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SFB 649 Discussion Paper 2005-031

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

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

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Abstract

Based on administrative data from the federal employment office in Germany, we apply matching techniques to estimate the stepping-stone function of temporary agency work for the unemployed, i.e. its short-run and long-run effects on their future employment prospects. Our results show that unemployed workers who take up a job in the temporary work agency (TWA) industry are on average more likely than unemployed workers not joining TWA work to be in agency employment in the four year period these workers are tracked after entering TWA work. However, we find no discernable effects on the probabilities of being either in regular employment or registered unemployment. Our findings therefore do not lend support to the stepping-stone function of temporary agency work.

Keywords: Temporary work agencies, stepping stone, evaluation, matching.
JEL Classification: C14, C41, J41, J64

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1 Introduction

Temporary agency 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 on this submarket, growing attention is being paid to the potential longer-term effects of temporary work agency (TWA) employment on the labor market prospects of workers. For labor turnover in this industry is exceedingly high, and TWA employment spells generally constitute but a short transitory period in the labor market histories of workers.

Because of its strong employment record and acclaimed stepping-stone function as a route for the jobless to reenter regular employment, policy makers across Europe increasingly take an active stance in further promoting TWA work. The latter motive proved particularly attractive in Germany. With stubbornly high rates of unemployment putting a drain on public resources and the efficiency of the federal employment service increasingly being questioned, policy makers in Germany discovered TWA work as a cost-effective complementary means to get the unemployed back into work. Deregulation of the TWA industry became a central pillar of the large-scale labor market reform package passed in Germany in late 2002. Furthermore, this latest reform of the Law on Placement Activity (LoPA) also sought to actively exploit the presumed stepping-stone function of TWA work through the large-scale creation by public tender of subsidized temporary work agencies, or personnel-service-agencies (PSA), in all of Germany's 181 employment office districts. These PSA operate as ordinary TWAs 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.

Empirical evidence on the stepping-stone function of TWA work, however, hardly exists (Storrie, 2002). By investigating the extent to which TWA work indeed facilitates transitions of previously unemployed workers into regular jobs in Germany, this paper therefore also provides a test of whether the latest reform of the LoPA in Germany has been enacted on false premises.

We confine the empirical analysis to an investigation of the stepping-stone function of TWA 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 TWA work for this particular group of workers. With about every second worker entering TWA work in Germany from unemployment, the population of interest chosen does, however, represent a significant share of all inflows into TWA 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 TWA, without seeking any steady employment. In the remainder of this

paper, we will, as is commonly done, refer to social-security employment outside the TWA industry as 'regular employment'. This is understood as but a terminological convention. For apart from the irregular triangular setup of the TWA submarket, workers in the German TWA 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). 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 5 years and thus are in a position to study both the short-term and long-term effects of TWA employment experience on their subsequent employment trajectories, rather than having to evaluate the effects of interest at a single, and often arbitrary point in time, as is often the case in the evaluation literature because of data constraints. The matching approach employed in this study is based on the methodology developed and applied by Barbara Sianesi in her studies on the short- and long-term effects of worker participation in Swedish labor market programmes (Sianesi, 2001/2004).

Our results show that unemployed workers who enter TWA employment within twelve months of unemployment registration benefit from both higher monthly employment (TWA or regular) and TWA employment probabilities throughout the four year period these workers are followed post treatment than unemployed workers not joining TWA work. TWA workers, however, do not enjoy greater chances of future regular employment, nor do they suffer from greater risks of unemployment. While our results, therefore, do not lend empirical support to the stepping-stone hypothesis of TWA work for unemployed job-seekers, they do neither confirm the existence of adverse effects of agency work on the future regular employment and unemployment probabilities of workers. If anything, TWA work seems to provide an access-to-work function for unemployed workers that leaves them with a higher probability of employment for the entire four years their subsequent labor market states are followed than comparable workers who do not join TWA work as of yet in their unemployment spell.

The paper is structured as follows. Section 2 surveys arguments for and against the existence of a stepping-stone function of TWA work, Section 3 reviews the existing literature on the subject, and Section 4 describes the data. Section 5 addresses the evaluation problem encountered in estimating the stepping-stone function of TWA work and proposes an appropriate framework for empirical evaluation. Section 6 contains the empirical results, and Section 7 concludes.

2 Preliminary Considerations

A number of reasons have been cited in the literature, why TWA work may provide a bridge to regular employment for the unemployed. First, TWA workers are frequently recruited among the

un- or nonemployed and are thus given access to paid work or entry-level jobs (see, for example, Mangum, Mayall, and Nelson, 1985), otherwise often denied to them on the general labor market. Surveys among TWA workers show that one of the main reasons for taking up a job in the TWA industry is the inability to find a regular job (see, for example, CIETT (2000) for Europe, Cohany (1998) for the US, or IWG (1995) for Germany). Second, the unemployed may acquire skills and gain work experience in TWA jobs that increase their productivity and thus improve their future labor market prospects (Autor, Levy, and Murnane, 1999; Paoli and Merri , 2001; Kvasnicka and Werwatz, 2003), which puts to a halt the depreciation of human capital otherwise the case in continued unemployment. Third, the search for regular employment may be more effective on a TWA job than in unemployment, as work assignments provide opportunities for workers to get to know different potential employers (Storrie, 2002), and to signal their ability (Ichino, Mealli, and Nannicini, 2004). Fourth, employers may, in turn, utilize temporary agency 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 TWA workers during a work assignment, on-the job screening is possible without subjecting oneself to any firing restrictions or direct firing costs. Finally, TWA workers are pre-screened 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, 2004). 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 TWA 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 TWA jobs with little opportunities for career advancement and highly paid permanent jobs (Mangum, Mayall, and Nelson, 1985; Segal and Sullivan, 1997a). In particular, TWAs 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. Finally, TWA employment may also 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.

3 Previous Research

Lack of longitudinal data on individual employment histories of temporary agency workers has largely precluded empirical research on the stepping-stone function of TWA work. Existing studies are mostly descriptive (see Storrie (2002) for a recent European survey; Rudolph and Schröder (1997), or Brose, Schulze-Böing, and Meyer (1990) for Germany), and as such do not permit any causal interpretation. Others fail to control for potential self-selection of workers into TWA employment by restricting the analysis to posterior transitions of an initial stock sample of workers already employed in the TWA industry (e.g. Finegold, Levenson, and van Buren (2003) and Segal and Sullivan (1997b) for the US, García-Pérez and Muñoz-Bullón (2003), Malo and Muñoz-Bullón (2002), and Amuedo-Dorantes, Malo, and Muñoz-Bullón (2004) for Spain, or Fridén, Hedén, and Wadensjö (2000) and Andersson and Wadensjö (2004) for Sweden), or suffer from data defects, in particular the ability to identify TWA workers across time (e.g. Segal and Sullivan (1997a) for the US), that raise questions about the robustness of the results obtained. Case studies based on data collected at a single TWA or specific geographic regions sometimes do find positive effects of TWA work (e.g. Almus, Engeln, Lechner, Pfeiffer, and Spengler (1999) for non-profit TWAs in Germany, or Ichino, Mealli, and Nannicini (2004) for Italy), and sometimes don't (e.g. Autor and Houseman (2002) for the US), which makes it hard to draw any generalizable conclusions.

For lack of alternative data, different types of flexible employment forms, such as fixed-term contract, casual, TWA 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 these jobs (see, for example, Booth, Francesconi, and Frank (2002) for the UK, or Zijl, Heyma, and van den Berg (2004) for the Netherlands). Differences in the respective contractual arrangements, employment compositions, and economic roles performed by these flexible work arrangements, however, make it all but clear, in how far results obtained in this vein do in fact apply to each single heterogeneous employment form. As this literature review illustrates, there is still a lack of solid evidence on whether or not temporary agency work provides a gateway to regular employment. With this study, we want to contribute to this insufficiently researched area.

4 The Data

The analysis is based on the IAB Employment Sample (IABES2) of the Institute for Employment Research (IAB), a 2% random sample of all employees registered by the social security system in Germany in the period 1975 to 2001. Data on East German workers is available from 1992 onwards. Employment information is based on statutory notifications of employers on their work-

force 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 IABES2 provides exact daily information on the employment and unemployment trajectories of more than one million individuals in the 27 years sampled. Large sample sizes and accurate flow information are indispensable for analyses of the TWA industry, because the latter still accounts for only a small employment share in the economy and is characterized by high rates of labor turnover. Moreover, typical problems besetting longitudinal survey data, such as panel mortality due to non-responses, or memory gaps in retrospective questions are not encountered in the IABES2 (Bender, Haas, and Klose, 2000), which will prove very useful in the current application.

However, the IABES2 also has a number of potential short-comings for the present analysis that need to be addressed. First, TWA workers cannot be differentiated from the mere administrative personnel of TWA firms in the dataset. However, this shortcoming is unlikely to be of major practical importance. Second, as the TWA firm alone issues the statutory employment notifications, the IABES2 neither contains information on client firms, nor work assignments of TWA workers. However, we will argue in Section 5.1.2 that this data deficiency is not really a disadvantage in the present analysis of the stepping-stone function of temporary agency work, in part because of the empirical strategy chosen. Nevertheless, 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 hypothesis for the stepping-stone function of TWA work. However, the object of the present analysis is deliberately chosen to be both more general and more fundamental, in that we test for the existence and the quantitative importance of the overall bridging function of TWA work, leaving an assessment of the relative importance of different explanations put forward for its alleged existence for future research. Given the paucity of empirical evidence on the stepping-stone function of TWA work, this is a natural starting point to take. Finally, covering only employment relationships that are subject to social security contributions, civil servants, the self-employed, and those in marginal dependent employment are not included in the IABES2.

The present evaluation problem of whether, and if so, to what extent TWA employment helps formerly unemployed workers find regular employment raises several methodological questions that have to be addressed. In Section 5.1, we discuss in detail its peculiar features in comparison to the standard evaluation problem, and in doing so, define key terms, such as treatment and non-treatment status (the counterfactual for the treated), outcome measures, and the like. This permits us to formulate testable causal questions about the stepping-stone function of TWA work, subject

to the restrictions imposed by the nature of 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.

5 The Evaluation Problem

5.1 Evaluating the Stepping-Stone Function of TWA Work

The standard 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 retraining programme for a certain skill group of workers), and linked to specific formal eligibility requirements for participation. None of these features, however, apply to the present evaluation problem. As an on-going programme, unemployed workers may join TWA work both at different calendar dates and at different individual elapsed unemployment durations. In addition, employment spells in the TWA industry vary endogenously in length. An unemployed worker may also hold a TWA job more than once, and thus be subject to multiple treatments with interspersed repeated spells of unemployment. As a consequence, one and the same worker may both be counted as treated at one point in time and as non-treated at another. Moreover, TWA workers may be assigned to different numbers of client firms for different durations and for different tasks, rendering TWA employment heterogeneous across workers in a number of respects that are endogenously determined but post entry. Formal requirements for participation are largely absent, with general 'profitable employability', a function of both individual characteristics of the unemployed job-seeker and general labor market conditions, alone being decisive for temporary work agencies in the recruitment process.

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 TWA employment spell and its end, depending on how TWA 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 TWA 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, i.e. the sample selected.

5.1.1 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. This allows us to observe the subsequent employment histories of those workers for an extended period of time in the IABES2 (up to a minimum total of 5 years). Entries into unemployment are sampled over a three year period to increase the absolute number of subsequent transitions to TWA 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 recall ban in the TWA industry. We further restrict this inflow sample to individuals who are between 18 and 55 years of age at the time of unemployment registration. The upper age limit is imposed so as 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 that lack some prior employment experience. This measure is imperative given the data collection process, for important worker attributes, in particular the educational-vocational qualification obtained, are recorded in employment notifications issued by employers, but not in the information collected in and contributed to the IABES2 from the federal unemployment register.

These restrictions leave us with a total of 101,475 workers in the sample selected that 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 Table 6 in the appendix. As documented in Table 1 below, 2.4% of these unemployment spells are right-censored at the end of 2001, and 11.7% end with no subsequent transition recorded within the sampling period 1994 to 2001. 82.8% of all unemployed workers enter a regular job, almost nine out of ten within one month of deregistering from unemployment. Another 3.2% of the unemployed leave for a TWA job, the great majority (85.2%) again within one month.

It is noteworthy that a much larger fraction, or 8.1% of all entries into unemployment (9.4% of all subsequent transitions) at some point until December 2001 do in fact take up a job in the TWA sector. With close to one in ten unemployed workers joining TWA work over this period, TWA 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 the Table 1, 84.2% of all direct entries into TWA 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

Table 1: Subsequent transitions to TWA and regular employment of unemployment inflows in 1994 - 1996

| Group of workers | Absolute number | Share (%) |
|---|-----------------|-----------|
| Total entries into unemployment: | 101,475 | 100 |
| with right-censored unemployment spells in 2001 | 2,401 | 2.4 |
| with no subsequent record in sampling period | 11,880 | 11.7 |
| ... who enter TWA work: | | |
| within < 1 month of exiting unemployment | 2,728 | 2.7 |
| after \geq 1 month of exiting unemployment | 473 | 0.5 |
| at some point within sampling period | 8,200 | 8.1 |
| ... who enter regular work: | | |
| within < 1 month of exiting unemployment | 72,460 | 71.4 |
| after \geq 1 month of exiting unemployment | 11,539 | 11.4 |

Source: IABES2.

suggests that the size of any permanent "underclass" of temporary workers must be small" (Segal and Sullivan, 1997a, p. 123). No subsequent transitions out of TWA work are observed for only 3.2% of direct entries into TWA employment. Moreover, only a third of these 3.2% are accounted for by right-censored TWA employment spells at the end of the sampling period. TWA job spells last for an average of 254 days, or 8.4 months, and are therefore very short indeed.

However, past work experience in the TWA sector seems to affect the probability of renewed entry into TWA work. As Table 2 shows, 16.4% of workers with some prior TWA work experience exit unemployment for a TWA job, compared to only 2.4% of workers who never worked in the TWA sector.

Table 2: Subsequent transitions to TWA and regular employment of unemployment inflows in 1994 - 1996 by prior TWA work experience

| TWA work experience prior to entry into unemployment | Subsequent transition to: | | | No subsequent transition observed |
|--|---------------------------|----------------|--------------------|-----------------------------------|
| | Regular work | TWA any agency | TWA same as before | |
| Anytime in the past | 73.5% | 16.4% | – | 10.2% |
| Entered unemployment from TWA work | 62.2% | 29.5% | 3.3% | 8.3% |
| None | 83.3% | 2.4% | – | 14.3% |

Source: IABES2.

An even larger fraction (29.5%) of workers that enter unemployment directly from TWA work again take up a TWA job, but only one in nine of these return to their previous TWA. Thus

a sizeable fraction of TWA workers, at least in the short to medium run, indeed appears to cycle between unemployment and temporary agency work spells before eventually finding regular employment. The vast majority of TWA workers, however, does not. Additional explorations, not shown in Table 2, underscore the importance of the TWA submarket for labor market flows, and of past TWA work experience for the likelihood of unemployed workers to enter TWA employment. Workers with prior TWA work experience and workers who enter unemployment directly from a TWA respectively account for 5.2% and 1.5% of all entries into unemployment and for 27.0% and 14.1% of all observed subsequent transitions from unemployment to temporary agency work. Recalls within the TWA industry, however, occur far less frequently than on the general labor market. Only 3.3% of all workers last employed at a TWA in fact return to the same agency when leaving unemployment. In contrast, 7.7% of workers entering unemployment from a regular job again return to the same employer when deregistering from unemployment. The recall 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 TWA workers, as we have seen, does in fact return to TWA work, albeit not to the same employer.

In the remainder of this paper, we restrict the analysis to transitions of individual workers to other labor market states (regular and TWA employment) that occur within less than one month of deregistering from unemployment. Apart from workers with some prior unemployment experience, we also retain workers in the analysis that have been employed in the TWA sector, i.e. "treated", before entering unemployment in 1994 to 1996, because of the scale of reentry into TWA work documented above. Exclusion of either of these two groups of workers from the analysis would very likely result in above-average productivity individuals being sampled. It would also restrict the treatment effect investigated to a significantly reduced subsample of TWA inflows from unemployment. From a policy perspective, however, this subsample does not necessarily represent the group of unemployed workers mostly concerned with in the context of the stepping-stone function of TWA work.

5.1.2 Treatment and Non-Treatment Status

With respect to the definition and the timing of the treatment, we consider the first entry of workers into TWA employment after having registered as unemployed in 1994 to 1996. Any subsequent treatments are thus taken as outcomes of the initial treatment. More specifically, we define treatment as entry into TWA employment, also called the intent of treatment. The causal effects of TWA work are therefore assumed to set in upon entry into the sector. We thereby disregard differences in TWA employment experience across workers (e.g. in terms of employment duration, number of work assignments, etc.), and focus instead on the overall or average effects

of joining TWA work on the subsequent labor market prospects of workers. Practical restrictions imposed by the data, notably 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 standard programme, virtually all aspects of TWA employment relationships formed are ultimately determined endogenously, depending on the post-entry decisions of both the temporary work agency and the TWA worker.¹ Moreover, the available, albeit limited evidence for Germany on the distribution of client assignments across TWA workers and on the transitions of TWA workers to regular employment suggests that the ability of TWA workers to sample many potential employers during temporary work assignments and the ability of client firms to screen TWA workers during such work assignments for permanent positions may not in fact be of primary importance for the acclaimed stepping-stone function of TWA work. As the case study by Kvasnicka (2003) has shown, most TWA 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 TWA 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 TWA 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 TWA 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 TWA workers in the majority prefer regular employment to holding a job in the TWA industry,² largely because of the higher pay and superior working conditions expected to accrue in the former. Moreover, surveys reveal that TWA workers frequently enter TWA 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 or not to enter TWA work in a given month of unemployment. This implies that for the construction of an adequate control group for those actually observed to enter TWA work in a given month of elapsed unemployment duration (u^1), only those unemployed workers should be chosen as potential controls that have been unemployed for at least u^0 and are not treated in u^0 , where $u^0 \geq u^1$. Note however that these workers may well enter TWA work and therefore be treated at a later month of elapsed unemployment duration. Thus while controls can be treated themselves at a later stage, treated workers may never subsequently become controls for workers that enter TWA work at

¹In addition, these aspects are varying over the course of the TWA employment relationship.

²See, for example, Storrie (2002) for the European evidence, Finegold, Levenson, and van Buren (2003) for the US, Hegewish (2002) for the UK, or IWG (1995) for Germany.

longer unemployment durations.

As individual months of entry into TWA work (treatment) differ across workers, we adopt a relative time scale 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 , i.e. elapsed unemployment duration before entry into TWA work, is an unobserved counterfactual for non-treated unemployed workers, it cannot be included as a regressor in the estimation of the propensity score. Nevertheless, we still condition the construction of matches on elapsed unemployment duration by estimating separate propensity scores for every month (u), each based on those treated in u and those not treated in u (see the matching protocol in the appendix). This approach is equivalent to estimating a discrete hazard rate model, where all estimated parameters are allowed to be duration-specific (Sianesi, 2004, p.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, i.e. the exclusive selection of potential controls from among those workers never observed to enter the particular programme investigated. For in the latter case the construction of a comparison group is in fact conditioning on the future and thus the outcome when programme starts are not restricted to a particular period (see Fredriksson and Johansson (2003) on this point). In the current application, this would introduce a downward bias in the estimated treatment effect of TWA work on regular employment probabilities, as unemployed workers that act as controls are likely to be never observed to enter TWA 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.

5.1.3 Formal Specification of the Evaluation Problem (ATT, CIA)

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 programmes in Sweden. The outcomes of interest are various labor market states of individuals over time, i.e. $\left\{ Y_{jt}^{(u)} \right\}_{t=u+1}^T$, where j denotes the type of outcome, i.e. the particular labor market status considered, and $t = u + 1, \dots, T$ 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 TWA 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 TWA work in u . We further denote the potential labor market states of an

individual at time t , where $t > u$, that joins TWA work in her u 'th month of unemployment with $Y_{jt}^{1(u)}$ and with $Y_{jt}^{0(u)}$ if an individual has not joined TWA 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 effect of joining TWA work in month u of elapsed unemployment duration ($Y_{jt}^{1(u)}$) compared to not joining TWA work in that month ($Y_{jt}^{0(u)}$) for those unemployed workers who actually take up a TWA job in that same month ($D^u = 1$), i.e.:

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

As discussed in Section ??, we have to assume stable unit treatment value and conditional independence to identify the second term in equation (1), i.e. the unobserved counterfactual. The latter assumption, in formal terms, requires that:

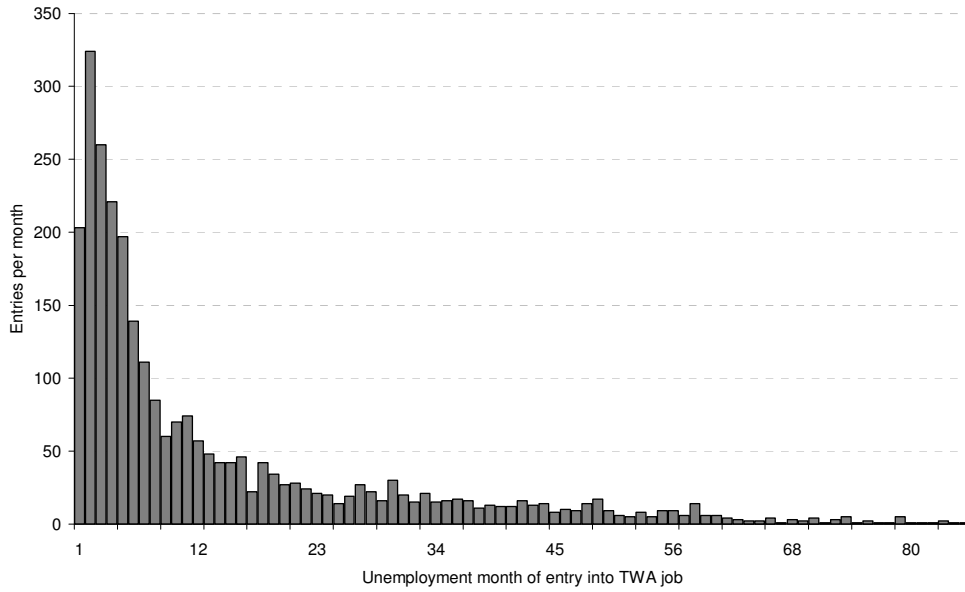
$$Y_{jt}^{0(u)} \perp D^u \mid X = x \quad \text{for } t = u + 1, \dots, T,\quad (2)$$

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

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

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 $U = 12$, i.e. we investigate the effect of taking up a TWA job within one year of registering as unemployed in the period 1994 to 1996. Restricting the period workers are followed while still in unemployment is necessary 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 IABES2. With data until December 2001, this restriction provides us with at least 48 months for each individual worker, irrespective of the particular calendar months she entered and exited her unemployment spell. Furthermore, as shown in Figure 1, the total number of transitions from unemployment to TWA work declines rapidly with elapsed months of unemployment duration. In fact, about two-thirds, i.e. 66.0%, or 1801 out of the 2728 transitions to TWA employment in the sampling period 1994 to 2001 take place within the first year of unemployment.

Figure 1: Treated unemployed workers by month of entry into TWA work



Source: IABES2.

5.1.4 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 TWA industry, we employ a set of four different outcomes measures $(Y_{jt}^{(u)})$, described in Table 3 below. These respectively forty-eight monthly post-treatment probabilities of regular employment, TWA employment, any type of employment (regular or TWA), and unemployment allow us to study the dynamics of the effects TWA employment exerts on the individual likelihoods of observing these states over time.

Table 3: Definition of outcome measures

Outcomes for each month up to 4 years post treatment:

Outcome 1 Monthly probability of regular employment

Outcome 2 Monthly probability of TWA employment

Outcome 3 Monthly probability of employment (TWA or regular)

Outcome 4 Monthly probability of unemployment

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 one for a particular group of workers. Moreover, workers that return to education, or general inactivity, i.e. states which are not recorded in the IABES2, are retained in the respective monthly base groups from which the four outcome measures are calculated. This design was inspired by a number of reasons.

First, employment spells are measured with daily accuracy in the IABES2. Unlike the situation encountered in monthly survey data, this inevitably requires a deliberate choice as to when exactly one should measure individual outcomes on a monthly basis. The primary alternative to the approach taken in this study would be to (arbitrarily) pick certain dates, be it the start or end of individual months, for measuring the outcomes of interest. The main drawback of such a design is that workers, by virtue of legal restrictions on employment termination in Germany, tend to be laid off or quit either at the 15th of a month or at its very end. As a consequence, measuring outcomes at the start (end), or in the first (second) half of individual months would introduce a negative (positive) bias in the estimates of monthly unemployment probabilities of groups of workers that exhibit a higher probability of employment termination over time. The reverse, in turn, would hold true for the respective monthly employment probabilities of this same group of workers. Ignoring this contingency is objectionable on methodological grounds, because it implies to condition a priori the analysis on assumed posterior outcomes yet to be investigated. In addition, this problem is likely to be a non-negligible one in the current context, for as documented in Section 5.1.1, TWA employment spells on average tend to be extremely short and frequently lead to renewed employment in the sector after intermittent spells of unemployment. Last but not least, the monthly chances of workers to be in either of the four states correspond more intuitively to the type of measurement adopted here, than to one that is based on likelihoods obtained from considering only a thirtieth of the respective reference periods, i.e. a single day.

Second, and by virtue of the data generation process, we have complete information on the employment (TWA and regular) and unemployment trajectories of workers over time that comprise our individual outcome measures of interest, i.e. employment subject to social security contributions and unemployment periods involving some sort of entitlement to financial support from the public authorities. As pointed out in Section 4, longitudinal survey data in contrast generally suffers from panel mortality due to nonresponses that prevents researchers from observing the true labor market states of individuals, i.e. the outcomes of interest. Monthly probabilities for indi-

vidual outcome measures in this case have to be calculated conditional on observing workers at the various sampling dates, which complicates the interpretation of the results obtained, i.e. the individual probabilities estimated. Accounting for this type of sample attrition in fact necessitates extensive additional explorations and in general introduces uncertainty about the robustness of the results obtained (see Sianesi (2004) for various steps employed to obtain bounds on the likely biases introduced by this measurement problem). The current application, in contrast, does not suffer from such a drawback. It therefore requires workers not observed in individual outcome months in the IABES2 to be retained in the monthly base groups from which the four probability measures are calculated, given the causal questions of interest, and the accuracy of the IABES2 in recording the four labor market states necessary to address them empirically.

While Outcome 1 is of primary interest for the empirical assessment of the stepping-stone function of TWA 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 TWA 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.

5.2 Implementation of Propensity Score Matching

5.2.1 Nearest-Neighbor Matching

We apply nearest-neighbor propensity score matching without replacement, but within caliper (Cochran and Rubin, 1973).³ In other words, conditional on elapsed unemployment duration u , each treated individual i in month u is matched to that non-treated 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, i.e. the degree of residual mismatch, does not exceed a certain maximum Ψ , or caliper (see, for example, Heckman, LaLonde, and Smith, 1999, p. 1954):

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

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 estimates the j times t ATTs (Δ_{jt}^u) for each entry month into TWA work, i.e. u , as the difference in mean outcomes between between

³The matching estimator 'psmatch2' by Leuven and Sianesi (2003) for STATA is used and adapted to the specific features of the present evaluation problem.

treated and matched controls:

$$\Delta_{jt}^u = \frac{1}{N^{u1}} \sum_{i=1}^{N^{u1}} \left(y_{jt}^{1(u)} - y_{jt}^{0(u)} \right), \quad (5)$$

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 non-dependence of the variance of the outcome on the propensity score (Lechner, 2001), the variances of the ATTs, Δ_{jt}^u can then be calculated as (see Sianesi, 2001, p. 28):⁴

$$Var(\Delta_{jt}^u) = \frac{1}{N^{u1}} Var\left(Y_{jt}^{1(u)} \mid D^u = 1\right) + \frac{\sum_{z=1}^{N^{u0}} \omega_z^2}{(N^{u1})^2} Var\left(Y_{jt}^{0(u)} \mid D^u = 0\right), \quad (6)$$

where $D^u = 1$ and $D^u = 0$ denote matched treated and non-treated 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. A description of the actual matching protocol used is provided in the appendix.

5.2.2 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, i.e. the ability to control for all factors that both determine selection into TWA work and affect potential outcomes in the two participation states. We discuss these factors in relation to the two principal actors involved, i.e. the TWA and the unemployed job-seeker, the potential restrictions imposed on their conduct by the LoPA, as well as the general labour market conditions they are confronted with.

The TWA: 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

⁴Note, however, that unlike Sianesi (2001), we do not have to condition on treated workers being observed at individual outcome months in this case, as we do not have any measurement error in the labor market states of interest that underly our outcome measures (see Section 5.1.4).

market conditions, are relevant for temporary work agencies in the recruitment process. Deferring a discussion of the latter for the time being, the former necessitates the consideration of attributes related to the productivity of individuals in the estimation of the propensity score. Besides personal characteristics, i.e. age, sex, foreign nationality, and marital status, 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 TWA job. The latter in particular appeared quite significant in the descriptive explorations of observed transitions from unemployment to TWA work in Section 5.1.1. 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 TWA workers are usually assigned to at client firms. In addition, and by virtue of the dataset, 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 TWA 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 Länder is included, where TWAs have only been able to operate from 1990 onwards, and annual as well as seasonal indicators are used to capture cyclical and seasonal variations in the demand of TWAs 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, as pointed out in the introduction, surveys regularly find unsuccessful search for a regular job to be one of the most important motives for taking up work in the TWA sector, thereby lending support to the notion of sequential decision taking on the part of unemployed job-seekers of whether or not to join TWA work. Second, benefit entitlement levels, and thus the reservation wage, decline with elapsed unemployed duration. As remuneration in the TWA sector generally falls short of levels attainable in other industries, workers with prolonged unemployment spells, and thus a lower reservation wage,

should be more likely to take up a TWA job than workers who have just entered unemployment. Third, elapsed unemployment duration provides an upper bound on 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 TWAs who each manage a whole portfolio of potential job opportunities. Registering in the new German Länder and the local unemployment rate at entry are likely to have an effect on the employment opportunities of individual job-seekers, both in the TWA sector and 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 that enter TWA employment 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, i.e. 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 LoPA: The only restriction in fact imposed by the LoPA before 2004 on the conduct of TWAs in the recruitment process was the recall ban, which prohibited an agency to rehire a laid-off worker within three months of prior employment termination.⁵ This ban was not eased until April 1997 (see also Jahn and Rudolph, 2002). By virtue of sampling only inflows into unemployment between 1994 and 1996, however, the reform is in fact immaterial for subsequent transitions of workers out of this spell of unemployment. However, we may not directly account for this regulatory constraint on the recruitment behavior of TWAs in the estimation of the propensity scores by way of a dummy variable that takes the value one if less than three months have elapsed since a worker has been laid off by a TWA, and zero otherwise. For 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, i.e. we will have no variation in the data, as the recall ban ceases to be binding for all workers still unemployed after three months. However, in the context of our matching algorithm, we would expect to have immediately preceding employment in the TWA sector less of a positive effect on the probability

⁵As we have seen in Section 5.1.1, recalls indeed occur much less frequently in the TWA sector than in the economy at large (see also Chapter 3, Section 3.3.1.2 for additional evidence from our case study).

to reenter TWA work in the first three months of unemployment than in the fourth, if the recall ban does indeed have a material effect. We do, in fact, find such evidence (see Table 7 in the appendix.).

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 TWA agencies. Labor demand of TWA firms is also 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 to 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 unemployment registration in the new German Länder proxies for persistent structural differences between East and West Germany.

A note is in order on a problem, which is commonly encountered in evaluation studies, i.e. anticipatory effects of future treatment on the behavior of workers that are later treated, and its likely pervasiveness in the current application. Anticipatory effects of unemployed job-seekers, leading to reduced job search prior to entry into TWA work (akin to Ashenfelter’s dip), should not be a major problem in the present evaluation problem of the stepping-stone function of TWA work. For TWAs in Germany tend to hire workers predominantly on-call in line with current levels of client demand (see Kvasnicka, 2003), which is unlikely to be predictable with certainty even one or two weeks in advance. In addition, worker recalls on the TWA submarket at the time were prohibited by the recall ban within three months of prior employment termination.

5.2.3 Matching Quality

The regression output of the probit estimations of the propensity scores for a number of treatment months are provided in Table 7 in the appendix. All covariates except current entitlement status are measured at entry into unemployment. In particular, previous TWA work experience and direct entry into unemployment from a TWA job have a sizeable and statistically significant positive effect on the probability of transition to a TWA job. Previous real earnings and wage levels at the last employer, as well as 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 TWA job, when other employment opportunities are scarce, TWAs may only be inclined to recruit more workers when client demand for their services is high, i.e. 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 TWA 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 TWA job in the first months of their unemployment spells. TWA 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 non-treated 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 non-treated 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 4 provides various summary statistics on covariate balancing and thus matching quality for all 12 probit regressions. Given the very large groups of potential controls available for each unemployment month (column 3), finding a suitable match partner for treated individual is not a problem. Only 2 out of the 1801 workers leaving unemployment for a TWA job, as shown in column 10, are excluded for lack of common support. 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 7% of the variance in treatment assignment across the matched subsamples, and thus only half the average respective figure obtained from the original samples of treated and non-treated workers. Associated probability values of likelihood ratio tests before and after matching are reported in columns 6 and 7. Whereas before matching, the joint significance of the covariates is never rejected for any of our twelve probit regressions, it is always rejected 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 , with median absolute standardized biases on average being reduced by a factor of three to four after

matching compared to the situation before matching (columns 8 and 9).⁶

Table 4: Statistics by month on covariate balancing, before and after matching

| Month (u) | Treated workers before | Nontreated workers before | Probit ps.-R ² before | Probit ps.-R ² after | Pr>χ ² before | Pr>χ ² after | Median bias before | Median bias after | Treated workers lost to CS after |
|-----------|------------------------|---------------------------|----------------------------------|---------------------------------|--------------------------|-------------------------|--------------------|-------------------|----------------------------------|
| (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| 1 | 203 | 90463 | 0.100 | 0.032 | 0.000 | 0.989 | 17.2 | 5.7 | 0 |
| 2 | 324 | 80813 | 0.134 | 0.028 | 0.000 | 0.891 | 21.6 | 5.4 | 0 |
| 3 | 260 | 69869 | 0.118 | 0.015 | 0.000 | 1.000 | 22.7 | 4.3 | 1 |
| 4 | 221 | 63723 | 0.169 | 0.027 | 0.000 | 0.993 | 24.8 | 4.6 | 0 |
| 5 | 197 | 58358 | 0.151 | 0.032 | 0.000 | 0.990 | 21.9 | 4.7 | 0 |
| 6 | 139 | 53884 | 0.128 | 0.046 | 0.000 | 0.988 | 21.3 | 6.1 | 0 |
| 7 | 111 | 49822 | 0.133 | 0.049 | 0.000 | 0.998 | 23.7 | 5.6 | 0 |
| 8 | 85 | 43760 | 0.124 | 0.113 | 0.000 | 0.697 | 26.0 | 9.4 | 0 |
| 9 | 60 | 42784 | 0.118 | 0.145 | 0.000 | 0.839 | 23.3 | 10.3 | 0 |
| 10 | 70 | 40659 | 0.154 | 0.112 | 0.000 | 0.912 | 26.6 | 9.6 | 1 |
| 11 | 74 | 37268 | 0.117 | 0.093 | 0.000 | 0.964 | 26.0 | 7.5 | 0 |
| 12 | 57 | 35596 | 0.140 | 0.189 | 0.000 | 0.574 | 30.9 | 8.6 | 0 |

Note: before = before matching, after = after matching.

(4), (5): Pseudo-R² 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.

Source: IABES2.

6 Empirical Findings

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_{jt}^u by month of entry into TWA work in Section 6.2, i.e. the causal effects identified under the CIA in equation (2). Following Sianesi (2004, p. 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:

$$E_U(\Delta_{jt}^u | D = 1) = \sum_{u=1}^{U=12} \left[E\left(Y_{jt}^{1(u)} - Y_{jt}^{0(u)} \mid D^u = 1\right) P(D^u = 1 | D = 1) \right], \quad (7)$$

where $E\left(Y_{jt}^{1(u)} - Y_{jt}^{0(u)} \mid D^u = 1\right) = \Delta_{jt}^u$, which are weighted in the summation by the monthly

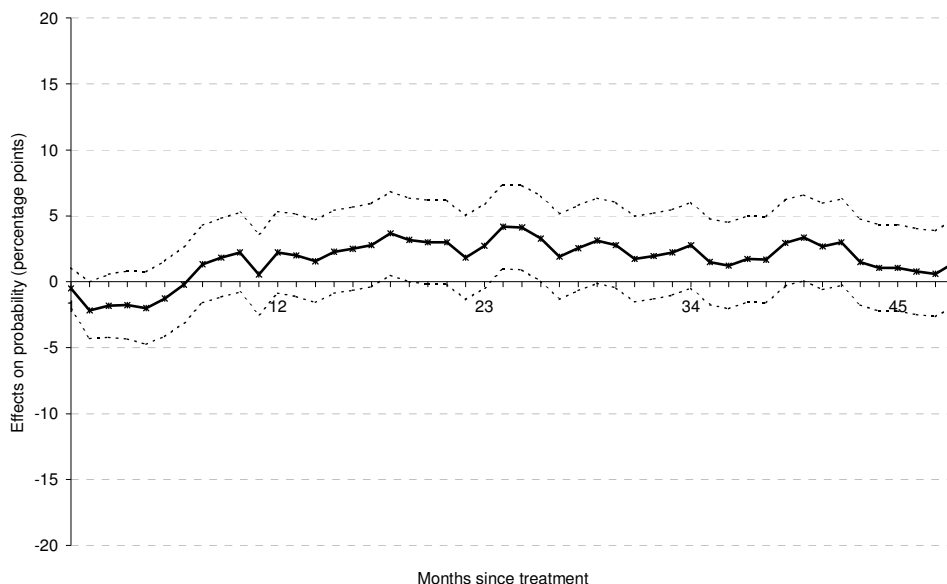
⁶The median is taken over all regressors and calculated for each unemployment month u following Rosenbaum and Rubin (1985) as: $Bias_{Before}(X) = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{[(V_1(X) + V_1(X))/2]}} \times 100$ before matching and as $Bias_{After}(X) = \frac{\bar{X}_1^M - \bar{X}_0^M}{\sqrt{[(V_1(X) + V_1(X))/2]}} \times 100$ after matching, where \bar{X}_1 and \bar{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 \bar{X}_1^M and \bar{X}_0^M the respective sample means in the group of matched treated individuals within the common support and nontreated individuals, i.e. controls (see Sianesi, 2004, p. 154).

entry distribution into TWA work for those actually leaving unemployment for TWA work, i.e. $P(D^u = 1 | D = 1)$. The following subsections graph estimates of these average monthly effects on our four outcome measures together with 95% 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 TWA work on average fared in their subsequent employment and unemployment trajectories by joining TWA work relative to the counterfactual situation in which they would have continued their job search in registered unemployment. Estimates of these treatment effects on our four outcome measures are reported in Table 8 in the appendix for selected months post treatment.

6.1.1 Outcome 1: Probability of Regular Employment

Figure 2 shows that entering a TWA job has no statistically significant effect, in the short run or long run, on the monthly probabilities of regular employment. In other words, neither a stepping-stone effect of TWA employment, nor an adverse effect on future probabilities of regular employment is discernable. If anything, TWA work tends to reduce the relative chances of being in a regular job in the first seven months, a differential effect that subsequently turns slightly positive, fluctuating around two percentage points.

Figure 2: Treatment effects over time on the probability of regular employment



Source: IABES2. - - - denote 95% confidence intervals.

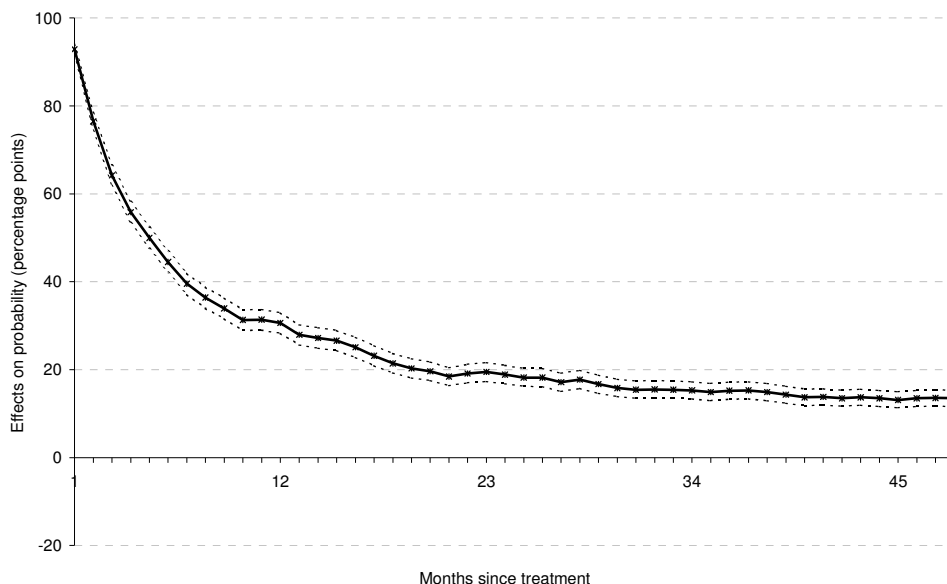
The estimated reduced probabilities of regular employment are most likely the result of a lock-in-effect of programme participation, as TWA employment spells of treated workers on average last

for less than 8.5 months, as pointed out before, and thus only marginally longer than the period in which the estimated difference in the probability of regular employment is negative. However, while some unemployed workers entering TWA work might still be employed in their job at later outcome months, others may also cycle between different TWA jobs. As a consequence, while inspection of Figure 2 provides evidence against the stepping stone hypothesis of TWA work, it does not give an answer as to whether unemployed workers benefit in their overall future probability of employment, be it regular or temporary agency work employment, from entering a TWA job from unemployment. Having explored the former constituent part of this outcome measure in Figure 2, we next turn to the latter component (Outcome 2), before considering both parts in combination, i.e. Outcome 3.

6.1.2 Outcome 2: Probability of TWA Employment

As is evident from Figure 3, individuals leaving unemployment for TWA work are significantly more likely throughout the four year period after they entered agency work to be employed in the TWA sector. While the treatment effect declines rapidly over the first eight to nine months, its decline becomes subsequently less marked and roughly stabilizes after two years at around 17 percentage points.

Figure 3: Treatment effects over time on the probability of TWA employment



Source: IABES2. - - - denote 95% confidence intervals.

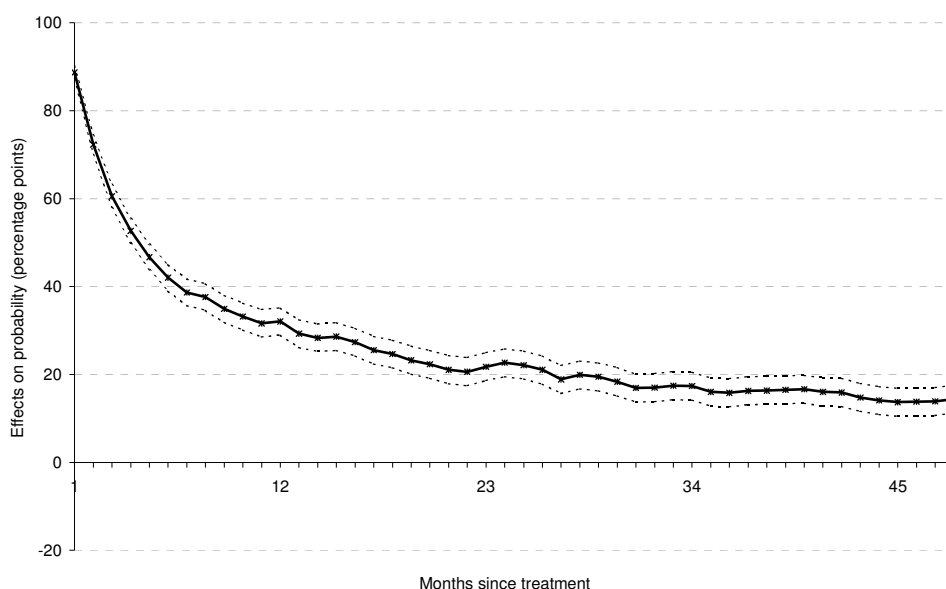
While prolonged program duration and repeated program participation may in general be a matter of concern if it keeps those workers from obtaining regular work, this does not seem to be

the case in the present context. For as we have seen in Figure 2, unemployed workers entering TWA work on average do not exhibit statistically significant lower monthly probabilities of regular employment than unemployed workers who chose not to join TWA work as yet.⁷

6.1.3 Outcome 3: Overall Probability of Employment

With respect to any social-security employment (TWA or regular), Figure 4 reveals that unemployed workers who take up a TWA job exhibit a higher probability than those unemployed workers who do not join TWA work in the same month of elapsed individual unemployment duration in each month following entry into the TWA sector for the entire 4 year period under investigation.

Figure 4: Treatment effects over time on the probability of employment



Source: IABES2. - - - denote 95% confidence intervals.

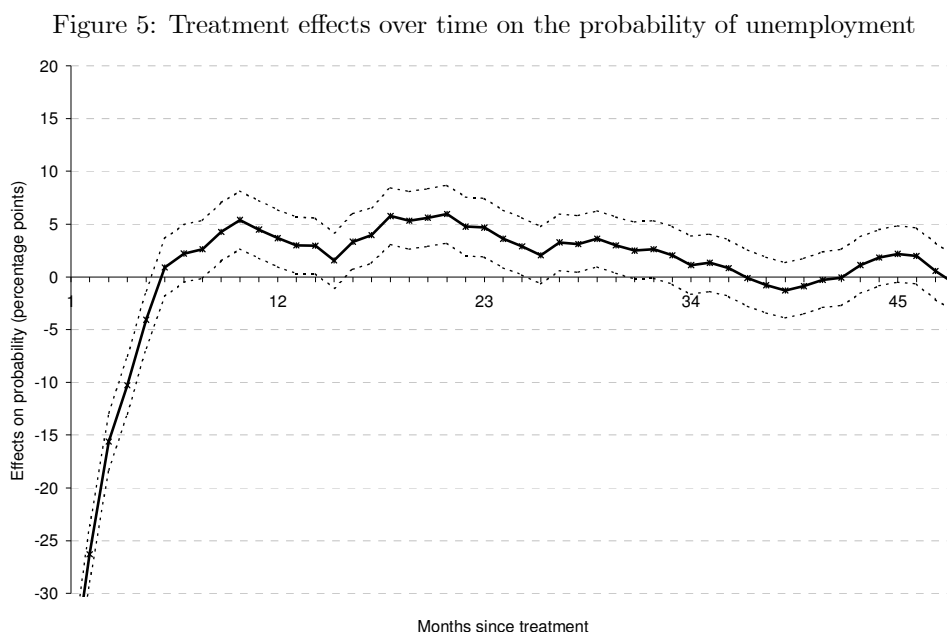
With Outcome 3 being a composite of Outcomes 1 and 2, and the general time pattern of treatment effects discernable in Figures 3 and 4, it is evident that the overall monthly employment probabilities quite closely resemble the levels and trend of the increased likelihoods of TWA employment for workers treated upon exit from unemployment. In sum, therefore, unemployed job-seekers who take up a TWA job on average fare overall better in terms of average employment probabilities over the 48-month period considered. It remains to be seen, however, how entry into TWA work affects the risks of future unemployment over time. As argued before, our outcome measures are neither mutually exclusive nor all inclusive, so that we cannot infer the treatment

⁷Moreover, as already noted in Section 5.1.1, only about 1% of entries to TWA work have right-censored TWA employment spells at the end of the sampling period of the IABES2 (December 2001).

effects of TWA work on a particular outcome from the treatment effects estimated for the other outcomes.

6.1.4 Outcome 4: Probability of Unemployment

Figure 5 documents that monthly probabilities of unemployment are significantly reduced for treated workers in their first six months after entering TWA work, then turn positive in the second halves of the first and second year, before becoming statistically indifferent from the risks of unemployment experienced by unemployed workers who have not joined TWA work as yet.



Source: IABES2. - - - denote 95% confidence intervals.

Apart from the initial lock-in effect of program participation, therefore, treated workers do at times appear to suffer from (albeit marginally) increased risks of unemployment, especially so in the first two years, with a spike around the time of average employment duration in TWA work, but increasingly converge in their future likelihood of unemployment to levels experienced by workers that were not treated as of yet. As was the case with regular employment, therefore, taking up a TWA job neither seems to worsen nor improve the long-term risks of unemployment for workers.

6.2 Treatment Effects by Month of Entry into TWA Work

Having so far explored the average dynamics of the different treatment effects, Table 5 reports the respective causal effects averaged over the forty-eight outcome months for different entry months into TWA work, as well as for the entire population of workers entering TWA work within their

first twelve months of unemployment. The former correspond to the causal effects identified under the CIA, equation (2), whereas the latter summarize Figures 2 to 5, i.e. relate 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 TWA work. Estimated baseline outcome probabilities for the respective control groups corresponding to the different groups of treated workers in Table 5 are provided in Table 9 in the appendix.

Table 5: Average treatment effects by unemployment month of entry into TWA work

| Outcomes: | Effect (percentage points) | | | | | |
|--------------------|----------------------------|---------------------|---------------------|---------------------|----------------------|---------------------|
| | u = 1-12 | u = 1 | u = 3 | u = 6 | u = 9 | u = 12 |
| Regular Employment | 1.7 (-1.5;4.9) | -1.0 (-10.7;8.7) | 2.4 (-6.1;10.9) | 3.4 (-7.9;14.7) | 7.5 (-9.6;24.6) | 1.9 (-16.0;19.8) |
| TWA Employment | 25.3 (23.1;27.6) | 26.1 (19.5;32.8) | 26.6 (20.6;32.5) | 24.6 (16.4;32.7) | 30.0 (17.8;42.2) | 16.3 (5.0;27.7) |
| Any Employment | 26.4 (23.3;29.6) | 24.5 (15.3;33.7) | 28.4 (20.3;36.5) | 27.3 (16.0;38.6) | 36.2 (19.6;52.9) | 18.0 (-0.0;36.1) |
| Unemployment | 0.2 (-2.5;2.9) | -4.6 (-12.2;3.1) | -6.8 (-13.6;0.0) | 3.3 (-6.6;13.1) | -1.7 (-16.6;13.3) | 7.4 (-7.9;22.6) |

Source: IABES2. 95% confidence intervals are reported in parentheses.

As is evident, the averaged ATTs on the probability of regular employment are never significantly different from zero, neither are those for the probability of unemployment, which contradicts our earlier results contained in Figure 5. The probabilities to be employed in the TWA sector, or in either TWA or regular work, however, remain strongly and positively affected by program participation, except for the averaged probability of any social-security employment for workers who entered in their twelve month of unemployment.

Overall it seems that unemployed workers joining TWA work very early (in the first month) and very late (in the twelve month) in their unemployment spells tend to fare worse on average than those workers who join in between these two periods. The former two groups of workers exhibit both lower treatment effects with respect to TWA employment and our composite employment measure than do workers treated in other months within their first twelve months of unemployment duration. In addition, the (albeit not statistically significant) treatment effects on regular employment are lower and those on unemployment are larger, the latter so at least for the 12th month joiners, for workers who join TWA work either very early or very late compared to the corresponding treatment effects of workers who enter TWA work in the third, sixth, and ninth month of unemployment. For the very early entries into TWA work these differential effects could be explained by workers having abandoned their search for a regular job out of registered unemployment too quickly, thus

losing out on potential regular employment opportunities because of reduced job search activity when being locked in TWA work straight after unemployment registration. The adverse outcomes observed for very late entries of workers into TWA work might be due to exactly the reverse, i.e. having waited for too long.

Overall, we may summarize the findings in Table 5 to suggest that unemployed workers benefit in their overall future employment chances from entering TWA work, because of the increased likelihood of future TWA employment, but are neither more likely to obtain regular employment, nor to suffer from increased risks of future unemployment. Sample sizes, however, are fairly small, and lead to large standard errors in the estimates of our ATTs obtained, as is evident from Table 5. Nevertheless, the estimated treatment effects on regular employment are quite small for entries into TWA work at the various months of unemployment duration considered in Table 5 compared to the baseline probabilities of regular employment for the respective matched control groups (see Table 9 in the appendix).

7 Conclusion

Applying statistical matching techniques, this paper has investigated the average effects of entering TWA work on the future labor market outcomes of unemployed workers relative to the counterfactual in which these workers would have continued their job-search in registered unemployment. The empirical analysis was based on the subsequent labor market trajectories of workers over a four-year period who registered as unemployed between 1994 and 1996. Unemployed workers who entered TWA employment within twelve months of unemployment registration turned out to benefit from both higher monthly employment and TWA employment probabilities throughout the four year period these workers were followed post treatment. They did not, however, enjoy greater chances of future regular employment, nor greater risks of unemployment. While our results, therefore, do not lend empirical support to the stepping-stone hypothesis of TWA work for unemployed job-seekers in Germany, they do neither confirm the existence of adverse effects on the future regular employment and unemployment probabilities of workers. If anything, TWA work seems to provide an access-to-work function for unemployed workers that leaves them with a higher probability of (TWA) employment for the entire four years their subsequent labor market states have been analyzed than workers who did not join TWA work as of yet in their unemployment spell. Personnel-Service-Agencies in Germany, introduced by the latest reform of the LoPA in 2002, were created as bridges for unemployed job-seekers into social security employment by way of temporary work assignments with client firms. The results of our empirical analysis, however, call into question the very existence of such a stepping-stone function of temporary agency work for

the unemployed, and therefore cast doubts on the very premises on which these new instruments of active labor market policy were introduced.

We have followed TWA workers for a longer time period than existing studies on the stepping-stone function of TWA work.⁸ Our results showed that this extended observation period for the measurement of outcomes proved mandatory in our study to fully capture the pattern of treatment effects as they evolved over time. Differences in the results obtained in our study to those of previous analyses, however, are hard to explain. Important differences exist in the sampling design, evaluation method employed, time periods considered, and regulatory frameworks encountered in the different countries for which the stepping-stone of TWA employment has been investigated empirically.

Future research for Germany will have to determine in how far these average treatment effects on the treated vary across different subpopulations of workers entering TWA work, i.e. explore potential heterogeneous treatment effects of TWA work, which for lack of sufficient sample sizes has not been pursued in this study (with the exception of different unemployment months workers entered into TWA work). Another fruitful area of research would be the consideration of an even larger set of outcome measures, including non-activity, marginal and self-employment, as well as indicators of the quality of jobs obtained by former TWA workers, proxied, for instance, by the type of employment contract held (open-ended or fixed-term). These extensions would require the use of new longitudinal data of sufficient sample size that record the labor market states of individuals necessary for constructing measures of these additional outcomes over time. Such data sources, however, do not yet exist for Germany.

⁸Ichino, Mealli, and Nannicini (2004), for instance, measured outcomes only one year and a half post treatment, and Autor and Houseman (2002) only between 90 days (job retention) and two years (program recidivism) later. Malo and Muñoz-Bullón (2003) followed TWA workers over a three year period, but as pointed out in Section 3, based their analysis on an initial stock sample of TWA employees.

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Appendix

Table 6: Summary statistics of the sample at entry into unemployment

| | | | |
|--------------------------------|------|---|------|
| Worker Characteristics: | | Previous real daily gross wage (€) | 47.5 |
| Female | 41.5 | Ø real daily gross wage at employer (€) | 54.8 |
| Foreign | 9.1 | Duration of last job less than 1 year | 44.3 |
| Age (years) | 34.3 | Ever before in TWA work | 5.2 |
| Married | 49.3 | Immediately before in TWA work | 1.5 |
| Educational/vocational degree: | | Unemployment Spell: | |
| secondary | 29.5 | First time unemployed | 49.5 |
| secondary with vocational | 66.1 | Registered in new German Lander | 33.7 |
| polytechnic or university | 4.3 | Local unemployment rate | 11.9 |
| Previous Employment History: | | Registration in: | |
| Sector: | | 1994 | 39.1 |
| Agriculture, forestry, fishery | 2.2 | 1995 | 31.2 |
| Energy | 0.5 | 1996 | 29.7 |
| Manufacturing | 28.9 | 1st quarter | 33.2 |
| Construction | 15.5 | 2nd quarter | 20.1 |
| Trade | 13.2 | 3rd quarter | 23.8 |
| Transport | 5.0 | 4th quarter | 22.9 |
| Credit | 1.2 | Entitlements: | |
| Services | 22.9 | Unemployment benefits | 91.6 |
| State | 4.7 | Unemployment assistance | 7.0 |
| Other | 5.0 | Unemployment support | 1.5 |
| Type of last occupation: | | | |
| In training | 9.5 | | |
| Unskilled blue-collar | 25.1 | | |
| Skilled blue-collar | 30.5 | | |
| White-collar | 24.5 | | |
| Part-time | 10.3 | | |

Note: Number of workers = 101,475. All entries are in percent, unless stated otherwise.

Source: IABES2.

Matching Protocol:

Steps 1. - 4. are repeated for the first twelve months u since unemployment registration in 1994 to 1996:

1. All workers treated in month u and all workers still unemployed after u are selected.
2. The conditional probability (propensity score) of joining TWA work in month u is estimated via a probit regression for the subsample of workers selected in step 1.
3. Based on the propensity score estimated in step 2, the group of treated workers is matched to the group of non-treated workers in month u by nearest-neighbor without replacement but within caliper Ψ .
4. The effects on the four monthly outcome measures (Δ_{jt}^u) of entering TWA work in month u of unemployment instead of not entering TWA work as yet is calculated as the difference in the respective outcomes between the treated and the matched controls identified in step 3.

Table 7: Estimation of propensity scores by month of entry into TWA work

| | Months 1 | 4 | 8 | 12 |
|-------------------------------|-------------------------------|-------------------------------|-----------------------------|-----------------------------|
| Personal characteristics: | | | | |
| Female | -.3080 (-4.52) ^{***} | -.2866 (-4.20) ^{***} | -.1559 (-1.59) | -.2648 (-2.09) [*] |
| Foreign | .0584 (0.71) | -.0372 (-0.41) | .0411 (0.35) | .1393 (1.10) |
| Age | .0069 (0.34) | -.0279 (-1.30) | .0137 (0.44) | .0203 (0.51) |
| Age ² | -.0002 (-0.85) | .0001 (0.42) | -.0004 (-0.94) | -.0006 (-1.01) |
| Married | -.0306 (-0.53) | .0002 (0.00) | -.0467 (-0.53) | -.0874 (-0.79) |
| Education: | | | | |
| (ref.: secondary degree) | | | | |
| Vocational degree | .2067 (2.64) ^{***} | .1528 (1.91) [*] | -.0922 (-0.83) | .0551 (0.41) |
| University | -.0941 (-0.46) | -.3879 (-1.30) | — | -.0096 (-0.03) |
| Last employment spell: | | | | |
| Job tenure less than 1 year | .0399 (0.74) | .1212 (2.07) ^{**} | .2865 (3.33) ^{***} | .0424 (0.41) |
| Occupation: | | | | |
| (ref.: unskilled blue collar) | | | | |
| Training | .1820 (1.70) [*] | .4933 (4.91) ^{***} | .1530 (0.94) | .0758 (0.40) |
| Skilled blue collar | -.0110 (.07) | .0109 (.07) | .1939 (1.66) [*] | .1219 (0.87) |
| White collar | -.0003 (-0.00) | .0283 (0.30) | .0465 (0.33) | -.1246 (-0.69) |
| Part time | .0641 (0.52) | -.1355 (-0.90) | -.1149 (-0.58) | -.0213 (-0.10) |
| Real gross daily income | | | | |
| Of worker | -.0004 (-0.35) | .0006 (0.89) | .0004 (0.46) | -.0004 (-0.19) |
| Average at employer | .0002 (0.13) | .0011 (0.73) | .0021 (0.89) | .0030 (1.07) |
| Sector: | | | | |
| (ref.: manufacturing) | | | | |
| Agriculture | -.1250 (-0.65) | .1760 (1.04) | — | — |
| Energy | .1943 (0.76) | .1908 (0.69) | — | — |
| Construction | -.0721 (-0.99) | .0065 (0.08) | .1124 (0.97) | .1314 (0.96) |
| Trade | -.0858 (-1.00) | -.0759 (-0.81) | -.0922 (-0.61) | .0539 (0.34) |
| Transport | -.1371 (-1.11) | -.1582 (-1.18) | .0387 (0.22) | -.3736 (-1.10) |
| Credit | -.0165 (-0.07) | .0864 (0.39) | .1171 (0.34) | — |
| Services | -.1495 (-1.84) [*] | -.1196 (-1.37) | .0394 (0.32) | .0239 (0.16) |
| State | -.6620 (-2.42) ^{**} | -.0928 (-0.67) | -.0596 (-0.30) | -.1375 (-0.53) |
| Other | -.1766 (-1.19) | -.2456 (-1.42) | -.1566 (-0.66) | -.0593 (-0.23) |
| Previous TWA work: | | | | |
| At some point in past | .5388 (6.75) ^{***} | .6027 (7.13) ^{***} | .3993 (3.30) ^{***} | .3818 (2.62) ^{***} |
| Last job was in TWA sector | .5735 (4.80) ^{***} | .7099 (5.80) ^{***} | .5312 (3.00) ^{***} | .2499 (1.05) |

... continued.

| | Months 1 | 4 | 8 | 12 |
|--------------------------------|-----------------|-------------------|------------------|-----------------|
| Unemployment characteristics: | | | | |
| First-time unemployed | .0726 (1.30) | -.0253 (-0.41) | -.0409 (-0.46) | .1210 (1.18) |
| Registered in new Lander | .1096 (1.26) | .1473 (1.66)* | .0669 (0.49) | -.3347 (-1.90)* |
| Local unemployment rate | .0021 (0.21) | .0084 (0.80) | -.0130 (-0.84) | .0138 (0.77) |
| Entitlements (ref.: benefits): | | | | |
| Support payments | — | -.0818 (-0.92) | -.1114 (-1.07) | .2273 (2.05)** |
| Living supports | -.1195 (-1.24) | -.7223 (-3.05)*** | -.3595 (-2.19)** | -.1177 (-0.72) |
| Registration: | | | | |
| (ref.: 1994, 1st quarter) | | | | |
| 1995 | .0790 (1.34) | -.1378 (-2.18)** | -.0387 (-0.44) | -.2215 (-1.71)* |
| 1996 | .0942 (1.57) | -.0709 (-1.17) | -.1082 (-1.14) | .1279 (1.23) |
| 2nd quarter | .2622 (3.89)*** | -.0618 (-0.97) | -.1167 (-0.97) | .3672 (2.61)*** |
| 3rd quarter | .2435 (3.65)*** | -.2780 (-3.88)*** | .0127 (0.12) | .3744 (2.72)*** |
| 4th quarter | .1093 (1.52) | -.3915 (-5.04)*** | .1730 (1.78)* | .1888 (1.22) |

Note: ***, **, * indicate statistical significance at the 1%, 5%, and 10% level. z-statistics in parantheses.

Source: IABES2.

Table 8: Average effects on the probabilities of being in different labor market states for various outcome months

| Outcomes: | Effect (percentage points): | | | | | |
|--------------------|-----------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | t = 3 | t = 6 | t = 12 | t = 24 | t = 36 | t = 48 |
| Regular Employment | -1.8 (-4.2;0.6) | -1.3 (-4.1;1.6) | 2.2 (-0.9;5.3) | 4.2 (1.0;7.4) | 1.2 (-2.0;4.5) | 1.4 (-1.8;4.7) |
| TWA Employment | 64.2 (61.9;66.5) | 44.5 (42.1;47.0) | 30.6 (28.3;33.0) | 18.9 (16.8;21.0) | 15.2 (13.2;17.1) | 13.5 (11.6;15.3) |
| Any Employment | 60.5 (57.9;63.1) | 42.0 (39.1;45.0) | 32.1 (29.0;35.2) | 22.7 (19.5;25.9) | 15.8 (12.7;19.0) | 14.4 (11.2;17.6) |
| Unemployment | -15.6 (-18.3;-12.9) | 0.9 (-1.9;3.6) | 3.7 (1.0;6.4) | 3.6 (0.9;6.3) | 0.8 (-1.8;3.5) | -0.6 (-3.2;2.0) |

Note: 95% confidence intervals are reported in parantheses.

Source: IABES2.

Table 9: Estimated probabilities for different outcomes and control groups by elapsed unemployment duration

| Outcomes: | Average probability (percent): | | | | | |
|--------------------|--------------------------------|---------------------|---------------------|---------------------|---------------------|---------------------|
| | u = 1-12 | u = 1 | u = 3 | u = 6 | u = 9 | u = 12 |
| Regular Employment | 37.7 (35.5;40.0) | 45.5 (38.7;52.4) | 41.3 (35.3;47.3) | 34.8 (26.8;42.7) | 32.1 (20.3;43.9) | 38.3 (25.7;50.9) |
| TWA Employment | 3.2 (2.4;4.0) | 3.1 (0.7;5.5) | 3.1 (1.0;5.2) | 4.0 (0.7;7.2) | 1.7 (-1.6;4.9) | 3.5 (-1.3;8.2) |
| Any Employment | 40.9 (38.6;43.1) | 48.5 (41.6;55.4) | 44.4 (38.4;50.5) | 38.6 (30.5;46.7) | 33.7 (21.8;45.7) | 41.6 (28.8;54.4) |
| Unemployment | 21.9 (20.0;23.8) | 21.4 (15.8;27.1) | 23.2 (18.0;28.3) | 21.1 (14.3;27.9) | 23.5 (12.7;34.2) | 18.7 (8.6;28.8) |

Note: 95% confidence intervals are reported in parantheses. Individual columns do not have to add up to 1.

Source: IABES2.

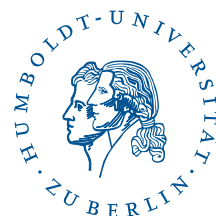
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