25.4)
THS empl.	24.0	24.7	25.1	26.6	27.8	20.1
	(21.6;26.3)	(18.1;31.4)	(19.0;31.1)	(18.4;34.8)	(13.9;41.6)	(5.9;34.4)
Any empl.	25.4	29.3	28.0	26.8	33.6	22.9
	(22.1;28.7)	(20.2;38.4)	(19.6;36.4)	(14.9;38.6)	(15.3;51.9)	(0.7;45.0)
Unemployment	–17.0	–20.6	–20.4	–12.5	–20.3	–12.6
	(–20.1;–14.0)	(–28.7;–12.5)	(–27.9;–12.8)	(–23.9;–1.2)	(–38.1;–2.5)	(–33.4;8.2)

Source: IABS.

Note: Ninety-five percent confidence intervals in parentheses; u = unemployment month of entry into THS work.

[a]Averaged over forty-eight months post-entry into THS work.

figures 10.2 to 10.5—that is, relates to the entire group of workers treated within their first year of unemployment and thus represent a benchmark against which to discuss the variations in ATTs by month of entry into THS work. Estimated baseline outcome probabilities for the respective control groups corresponding to the different groups of treated workers in table

10.5 are provided in appendix table 10A.3. As is evident, the averaged ATTs for the probability of regular employment are never significantly different from zero, whereas those for the probabilities to be employed in the THS sector, or in either THS or regular work (any employment) are always strong positively and statistically significantly affected if unemployment is left for a THS job. Future risks of unemployment, in turn, are in general significantly reduced for workers who take up a job in the THS sector. Although a marked systematic pattern by entry month into THS work is not observable for our four outcome measures, it appears that unemployed workers who join THS work very late in their unemployment spells (in the twelfth month) tend to fare worse, on average, than those workers who join earlier, with respect to both THS employment, any employment, and unemployment.

Overall, we may summarize the findings in table 10.5 to suggest that unemployed workers benefit in their overall future employment chances in the four-year observation period from entering THS work, because of the increased likelihood of future THS employment, but are neither, on average, more likely to obtain regular employment, nor to suffer from increased risks of future unemployment in the outcome period. Quite to the contrary, they appear to benefit substantially from reduced risks unemployment over the four-year post-treatment period considered. Sample sizes, however, are fairly small, which leads to large standard errors in the estimates of our ATTs obtained, as is evident from table 10.5. This is particularly a problem for our outcome measure of regular employment, for which all tabulated ATTs are insignificant, yet throughout positive. The estimated treatment effects on regular employment are, however, quite small for entries into THS work at the various months of unemployment duration considered in table 10.5 when compared to the baseline probabilities of regular employment for the respective matched control groups (see appendix table 10A.3).

The effects of THS employment on the future regular employment chances of unemployed workers, that is, our outcome of primary interest, may differ between subgroups of workers. In the following, a number of such groups are considered. Given the small sample size of unemployed workers that leave for a THS job and their rapidly declining numbers at longer-elapsed unemployment durations, however, we have to restrict the analysis to transitions to THS work that occur within the first six months of unemployment registration. Table 10.6 tabulates the causal effects for each subgroup on the probability of regular employment averaged over the forty-eight outcome months for different entry months into THS work, as well as, as a benchmark for the entire group of workers entering THS work within their first six months of unemployment, together with the estimated baseline probability of regular employment of their matched controls.

As is evident, average treatment effects over the four-year outcome period are in the majority positive, though at several instances also negative, never statistically significant for all entries months into THS work, and

Table 10.6 **Average treatment effects of THS work on probability of regular employment by unemployment month of entry into THS work for different subgroups of workers**

Groups of workers	Baseline probability[a]	Effect (percentage points)[a]			
		$u = 1$–6	$u = 1$	$u = 3$	$u = 6$
All workers (N = 1,286)	35.8	1.3 (–2.5;5.0)	5.3 (–4.1;14.7)	3.2 (–5.3;11.8)	0.7 (–7.8;9.2)
Aged 18–40 at entry into u. (N = 1,059)	36.9	2.0 (–2.1;6.2)	3.7 (–7.0;14.3)	4.1 (–5.3;13.4)	1.5 (–11.6;14.6)
No prior THS experience (N = 864)	38.9	2.3 (–2.3;6.9)	6.3 (–5.6;18.1)	1.1 (–9.1;11.4)	1.7 (–12.1;15.4)
Unempl. in W. Germany (N = 874)	34.9	3.7 (–0.8;8.2)	2.8 (–9.0;14.6)	–3.0 (–13.5;7.5)	5.2 (–8.8;19.3)
Men (N = 1,038)	37.8	–2.4 (–6.6;1.7)	1.2 (–9.5;11.9)	–4.0 (–13.5;5.5)	–1.7 (–14.1;10.8)

Source: IABS.

Note: Ninety-five percent confidence intervals are reported in parentheses; N = No. of treated in unemployment month of entry into THS work $u = 1$–6.

[a]Averaged over forty-eight months post-entry into THS work.

mostly modest in absolute value if compared to the baseline probabilities of matched controls for the entire entry period considered. It is notable that the reduction in the upper age limit of workers considered to forty years, and the sample restriction to workers without some prior THS experience do not result for each entry month into THS work in generally higher average treatment effects for regular employment probabilities than for all workers who enter THS work within the first six months of their unemployment spell. Furthermore, it appears that later entries into THS work ($u = 6$) once again appear to benefit less in their overall future regular employment chances than workers who enter THS employment earlier in their unemployment spell.

Finally, we want to look at the average treatment effects on the probability of regular employment for only the fourth year post-entry into THS work. For, as we have seen in figure 10.2, for the entire group of workers entering THS work within twelve months of unemployment registration, a positive differential in their monthly regular employment probabilities was discernable only in this fourth year of our observation period that outcomes are measured. Plotting the outcomes averaged over all entries into THS work within twelve months of their unemployment registration, this figure (like the other figures) did not have a causal interpretation, for the CIA, as noted, pertains only to treated workers and their matched controls who have identical elapsed unemployment durations at entry of the former into THS work. Considering only the averaged monthly regular employment differentials between treated and controls in the fourth year of our observation period, table 10.7 shows that neither for all workers that enter THS

Table 10.7 **Average treatment effects of THS work on probability of regular employment in the fourth year post-treatment by unemployment month of entry into THS work for different subgroups of workers**

	Effect (percentage points)[a]		
Groups of workers	$u = 1$	$u = 3$	$u = 6$
All workers (N = 1,286)	6.6	6.8	0.09
	(–3.0;16.2)	(–2.0;15.5)	(–11.0;12.9)
Aged 18 to 40 at entry into u. (N = 1,059)	7.9	8.3	–5.2
	(–2.8;18.6)	(–1.2;17.9)	(–18.9;8.5)
No prior THS experience (N = 864)	7.2	2.2	6.3
	(–4.7;19.0)	(–8.2;12.5)	(–7.9;20.5)
Unempl. in W. Germany (N = 874)	4.3	–2.3	–2.0
	(–7.6;16.2)	(–13.0;8.4)	(–16.6;12.7)
Men (N = 1,038)	3.2	0.9	–1.8
	(–7.6;14.1)	(–8.8;10.6)	(–14.9;11.4)

Source: IABS.

Note: Ninety-five percent confidence intervals are reported in parentheses; N = No. of treated in unemployment month of entry into THS work u = 1–6.

[a]Averaged over the fourth year post-entry into THS work.

work within six months of their unemployment registration, nor for any of the four subgroups already considered, are estimates statistically different from zero. Standard errors are, of course, once more very large due to the small sample sizes. Nevertheless, in the majority of cases, positive estimates are again small in magnitude, and in a quarter even negative, suggesting that workers do not significantly benefit in quantitative terms from generally higher chances of regular employment four years post-entry in THS work. Summing up the various analyses in this section, there is little evidence that suggests the existence of a general and significant stepping-stone function of THS work to regular employment for unemployed job-seekers in Germany in the time period considered.

10.7 Conclusion

Applying statistical matching techniques, this chapter has investigated the average effects of entering THS work on the future labor market outcomes over a four-year period of workers who registered as unemployed in 1994 to 1996 relative to the counterfactual, in which these workers would have continued their job search in registered unemployment.

Unemployed workers who entered THS employment within twelve months of unemployment registration turned out to benefit from both higher monthly chances of THS and overall employment (THS or regular employment) throughout the four-year period these workers were fol-

lowed post-treatment. Workers who took up a job in the THS sector also appeared to enjoy significantly reduced future risks of unemployment. They did not, however, seem to enjoy generally greater chances of future regular employment. While our results, therefore, do not lend empirical support to the stepping-stone hypothesis of THS work for unemployed job-seekers in Germany, they also do not confirm concerns about potential adverse effects on the future regular employment and unemployment probabilities of THS workers. If anything, THS work appears to provide an access-to-work function for unemployed workers that leaves them with a higher probability of (THS) employment for the entire four years their subsequent labor market states have been analyzed than workers who did not join THS work, as of yet, in their unemployment spell.

Appendix

Table 10A.1 Summary statistics of the sample at entry into unemployment

Worker characteristics		Previous real daily gross wage (€)	49.5
Female	43.2	Ø real daily gross wage at employer (€)	54.9
Foreign	9.4	Duration of last job less than one year	48.7
Age (years)	34.5	Ever before in THS work	5.1
Married	48.6	Immediately before in THS work	1.5
Kids	38.2	Unemployment spell	
Educational/vocational degree		First time unemployed	50.4
Secondary	28.9	Registered in new German Lander	32.5
Secondary with vocational	66.3	Local unemployment rate	11.7
Polytechnic or university	4.8	Registration in	
Previous employment history		1994	38.7
Sector		1995	31.5
Farming and energy	2.7	1996	29.9
Manufacturing	26.8	1st quarter	32.1
Construction	15.9	2nd quarter	20.4
Trade	13.8	3rd quarter	24.2
Transport	5.0	4th quarter	23.2
Services	28.9	Entitlements	
State	6.0	Unemployment benefits	90.3
Other	0.8	Unemployment assistance	7.6
Type of last occupation		Unemployment support	2.1
In training	9.9		
Unskilled blue collar	24.3		
Skilled blue collar	28.6		
White collar	26.7		
Part-time	10.6		

Source: IABS.

Note: Number of workers = 106,383. All entries are in percent, unless stated otherwise.

Table 10A.2 **Estimation of propensity scores by month of entry into THS work**

	$u = 1$	$u = 4$	$u = 8$	$u = 12$
Personal characteristics				
Female	–.2582 (.0658)***	–.2038 (.0680)***	–.3076 (.1121)***	–.1689 (.1542)
Foreign	.0544 (.0762)	.0477 (.0778)	.0518 (.1151)	.2354 (.1656)
Age	.0084 (.0222)	–.0369 (.0241)	–.0183 (.0374)	–.0901 (.0546)*
Age^2	–.0003 (.0003)	.0002 (.0003)	–.0000 (.0005)	.0008 (.0007)
Married	–.0218 (.0647)	.0296 (.0735)	–.0244 (.1108)	–.1095 (.1675)
Kids	–.0249 (.0660)	–.0961 (.0752)	–.1194 (.1136)	.0090 (.1665)
Education				
(ref.: secondary degree)				
Vocational degree	.2328 (.0776)***	.0690 (.0801)	–.0128 (.1237)	.2611 (.1810)
University	–.2668 (.2410)	–.4419 (.2972)		.6767 (.3159)**
Last employment spell				
Job tenure less than one year	.0896 (.0524)*	.0721 (.0588)	.2423 (.0949)**	.0987 (.1274)
Occupation:				
(ref.: unskilled blue collar)				
Training	.2229 (.1051)**	.3906 (.1024)***	.2356 (.1811)	.2981 (.2314)
Skilled blue collar	.0156 (.0737)	.0521 (.0833)	.2479 (.1283)*	.0602 (.1802)
White collar	.0664 (.0879)	.0668 (.0976)	.0752 (.1556)	–.3035 (.2312)
Part time	.0495 (.1223)	–.2019 (.1632)	–.0161 (.2066)	–.0566 (.2817)
Real gross daily income				
Of worker	–.0016 (.0010)	–.0006 (.0010)	–.0012 (.0016)	.0015 (.0018)
Average at employer	.0026 (.0015)*	.0031 (0.73)*	.0020 (.0026)	.0019 (.0034)
Sector				
(ref.: manufacturing)				
Agriculture / energy	.0854 (.1369)	.2471 (.1420)*		
Construction	–.0463 (.0738)	.0645 (.0833)	.1128 (.1289)	.1517 (.1764)
Trade	–.1020 (.0881)	–.0270 (.0953)	–.0429 (.1622)	–.0189 (.2084)

Transport	–.0595 (.1159)	–.1660 (.1510)	.1829 (.1775)	–.2767 (.3723)
Services	–.1413 (.0776)*	–.0984 (.0863)	.1042 (.1281)	–.0351 (.1794)
State	–.4917 (.2116)**	–.1126 (.1481)	–.3059 (.2996)	
Other	–.1465 (.3168)	.2503 (.2638)	.3850 (.3773)	
Previous THS work				
At some point in past	.6053 (.0776)***	.5850 (.0903)***	.4674 (.1306)***	.4801 (.1907)**
Last job was in THS sector	.6075 (.1134)***	.7466 (.1263)***	.2443 (.2019)	.3823 (.3015)
Unemployment characteristics				
First-time unemployed	.0657 (.0555)	.0055 (.0621)	–.0555 (.0973)	.0481 (.1405)
Registered in new Lander	.1188 (.0862)	.0742 (.0921)	.0479 (.1513)	–.4497 (.2256)**
Local unemployment rate	.0041 (.0099)	.0132 (.0106)	–.0107 (.0171)	.0244 (.0234)
Entitlements (ref.: benefits)				
Assistance payments	–.2045 (.0978)*	–.0884 (.0903)	–.0811 (.1125)	–.0027 (.1482)
Living supports		–.4684 (.1665)***	–.3018 (.1548)**	–.1329 (.1699)
Registration				
(ref.: 1994, 1st quarter)				
1995	.0625 (.0594)	–.0629 (.0648)	.0013 (.0982)	–.3129 (.1956)
1996	.1008 (.0591)*	–.0311 (.0634)	–.0916 (.1060)	.2421 (.1360)*
2nd quarter	.2172 (.0676)***	–.0896 (.0670)	–.1184 (.1330)	.4184 (.1806)**
3rd quarter	.2460 (.0656)***	–.2669 (.0735)***	–.0651 (.1214)	.3729 (.1798)**
4th quarter	.0658 (.0178)	–.4032 (.0796)***	.1751 (.1069)	.1331 (.2085)

Source: IABS.

Note: Standard errors in parentheses, u = unemployment month of entry into THS work.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 10A.3 **Average probabilities of different labor market states for control workers by elapsed unemployment duration of their matched treated workers**

	Average probability (percent)[a]					
Outcomes	$u = 1–12$	$u = 1$	$u = 3$	$u = 6$	$u = 9$	$u = 12$
Regular employment	34.8	37.7	36.2	31.6	30.6	37.3
	(32.6;37.0)	(31.1;44.3)	(30.3;42.1)	(24.0;39.3)	(19.1;42.0)	(24.3;50.3)
THS employment	3.9	3.8	3.5	2.0	4.0	2.2
	(3.0;4.8)	(1.2;6.4)	(1.2;5.7)	(–0.3;4.4)	(–0.9;9.0)	(–1.7;6.1)
Any employment	38.6	41.5	39.5	33.6	34.5	39.4
	(36.4;40.9)	(34.8;48.2)	(33.5;45.5)	(25.8;41.4)	(22.7;46.4)	(26.3;52.6)
Unemployment	37.6	35.6	36.9	37.9	43.1	36.8
	(35.4;39.9)	(29.1;42.1)	(30.1;42.8)	(29.9;45.9)	(30.7;55.4)	(23.8;49.8)

Source: IABS.

Note: Ninety-five percent confidence intervals are reported in parentheses; u = unemployment month of entry into THS work of corresponding matched treated.

[a]Averaged over forty-eight months post entry into THS work of corresponding matched treated.

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