Labor Market Dynamics and Policy Evaluation: Empirical Evidence from Micro Data

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To my parents

and my partner Gerard

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Contents

Chapter 1

Introduction

1.1 Introduction – English Version

The labor market reform proposals by the Hartz commission have put the topic of the flexibility of the German labor market again into the center of the political debate. In this thesis different aspects of labor market flexibility are analyzed. In lines with the question of labor market flexibility it is focused on dynamic aspects in the employment trajectories of individuals which are investigated in three separated studies.

First, by looking at changes in the stability of employment relationships it is studied whether employment relationships became more flexible.

In recent years, a perceived decrease in the stability of employment relationships has been intensively discussed. On the one hand, concerns about a potential decline in the duration of employment relationships have been raised with respect to the consequences on the individual careers paths and the human capital accumulation. For example, many job switches and interrupting unemployment spells might have negative effects on the individual careers. Depreciation of human capital and negative signals to potential future employers might lead to losses in earnings potential and difficulties in job search. In addition, from a macroeconomic point of view, a decline in the duration of employment relationships might threaten the foundations of a highly educated workforce, as it is sometimes mentioned in connection with the German apprenticeship system.

On the other hand, an increase in the flexibility of employment relationships might be part of a necessary adjustment process to structural changes. It is not clear whether this adjustment has already proceeded to a sufficient degree in European countries. In the context of these considerations concerning the evolution of the stability of employment relationships a careful empirical analysis of the development of the duration of employment relationships is necessary, where business cycle effects are separated from a potential secular trend.

In view of a potential decline in the stability of employment relationships it is conceivable to suggest additional governmental support on the individual level in order to overcome dips in the employment situation. An outstanding example is the case of East Germany where support was massively offered to smooth the consequences of the transformation process after the reunification.

Active Labor Market Policy, like training programs and job creation scheme, might provide help in this context. Additionally, they might also increase the flexibility of the labor market, especially in the presence of minimum wages.

Unemployment might occur in the presence of minimum wages due to an insufficient level of human capital. In this case is the individual marginal productivity lower than the minimum wage. Programs of Active Labor Market Policy, like training programs and job creation schemes, try to counteract here. Their intention is to compensate a lack of human capital which can result from different origins. For example, human capital might be depreciated due to structural changes or due to long time of unemployment. But also an initial low level of professional education could be responsible.

Consequently, these programs try to provide human capital, where training programs mainly focus on cognitive skills whereas job creation schemes focus on noncognitive skills. There are also additional channels how these programs might increase the employment chances. For example, participation in these program might raise the attachment of the participants with the labor market or successful participation might send positive signals to potential future employers about the learning capabilities.

In the following, Chapter 2 contains an in–depth empirical analysis of the duration of employment relationships (here also called job duration or job stability) for 1980's and 1990's in West Germany. A descriptive analysis of the evolution of elapsed job duration of those currently in work is conducted based on data of the German Socio-Economic Panel (GSOEP) 1984-1999.

In addition, the evolution of the completed job duration is analyzed. A competing risk Cox Proportional Hazard Rate model is estimated. It is distinguished between different reasons for ending a job and different exit states. This way, not only a secular trend can be identified but also potential reasons for the observed changes in job stability can be empirically investigated.

The results show that job stability of men declined. Part of this can be attributed to an increase in layoffs and part to an increase in transitions to unemployment. However, these two developments are not significantly related to each other. For women no significant change in job stability is found. Some evidence is presented that downsizing of large firms might be responsible for part of the decline in job stability for men, whereas no significant impact of skill–biased technological change on job stability or evidence for a general weakening of the attachment between firms and their employees is found.

This chapter 2 is based on Bergemann and Mertens (2004). Following Bergemann and Mertens (2001) the empirical approach with respect to the completed job duration analysis is described in more detail. In addition, for both measures of job duration sensitivity analyzes are included. The sensitivity analyzes are taken from Bergemann and Mertens (2001) where here new estimations for the completed job duration are presented. Furthermore, it is commented on similarities and differences in the results and approaches of the main and the sensitivity analyzes.

Chapter 3 and 4 are evaluation studies for two different Active Labor Market programs, namely for training programs and job creation schemes. It is investigated whether these programs increase the flexibility on the labor market by helping the participants finding a job and/or remaining employed.

Chapter 3 evaluates the effects of training programs in East Germany on the employment chances of the actual participants for the time period 1990-1999. In course of the economic transformation process after the reunification the human capital of the East German labor force was heavily depreciated, while the wages were set on a quite high level. In order to fight the high unemployment occurring shortly after the reunification the training programs were designed to provide skills which were in demand in a market economy but not sufficient in supply due to the former educational system.

In order to evaluate the effects of training programs on the employment chances of the actual participants chapter 3 develops a novel evaluation approach. This approach builds upon a dynamic employment model which takes account of an important stylized fact concerning the employment probability. One can commonly observe that the probability to be employed in the next period is higher in case a person is currently employed than if s/he were currently unemployed. On the basis of this dynamic employment model the treatment–on–the–treated effect can be estimated with an innovation of this paper. A conditional difference–in–differences in hazard rate estimator is developed as an extension of the conditional difference–in–differences estimator, which is a popular evaluation method usually applied to employment rates or earnings. The conditional difference–in–differences in hazard rate estimator assesses the treatment effects separately for the different transition rates, here the reemployment rate and the rate to remain employed. The results of this estimator are contrasted with evaluation results of a conditional difference–in–differences estimator on unconditional employment rates, where – as commonly done – state dependence is not taken into account. Additionally, a sensitivity analysis is conducted in order to compare the results of the conditional difference–in–differences in hazard rate estimator with a further way to model state dependency.

Especially in East Germany unemployed often do not participate only once in a program of Active Labor Market Policy but several times. Potential complementary effects which can occur in the presence of multiple participation are often ignored in evaluation studies. Here the effect of a training as the first participation in a program of Active Labor Market Policy is estimated. In addition, it is differentiated between different treatment sequences where the incremental and combined effect of multiple participation is evaluated. The results are estimated on the basis of survey data of the Labor Market Monitor of the state of Sachsen–Anhalt. This data set is unique in the sense that it offers a monthly employment calendar for the years 1990-1999.

With regard to the transition rates it is found that the employment effects of participation in a first training program are mostly insignificant but that there are some significantly positive effects for selected starting dates of training programs. In contrast, with respect to unconditional employment rates the results show significantly negative effects. Combined sequences of two programs with a first training program are not successful with respect to the transition rates whereas the incremental effect of the second treatment appears to have slightly positive effects on the probability to remain employed.

In the sum, the results of chapter 3 indicate that modeling transition rates is more appropriate than using unconditional employment rates. Using only employment rates as success criterion could result in misleading conclusions concerning the effectiveness of programs of Active Labor Market Policy. The results also show that modeling transition rates is more informative as it is possible to deduce whether programs rather help to find a job and/or whether they rather stabilize employment. Furthermore, the results reveal the importance to take into account the time the training program took place as the results show significant variations over time concerning the outcome variables which corresponds to institutional changes.

This chapter 3 is based on a study of Bergemann, Fitzenberger and Speckesser (2004). It is enriched by a sensitivity analysis with respect to a different approach to take account of state dependence in the outcome variable. This sensitivity analysis is part of Bergemann, Fitzenberger and Speckesser (2001). The results of the two approaches to take account of state dependency are compared.

Job creation schemes are also heavily used as programs of Active Labor Market Policy in East Germany. Job creation schemes intend to created additional temporary jobs mainly in the public or non–profit sector for the time of the subsidy. Chapter 4 evaluates the effects of job creation schemes for the time 1990–1999 on the employment chances on the basis of the Labor Market Monitor Sachsen–Anhalt.

The individual treatment effects are estimated for two different groups, the group of participants and a group which consists of the labor force which was hit fully by the transformation shock, including participants. By estimating the average employment effects for this second group the size of the treatment effect is assessed for the case that the group of participants would change. Thus, potential substitution and displacement effects are not taken into account.

Population average treatment effects, as done here for the specific group of the labor force in East Germany, are rarely estimated as it is difficult to set the hypothetical treatment time of nonparticipants and to estimate the employment effects conditional on the treatment times. However, such an approach is necessary, especially in the context of East Germany where the institutional arrangements changed strongly over the period considered.

Here, a solution in the context of the conditional difference–in–differences in hazard rate estimator is proposed. First, the hypothetical start dates for the nonparticipants are distributed over the whole time range 1990–1999, where the influence of time invariant individual characteristics on the starting date is taken into account. Secondly, when estimating the population average treatment effect provisions are made in order to consider that individuals start a program at different points in time.

The results indicate zero to positive effects on the reemployment probability and mainly significantly positive effects on the probability to remain employed for the actual participants. The effects are larger for programs that start later in time. The estimated population average treatment effect does not deviate strongly from the estimated treatment–on–the–treated effect. Thus, it is concluded that a change of the participation group towards the average population would not influence strongly the size of employment effects. As the treatment effects vary with the time the programs started, the results confirm the relevance of the explicit modeling of the starting date of the program for the nonparticipants and taking account of the influence of the start date on the employment effect.

1.2 Einleitung – Deutsche Version

Die Vorschläge zur Reform des deutschen Arbeitsmarktes durch die Hartz–Kommission hat das Thema der Flexibilität des deutschen Arbeitsmarktes wieder in den Mittelpunkt der politischen Diskussion gerückt. Die vorliegende Dissertation beleuchtet verschiedene Aspekte der Arbeitsmarktflexibilität. Im Einklang mit der Themenstellung findet hier in drei – auch unabhängig voneinander zu sehenden – Studien die Bedeutung der Dynamik von Beschäftigungsverläufen besondere Berücksichtigung.

Die erste Studie beschäftigt sich mit der Frage, ob Veränderungen bezüglich der Stabilität von Beschäftigungsverhältnissen eingetreten sind. Hierdurch lassen sich Rückschlüsse auf eine potentielle Flexibilisierung von Beschäftigungsverhältnissen ziehen.

In jüngster Vergangenheit wurde die Bedeutung eines möglichen Rückgangs der Stabilität von Beschäftigungsverhältnissen verstärkt diskutiert. Einerseits wurden Bedenken geäußert, dass sich ein Rückgang der Dauer von Beschäftigungsverhältnissen negativ auf die individuellen Beschäftigungsverläufe und Akkumulation von Humankapital auswirken könnte. Genannt werden beispielsweise häufiger Stellenwechsel und zwischenzeitliche Arbeitslosigkeitsperioden, die zu einer Abwertung von Humankapital und nachteiligen Signalen an potentielle Arbeitgeber führen könnten. Hiermit wären Einkommensverluste und Schwierigkeiten bei der Arbeitssuche verbunden. Darüber hinaus könnten mit einem Rückgang der Dauer von Beschäftigungsverhältnissen die Grundlagen für ein gesamtgesellschaftlich hohes Qualifikationsniveau gefährdet werden. Dieses Argument wird häufig im Zusammenhang mit dem deutschen System der Lehrlingsausbildung angeführt.

Andererseits könnte eine Flexibilisierung von Beschäftigungsverhältnissen einen Teil eines notwendigen Anpassungsprozesses im Rahmen des Strukturwandels darstellen. In dieser Sichtweise stellt sich insbesondere für europäische Volkswirtschaften die Frage, ob eine entsprechende Anpassung bereits in hinreichendem Maße vorangeschritten ist.

Vor dem Hintergund dieser Überlegungen ist eine sorgfältige empirische Analyse der Entwicklung der Dauer von Beschäftigungsverhältnissen empfehlenswert, bei der insbesondere zwischen zyklischen Effekten und Trends unterschieden wird.

Um die negativen Wirkungen eines Rückgangs der Stabilität von Beschäftigungsverhältnissen für die einzelnen Betroffenen abzufedern, wäre es denkbar, dass verstärkt staatliche Unterstützung angeboten wird. Ostdeutschland ist hierfür ein Extrembeispiel. Im Rahmen des Transformationsprozesses nach der Wiedervereinigung wurde hier massiv staatliche Unterstützung geleistet.

Aktive Arbeitsmarktpolitik, wie Weiterbildungsmaßnahmen und beschäftigungsschaffende Maßnahmen, wären mögliche Instrumente für eine derartige Intervention. Darüber hinaus könnten, sie insbesondere bei der Existenz von Mindestlöhnen, zur Flexibilisierung des Arbeitsmarktes beitragen.

Mindestlöhne können zu Arbeitslosigkeit führen, falls Individuen nur über ein geringes Niveau an Humankapital verfügen. In diesem Fall ist die individuelle Grenzproduktivität niedriger als der Mindestlohn. Instrumente der aktiven Arbeitsmarktpolitik, wie Weiterbildungsmaßnahmen und beschäftigungsschaffende Maßnahmen, versuchen hier entgegenzuwirken. Ihre Zielsetzung besteht in der Kompensation der Humankapitallücke, welche unterschiedlicher Ursache sein kann. Beispielsweise kann Humankapital durch Strukturwandel oder Arbeitslosigkeitsperioden abgewertet werden. Auch eine unzureichende berufliche Erstausbildung kann hierfür verantwortlich sein.

Weiterbildungsmaßnahmen und beschäftigungsschaffende Maßnahmen versuchen daher Kenntnisse zu vermitteln, die die Produktivität der Teilnehmer erhöhen. Dabei zielen Weiterbildungsmaßnahmen im Wesentlichen auf kognitive und beschäftigungsschaffende Maßnahmen auf nonkognitive Kenntnisse ab. Auch weitere Kanäle, durch die die Maßnahmen die Beschäftigungsfähigkeit der Teilnehmer erhöhen können, sind denkbar. Beispielsweise, könnte durch sie die Arbeitsmarktbindung des Teilnehmers erhöht werden oder aber die erfolgreiche Teilnahme an einer Weiterbildungsmaßnahme Lernfähigkeit signalisieren.

Im Folgenden beinhaltet Kapitel 2 eine tiefgehende empirische Analyse der Entwicklung der Dauer von Beschäftigungsverhältnissen in Westdeutschland, wobei im weiteren vereinfachend von Beschäftigungsdauer oder Beschäftigungsstabilität gesprochen wird.

Eine deskriptive Analyse der Entwicklung der bisherigen Beschäftigungsdauer wird auf Basis von Daten des Sozio-Oekonomischen Panels für die Zeit von 1984 bis 1999 erstellt. Das Hauptaugenmerk des Kapitels 2 liegt auf einer Analyse der abgeschlossenen Beschäftigungsdauer für die Zeit von 1984 bis 1997. Ein proportionales Cox–Hazardraten–Modell mit konkurrierenden Risiken wird geschätzt, wobei unterschieden wird zwischen den verschiedenen Gründen für die Beendigung eines Beschäftigungsverhältnisses und den verschiedenen Übergangszuständen. Diese Vorgehensweise ermöglicht nicht nur einen potentiellen Trend sondern auch Gründe für die Veränderung der Beschäftigungsstabilität zu identifizieren.

Die Ergebnisse zeigen, dass die Beschäftigungsstabilität von Männern abgenommen hat. Ein Teil dieser Entwicklung ist vermehrten Entlassungen zuzuschreiben, ein weiterer Teil vermehrten Übergängen in Arbeitslosigkeit, wobei diese Entwicklungen nicht miteinander verbunden sind. Hingegen sind für Frauen keine signifikanten Veränderungen zu verzeichnen. Des Weiteren sind Hinweise zu finden, dass insbesondere die Entlassungen in großen Firmen eine bedeutende Quelle für den Rückgang der Beschäftigungsstabilität von Männern darstellen. Technischer Fortschritt, der Qualifizierte und Hochqualifizierte begünstigt, scheint hier keinen besonderen Einfluss auszuüben. Ebenso gibt es keine Hinweise für eine generelle Schwächung der Bindung zwischen Arbeitnehmer und Arbeitgeber.

Kapitel 2 basiert auf einer gemeinsamen Arbeit von Bergemann und Mertens (2004). In der vorliegenden Arbeit wird jedoch der empirische Ansatz für die Analyse der abgeschlossenen Beschäftugungsdauer in Anlehnung an Bergemann und Mertens (2001) detaillierter beschrieben. Darüber hinaus sind für die beiden Maßeinheiten der Beschäftigungsdauer Sensitivitätsanalysen hinzugefügt. Die Sensitivitätsanalysen sind der Studie von Bergemann und Mertens (2001) entnommen, wobei neue Schätzungen für die abgeschlossene Beschäftigungsdauer durchgeführt wurden. Auch werden zusätzlich Übereinstimmungen und Unterschiede zwischen der Hauptanalyse und den Sensitivitätsanalysen beschrieben.

Kapitel 3 und 4 umfassen Evaluationsstudien von zwei unterschiedlichen Instrumenten der aktiven Arbeitsmarktpolitik, namentlich Weiterbildungsmaßnahmen und beschäftigungsschaffende Maßnahmen. Hierbei wird untersucht, ob diese Programme zur Erhöhung der Arbeitsmarktflexibiliät beitragen, indem sie den Teilnehmern helfen, eine Beschäftigung zu finden und erwerbstätig zu bleiben.

Kapitel 3 evaluiert die Wirkung von Weiterbildungsmaßnahmen in Ostdeutschland für die Zeitraum von 1990 bis 1999 im Hinblick auf eine Erhöhung der Erwerbschancen für die tatsächlichen Teilnehmer. Das Humankapital der ostdeutschen Erwerbsbevölkerung war durch den Transformationsprozess in Folge der Wiedervereinigung einer starken Abwertung unterworfen. Gleichzeitig lagen die Löhne auf einem relativ hohen Niveau. Die hohe Arbeitslosigkeit in Ostdeutschland lässt sich zumindest teilweise auf diesen Zusammenhang zurückführen. Um dem entgegenzuwirken wurden im großen Stil Weiterbildungsmaßnahmen eingesetzt, die marktgängiges Wissen vermitteln sollten.

Um die Wirkung von Weiterbildungsmaßnahmen auf die Erwerbschancen der tatsächlichen Teilnehmer evaluieren zu können, entwickelt Kapitel 3 einen neuartigen Evaluationsansatz. Dieser Ansatz baut auf ein dynamisches Beschäftigungsmodell auf, welches einem wichtigen stilisierten Faktum in Bezug auf Beschäftigungswahrscheinlichkeit Rechnung trägt. Im Allgemeinen kann man beobachten, dass die Wahrscheinlichkeit morgen erwerbstätig zu sein größer ist, im Falle man ist heute erwerbstätig anstelle man ist nicht erwerbstätig. Auf Basis des dynamischen Beschäftigungsmodells kann die durchschnittliche Teilnahmewirkung auf die tatsächlichen Teilnehmer mit einer Innovation dieses Kapitels geschätzt werden. Ein konditionaler Differenz–von–Differenzen in Hazard Raten Schätzer wird eingeführt als Weiterentwicklung des konditionalen Differenz–von–Differenzen Schätzers. Der konditionale Differenz–von–Differenzen Schätzer ist ein beliebter Schätzansatz, der üblicherweise in Bezug auf unbedingte Erwerbstätigkeitsraten oder Löhne angewandt wird. Der konditionale Differenz–von–Differenzen in Hazard Raten Schätzer hingegen untersucht die Teilnahmewirkung auf Übergangsraten. In dieser Studie finden die Wiederbeschäftigungsrate und die Verbleibsrate in Erwerbstätigkeit Berücksichtigung. Die mit diesem Ansatz erzielten Ergebnisse werden den Ergebnissen auf Basis von unbedingten Erwerbstätigkeitsraten gegenüber gestellt, bei denen, wie es üblicherweise geschieht, die Zustandsabhängigkeit nicht berücksichtigt wird. Zusätzlich wird eine Sensitivitätsanalyse durchgeführt, die die Ergebnisse des konditionalen Differenz–von–Differenzen in Hazard Raten Schätzers mit einer weiteren Möglichkeit vergleicht, die Zustandsabhängigkeit zu berücksichtigen.

Insbesondere in Ostdeutschland lässt sich beobachten, dass Arbeitslose häufig nicht nur einmal an einem Programm der aktiven Arbeitsmarktpolitik teilnehmen, sondern mehrmals. Evaluationsstudien ignorieren in der Regel potentielle Komplementärwirkungen, die sich hieraus ergeben. Demgegenüber wird in Kapitel 3 nicht nur die Wirkung einer Teilnahme an einer Weiterbildungsmaßnahme als erste Teilnahme an einem Programm der aktiven Arbeitsmarktpolitik geschätzt, sondern auch verschiedene Teilnahmesequenzen unterschieden, deren inkrementelle und kombinierte Wirkung geschätzt wird. Die Evaluationsstudie verwendet dabei Umfragedaten des Arbeitsmarktmonitors Sachsen-Anhalt. Dieser Datensatz ist einzigartig, da er einen Kalender enthält, dem die Erwerbszustände der Befragten für den Zeitraum von 1990 bis 1999 auf monatlicher Basis entnommen werden können.

Die Ergebnisse weisen vorwiegend auf eine insignifikante Wirkung einer ersten Teilnahme an einer Weiterbildungsmaßnahme auf die Übergangsraten hin, wobei jedoch für einzelne Eintrittszeitpunkte signifikante positive Wirkungen vorzufinden sind. Die Ergebnisse bezüglich der unbedingte Erwerbstätigkeitsrate legen hingegen eine negative Wirkung der Weiterbildungsmaßnahmen nahe. Die kombinierte Wirkung einer Mehrfachteilnahme mit einer ersten Teilnahme an einer Weiterbildungsmaßnahme, hat keine signifikante Wirkung auf die Übergangsraten. Die inkrementelle Wirkung der zweiten Teilnahme auf die Übergangsraten scheint hingegen positiv zu sein.

Bei einer Gesamtbetrachtung der Ergebnisse des 3. Kapitels kann der Schluss gezogen werden, dass es angemessener ist, die Programmteilnahme im Hinblick auf ihre Wirkung auf Übergangsraten als auf unbedingte Erwerbstätigkeitsraten zu evaluieren. Falls ausschließlich unbedingte Erwerbstätigkeitsraten als Erfolgskriterium herangezogen werden, besteht die Gefahr von irreführenden Schlussfolgerungen bezüglich der Effizienz von aktiver Arbeitsmarktpolitik. Die Ergebnisse zeigen auch die Erkenntnisgewinne, die die Modellierung von Übergangsraten mit sich bringen. Es kann bestimmt werden, ob die Programme eher helfen eine Beschäftigung zu finden, und/oder ob sie eher helfen erwerbstätig zu bleiben. Darüber hinaus zeigen die Ergebnisse die Bedeutung auf, den Zeitpunkt des Eintritts in die Maßnahme zu berücksichtigen, da die Wirkung diesbezüglich Variationen, die mit institutionellen

Änderungen der Programme zusammen hängen könnten, aufweist.

Das Kapitel 3 basiert auf einer Studie von Bergemann, Fitzenberger und Speckesser (2004). Es ist erweitert um eine Sensitivitätsanalyse, bei der ein zusätzlicher Ansatz zur Berücksichtigung der Zustandsabhängigkeit in der Erwerbstätigkeitsrate geschätzt wird. Diese Sensitivitätsanalyse ist Teil von Bergemann, Fitzenberger und Speckesser (2001). Die Ergebnisse der zwei unterschiedlichen Wege zur Berücksichtigung von Zustandsabänigkeit werden miteinander verglichen.

Beschäftigungsschaffende Maßnahmen sind ein weiteres Instrument der aktiven Arbeitsmarktpolitik, das im starken Maße in Ostdeutschland eingesetzt wird. Diese Maßnahmen beabsichtigen zusätzliche, zeitlich befristete Beschäftigung zu schaffen. Kapitel 4 evaluiert die Wirkung dieser Maßnahmen für den Zeitraum von 1990 bis 1999 im Hinblick auf eine Erhöhung der Beschäftigungschancen auf Basis der Daten des Arbeitsmarktmonitors Sachsen-Anhalts. Die durchschnittliche Teilnahmewirkung wird für zwei unterschiedliche Gruppen geschätzt: die Gruppe der tatsächlichen Teilnehmer und die Gruppe der erwerbsfähigen Bevölkerung, welche dem Transformationsschock unterlegen war. Indem die durchschnittliche Wirkung einer (potentiellen) Teilnahme der Gruppe der erwerbsfähigen Bevölkerung auf deren Erwerbschancen geschätzt wird, wird untersucht wie sich die durchschnittliche Teilnahmewirkung bei einer Modifikation der Teilnehmergruppe verändern würde. Angesichts dieser Fragestellung werden keine Substitutions- oder Verdrängungswirkungen berücksichtigt.

Durchschnittliche Teilnahmewirkungen für eine Grundgesamtheit, die über die Teilnehmer hinausgeht, sind bislang selten geschätzt worden. Die Ursache mag in den Problemen zu finden sein, die die Setzung von hypothetischen Eintrittszeitpunkten, sowie die Schätzung der Teilnahmewirkung in Abhängigkeit von diesen Eintrittszeitpunkten mit sich bringen. Jedoch ist ein solcher Ansatz insbesondere für Ostdeutschland notwendig, da sich wesentliche Änderungen bei den Teilnahmeregelungen der beschäftigungsschaffenden Maßnahmen ergeben haben.

Eine Lösung dieses methodischen Problems wird hier im Kontext des konditionalen Differenz–von–Differenzen in Hazard Raten Schätzers vorgeschlagen. Zuerst werden die hypothetischen Eintrittszeitpunkte für die Nichtteilnehmer über den Zeitraum von 1990 bis 1999 verteilt, wobei der Einfluss von zeitlich konstanten individuellen Eigenschaften auf die Eintrittszeitpunkte berücksichtigt wird. Zweitens werden bei der Schätzung der Teilnahmewirkung Vorkehrungen getroffen, um den Einfluss des Eintrittszeitpunktes in angemessenem Maße zu berücksichtigen.

Für die Gruppe der tatsächlichen Teilnehmer zeigen die Ergebnisse eine bedingt positive Wirkung auf die Wiederbeschäftigungswahrscheinlichkeit und eine signifikant positive Wirkung auf die Verbleibswahrscheinlichkeit in Erwerbstätigkeit. Im Allgemeinen ist die Wirkung von Programmen, die zu einem späteren Zeitpunkt begonnen haben größer. Die geschätzte Teilnahmewirkung der Gruppe der erwerbsfähigen Bevölkerung weicht nicht stark von der geschätzten Teilnahmewirkung auf die tatsächlichen Teilnehmer ab. Somit wäre von einer Variation der Teilnahmergruppe keine wesentliche Veränderung der durchschnittlichen Teilnahmewirkung zu erwarten. Das Ergebnis, dass die Teilnahmewirkung im Hinblick auf den Eintrittszeitpunkt variiert, bestätigt die Bedeutung der expliziten Modellierung des Eintrittszeitpunkts für die Nichtteilnehmer sowie die Berücksichtigung dieser bei der Schätzung der Teilnahmewirkung.

Chapter 2

Job Stability Trends, Layoffs, and Transitions to Unemployment – An Empirical Analysis for West Germany

2.1 Introduction

In the recent past the question of job security and the stability of employment relationships has been increasingly discussed. The general notion is that job stability is on the decline in most OECD countries, although the actual empirical evidence is scarce and ambiguous. Several studies for Germany and other OECD countries like the United States, the United Kingdom and France show some limited evidence of increasing job instability.[1]

It is well known to economists that job stability is not necessarily always a good thing. Indeed, if we believed that separations were always efficient (see e.g. McLaughlin 1991 or Parsons 1986), there would be nothing to worry about. However, most contracting mechanisms – like the bonding schemes or fixed wage contracts in the early models by Oi (1962), Becker (1962) and Parsons (1972), for example, or the later models with costly or suppressed renegotiations of wages by Hashimoto (1981) and Hall and Lazear (1984) – yield separations that are not efficient. Too much job stability can in fact be harmful at the macroeconomic level; for example, if firms have difficulties restructuring their workforce in times of structural change. Indeed, job relationships in Europe have often been termed too inflexible,

[1] See Bergemann and Schneider 1998; Burgess and Rees 1996, 1997, 1998; Booth et. al. 1999; Swinnerton and Wial 1995; Diebold et al. 1997; Schmidt and Svorny 1998; Givord and Maurin 2004; and the articles in the Journal of Labor Economics 17 (4, part 2) and in Neunmark 2000; OECD 1997; ILO 1996.

as have the labor market institutions themselves; this phenomenon is sometimes dubbed *Eurosclerosis.* Even at the individual level, job stability is not always desirable, as reflected by voluntary quits.

One major reason for some economists to be concerned about declines in job stability is the potential effect on individual career paths. Too many job switches and periods of unemployment may lead to losses in human capital, decreasing earnings potentials and a limited capability to obtain work due to disadvantage signals (Spence 1973). Furthermore, economy–wide long–term labor relationships might be one prerequisite for a highly educated workforce (Acemoglu and Pischke 1998), which is, in turn, amongst other things responsible for the economic success of a country. Therefore, it is important to interpret potential declines carefully. The question of whether jobs terminate due to quits or layoffs is of particular relevance, and is too often neglected in the literature. While quits tend to be associated with improved job conditions elsewhere, permanent layoffs and worker displacement usually lead to at least temporary unemployment and frequently to wage losses upon re–employment. In Germany the latter problem is of less relevance than in the United States, as wage losses tend to be relatively small (compare Burda and Mertens 2001; Dustmann et al. 2002). After looking at job stability patterns, we will therefore examine whether individuals find another job immediately after leaving their old one. Furthermore, we will test a number of hypotheses that have been put forward as potential explanations for a decline in job stability: downsizing of firms, skill–biased technological change, weakening bonds with the firm, and flexible work arrangements. Finally, another important question is how to measure a potential decline in job stability properly. We will address these questions using data from the German Socioeconomic Panel (hereafter GSOEP).

After surveying very briefly the literature on job stability and describing the data set, we will proceed to a detailed analysis of elapsed firm tenure. The elapsed firm tenure of those currently in work is the most commonly used measure of job stability and has not yet been properly explored for Germany. The GSOEP data show that there was indeed some decline in elapsed tenure in West Germany during the 1980s and 1990s. However, one serious problem of elapsed tenure is that it does not take into account the problem of right censoring. We do not know how long the jobs will actually last. This is a particular problem in times when many new hires are made. Logically, we will observe a decline in average elapsed tenure in that case. To combat the problem of right censoring, we use competing risk hazard rate models in our major analysis of separation risks in section 2.5. These models show that the decline in job stability can be attributed primarily to an increase in layoffs and transitions to unemployment. Finally, in section 2.6 we summarize our findings and give an outlook for future research.

2.2 The Literature

Although the literature on job stability is primarily empirical,[2] it is firmly based on well–known theories describing mobility in the labor market. Human capital theory offers an explanation for why separations usually decline with labor market experience and tenure (see e.g. Becker 1962, Mincer 1962 and 1974, Oi 1962, Parsons 1972 and Hashimoto 1981). Search and matching theory also concludes that mobility decreases with tenure and experience, as good matches are the ones that survive the longest and older workers have simply had more time to locate well–paid jobs (see e.g. Stigler 1962, Mortensen 1970, Burdett 1978, Jovanovic 1979a, 1979b). Therefore, it seems reasonable to use the elapsed tenure of those currently employed, i.e. the time spent with a particular employer, as a common measure of job stability. If we find a general tendency toward decreasing average tenure over time, this will be interpreted as an indication of declining job stability.

Most of the original US studies found little evidence for a drop in job stability between the 1970s and early 1990s (see Farber 1995; Diebold et al. 1996, 1997).[3] Only Swinnerton and Wial (1995, 1996) reported declines in job stability, although these declines were far smaller in their re–estimated results. Farber (1995) notes that men are increasingly less likely to be in long–term jobs, while women's chances of being in long–term employment have increased significantly. Moreover, Farber concludes that groups that have experienced greater declines in earnings, such as the young and especially the less educated, have also experienced a greater decline in job stability. Some more recent evidence of declining job stability is collected in an edited volume (Neumark 2000) and in a special issue of the Journal of Labor Economics (1999 [4, part 2]). In the latter, Neumark et al. (1999) report that job stability declined modestly in the first half of the 1990s. However, men with substantial tenure experienced a sharp decline in job stability in the first half of the 1990s. These results were confirmed by Jaeger and Stevens (1999), who show a declining proportion of workers with less than 10 years of tenure. However, Gottschalk and Moffitt (1999) found no such evidence when estimating Cox Proportional Hazard Rate Models for different demographic groups. Separation rates did not increase; on the contrary, the results indicate a *decline* in the hazard of job exits for white males of all educational levels. These findings persist even when considering only those workers who report involuntary job terminations. However, while there is evidence

[2]One exception being Valetta (1999b), who offers an implicit contract model to explain inefficient separations. In this model workers' job security declines if they are dismissed although they had reasonable expectations of not being dismissed.

[3]For overviews of the US literature see Schmidt and Svorny (1998) or Valetta (1999a). See also the special issue of the Journal of Labor Economics 17 (October 1999, part 2), where Gottschalk and Moffit present an interesting comparison of studies. The latest compilation of articles on the topic can be found in Neumark (2000). Comparable studies for Europe are only available for the UK (see e.g. Burgess and Rees 1996, 1997, 1998; Booth et al. 1999) and France (e.g. Givord and Maurin, 2004).

for some changes in job stability in the 1990s, these changes did not persist for very long and, as Neumark (2000) argues, it would be premature to infer long–term trends towards a decline in long–term employment relationships.

Apart from studies directly aimed at analyzing job stability patterns, the literature on worker displacement yields some further interesting insights. In the United States job losses have increased since the 1970s, with high–tenure and white–collar workers being increasingly affected (see Hamermesh 1989; Farber 1993, 1997; Hall 1995; Fallick 1996 and Kletzer 1998 for surveys). The figures reported by researchers such as Farber (1997) cast some doubt on the notion that job stability has not changed: some 13% of the workforce experienced job loss in the recession of 1981–1983. The three–year rate of job loss decreased until 1987–89, and then rose to its highest level since 1981: 15% of the workforce lost their job in the period of expansion between 1993 and 1995. As Kletzer (1998) puts it,"These high rates of job loss are consistent with public perceptions of rising job insecurity."

Previous studies of job stability in Germany have applied different measures, and yielded apparently conflicting results. Winkelmann and Zimmermann (1998) report decreasing numbers of job changes as evidence for an increase in job stability, whereas Bergemann and Schneider (1998) use descriptive duration analysis to show that job stability has declined. Likewise, Grotheer and Struck (2003) report that the percentage of short jobs increased in the 1990s. Prolonged periods of unemployment and nonparticipation might make explain these disparate results. With the present paper, we intend to give a detailed overview of the evolution of job duration. We start by presenting some statistics on elapsed tenure to gain a first insight into the information provided by the GSOEP data.

Even more tricky than analyzing the pattern of job stability is explaining the reasons for any changes observed. Several hypotheses have been formulated in the literature; these will be tested in our empirical analysis. First of all, the business cycle has a strong impact. We know that (voluntary) quits are pro–cyclical, while (involuntary) layoffs are counter–cyclical, both influencing the tenure distribution. In boom periods more new jobs are created, automatically leading to more jobs of short duration. Hence, tenure decreases, even if layoffs are reduced. In recessions there are more layoffs, fewer quits, and average tenure is likely to increase as new hires are rare (Burgess and Rees 1996, Schettkatt 1996). It is therefore vital to control for business cycle effects. Secondly, changes in the composition of the workforce may influence average job stability. We are able to control for such effects using the abundant information provided by the GSOEP data.

Apart from those two rather conventional hypotheses, the influence of flexible work arrangements (e.g. Levenson 2000), the downsizing of firms (Capelli 2000), skill–biased technological change (Givord and Maurin 2003; Autor et al. 2002) and weakening bonds with the firm (Valetta 1999b) will be discussed. We will test for all these hypotheses in our multivariate analysis in section 2.5. Before that, however, we will take a look at the data and some cross–sectional findings.

2.3 The Data Set

The GSOEP is a representative panel survey of households and their members, which has been running in West Germany since 1984. The concept of the GSOEP is to annually re–interview the households and their split–offs, usually in March.[4] In 1984, the sample consisted of approximately 4500 households and 9000 persons. The GSOEP questionnaire covers a wide variety of economic and social characteristics of households and their members. In particular, the occupational situation of the interviewees is one of the main themes. No other German data set provides this breadth of information, especially where the reasons for a job termination are concerned. Of course, this data set also has its limitations, which we will take into account in our empirical analysis as far as possible.

In the GSOEP, employed respondents are asked how long they have already been with their current employer. This information allows us to easily calculate our first major indicator *"median of elapsed tenure"*. However, extracting data on the length of job spells for the duration analysis is not as trivial as it might seem. Workers report changes in their employment situation together with the reasons for this change in the year before or during the year of the interview (starting with interview year 1985). Parallel to this, respondents complete a monthly calendar providing information on their labor market states.

There are two main ways of extracting job spells from this information: either by completely relying on the information on the reported changes in the employment situation or by combining this information with the monthly calendar. Appendix 2.D.1 contains a short description of the first approach. In our main analysis we decide to use as much information as possible by combing both data sources, although in this case we have to make specific setting if an individual reports contradictory information.[5] Combing these two data sources is preferable for two main reasons. First, when exclusively using the information on job changes one is only able to extract job spells which cover at least one interview date. Thus, one would loose information on a significant part of short job spells. Secondly, only the calendar offers information on the destination states after the end of a job.

[4]For further information about the GSOEP, see SOEP Group (2001).
[5]For a more extensive discussion, consult the Appendix 2.B.

After combining the two data sources, we add the information on the elapsed tenure to the job spell information in case a job spell is left censored, information provided mainly by way of the calendar. This results in a so–called *stock sample*, which must be taken into account in the duration analysis.

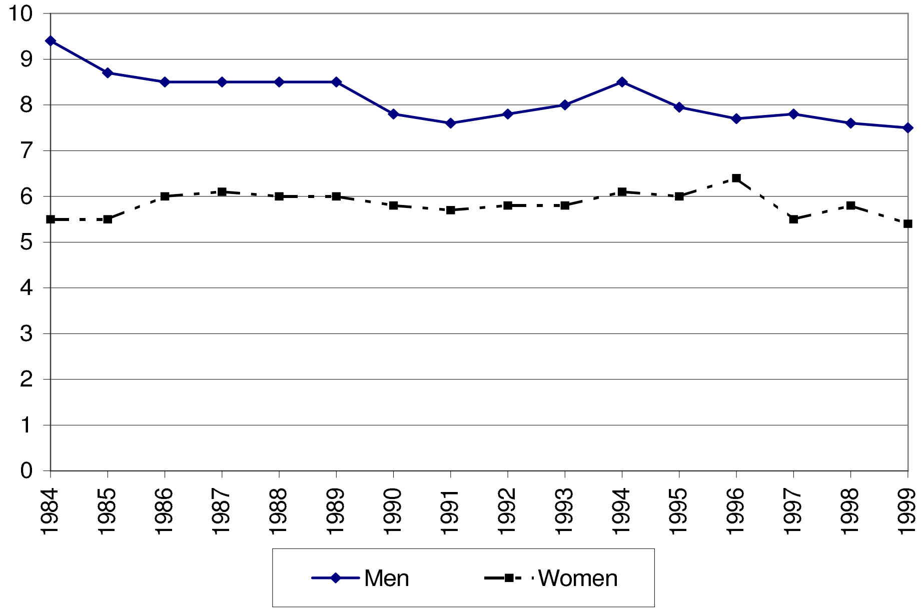
Having thus created a (job) spell data set, we add individual and job–specific information to each spell. The individual information is available for all job spells, whereas the job–specific information is only available if the respondent was actually in the job at the time of an interview. Consequently, we only have job–specific information on a representative basis for jobs lasting at least 12 months.

The analysis distinguishes two different sets of destination states. The first set relates to the reason for job termination: quits, which are initiated by the employee; layoffs, which are initiated by the firm; and a third category, termed 'other reasons', that includes such reasons as the end of a fixed–term contract and retirement. It also includes sabbatical leave and maternity leave if these result in termination of the job. The GSOEP only taps the reason for the termination of the job that ended closest to the interview date, even if the individual had more than one job between two interviews. Therefore, we are less likely to know the reasons for exits from short jobs. Finally, it is important to note that the questions tapping the reasons for job separation were changed in 1991. We conducted extensive checks, which confirmed that the variable we generated to reflect the reasons for job change is essentially consistent over time. However, there is one minor exception: in 1990, maternity leave as reason for job termination was introduced as an explicit response alternative. Therefore, we find an increase in exits in our variable *"other reasons"* as this variable contains job exits due to maternity leave. In order to correct for the effect of this change in the questionnaire, we incorporate a dummy variable in our multivariate analysis. Unfortunately, the wording of the question changed again after 1997, but now making it impossible to create a consistent variable. Therefore, our multivariate analysis only covers the 1990s until 1997.

In our second part of the analysis, we investigate a second set of destination states. These are the labor market states that an individual enters after exiting a job: unemployment, employment (full– or part–time) or education and nonparticipation combined. The data on these destination states are drawn from the monthly calendar. The same problem arises with respect to maternity leave as, here again, maternity leave was not introduced as an employment status in its own right until the 1991 questionnaire.

We selected the original West German sample A, containing German citizens only, for our analysis. We did not include East Germany, where job tenure patterns in the 1990s were obviously subject to different mechanisms than in West Germany. Only workers aged between 16 and 56 were included in the analysis of elapsed tenure, and only workers who started the job aged between 16 and 56 in the multivariate analysis.

Figure 2.1: Evolution of Median Elapsed Tenure by Gender (in Years)*



* Own calculations based on the GSOEP 1984–1999. Only German citizens living in West Germany (Sample A), aged 16–56, working full– or part–time. Excluding civil servants, apprentices and self–employed respondents, workers in agriculture, non–profit organizations and private households, and observations with missing values for age, sex, job status, industry affiliation or tenure.

Civil servants, apprentices and the self–employed were dropped from our sample, as were workers in agriculture, non–profit organizations and private households. Finally, we did not include workers with missing values for age, sex, job status, industry affiliation or tenure in the elapsed tenure analysis.[6]

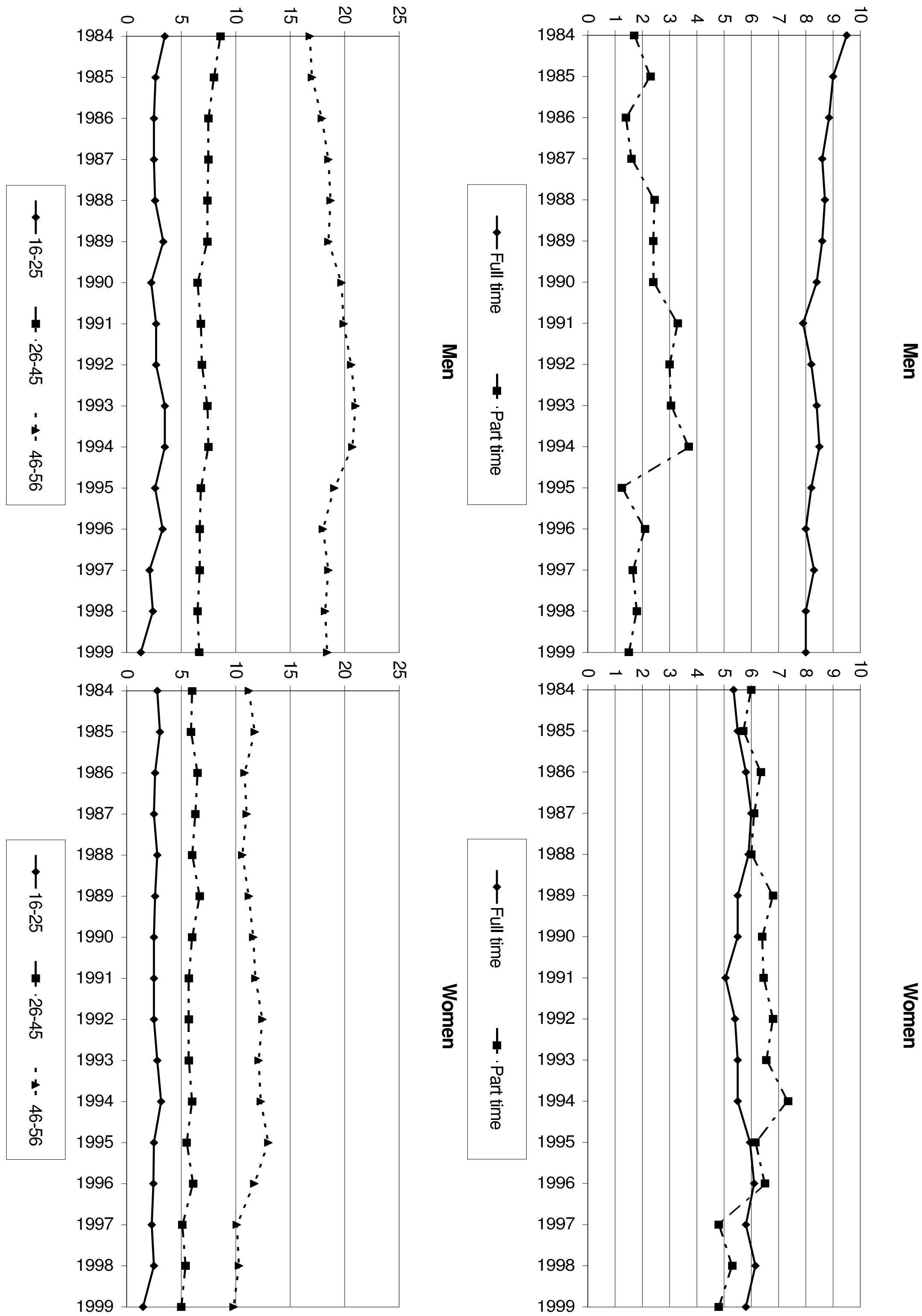
2.4 The Empirical Analysis of Elapsed Tenure

The results presented in Table 2.1 and Figure 2.1 substantiate the established fact that median elapsed tenure differs significantly by gender.[7] Women's median tenure fluctuates at around 6 years. Men's median tenure drops from 9.4 years in 1984 to 8.7 years in 1985, and stays relatively constant until the end of the 1980s. It declines further at the beginning of the 1990s, but returns to 8.5 years in 1994. Thereafter, median tenure falls to 7.5 years in 1999. Even if we assume that 1984 was a year with exceptionally high elapsed tenure, there are some signs of a decline in median elapsed tenure for men, primarily in the 1990s.

[6]For the missing value treatment in the duration analysis, see section 2.5.2

[7]Here, as in the following duration analysis, we do not use weights. We conducted, however, a sensitivity analysis for the elapsed tenure measure using the GSOEP weights for the time 1984–1997. Here we essentially receive the same results. Compare Appendix 2.C, where we also use a different selection rule with respect to age. Individuals up to age 65 are included.

Figure 2.2: Evolution of Median Elapsed Tenure by Age and Hours Worked (in Years)*



*Source: Own calculations based on the GSOEP 1984–1999. For sample selection, see note to Figure 2.1

It is also important to distinguish different age groups because older workers are obviously able to accrue longer tenure than young workers. Age is therefore used as a (non–ideal) proxy for labor market experience. In Figure 2.2, median elapsed tenure is reported for the groups aged 16–25, 26–45 and 46–56 years. A gender difference is evident only in the older age groups. The tenure of female workers aged between 16 and 25 is comparable to that of their male counterparts. What is noticeable, however, is that median elapsed tenure decreases slightly for the young and middle–aged men between 25 and 45 years of age, but increases substantially for males aged between 46 and 56 during the early 1990s before starting to fall again. The female experience is strikingly different from the results reported by Farber (1995), Marcotte (1995) and Burgess and Rees (1998) for the United States and the UK. It seems that, while women in the US and the UK have been able to accrue longer tenure over time, West German women have only been able to maintain the level acquired in the mid–1980s.

Another interesting detail in the tenure pattern has been pointed out by Gregg and Wadsworth (1995), who show significants differences in the median elapsed tenure of part–timers and full–timers in the UK. Moreover, part–timers face increasing separation rates over time. Therefore, Figure 2.2 shows median elapsed tenure by regular hours worked. Men in part–time jobs (including marginal employment)[8] have lower median elapsed tenure than full–time workers. However, only around 3% of male respondents were not in full–time jobs (own calculations from the GSOEP, see also Hoffmann and Walwei 1998).[9] As no clear pattern can be observed for part–time workers, the reported decrease in median elapsed tenure can be assumed to be caused by males in full–time work only. As expected, women are more likely to work part–time (around 30%). It is interesting to see that, for women, part–time work is in fact associated with slightly higher median tenure than full–time work – at least until 1996. Apart from that, there are no clear tendencies over time for females in either full–time or part–time work.

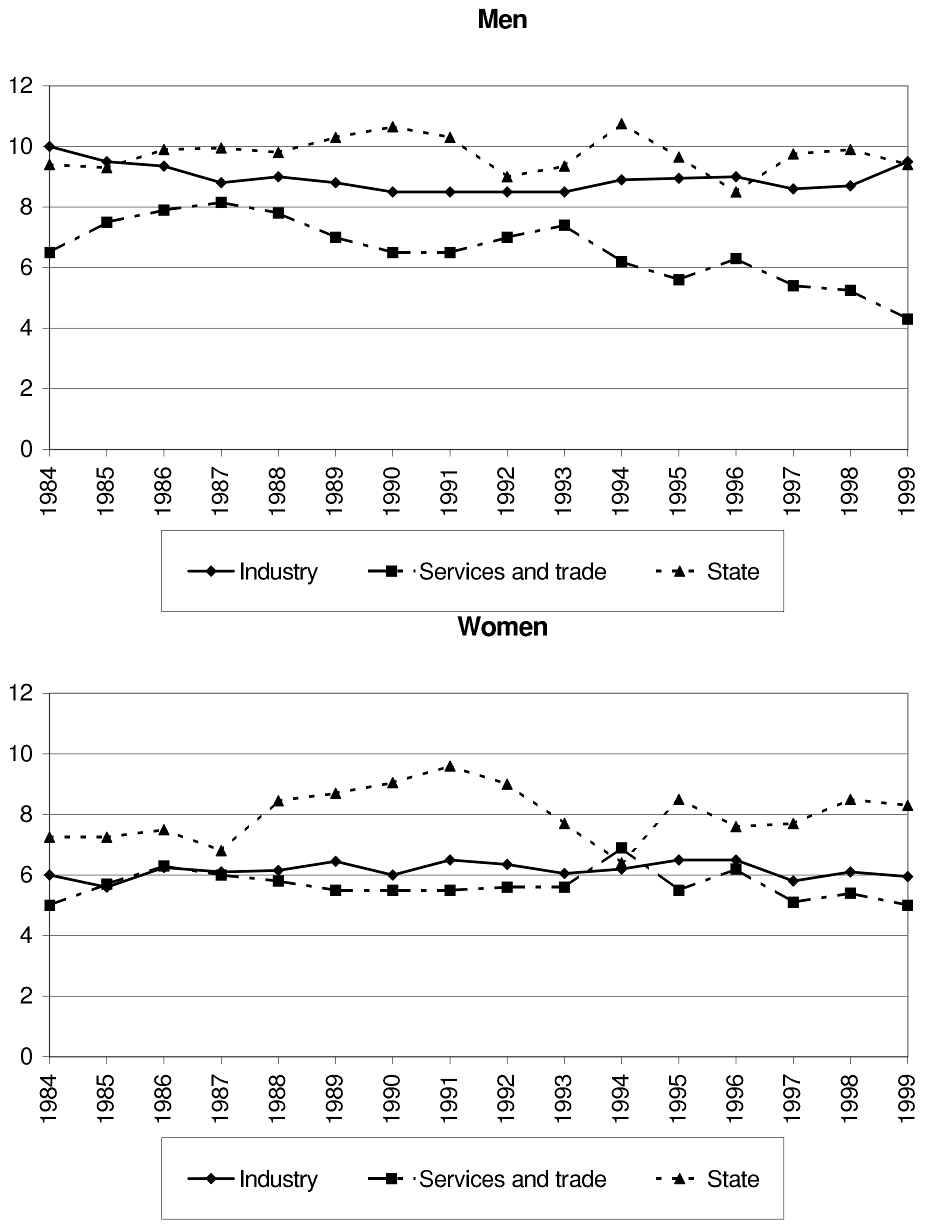
Similarly, tenure by industry differs more strongly for men than for women, as shown by Figure 3, which depicts the most important sectors. We subsumed manufacturing, construction, energy and mining to the "*industry*" sector, where we observe a decrease of around one year of tenure. In the late 1990s, however, this trend reversed. Services, credit and insurance, and trade are pooled into "*trade and services*". Here, we observe a significant decline in median tenure of around 2 years.

Because we still do not know the reasons for the slight decline observed in median elapsed tenure for men, we cannot know whether this is a positive or an alarming

[8]Marginal employment in Germany was defined as either working less than 15 hours per week in the period of observation or receiving monthly wages below a certain threshold (e.g. approximately 310 Euro in 1998).

[9]In 1988, around 3000 male workers were in full–time employment and only around 60 in part–time or marginal employment.

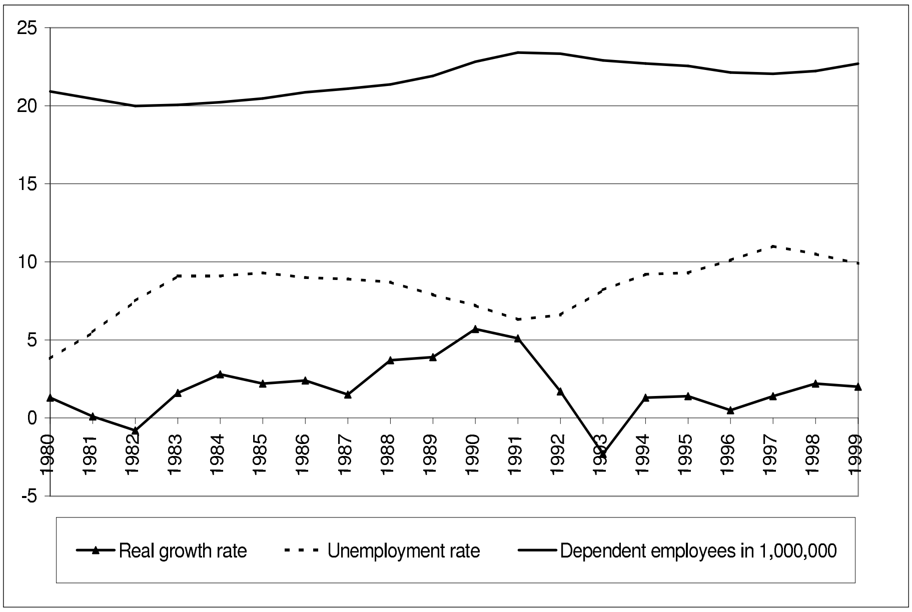
Figure 2.3: Evolution of Median Elapsed Tenure by Industry (in Years)*



*Source: Own calculations based on the GSOEP 1984–1999. For sample selection, see note to Figure 2.1

development. Considering the business cycle, as pointed out in section 2.2, (voluntary) quits are pro–cyclical, while (involuntary) layoffs are counter–cyclical, and both influence the tenure distribution. Between 1984 and 1989, the West German economy recovered from the recession of the early 1980s, leading to falling unemployment rates and slightly better job prospects for workers, as shown in Figure 2.4. In 1990, the year of re–unification, however, there was a pronounced boom, bringing West German capacities to their limits. This was primarily due to the increased demand for West German products in East Germany. This boom ended dramatically

Figure 2.4: Unemployment and the Business Cycle in West Germany *



*Source: Bundesanstalt für Arbeit (1988, 1992, 1996, 2000) as well as Working Group "Volkswirtschaftliche Gesamtrechnung der Länder" (2004).

in 1993, followed by a recession from which the West German economy has been recovering very slowly ever since. Unemployment in West Germany has increased and growth rates are at relatively low levels. The business cycle does seem to have had some influence on the evolution of elapsed tenure, but it cannot account fully for the pattern observed. Figure 2.1 shows that median tenure was slightly higher in the recession of 1993–94 than in the following years. But comparing 1987 and 1997, two years with relatively similar GDP growth rates, we see that median tenure declined by nearly one year.

A related and alternative explanation for the decline in job tenure is the increasing number of employees in West Germany, as increasing numbers of new hires lead to a decline in elapsed tenure. If new hires were the reason for decreasing job stability, there would be nothing to worry about. As shown by Figure 2.4, new hires only accounted for the decline in median elapsed tenure in the last two years of our observation period. From 1993 to 1997, the number of employees declined.

Having ruled out these two possibilities, the effects of the "usual suspects" can be investigated, namely, structural change, "globalization" and technological progress leading to both increased layoffs and quits as new job opportunities arise. The result would again be reduced average tenure. Astonishingly little is known about these interdependencies, although the effects on wage differentials have been of some concern.[10] Moreover, few previous studies have focused on the reasons for increasing

[10]See e.g. Katz and Murphy 1992; Levy and Murnane 1992; Krugman 1994; Leamer 1994, 1997.

job instability. Booth et al. (1999) show that, over a period of some forty years, the likelihood of individuals exiting their jobs has increased in the UK, with layoffs increasing more than quits. Their findings clearly illustrate the increased job instability in the 1980s. For the US, Valetta (1999b) shows that male workers with substantial job tenure experienced a rising incidence of permanent layoffs between 1976 and 1992. Keeping this in mind, we now go on to explore the development of job duration in West Germany, the reasons for separation and the employment state after separation in more detail.

2.5 Do Jobs Tend to End Earlier and, if so, Why?

A good way to look at the development of job duration is to perform duration analysis. This type of analysis has several advantages. The basic building blocks of duration analysis are hazard rates, which indicate the rate at which a job will end at a specific point of time, given that it has lasted until that time. Changes in this rate can be interpreted as changes in the evolution of job stability. A second advantage of duration analysis is that it offers estimation approaches that are insensitive to changes in the inflow rate. Remember that elapsed tenure is a retrospective measure, where the inflow rate is very influential.

Thirdly, duration models use information on the exact termination date of each job and control for the right censoring of spells (i.e. the fact that the jobs observed at a certain point in time will continue for an unknown time span). Furthermore, elapsed tenure is usually only considered on a yearly basis (as are retention rate estimates). The GSOEP, however, includes monthly information on job duration that can and should be exploited. Finally, our duration analysis in the tradition of Booth et al. (1999) and Gottschalk and Moffitt (1999) is multivariate, with all observations and influences being combined in a single estimation, adding to the clarity of the results. In more conventional multivariate analyses separate models are needed for workers with different elapsed tenures.[11]

Nevertheless, it is important to note that this shift in the type of analysis also entails a shift in perspective. We sample jobs rather than people. As most people tend to be in long–term jobs, but most jobs are short lived, the average duration of a job spell is rather short compared to the elapsed firm tenure (Topel and Ward 1992, Farber 1999). Indeed, we believe that it is important to test the influence of short jobs when addressing the question of changing job stability.

Many previous studies on job stability have neglected another important aspect – what happens to workers who leave a job? Do they become unemployed, employed, or do they even leave the labor force altogether (see also Neumark, 2000)? It would

[11] For Logit regressions on the probability of being in short or long jobs, see Mertens 1999.

certainly be more alarming to find that job duration declines due to increased transitions to unemployment rather than to increased transitions to new jobs or training. Therefore, following our analysis of job duration until layoffs or quits, we perform an analysis of job duration until transition to unemployment or to a new job in another firm. An advantage of this analysis is that the employment calendar in the GSOEP provides us with data on the transition state for a large proportion of the short jobs. However, the downside is that we do not have job–specific information for a significant share of the short jobs.

2.5.1 Empirical Modeling

The basic tools to model duration data are survival functions $\bar{F}(x)$ and hazard functions $h(t)$ at some duration t . Duration t is commonly defined as a measure of length of a spell between certain events.

$\bar{F}(x)$ gives the probability that a duration will last longer than t. Formally, for continuous time:

$$\bar{F}(x) = P[T \geq t] = 1 - F(x) = 1 - \int_0^t f(s)\,ds \tag{2.1}$$

with denoting $F(x)$ the distribution function and $f(t)$ the density function for some duration t.

The hazard function $h(t)$ gives the rate per time period at time that the probability of a spell terminating is amassed conditional on the spell not being terminated prior to t. For continuous duration the hazard function $h(t)$ is defined by

$$h(t) = \frac{f(t)}{1 - F(t)} = \frac{f(t)}{\bar{F}} \tag{2.2}$$

It should be noted, that the hazard and survival function are closely related (as well as with the density and the distribution function). One can be derived by the other using the following relationship.

$$\bar{F}(x) = \exp\left(-\int_0^t h(s)\,ds\right) \tag{2.3}$$

However, individuals might face different risks of terminating a spell according to their environmental and individual characteristics. Furthermore, the risk might change over the duration of a spell: an observation, which is commonly subsumed under the heading 'duration dependence'. Consequently, the hazard function should be modeled such that it not only depends on time but also on covariates i.e.:

$$h(t) = \theta(t|x) \tag{2.4}$$

We chose the popular Semi–Parametric Proportional Cox Model as a basis for our estimation. The model works on the usual assumption of a proportional effect of covariates on the hazard rate $\theta(t)$.

$$\theta(t|x) = \theta_0(t)\exp(x\beta) \tag{2.5}$$

The major advantage of this model is that it leaves the form of the so–called 'baseline hazard' $\theta_0(t)$ unspecified. Thus, no special assumption concerning the duration dependence is necessary.

We extend this standard Cox Model in three ways to address our research question. First, the model is specified in an independent competing risk form to distinguish between the determinants that are responsible either for the different reasons of job termination or for the different transition states. In the independent competing risk specification, the hazard of dismissal is, for example, estimated by treating termination due to quitting or due to other reasons as censored, and vice versa.

Second, to capture time trends in the hazard of job termination, we incorporate covariates into the regression (x^a_τ) which capture the influence of calendar time τ on the hazard rates (either dummies for each year of the 1984–1997 period or a time trend variable taking value 1 for 1984 to 14 for 1997).

Third, we remove the proportionality assumption for these calendar time variables and allow for different effects on the hazard rates, depending on how long the job had already lasted. In varying combinations, we distinguish job durations of up to one year, more than one year but less than or equal to 10 years, and more than 10 years. Furthermore, we try to take account of the changing economic conditions as well as changing individual determinants over the length of a spell x^b_τ by allowing these covariates to vary on an annual basis.

We estimate exit–specific hazard rates of the Semi–Parametric Proportional Cox Model consisting of elements of the following equation:

$$\theta_e(t|x) = \theta_{e0}(t)\exp(x^a_\tau\beta^a_e + I(t \leq 12)x^a_\tau\beta^{a1}_e + I(t \leq 120)x^a_\tau\beta^{a2}_e + x^b_\tau\beta^b_e) \tag{2.6}$$

where $\theta_{e0}(t)$ is the 'baseline' hazard for the respective termination state e and $x = (x^a_\tau, x^b_\tau)$ are the time–varying covariates. These hazard rates are estimated separately for each gender, as the employment behavior of men and women shows clear differences.

Note that our model can deal with left–truncated spells. This is necessary as part of our sample is a stock sample, meaning that our analysis includes job spells for which we know the start date and that they had not ended by the date on which the stock sample was drawn. Had these spells ended before this date, we would not have observed them. By far the largest part of our stock sample was drawn in January 1984. Starting with January 1984, we have data on the monthly employment status and job changes of interviewees who regularly took part in the GSOEP. We therefore observe the end (or the time of censoring) of all jobs started in 1984 and later. However, jobs started before 1984 are only included in our sample if they ended in 1984 or later. The rest of the stock sample is drawn from newcomers to the GSOEP.

It is crucial to use a stock sample approach in order to be able to analyze jobs that have already existed for a long time. If we used only a flow sample, we would only be able to analyze jobs lasting a maximum of 14 years. Furthermore, by adding the stock sample we are able to increase our sample size considerably. We are not aware of other studies in the area of job stability that have exploited these advantages of a stock sample.[12] To incorporate the stock sample in our empirical analysis, we assume that there is no unobserved heterogeneity – a common assumption in the job stability literature. If we rule out unobserved heterogeneity, the following relationship is valid and is taken into account in the likelihood contribution of the stock sample:

$$\theta_{e|p}(r|p, x, S) = \theta(r + p|x) \tag{2.7}$$

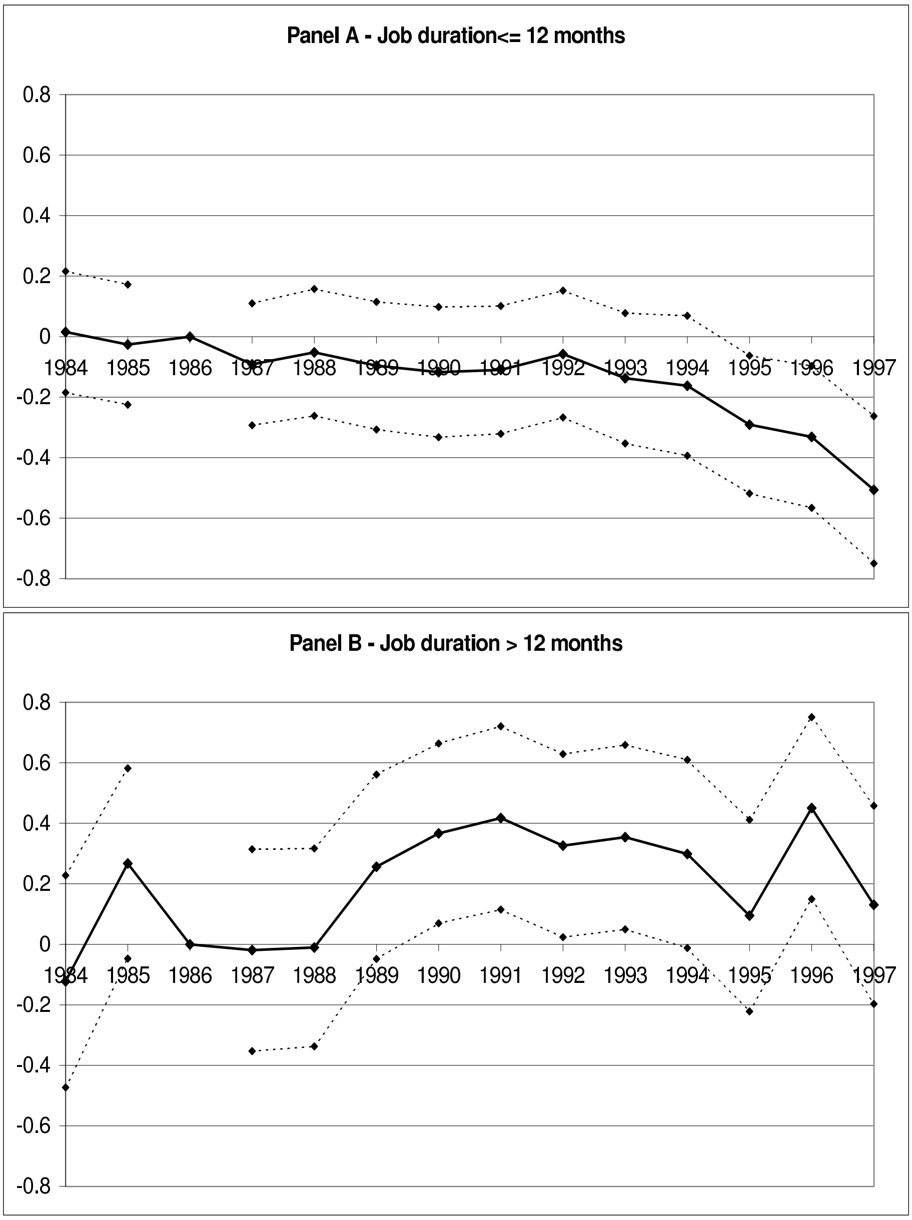
with p denoting the elapsed duration, r the residual duration and S presence in the stock.

2.5.2 Multivariate Estimation Results

Let us first consider whether the risk of a job spell ending has increased in recent years. As mentioned in the data section, we can only use data collected between 1984 and 1997, as a subsequent change in the wording of the job change questions made it impossible to create a consistent variable over time. We start with an analysis as comparable as possible to the analysis of elapsed tenure by ignoring the reason for separation. Therefore, we regress job duration on yearly dummies for job durations of up to 12 months and job durations of more than 12 months

[12]See Appendix 2.D for a duration analysis of job spell data on the basis of a flow sample. Here the job spell data is used as described in Appendix 2.D.1. The basic results are similar to the one in section 2.5.2. However, due to the small sample size and the different extraction method, we can not conduct such a differentiated analysis with respect to the reasons of change in job duration and the transition rates.

Figure 2.5: Estimated Coefficients for the Evolution of the Hazard of Job Termination – Men*

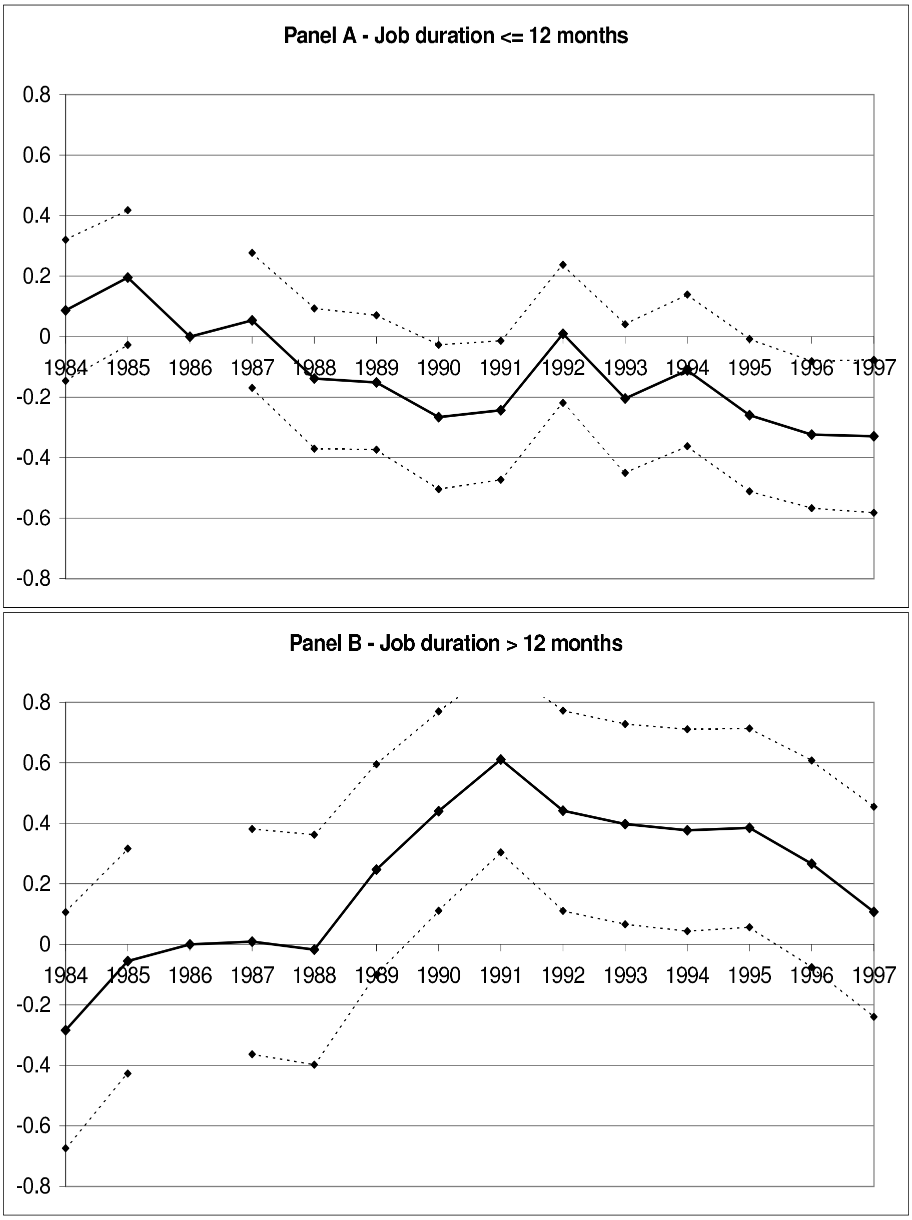


*Note: Results are based on a duration models. The estimated coefficients on calendar time dummies are depicted. See text for details. Dotted lines represent 95% confidence intervals.
Source: Own calculations based on the GSOEP 1984–1998. Only German citizens living in West Germany (Sample A), aged 16–56 at the beginning of the job spell, working full– or part–time. Excluding civil servants, apprentices and self–employed respondents, workers in agriculture, non–profit organizations and private households, and observations with missing values for age, sex or education. For job spells longer than 12 months, observations with missing values on the reason for separation, job status, industry or firm size are also excluded.

(see Figures 2.5 and 2.6).[13] Our base category is the year 1986, which is the third

[13]For jobs of up to 12 months duration, we do *not* exclude spells with missing values for variables such as job status and industry affiliation, as the sample size would otherwise be too small.

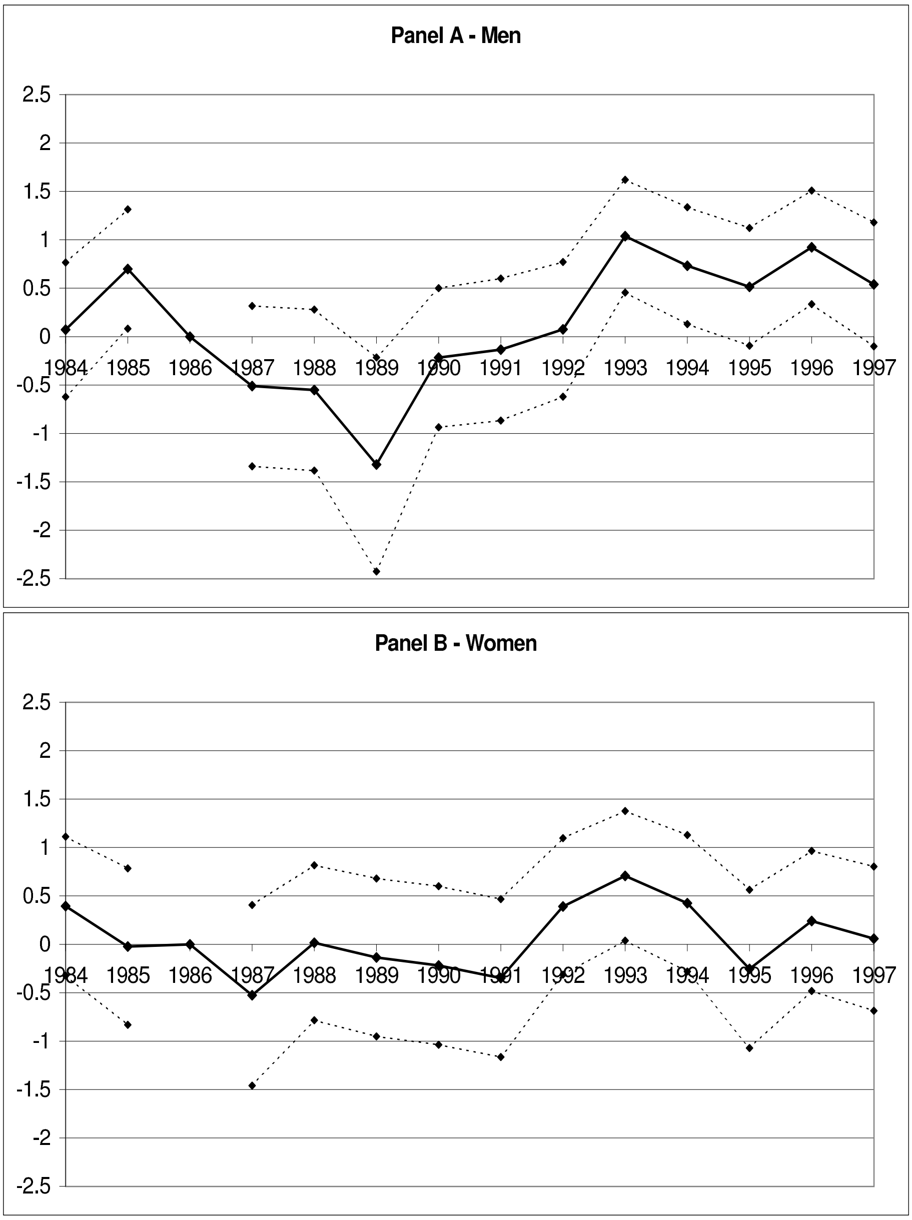
Figure 2.6: Estimated Coefficients for the Evolution of the Hazard of Job Termination – Women*



*Note: Results are based on a duration models. See text for details. Dotted lines represent 95% confidence intervals.
Source: Own calculations based on the GSOEP 1984–1998. For sample selection, see note to Figure 2.5.

year in a row with relatively stable growth rates (see figure 2.4). For jobs lasting longer than 12 months, the results are consistent with the results of our preceding analysis on elapsed tenure. There are some indications of an increase the hazard of job termination for men, less so for women. In contrast to job durations of more than 12 months, we find a slightly declining hazard for jobs lasting up to 12 months, indicating that short jobs somewhat became longer, especially for men. It should be

Figure 2.7: Estimated Coefficients for the Evolution of the Hazard of Job Termination – Competing Risk Model — Hazard of being Laid off*

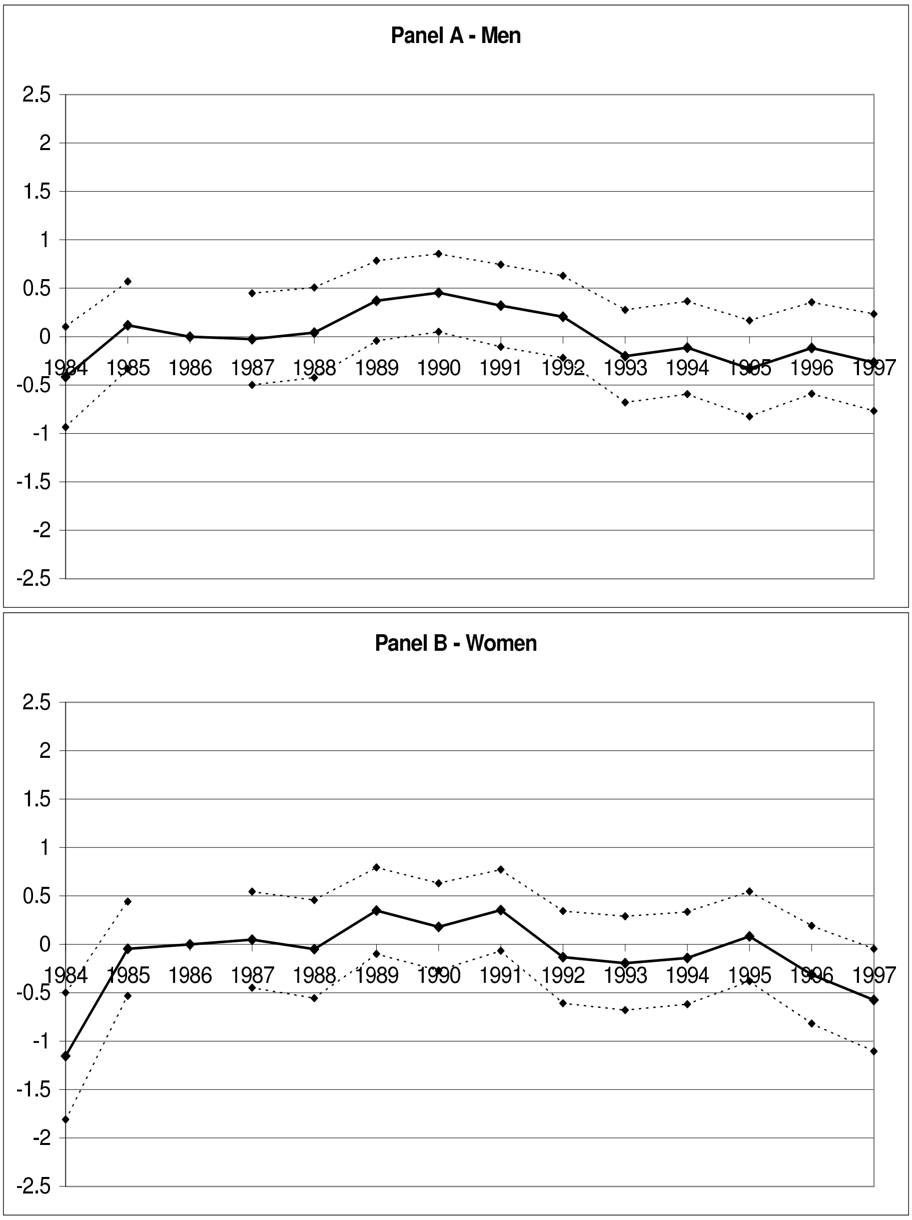


*Note: Results are based on competing risk models. See text for details. Dotted lines represent 95% confidence intervals.
Source: Own calculations based on the GSOEP 1984–1998. For sample selection, see note to Figure 2.5.

mentioned that here, and in the following step of our duration analysis, the results for women have to be interpreted very carefully, as due to identification problems, we cannot yet control for the change in the wording of the questionnaire since 1991.

With these results in mind, we now go on to look at whether there are any changes in the way a job ended. Using our spell data, we are able to address this question in a differentiated analysis. As outlined above, job–specific information is consistently

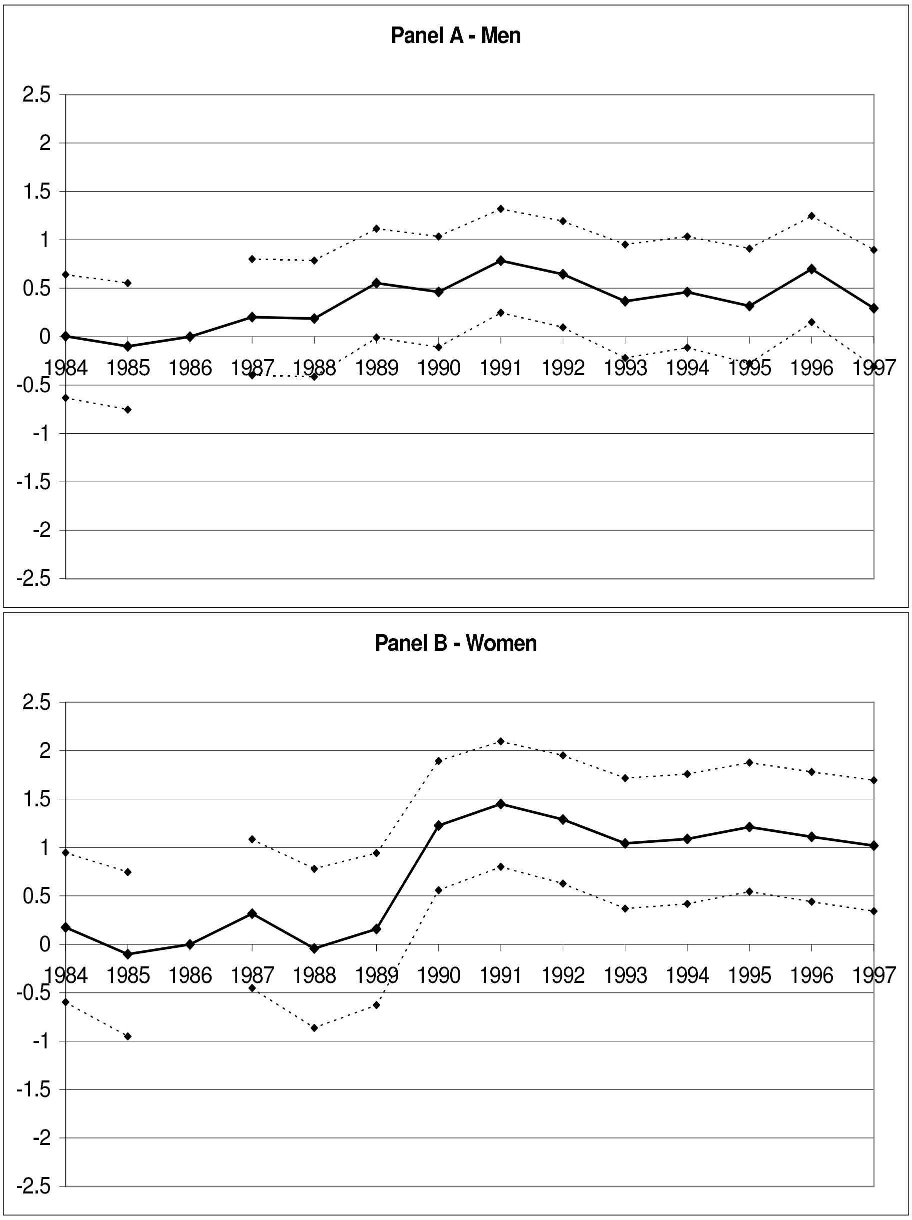
Figure 2.8: Estimated Coefficients for the Evolution of the Hazard of Job Termination – Competing Risk Model – Hazard of Quitting*



*Note: Results are based on competing risk models. See text for details. Dotted lines represent 95% confidence intervals.
Source: Own calculations based on the GSOEP 1984–1998. For sample selection, see note to Figure 2.5.

available only for jobs lasting longer than 12 months. Moreover, these are the jobs for which data on the reason for separation are most often available. Therefore, only jobs lasting longer than 12 months are included in the analysis of the reason for job termination. This approach is justified by the fact that short jobs are often substantially different in nature from jobs lasting longer than 12 months, consisting mainly of holiday jobs for students or short–term replacement jobs. This might

Figure 2.9: Estimated Coefficients for the Evolution of the Hazard of Job Termination – Competing Risk Model – Hazard of Leaving due to Other Reasons*



*Note: Results are based on competing risk models. See text for details. Dotted lines represent 95% confidence intervals.
Source: Own calculations based on the GSOEP 1984–1998. For sample selection, see note to Figure 2.5.

also explain the diverging evolution over time of the hazard of job termination. Nevertheless, when analyzing jobs by destination state, we will again consider job durations of up to 12 months.

In order to get an idea of how job duration evolved until dismissal, quit or termination for other reasons, we first regress job duration until each of these three exit states on calendar time dummies. Figures 2.7, 2.8 and 2.9 present the estimation results. The coefficients on the yearly dummies vary strongly over time. Roughly speaking, the hazard of men being laid off declines until 1989, increases again until 1993, and then remains at a relatively high level compared to the 1984–1992 period (the year 1985 being an exception, with relatively high hazard rates). There is a similar pattern for women, but neither the decline up to the year 1989 nor the subsequent increase is as pronounced as for men. Indeed, women's hazard of being laid off in the mid–1980s is similar to that in the late 1990s, following the peak in 1993. Because the coefficients on the year dummies capture not only a potential secular trend, but also other calendar time effects, parts of the movement can logically be explained by business cycle effects. The direction of movement strongly coincides with the West German economic cycle (see Figure 2.4 and section 2.4).

Turning now to the hazard of quitting, we find exactly the opposite pattern to the hazard of being laid off. Compared to the year 1986, however, the coefficients are rarely significant and the trend is much less pronounced. The hazard of men leaving their job due to other reasons increased in the economic boom period of 1991, as compared to 1986, and remained at a relatively high level. For women, however, we see a strong increase in the hazard of job termination due to other reasons starting in 1990. This can probably be partly explained by the introduction of maternity leave as an explicit response category in the questionnaire (see the data section above).

To examine whether the evolution of the hazard of job termination due to dismissal, quit or other reasons can be understood as a secular trend, we will now partly compress and partly expand our econometric model. Instead of the yearly dummies, we include a time trend variable, taking value 1 in 1984, 2 in 1985 and so forth, up to value 14 in 1997. We expand the model by controlling for the influence of the business cycle by including the average growth rate of real GDP at the current point in time and lagged by one year, as well as the average of the two– and three–year lagged growth rates. Additionally, we control for possible changes in the composition of the workforce by including determinants known to influence job duration (e.g. Mertens 1998) in the econometric model. Two different types of covariates can be distinguished. First, individual characteristics known to influence job duration such as age and kind of professional education. Second, job–specific variables such as part–time status (regular part–time and marginal employment), firm size and industry affiliation. Finally, we incorporate a dummy in the econometric model for women to account for the change in the wording of the questionnaire as of 1990.

The evolution of layoffs and quits over time is of particular interest. Therefore, let us first consider the coefficient on the time trend in Table 2.2, Appendix 2.A. Despite controlling for the business cycle, as well as individual and job characteristics, we find a clear tendency towards an increase in the hazard of men being laid off. The coefficient is positive and significant. The hazard of men quitting, on the other hand, is not influenced by calendar time, but there is an increasing trend in the hazard of men leaving a job due to other reasons. For women, there is no significant trend in the hazard of being laid off, quitting or leaving due to other reasons.

Let us now consider the influence of the economic conditions on job duration. As expected, the state of the economy has a significant impact on job duration. In an economic slowdown, the risk of being given notice increases, whereas in an economic upturn, the odds of resigning increase. The hazard of men leaving due to other reasons increases in times of positive economic development. These results again emphasize the importance of distinguishing between business cycle influences and secular trends in job duration. This fact was not taken into consideration by Gottschalk and Moffitt (1999), for example. Similar to our study, they included a linear trend term for the year of observation in their analysis, but they did not include measures of labor market tightness.

The results concerning the influence of age on job duration can be summarized as follows: Men aged 56 and older are more likely to be laid off. The younger workers are, the more likely they are to quit voluntarily. For men, the hazard of leaving due to other reasons initially decreases with age, but increases again among those aged 56 and older. For women, it increases for the age 26–35 group, then decreases, and increases again among those aged 56 and older. Education also plays only a limited role in determining job duration. Having a college degree increases the odds of men resigning, whereas women with a college degree are more likely to leave the job due to other reasons.

Working part–time increased the odds of both men and women quitting, and of men ending their job due to other reasons. For women, working part–time is associated with less risk of being laid off.

Firm size has the expected effect on job separation. The risk of being laid off is higher in small firms than in medium–sized or larger firms, and individuals employed in large firms are less likely to quit. As expected, industry affiliation also influences job duration. For example, working in the credit and insurance sector decreases the hazard of being laid off, and working in public administration reduces the hazard of women quitting. Finally, it should be mentioned that the hazard of women ending a job due to other reasons has increased significantly since 1990. This can be attributed to the introduction of maternity leave as a potential reason for job termination in the GSOEP questionnaire.

We will now continue to study potential reasons for the decline in job security. Naturally, one reason that suggests itself is a change in labor market regulations. However, only a few small changes occurred during the observation period, and were introduced either at the beginning or at the end of this period (see Walwei, 1998). Thus, they can hardly explain the trend observed. Since 1985, the regulations concerning fixed–term contracts have become slightly less restrictive, and a formal reason is no longer necessary to justify a contract of this type. Furthermore, since

October 1996, the maximum length of fixed–term contracts has been increased from 18 to 24 months, and employment protection has been slightly weakened. The list of social criteria used to determine which employees should be dismissed last has been trimmed. Finally, firms with less than 11 employees no longer fall under the employment protection law. Before October 1996, the number of employees was 6. Finally, more account should now be taken of the firm's interests when applying employment protection law – a ruling which is difficult to implement.

The rise in flexible work arrangements such as part–time work has also been suggested as a possible explanation for the decline in job stability (see e.g. Levenson, 2000). As we controlled for potential changes in the composition of part–time vs. full–time contracts in our reference model, the trend towards an increase in the hazard of men being laid off or ending a job due to other reasons cannot attributed to any change in the composition of hours worked. In order to identify any changes in the hazard of ending a part–time job, we interact the time trend with a part–time dummy in an extended model.[14]

Other reasons that have been proposed for a decline in job security include downsizing of firms (see e.g. Cappelli, 2000), skill–biased technological change (Givord and Maurin, 2003), and weakening bonds with the firm (Valetta, 1999b). We will test the downsizing hypothesis by interacting the trend variable with firm size in order to test whether larger firms are mainly responsible for the increased incidence of layoffs. To test for skill-biased technological change, we will interact the skill level of the individual with the time trend. If skill–biased technological change is responsible for the trend in the hazard of job termination, we would expect low–skilled workers to be most strongly affected by the changes.

In addition, we examine whether long–tenured workers are at higher risk of being laid off by including a time trend variable only valid for individuals with more than 10 years of tenure. This constitutes another aspect of skill–biased technological change, which eliminates routine tasks where high–tenured workers are at a comparative advantage (Autor et al., 2003). If high–tenured workers are found to have not only an increasing hazard of being laid off, but also an increasing hazard of quitting, this might be understood as a general weakening of the bonds between the firms and their workers (Valetta, 1999b).

The results of our extended model (see Appendix 2.A, Table 2.3) show that men in part–time jobs do not face an increasing hazard of being laid off as compared to men in full–time jobs; rather, the hazard of being laid off from a part–time job declines over time.

[14] Another flexible work arrangement which might be responsible for the trends in the hazard rate is fixed–term contracts. Unfortunately, the GSOEP variable on limited term contracts has many missings, meaning that a reliable analysis of limited term contracts is not possible in our context.

Another important result of our extended model is that the increased risk of men being laid off is concentrated in large firms. This can be interpreted as some evidence for the hypothesis that the downsizing of large firms has lead to decreased job security.

We find no evidence for an increase in the risk of long–tenured employees being laid off. In fact, we see the opposite pattern, with long–tenured male employees experiencing a decline in the hazard of being laid off. This might be due to stricter court rulings concerning the dismissal of long–tenured employees (see Franz and Rüthers 1999). Likewise, there is no evidence that low–skilled employees experience an increase in the hazard of being laid off. As neither the hazard of quitting nor the hazard of being laid off rises for long–tenured employees, we find no evidence for a general weakening of the bonds between firms and their employees.

We now turn to the second aspect of job stability, namely, whether individuals are unemployed, employed, in training or out of the labor force after a job ends. Table 2.4 in Appendix 2.A summarizes the estimation results when time trend variables are used to capture the evolution of the hazard of job termination with transition to unemployment, employment or nonparticipation. We use all job durations in this analysis because, in contrast to the reason for job termination, the transition state is available for nearly all jobs, irrespective of their length. The downside of this, however, is that we cannot control for job–specific characteristics, except for part–time status.[15] To capture potential diverging trends for jobs of different durations, we incorporate interaction terms for job durations of up to 12 months.

Let us first summarize the results for men. There is a significant increase over time in the hazard of entering unemployment for men in a job already lasting one year or longer. For men in short jobs, in contrast, the hazard of entering unemployment declines significantly over time, as a test of the joint significance of the time trend and the interaction term for short job durations revealed. With respect to transitions towards new jobs or nonparticipation, there are no significant changes in the hazard rates for men who had already been in a job for more than 12 months. For women, we do not detect any significant changes in the hazard of entering unemployment, but the hazard of changing jobs increased slightly and the hazard of entering nonparticipation increased significantly for women who had already been in a job for longer than 12 months. The fact that short jobs are substantially different in nature from jobs lasting longer than a year is again shown in the hazard of entering nonparticipation. This hazard declined significantly for both men and women for jobs of up to 12 months duration.

[15] The results for the hazard of transitions from jobs lasting more than 12 months to unemployment, a new job or nonparticipation persist when we analyze these jobs only and include further job–specific covariates. Extending the model concerning termination due to layoffs, quits and other reasons in the same way does not provide any additional insights into the reasons for the evolution of the hazard of entering one of the three transition states. Therefore, we only present this short version.

An important result of this analysis of transition states is the increasing hazard over time of entering unemployment for men in jobs of more than 12 months. A natural hypothesis is that this development coincides with the increase in the hazard of men being laid off. To investigate this, we now distinguish between layoffs leading to unemployment and layoffs leading to a new job or nonparticipation. Furthermore, we analyze transitions into unemployment initiated by quits or other reasons. The hazards of these three competing risks are again only estimated for job durations of more than 12 months, as the reasons for job termination play a role here (see Table 2.5 and 2.6 in Appendix 2.A).

Astonishingly, we do not find such a close relationship between layoffs and transitions to unemployment. The results for men show a positive but insignificant coefficient on the trend variable. In contrast, there is a slight trend towards an increase in the hazard of men being laid off when the dismissal is followed by a new job or nonparticipation Furthermore, there is a slightly significant trend for transitions into unemployment initiated by quits or other reasons.

2.6 Conclusion

Our results provide some evidence to support the view that job stability in West Germany declined in the 1980s and 1990s. However, not all demographic groups in the labor market were equally affected. Using repeated cross–sections from the German Socioeconomic Panel (GSOEP), we show that male workers aged between 16 and 56 experienced a decrease in median elapsed tenure from 9.4 years in 1984 to 7.5 years in 1999. While women in countries like the US or the UK were able to accrue higher median tenure, there was no such an increase in Germany between 1984 and 1997.

These simple cross–sectional results are confirmed by our analysis of the hazard of job termination. Estimating a Cox Model with only calendar time dummies, we show that there are signs of an increase in the risk of job termination over the observation period for men who had already been in a job for 12 months.

Extending our analysis to a Competing Risks Model with respect to the reasons for job termination, and controlling for the business cycle as well as demographic and job characteristics, we are able to show that this increase was caused primarily by a rise in layoffs and terminations due to other reason.

When analyzing the hazards of becoming unemployed, entering a new job or nonparticipation, we find that men face an increasing risk of becoming unemployment if they have already been in their job for more than one year. In addition, women face an increasing hazard of nonparticipation if they have already been in a job for more than one year.

However, the increased risk of men being laid off does not coincide with an increased risk of men entering unemployment, as one might have expected. Instead, there are signs that the increased hazard of being laid off coincides with transitions to a new job or to nonparticipation. Similarly, the increased hazard of entering unemployment is related to transitions that are initiated by quits or other reasons.

The results of the duration analysis described thus far are only valid for jobs which have already lasted one year. The pattern of results for short jobs is substantially different, however. For job durations of up to one year, we find a decreasing hazard of job termination, especially for men, coinciding with a decreasing hazard of being laid off, becoming unemployed, and entering nonparticipation.

Where the reasons for the decline in job stability are concerned, some of the increased risk of job termination seems to be related to the downsizing of large firms, as the increased risk of men being laid off is concentrated in these firms. However, the hypothesis that skill–biased technological change or a general weakening of the bonds between firms and their workers are responsible for this development cannot be confirmed for West Germany.

Considering our empirical results, it would certainly be exaggerated to claim that there has been a serious deterioration in job stability. Still, there is cause for concern. With men increasingly exposed to layoffs and increased transitions to unemployment, the willingness to accrue education and specific capital may be limited.

Moreover, although wage losses upon re–employment are not as large as in the United States for the majority of workers, prolonged unemployment may lead to severe income losses (Burda and Mertens 2000). Finally, it may be the case that increasing layoff risks affect ‘outsiders’ more severely, leading to stronger dualization of the labor market. On the other hand, it could be argued that the decrease in job stability simply shows that the German economy is adjusting to the globalization and technological innovation process – especially considering that the increased risk of men being laid off is not accompanied by a similar significant increase in the risk of transitions to unemployment. The interpretation that the economy is becoming more flexible, with positive side–effects on macroeconomic development is, however, questionable in view of persistently high unemployment rates. Further studies are clearly needed to shed more light on the background to this development.

Appendices to Chapter 2

2.A Tables

Table 2.1: Median Tenure in Years

	Men		Women	
	Median tenure	N of observations	Median tenure	N of observations
1984	9.4	1891	5.5	1305
1985	8.7	1726	5.5	1258
1986	8.5	1649	6	1189
1987	8.5	1629	6.1	1160
1988	8.5	1560	6	1140
1989	8.5	1540	6	1158
1990	7.8	1541	5.8	1172
1991	7.6	1548	5.7	1212
1992	7.8	1509	5.8	1184
1993	8	1486	5.8	1179
1994	8.5	1398	6.1	1141
1995	8	1372	6	1072
1996	7.7	1361	6.4	1083
1997	7.8	1318	5.5	1093
1998	7.6	1261	5.8	1057
1999	7.5	1277	5.4	1087

Source: Own calculations based on the GSOEP 1984–1999. Only German citizens living in West Germany (Sample A), aged 16–56, working full– or part–time. Excluding civil servants, apprentices, self–employed respondents, workers in agriculture, non–profit organizations and private households, and observations with missing values for age, sex, job status, industry affiliation or tenure.

Table 2.2: Competing Risk Model of the Hazard of Job Termination with Respect to the Reasons for Termination of Jobs Lasting Longer than One Year – Reference Model

	Men			Women		
	Layoff	Quit	Other Reasons	Layoff	Quit	Other Reasons
Time Trend	**0.040*** (0.016)	**-0.009** (0.012)	**0.052**** (0.014)	**-0.049** (0.041)	**0.032** (0.025)	**-0.028** (0.030)
Average of Current and One-Year Lagged Growth Rate (State Level)	**-0.173**** (0.039)	**0.105**** (0.024)	**0.062*** (0.031)	**-0.146**** (0.040)	**0.102**** (0.027)	**0.029** (0.030)
Average of Two- and Three-Year Lagged Growth Rate (State Level)	**-0.021** (0.032)	**0.046*** (0.023)	**0.042** (0.027)	**0.051** (0.037)	**0.035** (0.025)	**-0.005** (0.027)
Age Base Category: *16-25 Years*						
26-35 Years	**-0.130** (0.216)	**-0.079** (0.128)	**-1.239**** (0.205)	**0.147** (0.215)	**-0.214** (0.132)	**0.339*** (0.158)
36-45 Years	**0.143** (0.238)	**-0.568**** (0.154)	**-1.469**** (0.284)	**0.176** (0.230)	**-0.428**** (0.154)	**-1.361**** (0.239)
46-55 Years	**0.178** (0.251)	**-1.314**** (0.230)	**-0.896**** (0.234)	**0.395** (0.253)	**-1.070**** (0.201)	**-1.553**** (0.264)
56 and Older	**0.580+** (0.297)	**-1.394**** (0.346)	**1.508**** (0.206)	**0.120** (0.427)	**-1.090**** (0.380)	**0.900**** (0.211)
Vocational Education Base Category: *Vocational Training*						
No Vocational Training	**0.270** (0.170)	**-0.138** (0.145)	**0.206** (0.146)	**0.316+** (0.163)	**-0.151** (0.123)	**0.142** (0.111)
College Degree	**-0.249** (0.234)	**0.422**** (0.116)	**-0.025** (0.160)	**-0.052** (0.362)	**0.231** (0.219)	**0.550**** (0.178)
Part-time or Marginal	**0.227** (0.455)	**0.520*** (0.244)	**1.239**** (0.268)	**-0.471**** (0.163)	**0.246*** (0.112)	**0.123** (0.108)
Firm Size Base Category: *20-199 Employees*						
1-19 Employees	**0.503**** (0.155)	**0.119** (0.107)	**0.055** (0.169)	**0.578**** (0.164)	**0.227+** (0.119)	**0.148** (0.143)
>= 200 Employees	**-0.437**** (0.153)	**-0.611**** (0.107)	**-0.018** (0.114)	**-0.528**** (0.186)	**-0.229*** (0.116)	**0.133** (0.115)

Table continues next page...

…Table continued

	Men			Women		
	Layoff	Quit	Other Reasons	Layoff	Quit	Other Reasons
….						
Industry Affiliation Base Category: *Manufacturing*						
Energy	**-1.871+** (0.985)	**-0.987*** (0.471)	**-0.315** (0.355)		**0.295** (0.634)	**-0.100** (0.656)
Mining	**-0.537** (0.980)		**0.615*** (0.308)			
Construction	**0.219** (0.171)	**0.262*** (0.127)	**0.178** (0.154)	**0.451** (0.332)	**-0.452** (0.414)	**-0.121** (0.399)
Trade	**-0.068** (0.211)	**0.416**** (0.142)	**-0.369** (0.231)	**-0.223** (0.191)	**0.134** (0.134)	**-0.371*** (0.150)
Traffic and Communication	**0.151** (0.226)	**0.484**** (0.157)	**-0.049** (0.208)	**-1.040+** (0.600)	**0.077** (0.264)	**-0.160** (0.267)
Credit and Insurance	**-2.345*** (1.000)	**0.113** (0.261)	**-0.361** (0.307)	**-1.780**** (0.588)	**-0.195** (0.220)	**-0.408+** (0.212)
Other Services	**0.036** (0.215)	**0.471**** (0.136)	**0.236** (0.171)	**-0.416*** (0.175)	**0.088** (0.124)	**-0.040** (0.125)
Government and Social Security	**-0.377** (0.314)	**-0.558*** (0.275)	**0.221** (0.159)	**-2.140**** (0.586)	**-0.417+** (0.220)	**-0.360*** (0.179)
Change of Questionnaire				**0.412** (0.329)	**-0.221** (0.199)	**1.283**** (0.245)
No. of Spells No. of Destination States	2988 290	2988 543	2988 390	2415 216	2415 493	2415 426
LR Chi2	147.54	212.76	688.77	109.53	95.11	381.78

Note: Robust standard errors with respect to individuals reported in brackets; * indicates significance at the 5% significance level, ** at the 1% significance level and + at the 10% significance level.
Source: Own calculations based on the GSOEP 1984–1998. Only German citizens living in West Germany (Sample A), aged 16–56 at the beginning of the job spell, working full– or part–time. Excluding civil servants, apprentices and self–employed respondents, workers in agriculture, non–profit organizations and private households, and observations with missing values on the reason for separation, age, sex, education, job status, industry or firm size. Data on GDP growth rates are taken from the Working Group "Volkswirtschaftliche Gesamtrechnung der Länder"(2004).

Table 2.3: Competing Risk Model of the Hazard of Job Termination with Respect to the Reasons for Termination of Jobs Lasting Longer than One Year – Extended Model

	Men			Women		
	Layoff	Quit	Other Reasons	Layoff	Quit	Other Reasons
Time Trend	**0.054+** (0.030)	**-0.018** (0.021)	**0.088**** (0.033)	**0.002** (0.054)	**0.003** (0.032)	**-0.006** (0.040)
Time Trend*I(t>120)	**-0.066+** (0.034)	**0.018** (0.036)	**-0.045** (0.029)	**0.013** (0.041)	**0.014** (0.039)	**0.023** (0.029)
Time Trend*No Education	**-0.058** (0.040)	**-0.012** (0.034)	**-0.041** (0.038)	**-0.007** (0.037)	**0.083**** (0.030)	**-0.048** (0.032)
Time Trend*College	**-0.065** (0.045)	**0.015** (0.030)	**-0.052** (0.038)	**0.014** (0.078)	**0.025** (0.049)	**-0.046** (0.045)
Time Trend*Part-time	**-0.204*** (0.087)	**0.097** (0.074)	**-0.088** (0.054)	**-0.016** (0.038)	**0.050*** (0.024)	**-0.020** (0.030)
Time Trend*Small Firm	**0.020** (0.037)	**0.028** (0.030)	**0.005** (0.044)	**-0.079*** (0.040)	**-0.024** (0.027)	**0.006** (0.039)
Time Trend*Large Firm	**0.061+** (0.036)	**-0.016** (0.027)	**0.015** (0.031)	**-0.041** (0.049)	**-0.000** (0.030)	**-0.019** (0.030)
Further covariates included correspond to reference model.						
No. of Spells No. of Destination States	2988 290	2988 543	2988 390	2415 216	2415 493	2415 426
LR Chi2	153.83	216.94	691.77	117.40	107.16	392.68

For notes and sources, see Table 2.2.

Table 2.4: Competing Risk Model of the Hazard of Job Termination with Respect to Transition States

	Men			Women		
	Unemp-loyment	New Job	Non-participa-tion	Unemp-loyment	New Job	Non-participa-tion
Time Trend	**0.047**** (0.014)	**0.007** (0.011)	**0.021** (0.015)	**-0.036** (0.028)	**0.043+** (0.023)	**0.040*** (0.017)
Time Trend*I(t<=12)	**-0.079**** (0.017)	**0.003** (0.016)	**-0.053**** (0.017)	**-0.030** (0.021)	**0.008** (0.018)	**-0.121**** (0.014)
Average of Current and One-Year Lagged Growth Rate (State Level)	**-0.099**** (0.022)	**0.064**** (0.020)	**0.026** (0.018)	**-0.099**** (0.028)	**0.083**** (0.025)	**-0.003** (0.017)
Average of Two- and Three-Year Lagged Growth Rate (State Level)	**-0.035+** (0.018)	**0.017** (0.018)	**0.067**** (0.018)	**0.029** (0.026)	**0.050*** (0.022)	**0.000** (0.015)
Age Base Category: *16-25 Years*						
26-35 Years	**-0.118** (0.114)	**0.022** (0.089)	**-0.863**** (0.095)	**-0.117** (0.122)	**-0.199+** (0.104)	**-0.144+** (0.079)
36-45 Years	**0.149** (0.128)	**-0.187+** (0.110)	**-1.460**** (0.166)	**-0.343*** (0.152)	**-0.306*** (0.126)	**-0.583**** (0.108)
46-55 Years	**0.550**** (0.139)	**-0.813**** (0.161)	**-1.058**** (0.174)	**-0.077** (0.180)	**-0.798**** (0.175)	**-0.767**** (0.137)
56 and Older	**1.415**** (0.180)	**-1.369**** (0.366)	**1.729**** (0.154)	**0.049** (0.298)	**-1.588*** (0.666)	**0.676**** (0.147)
Vocational Education Base Category: *Vocational Training*						
No Vocational Training	**0.449**** (0.106)	**-0.228*** (0.115)	**0.639**** (0.089)	**0.157** (0.111)	**-0.042** (0.098)	**0.442**** (0.068)
College Degree	**-0.633**** (0.211)	**0.182+** (0.093)	**-0.236** (0.150)	**0.110** (0.223)	**0.452**** (0.149)	**0.606**** (0.148)
Part-time or Marginal	**-0.284** (0.208)	**0.665**** (0.147)	**1.470**** (0.092)	**-0.398**** (0.111)	**-0.141** (0.100)	**0.846**** (0.066)
Change of Questionnaire				**0.235** (0.192)	**-0.275** (0.178)	**0.335**** (0.117)
No. of Spells No. of Destination States	5266 996	5266 996	5266 1179	4582 628	4582 751	4582 1593
LR Chi2	177.76	87.13	1451.70	52.97	70.62	521.98

Note: Robust standard errors with respect to individuals reported in brackets; * indicates significance at the 5% significance level, ** at the 1% significance level and + at the 10% significance level.
Source: Own calculations based on the GSOEP 1984–1998. Only German citizens living in West Germany (Sample A), aged 16–56 at the beginning of the job spell, working full– or part–time. Excluding civil servants, apprentices and self–employed respondents, workers in agriculture, non–profit organizations and private households, and observations with missing values for age, sex, or education. For jobs lasting more than 12 months, observations are also excluded if reasons for job separation, job status, industry or firm size are missing. Data on GDP growth rates are taken from the Working Group"Volkswirtschaftliche Gesamtrechnung der Länder" (2004).

Table 2.5: Competing Risk Model of the Hazard of Job Termination with Combinations of Reasons for Job Separation and Transition States for Jobs Lasting Longer than 12 Months – Men

	Men		
	Layoff + Unemployment	Layoff + (New Job or Non-participation)	(Quit or Other Reason) + Unemployment
Time Trend	**0.029** (0.020)	**0.054+** (0.028)	**0.042+** (0.023)
Average of Current and One-Year Lagged Growth Rate (State Level)	**-0.254**** (0.048)	**-0.042** (0.066)	**-0.040** (0.047)
Average of Two- and Three-Year Lagged Growth Rate (State Level)	**-0.025** (0.042)	**-0.023** (0.049)	**-0.021** (0.039)
Age Base Category: *16-25 Years*			
26-35 Years	**-0.370** (0.263)	**0.330** (0.395)	**-0.361** (0.299)
36-45 Years	**-0.124** (0.299)	**0.585** (0.418)	**-0.266** (0.340)
46-55 Years	**0.228** (0.291)	**0.077** (0.459)	**-0.170** (0.334)
56 and Older	**0.771*** (0.333)	**-0.197** (0.674)	**1.356**** (0.324)
Vocational Education Base Category: *Vocational Training*			
No Vocational Training	**0.541**** (0.187)	**-0.411** (0.366)	**0.551*** (0.222)
College Degree	**-0.596+** (0.326)	**0.076** (0.322)	**-0.127** (0.267)
Part-time or Marginal	**-1.278** (1.023)	**1.425**** (0.541)	**0.813** (0.506)
Firm Size Base Category: *20-199 Employees*			
1-19 Employees	**0.574**** (0.190)	**0.389** (0.257)	**0.447+** (0.240)
>= 200 Employees	**-0.462*** (0.195)	**-0.525*** (0.248)	**-0.166** (0.209)
…Industry Affiliation as in reference model.			
No. of Spells No. of Destination States	2988 186	2988 104	2988 150
LR Chi2	192.38	50.63	125.22

For notes and sources. see Table 2.2.

Table 2.6: Competing Risk Model of the Hazard of Job Termination with Combinations of Reasons for Job Separation and Transition States for Jobs Lasting Longer than 12 Months – Women

	Layoff + Unemployment	Layoff + (New Job or Non-participation)	(Quit or Other Reason) + Unemployment
Time Trend	**-0.086** (0.054)	**0.019** (0.064)	**0.049** (0.047)
Average of Current and One-Year Lagged Growth Rate (State Level)	**-0.188**** (0.049)	**-0.080** (0.063)	**0.043** (0.053)
Average of Two- and Three-Year Lagged Growth Rate (State Level)	**0.037** (0.048)	**0.084** (0.059)	**0.032** (0.049)
Age Base Category: *16-25 Years*			
26-35 Years	**0.678*** (0.309)	**-0.411** (0.313)	**-0.103** (0.275)
36-45 Years	**0.831*** (0.330)	**-0.606+** (0.334)	**-0.475** (0.337)
46-55 Years	**0.959**** (0.356)	**-0.213** (0.369)	**-0.183** (0.363)
56 and Older	**0.607** (0.530)	**-0.319** (0.746)	**0.710** (0.445)
Vocational Education Base Category: *Vocational Training*			
No Vocational Training	**0.255** (0.205)	**0.387** (0.265)	**-0.184** (0.246)
College Degree	**-0.219** (0.483)	**0.215** (0.522)	**0.463** (0.383)
Part-time or Marginal	**-0.678**** (0.209)	**-0.135** (0.251)	**-0.053** (0.247)
Firm Size Base Category: *20-199 Employees*			
1-19 Employees	**0.694**** (0.217)	**0.412+** (0.250)	**0.567*** (0.262)
>= 200 Employees	**-0.473*** (0.240)	**-0.606*** (0.301)	**0.212** (0.236)
…Industry affiliation as in reference model.			
Change of Questionnaire	**0.512** (0.409)	**0.186** (0.558)	**-0.318** (0.363)
No. of Spells No. of Destination States	2415 129	2415 87	2415 120
LR Chi2	94.41	48.10	22.51

For notes and sources. see Table 2.2.

2.B The Data Set Combining Reported Job Changes with Monthly Spell Data

The data were extracted using the CD–Rom Version 2000 and SAS V8.

Steps:

1. Extraction of Cross–Sectional Data
2. Calendar Information
3. Creation of Change Variables
4. Splitting Spells
5. Adding Cross–Sectional Information to the Calendar Information
6. Reasons for Job Termination
7. Combining Spells with Adjacent Spells
8. Left–Censored Spells
9. Additional Data Sources
10. Selection

1. Extraction of Cross–Sectional Data The cross–sectional information is extracted and pooled into one data set where variables are named according to the wave of origin, i.e. branch85, branch86, etc.

2. Calendar Information The calendar information is rather simple and includes only a few important variables: persnr, spelltype, begin, end, censor. Spelltyp can take the following values:

1=full–time
2=short work hours
3=part–time
4=vocational training
5=unemployed
6=retired
7=maternity leave (only since 1990)
8=school, college
9=military

10=housewife, husband
11=second job
12=other

These form the basis for our analysis. However, there are several steps necessary to transform calendar information into job–spell information. First, we create a new variable “typnew” as follows, to distinguish major labor market groups:

typnew=.;
IF (spelltyp=1 or spelltyp=2) THEN typnew=1; /* Full–time*/

IF (spelltyp=4 or spelltyp=8) THEN typnew=2; /* School, training*/

IF spelltyp=5 THEN typnew=3; /* Unemployment */

IF spelltyp=3 THEN typnew=4; /* Part–time */

IF (spelltyp=6 or spelltyp=7 or spelltyp=9 or spelltyp=10 or spelltyp=11 or spelltyp=12) THEN typnew=5; /*Non–participation */

IF spelltyp=99 THEN typnew=99; /*missing*/

Parallel spells might be problematic in the following analysis. Take, for example, a worker with a full–time job, who reports a parallel spell at school or in a part–time job. For our job change analysis we are primarily interested in the main occupation. Therefore we eliminate parallel spells using the following procedure:
i) Within spelltypes ”typnew” parallel spells are eliminated by shortening the first spell.
ii) Between spelltypes ”typnew” the following hierarchy is used: full–time work, school/training, unemployment, part–time work, non–participation.

3. Creation of Change Variables The cross–sectional and calendar information is then combined into a single data set. First of all, two switch variables are created from the information on changes in the current year (e.g. in the 1989 interview for switches since January 1989) and from the information on switches in the previous year (e.g. in the 1989 interview for switches between January 1988 and December 1988). The data structure is such that one switch can be mentioned twice if there is a switch, for example, in March 1988 and there are interviews in April 1988 and March 1989. The exact question asked in the GSOEP in 1989 is:

”When did you give up your last job?
–1988 in the month of...... (previous year)
–1989 in the month of...... (current year)

The creation of two change variables, however, might create unrealistically short spells if respondents remember switches inaccurately. Therefore we assume that switches occurring in the interval [-2 months, 2 months] are the same job change. Thus, if a switch is reported for"March 1988" in 1988 and for "April 1988" in 1989, we use the information provided closer to the event reported, i.e. March 1988.

4. Splitting Spells The change variables are then used to split employment spells, full–time and part–time spells. Again, there might be problem with the information provided in the data set when respondents switch between full–time and part–time work (reported in the calendar) and report that switch with slight deviations in the cross–sectional question. To avoid unrealistically short spells, we check whether the split procedure creates short spells of up to two months. If this is the case, the split is not performed.

5. Adding Cross–Sectional Information to the Calendar Information Following the split procedure, we match spells with the relevant cross–sectional information. We create a data set with time–varying variables. First, there is information relevant to all spells within a year, like education and age. These data are added by simply checking whether the spell includes a certain year. By running the match procedure backwards from 1998 to 1985, we are sure to catch the earliest observation for each spell.

Second, there is job–relevant information (industry, occupation, firm size, worker status) from the cross–section that should only be used for the employment spell at the time of the interview. Therefore, we use the interview date to match the information. If the interview date is missing we use March, because this is the most frequent interview month.

6. Reasons for Job Termination If the interviewee indicates that s/he has given up a job since the beginning of the year prior to the interview, the following questions are posed :
(i) When did you give up your last job?
(ii) Why did you leave this job? Which of the following applies?

We classify the answers to this question into three groups.
1) Lay–off: Terminations initiated by the firm.
2) Quits: Terminations initiated by the worker.
3) Other reasons: End of fixed–term contract, maternity leave, (early) retirement, and leave of absence.
However, the possible answers to the question "Why did you leave the job" changed over our period of observation. One change was that severance by mutual agreement was a possible answer in the 1985–1990 period, whereas no such answer was possible

in the interviews of 1991–1998. However, in the first period an additional question was posed concerning the motivation for job termination, such that we are able to deduce whether, in case of a mutual agreement, the termination was initiated by the firm or by the worker. We conducted a number of sensitivity checks and did not find any indication for a structural break in the frequency of our reason for job termination variable for the questionnaire years 1990 to 1991 (when using our classification). A problematic change in the questionnaire was that, as of 1991, maternity leave was given as a possible reason for job termination. We could not generate corresponding information for the preceding years. Therefore we will take this structural break into account in our multivariate analysis.

7. Combining Spells with Adjacent Spells The calendar reports short–time work which is not a job switch. Therefore, we combine short–time work with the immediately preceding employment spell. Along the same lines, we combine adjacent full–time and part–time spells for which no reason for separation is known. The latter does not seem to influence the results significantly.

8. Left–Censored Spells The GSOEP gives a left–censored status to employment spells for which the start date is not known. In these cases, we match cross–sectional information on the time spent with the current employer, in order to deduce the start date of the job. In this way, left–censored spells become left truncated (a stock sample) and can be used in the duration analysis. We also use this approach for employment spells starting before 1984, as we do not have spell split information for the year 1983, the first year the monthly calendar was implemented in the GSOEP.

9. Additional Data Sources We merged information on the yearly growth rate of real GDP in the German states to the individual job spells. These data are provided by the Working Group "Volkswirtschaftliche Gesamtrechnung der Laender", see $http://www.vgrdl.de/Arbeitskreis - VGR$.

10. Selection The following selection applies:

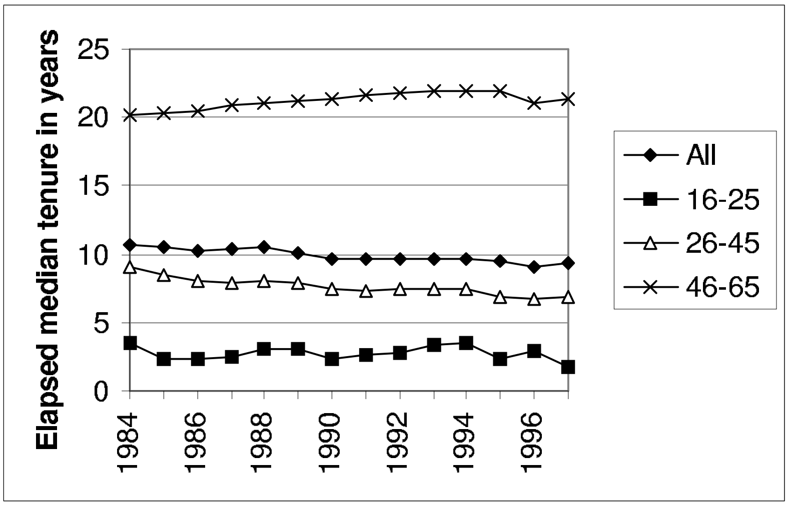
- Aged 16–56 at the beginning of the spell
- Without self–employed respondents
- Without apprentices
- Without civil servants
- Without agriculture (including forestry and fisheries)

- Without non–profit organizations
- 1984–1997
- NB: We could not include years 1998 onwards because the wording of the questionnaire changes in 1999 with respect to the job change information and thus, from 1998 onwards, we are unable to create the same variable for job change reason as before.
- Without spells that report end of training as reason for switch
- Without spells that report end of own business as reason for switch
- Full–time and part–time
- Sample A (West Germans)

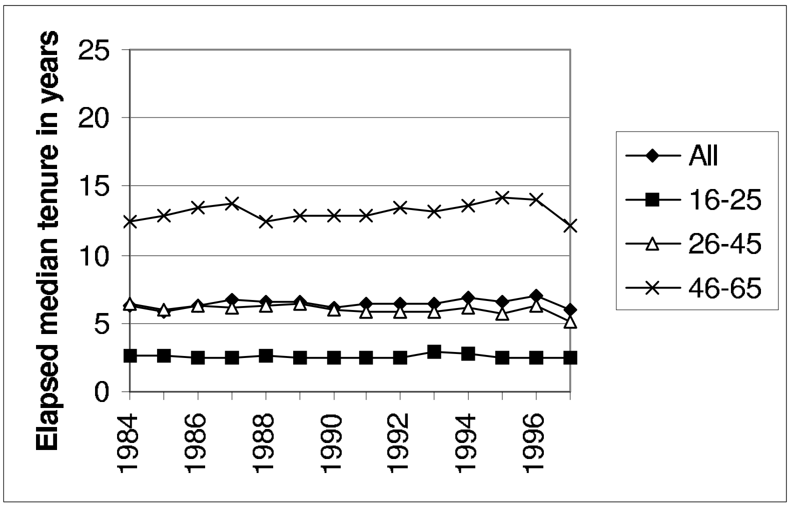
2.C Sensitivity Analysis for Elapsed Tenure 1984–1997 Using Weights

Figure 2.10: The Evolution of Median Elapsed Tenure by Age Groups Using Weights

Panel A - Men



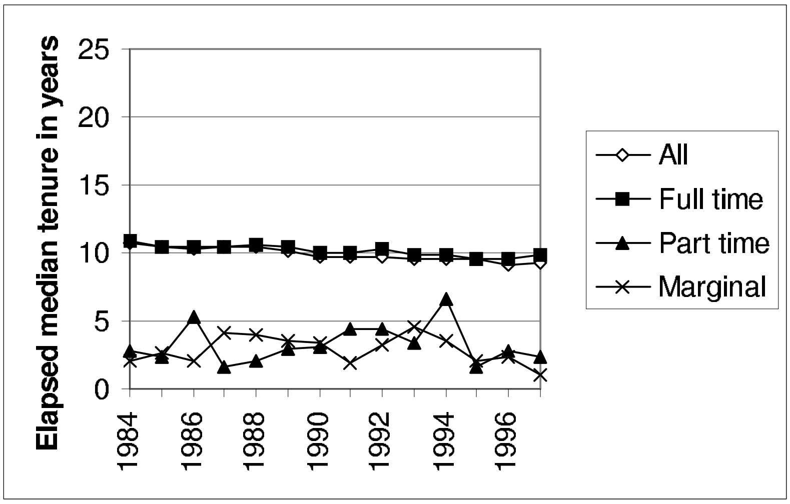
Panel B - Women



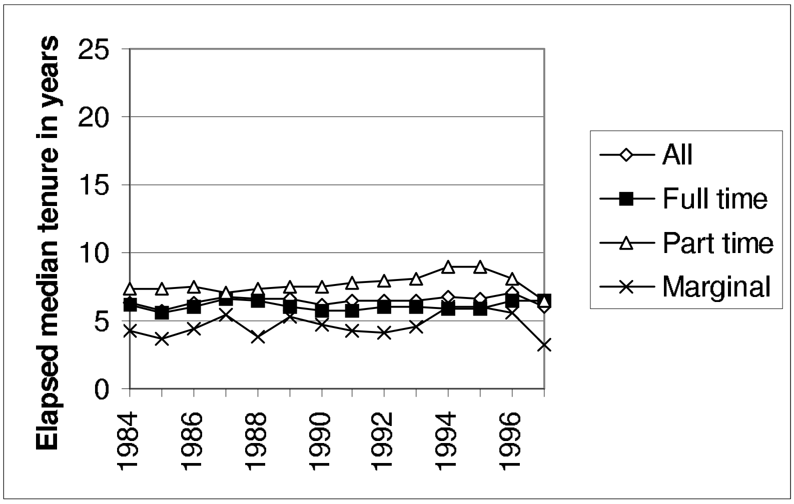
Note: All observations are weighted by the GSOEP sample weight. *Source:* Own calculations based on the GSOEP 1984–1997. Only German citizens living in West Germany (Sample A), aged 16–65, working full– or part–time. Excluding civil servants, apprentices and self–employed respondents, workers in agriculture, non–profit organizations and private households, and observations with missing values for age, sex, job status, industry affiliation or tenure.

Figure 2.11: The Evolution of Median Elapsed Tenure by Hours Worked Using Weights

Panel A - Men



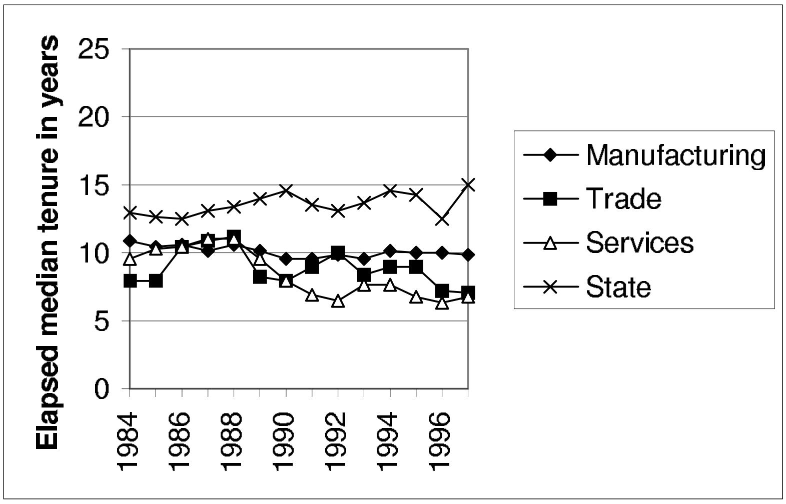
Panle B - Women



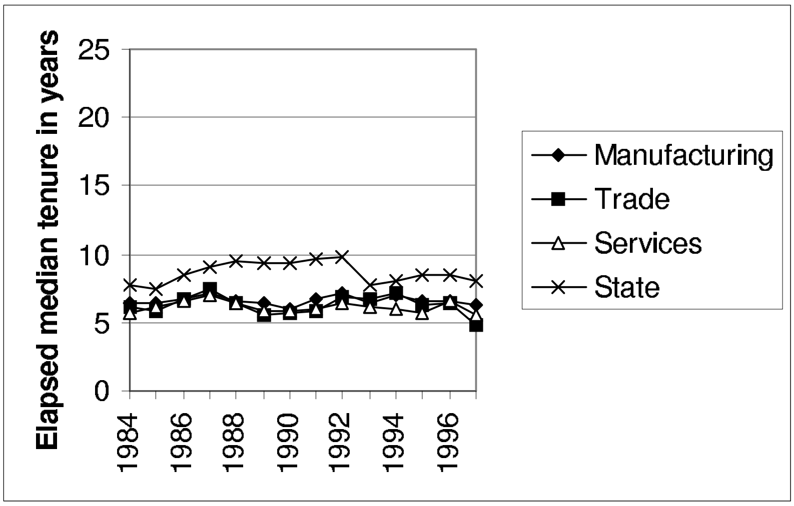
Notes and sources see figure 2.10

Figure 2.12: The Evolution of Median Elapsed Tenure by Industry Using Weights

Panel A - Men



Panel B - Women



Notes and sources see figure 2.10

2.D Sensitivity Analysis for Duration Analysis Using a Flow Sample from Reported Job Changes

2.D.1 The Data Set

In the GSOEP, workers report changes of the employment situation in the year before or during the year of the interview. With the help of this information we trace the job back to the point of time when it started. In the consecutive waves, we check whether and why jobs possibly ended. In this way, jobs, which cover the minimum of one interview date, can be detected. As the GSOEP consistently offers this information on the job start date only since the beginning of 1985, we only take spells into considerations, which began since 1985. The observation period ends in December 1996. There exists a problem if more than one job change occurred between two interview dates. In this case, the exact termination date of the first job is not available. However, we know the time period (maximum of one year), in which the job ended. We set the end of the job heuristically in the middle of this period. Jobs of people who drop out of the GSOEP are treated as censored.

We select for our analysis only the original West German sample A, containing German citizens only. In the multivariate analysis we only select workers, who started the job at age between 16 and 56. Civil servants, apprentices and the self–employed were equally dropped from our sample, as were workers in agriculture, non–profit organizations and private households. Finally, we did not include workers with missing values on either reason for termination, age, sex, education, firm size, and industry affiliation.

Here, we restrict ourselves on extracting a flow sample, which is equivalent to saying that we only extract job spells which start during the observation period, in which we can also observe their end, in case they would end. This is the approach usually taken when analyzing job stability with duration analysis (see Booth, et. al., 1999 or Gottschalk and Moffitt, 1999).

2.D.2 Multivariate Estimation Results

To receive results that are comparable to the analysis of elapsed tenure, we first ignore the reason for separation. We regress job duration with the aid of the Cox Proportional Hazard Rate Model on a time trend (and a dummy for the change in questionnaire for women), whereby the time trend is modeled as a time varying variable which takes the value 1 when the job was held in 1985, 2 when held in 1986 and so forth. Note that here and in the following we analyze all jobs, irrespectively of the job duration. The results are documented in Appendix section 2.D.3, Table

2.7. The data confirm the statements of the analysis on elapsed tenure. We find a tendency for a decline in job duration for men over time but no such result for women. In a second step, job duration until dismissal, quit or termination due to other reasons is purely regressed on the time trend and the dummy for the change in questionnaire for women. Appendix section 2.D.3, Table 2.8 presents the estimation results. As the coefficients show, job duration of men until layoff decreased significantly over time. For women, there also seems to be such a tendency over time, but this might also be due to the interaction with the change of questionnaire dummy. There are indications that job duration until quitting of men has slightly increased. Job duration of men ending out of other reasons decreased slightly over time. Since the change of questionnaire there is a significant increase in the frequency of answers of women to leave the job due to other reasons. Further determinants are included in the econometric model. Firstly, the growth rate of the real GDP is incorporated to capture the changes in the economic conditions of West Germany. Secondly, individual and job specific characteristics, similar to the reference model are included.

Table 2.9 in Appendix section 2.D.3 presents the estimation of the hazards of job termination for this model. Let us first consider the influence of the economic conditions on job duration. The state of the economy has a significant impact on job duration. In an economic slowdown, the risk of being given notice increases, whereas in an economic upturn the odds to resign increase. However, the significance of the coefficient on the growth rate in the model of quits for women on the 10% level might indicate that the relationship is not as simple as it is modeled here.

The issue of particular interest is the evolution of layoffs and quits over time. Therefore, let us turn to the coefficient on the time trend. First, the results for men are reported. There is still a clear tendency towards an increase in the hazard of being laid–off in this model. The coefficient is positive and significant. The hazard of quitting, on the other hand, is not influenced by the time. There is still an increasing trend in the hazard of leaving a job due to other reasons. The hazard of being laid off for women looses its significant time trend in the reference model and female quits and leaving due to other reasons continues depicting no significant change.

One might conjecture that the decrease of the job duration of men until termination due to other reasons is the result of the extended early retirement programs and so called 'social plans'. With the aid of the social plans to reduce the workforce, it was a relatively frequent practice to dissolve the working contract of older workers while paying the difference between unemployment benefits up to the year of early retirement (Börsch–Supan and Schnabel 1997). This can be interpreted as a kind of discharge in early retirement, which often occurred in mutual agreement.

The results concerning the influence of age on job duration can be summarized as follows: Young men at the age of 25 to 35 years are less likely to be laid off. Voluntary quitting is, however, more likely the younger the workers are and the probability to leave the job due to other reason decreases first with age but increases again in the age of 56 and older.

This fact as well as the argumentation on the early retirement programs suggests a special influence of age on job duration. Therefore, we interact the time trend with the age dummies. As Table 2.10 in Appendix section 2.D.3 shows, however, that the increased risk over time for men of being laid off and to end a job due to other reasons is relative equally distributed over age. Thus, the conjecture that early retirement programs and social plans might play a role for the increased risk for men of ending a job can not be confirmed with this data. However, it should be noted that due to the flow sample approach we have only few to zero observations for the oldest age categories. Women, in contrast, who are of age 36 to 45 became more likely whereas older women became less likely to be laid off over time. With respect to the hazard of job termination due to other reasons there is a slight decline in the female age category of 26–35.

Further results on the determinants of job duration can be summarized as follows. Vocational education has surprisingly little influence in determining job duration. Working in a small firm increases the odds to be laid off for men and to quit for men and women. Working in a large firm reduces the odd to quit and to leave the job due to other reasons for men, and it reduces the risk to be laid off for women.

Finally the results concerning industry affiliation should be reported. As expected, industry affiliation influences job duration, too. The dummies are in the specification for men to leave the job for other reasons and for women in the specification of layoffs and quits jointly significant on the 5% level. The direction of the effects is also less surprising. For example, working in the construction industry increases the hazard of being laid off for men, whereas working in public administration reduces the risk for men and women.

To summarize, the main results of this sensitivity analysis with respect to the development of job stability on the basis of a pure flow sample and a different data extraction method are similar to the main analysis. Slight differences emerge with respect to the influence of age and industry affiliation which are most likely due to the underrepresentation of long job spells in the flow sample.

2.D.3 Tables

Table 2.7: Hazard of Job Termination Using a Flow Sample from Reported Job Changes

	Men	Women
Time Trend	**0.034*** (0.017)	**-0.015** (0.024)
Change of Questionnaire		**0.194** (0.155)
No. of Spells	1006	1009
No. of Destination States	526	563
LR Chi2	4.09	1.71

Note: Robust standard errors in brackets clustered on individuals, * indicates significance at the 5% significance level, ** at the 1%significance level and + at the 10% significance level.
Source: Own calculation using the GSOEP.Only German citizens living in West Germany (Sample A) full–time and part–time employees, without missing values on reason for separation, age, sex, education, firm size and industry affiliation.

Table 2.8: Competing Risk Model of the Hazard of Job Termination with Respect to the Resons for Termination Using a Flow Sample from Reported Job Changes

	Men			Women		
	Layoff	Quit	Other Reasons	Layoff	Quit	Other Reasons
Time Trend	**0.159**** (0.033)	**-0.039+** (0.022)	**0.073+** (0.039)	**0.112*** (0.048)	**-0.043** (0.035)	**-0.056** (0.042)
Change of Questionnaire				**-0.610*** (0.300)	**0.066** (0.226)	**1.120**** (0.317)
No. of Spells	1006	1006	1006	1009	1009	1009
No. of Destination States	144	289	93	121	275	167
LR Chi2	22.97	3.13	3.45	5.78	2.39	14.00

For note and sources, see Table 2.7.

Table 2.9: Competing Risk Model of the Hazard of Job Termination with Respect to the Reasons for Termination Using a Flow Sample from Reported Job Changes

	Men			Women		
	Layoff	Quit	Other Reasons	Layoff	Quit	Other Reasons
Time Trend	**0.121****	**-0.023**	**0.089+**	**0.051**	**0.003**	**-0.003**
	(0.036)	(0.024)	(0.048)	(0.060)	(0.040)	(0.052)
Growth Rate of Real GDP	**-0.103***	**0.081****	**0.065**	**-0.166****	**0.060+**	**0.062**
	(0.042)	(0.031)	(0.059)	(0.042)	(0.035)	(0.044)
Age: Base Category: *16-25 Years*						
26-35 Years	**-0.444+**	**-0.266**	**-1.051****	**0.104**	**-0.098**	**0.154**
	(0.248)	(0.174)	(0.274)	(0.266)	(0.154)	(0.215)
36-45 Years	**-0.128**	**-0.725****	**-1.144****	**-0.301**	**-0.268**	**-0.886****
	(0.270)	(0.207)	(0.385)	(0.271)	(0.180)	(0.288)
46-55 Years	**0.368**	**-1.098****	**-2.026****	**-0.176**	**-1.124****	**-0.754***
	(0.298)	(0.339)	(0.639)	(0.335)	(0.289)	(0.339)
56 and Older		**-0.630**	**0.267**	**-0.136**	**-1.344**	**-0.821**
		(0.525)	(0.541)	(0.777)	(1.028)	(0.876)
Vocational Education Base Category: *Vocational Training*						
No Vocational Training	**0.279**	**-0.186**	**0.187**	**0.173**	**0.016**	**0.229**
	(0.272)	(0.209)	(0.318)	(0.256)	(0.173)	(0.204)
College Degree	**-0.382**	**0.178**	**0.021**	**-0.224**	**0.095**	**0.621****
	(0.282)	(0.152)	(0.315)	(0.436)	(0.258)	(0.232)
Firm Size Base Category: *20-199 Employees*						
1-19 Employees	**0.731****	**0.265+**	**0.166**	**0.176**	**0.319+**	**0.129**
	(0.210)	(0.157)	(0.249)	(0.215)	(0.174)	(0.213)
> 200 Employees	**-0.198**	**-0.433****	**-0.688****	**-0.883****	**-0.216**	**0.150**
	(0.237)	(0.156)	(0.260)	(0.260)	(0.178)	(0.189)

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Industry Affiliation Base Category: *Manufacturing*						
Mining			**1.252**** (0.422)		**4.418**** (0.387)	
Energy	**-0.807** (1.064)		**0.230** (0.708)		**0.560** (0.644)	**0.528** (0.687)
Construction	**0.419+** (0.251)	**0.009** (0.190)	**-0.136** (0.359)	**0.294** (0.424)	**-0.654** (0.480)	**0.175** (0.394)
Trade	**0.287** (0.294)	**0.335** (0.214)	**-0.271** (0.371)	**0.122** (0.253)	**0.160** (0.175)	**-0.065** (0.235)
Traffic and Communication	**0.244** (0.310)	**0.060** (0.259)	**-0.806** (0.592)	**-0.339** (0.738)	**-0.097** (0.440)	**0.017** (0.563)
Credit and Insurance		**0.247** (0.290)	**-0.241** (0.755)	**-0.845** (0.609)	**-0.281** (0.311)	**-1.172*** (0.583)
Other Services	**0.083** (0.285)	**0.396*** (0.174)	**0.594*** (0.294)	**-0.446+** (0.233)	**-0.022** (0.170)	**0.127** (0.205)
Government and Social Security	**-1.806+** (1.028)	**-0.106** (0.301)	**0.843*** (0.377)	**-2.444*** (0.993)	**-0.257** (0.288)	**-0.181** (0.321)
Change of Questionnaire				**-0.474** (0.363)	**-0.061** (0.234)	**0.931**** (0.326)
No. of Spells No. of Destination States	1006 144	1006 289	1006 93	1009 121	1009 275	1009 167
LR Chi2	95.55	62.93	102.55	69.25	386.50	60.90

For notes and sources, see Table 2.7. Data on GDP growth rates are taken from the Working Group "Volkswirtschaftliche Gesamtrechnung der Länder" (2004).

Table 2.10: Competing Risk Model of the Hazard of Job Termination with Respect to the Reasons for Termination Using a Flow Sample from Reported Job Changes – Extended Model

	Men			Women		
	Layoff	Quit	Other Reasons	Layoff	Quit	Other Reasons
Time Trend	**0.252****	**-0.100+**	**0.119+**	**0.000**	**-0.058**	**0.113**
	(0.085)	(0.054)	(0.072)	(0.080)	(0.056)	(0.074)
Growth Rate of Real GDP	**-0.101***	**0.085****	**0.064**	**-0.173****	**0.065+**	**0.059**
	(0.042)	(0.031)	(0.060)	(0.042)	(0.035)	(0.044)
Age Base Category: *16-25 Years*						
26-35 Years	**1.016**	**-0.824***	**-0.660**	**-0.475**	**-0.687+**	**1.221+**
	(0.924)	(0.398)	(0.763)	(0.693)	(0.376)	(0.643)
36-45 Years	**1.097**	**-1.373***	**-0.306**	**-1.975***	**-0.587**	**0.308**
	(0.971)	(0.549)	(1.011)	(0.861)	(0.456)	(0.908)
46-55 Years	**1.607**	**-1.851+**	**-4.048**	**1.036**	**-2.340***	**1.510**
	(1.105)	(1.008)	(2.878)	(0.864)	(0.939)	(1.552)
56 and Older		**-0.833**	**-2.955**	**6.671****	**-6.285****	**-1.130**
		(1.267)	(2.343)	(1.842)	(1.916)	(1.268)
Time Trend*Age Base Category: Time Trend*16-25 Years						
*Time Trend*26-35 Years*	**-0.172+**	**0.088**	**-0.051**	**0.076**	**0.086+**	**-0.137+**
	(0.099)	(0.059)	(0.094)	(0.087)	(0.051)	(0.073)
*Time Trend*36-45 Years*	**-0.144**	**0.100**	**-0.105**	**0.195***	**0.052**	**-0.152**
	(0.104)	(0.073)	(0.123)	(0.098)	(0.061)	(0.106)
*Time Trend*46-55 Years*	**-0.146**	**0.113**	**0.213**	**-0.147**	**0.160**	**-0.289**
	(0.122)	(0.126)	(0.287)	(0.110)	(0.108)	(0.201)
*Time Trend*56 and Older*		**0.044**	**0.311**	**-0.735****	**0.498****	**0.006**
		(0.162)	(0.232)	(0.227)	(0.161)	(0.149)
Vocational Education Base Category: *Vocational Training*						
No Vocational Training	**0.273**	**-0.183**	**0.184**	**0.165**	**0.021**	**0.220**
	(0.273)	(0.207)	(0.321)	(0.254)	(0.173)	(0.207)
College Degree	**-0.395**	**0.177**	**0.012**	**-0.221**	**0.110**	**0.621****
	(0.279)	(0.154)	(0.315)	(0.434)	(0.260)	(0.233)

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Firm Size Base Category: *20-199 Employees*						
1-19 Employees	**0.715**** (0.211)	**0.273+** (0.157)	**0.130** (0.254)	**0.215** (0.218)	**0.307+** (0.175)	**0.134** (0.212)
> 200 Employees	**-0.185** (0.237)	**-0.439**** (0.156)	**-0.686*** (0.270)	**-0.880**** (0.261)	**-0.224** (0.178)	**0.155** (0.189)
Mining			**1.244**** (0.421)		**4.780**** (0.532)	
Energy	**-0.761** (1.068)		**0.221** (0.723)		**0.529** (0.645)	**0.541** (0.670)
Construction	**0.421+** (0.253)	**0.011** (0.190)	**-0.139** (0.359)	**0.363** (0.423)	**-0.672** (0.479)	**0.187** (0.399)
Trade	**0.276** (0.295)	**0.346** (0.215)	**-0.314** (0.374)	**0.155** (0.253)	**0.164** (0.176)	**-0.064** (0.237)
Traffic and Communication	**0.258** (0.310)	**0.059** (0.259)	**-0.811** (0.607)	**-0.406** (0.760)	**-0.131** (0.437)	**0.057** (0.579)
Credit and Insurance		**0.243** (0.291)	**-0.261** (0.755)	**-0.757** (0.615)	**-0.291** (0.310)	**-1.153*** (0.582)
Other Services	**0.087** (0.284)	**0.394*** (0.176)	**0.576*** (0.294)	**-0.456+** (0.233)	**-0.024** (0.170)	**0.130** (0.207)
Government and Social Security	**-1.812+** (1.023)	**-0.098** (0.300)	**0.791*** (0.397)	**-2.454*** (0.990)	**-0.273** (0.288)	**-0.154** (0.322)
Change of Questionnaire				**-0.425** (0.363)	**-0.014** (0.240)	**0.902**** (0.324)
No. of Spells No. of Destination States	1006 144	1006 289	1006 93	1009 121	1009 275	1009 167
LR Chi2	115.19	66.71	121.24	90.99	396.93	68.73

For notes and sources, see Table 2.7. Data on GDP growth rates are taken from the Working Group “Volkswirtschaftliche Gesamtrechnung der Länder”(2004).

Chapter 3

Evaluating the Dynamic Employment Effects of Training Programs in East Germany Using Conditional Difference–in–Differences

3.1 Introduction

After the formation of the German "Social and Economic Union" in 1990, the East German economy underwent enormous changes. It had to transform from a command driven backward economy to a market economy at an unprecedented speed. The transformation process brought about high unemployment in East Germany. To increase the employment chances of the unemployed, the German government decided to provide on a high scale Active Labor Market Policies (ALMP) in East Germany. These programs mainly consisted of training and temporary employment schemes. In 2002, more than a decade after the reunification, the German Federal Employment Service (*Bundesanstalt für Arbeit*, BA) still spent around 20 Billion Euro ($\approx$ 0.9% of the GDP) for ALMP (Bundesanstalt für Arbeit, 2003). About 50% of this budget is spent in East Germany even though the labor force in East Germany is less than one sixth of Germany as a whole. Quite a significant share of the labor force in East Germany has been participating in programs of ALMP since 1990.

Contributing to the debate on the effectiveness of ALMP, this paper estimates the employment effect of public sector sponsored training programs in East Germany at the individual level for the time period 1990 to 1999. In the early 90s, training was often considered to be the most effective among the ALMP programs. It was intended to provide skills that are in demand in a market economy but not in sufficient supply due to the former educational system. Consequently, it took a prominent role in the ALMP programs in East Germany.

In our empirical analysis we focus on the group of individuals who belonged to the active labor force in 1990. This group was hit fully by the transformation shock. We use data from the Labor Market Monitor for the state of Sachsen–Anhalt (*Arbeitsmarktmonitor Sachsen–Anhalt* LMM–SA), a data set allowing for monthly information on employment and program participation.

We implement a semiparametric conditional difference–in–differences estimator (CDiD) (Heckman/Ichimura/Smith/Todd, 1998). In the light of the state dependence of the employment process we extend the CDiD approach to use transition rates between different labor market states as outcome variables instead of exclusively use employment rates in levels as often done in the literature.

For the implementation of the CDiD estimator, we apply propensity score matching in the first stage and then estimate average effects of treatment–on–the–treated. The analysis matches treated individuals to nonparticipants using kernel matching to account for selection on observables. Selection on time invariant unobservable characteristics is controlled for using a conditional difference–in–differences estimator. Our inference uses a bootstrap approach taking account of the estimation error in the propensity score. We perform a sensitivity analysis on the implementation details of the evaluation approach.

Our results indicate that modeling transition rates is more appropriate than using unconditional employment rates. Using only employment rates as success criterion could result in misleading conclusions concerning the effectiveness of ALMP programs. With regard to the transition rates, we find that the employment effects are mostly insignificant and that there are some significantly positive effects for selected start dates.

In addition, our results show the usefulness of exploiting the additional information which transition rates can provide as opposed to unconditional employment rates. With the aid of transition rates we are able to determine whether ALMP programs help to find a job and/or whether they rather stabilize employment. Our results show significant variation over time concerning these two outcomes.

We make three additional points in the methodological debate on program evaluation: First, anticipation effects regarding future participation or eligibility criteria (Ashenfelter's Dip) requiring a certain elapsed duration of unemployment for participation are likely to affect strongly the results of any difference–in–differences estimator (Heckman and Smith, 1999). Using institutional knowledge to bound the start of the Ashenfelter's Dip, we suggest a long–run difference–in–differences estimator to take account of possible effects of anticipation or participation rules.

Second, we suggest a heuristic cross–validation procedure for the bandwidth choice which is well suited to the estimation of conditional expectations for counterfactual variables.

The third point relates to the fact that in East Germany individuals often participate more than once in a program during a short time period. Some observers (e.g. Hagen and Steiner, 2000) suggest that multiple program participation occurs because the participants cannot (or do not want to) find a job after the end of the first program. In order to keep their transfer income (and possibly in order to lower the level of official unemployment), these persons participate in a further program "carousel effect"). To address this issue, our study estimates the effects of participation in training as first program and of participation in a second program afterwards, be it a second training program or a job creation program. Regarding participation in a second program, we estimate both the incremental effect of the second program and the combined effect using our difference–in–differences approach.[1]

Former studies on the effect of ALMP in East Germany on the individual employment chances provide mainly negative though unclear evidence, see the surveys in Fitzenberger and Speckesser (2002) and Hagen and Steiner (2000). The existing studies suffer greatly from data limitations, either they are plagued by a small number of participants (German Socio-Economic Panel, e.g. Lechner, 1998) or the data is limited to the early 1990's and does not allow for constructing the employment history on a monthly basis (Labor Market Monitor for East Germany, e.g. Fitzenberger/Prey, 2000).[2]

The paper is organized as follows: Section 3.2 gives a short description of the institutional background for ALMP in East Germany and discusses descriptive evidence. Section 3.3 develops the microeconomic evaluation approach used here. The implementation of the approach is described and the empirical results of the evaluation are discussed in section 3.4. Section 3.5 concludes. The appendix 3.A includes detailed tables and appendix 3.B a sensitivity analysis with respect to the underlying behavioral model.

[1] Some progress has been made in the methodological literature in order to extend standard static evaluation approaches to the dynamic selection issue involved here, see Lechner/Miquel (2001). The requirements on the data when applying the Lechner/Miquel approach are unlikely to be satisfied in our case (see section 3 below) and we are not aware of an actual application of this approach.

[2] Our earlier paper Bergemann et al. (2000), where the impact on employment rates is estimated for the period 1990 to 1998, is an exception. Reliable administrative data on ALMP has so far not been made available.

3.2 Training in East Germany

3.2.1 Background

Between 1969 and 1997, training as part of Active Labor Market Policy in Germany was regulated by the Labor Promotion Act (*Arbeitsförderungsgesetz*, AFG). Despite a number of changes in the regulation over this time period, the basic design of training programs remained almost unchanged until the AFG was replaced by the new Social Law Book III (SGB III) in 1998. The German Federal Labor Office (*Bundesanstalt für Arbeit*, BA) was in charge of implementing these programs in addition to being responsible for job placement and for paying out unemployed benefits. Training programs under the AFG rule fell into four categories:[3] Further Vocational Training (*Fortbildung*), Re–training (*Umschulung*), Short–term training (*Kurzzeitmaßnahmen nach § 41a AFG*) and Integration subsidies (*Einarbeitungszuschuss*, §§ 33 – 52 AFG). With German unification, these programs were extended to East Germany after July 1990 (§ 249 AFG). Policy makers intended to foster the adjustment of the East German human capital stock to Western levels. The large and prolonged use of ALMP was also justified by equity goals (the standard of living in East Germany should converge quickly to Western levels) and by political goals (political stability after a massive transformation shock and avoiding large scale outmigration).

3.2.2 Training under the Labor Promotion Act 1990–1997

Further vocational training (§ 41 AFG) consists of the assessment, maintenance, and extension of skills. The duration of the courses depends on the characteristics of the participants. The courses regularly take between 2 and 8 months and are mainly offered by private sector training companies.

Re–training enables vocational re–orientation if no adequate employment can be found because of skill obsolescence. Re–training is supported by the BA for a period up to 2 years and aims at providing a new certified vocational training degree.

Short–term training aim at increasing the employment chances by skill assessment, orientation, and guidance. The courses are intended to increase the placement rate of the unemployed. Mostly, they do not provide occupational skills but aim at maintaining search intensity and increasing hiring chances. The courses usually last from two weeks to two months.

Integration Subsidies involve payments to employers providing employment to previously unemployed workers who need a training period. The worker earns a regular

[3] We ignore German language courses which have different target groups.

wage from the employer. This program is included in official numbers of participation in training programs. However, it is not analyzed in our empirical analysis because the data used do not allow to identify it.

Except for integration subsidies, all participants in full–time courses are granted an income maintenance payment (*Unterhaltsgeld*) if the conditions of entitlement are satisfied. To qualify, persons must meet the requirement of being previously employed for a minimum duration during a set period of time, i.e. at least one year in employment or receipt of unemployment benefit or subsequent unemployment assistance. The set period may be extended for individuals returning to the labor market.

The income maintenance payment amounts to the same level as unemployment benefits, i.e. to 67% (60%) of previous net earnings for participants with (without) at least one dependent child. The income maintenance payments used to be higher in the early 1990's (see below). If a person does not fulfill the requirement of previous employment, but received unemployment assistance until the start of treatment, the income maintenance may be paid as well. Participants re–qualify for unemployment benefits providing an additional incentive for participation. The BA also covers all the direct training costs such as course fees.

3.2.3 Changes in Programs and Incentives

During the 1990's, legislation modified the types of programs, the level of income maintenance payments, and the eligibility criteria. *Short–term training programs* were abolished formally in 1992 and in 1993, a new program started with the same purpose. However, participants were no longer considered as taking part in training programs and were therefore recorded as unemployed. *Income maintenance payments* were reduced after 1993 from 68% (63%) of the net earnings during previous employment for participants with (without) children to 63% (60%).

Before 1994, participation in a training program was accessible for participants without having experienced unemployment beforehand as long as the case worker considered participation in training as "*advisable*". This type of training intended to prevent future unemployment, to increase the labor market prospects of the employed in the future, or to foster re–integration of individuals returning to the labor market. Starting in 1994, the access was restricted to individuals fulfilling the criteria for "*necessary*" training which basically restricted the program to formerly unemployed participants. However, especially in East Germany, the participation under the weak criterion of "being threatened by unemployment" was still possible.

The changes resulted in a new mix of participants in training programs and they somewhat shifted the focus of training. A credible evaluation strategy has to account for these changes.

The end of explicit short–term training programs made the remaining programs longer and more expensive on average. In addition, the remaining program mix was less explicitly focussed on a immediate placement of participants. The mix of programs observed after the change is more strongly focussed on providing additional skills and helping participants to signal their skills. We suspect therefore that, on the one hand, incentives to participate are stronger on average than for the program mix before the reform. This may result on average in stronger anticipation effects such that in the prospect of participation unemployed individuals decrease their search effort for a new job.

On the other hand, training programs became less attractive, especially for workers who are still employed, both due to the lower income maintenance payments and due to the focus on previously unemployed individuals. Over time, a change in the selection of the program group occurred restricting training to problem groups with a priori significantly lower employment chances.

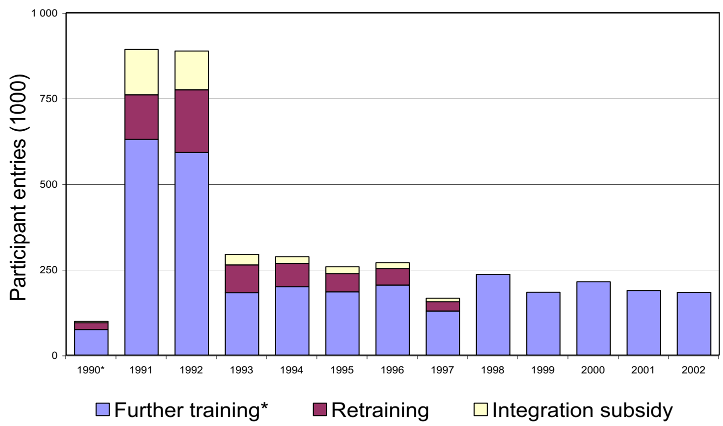
3.2.4 Aggregate Participation

Training programs were implemented in East Germany immediately after unification (see figure 3.1): 98,500 persons started to participate during the last three months of 1990. In 1991, the maximum was reached with 892,145 entries. Only in 1992, there was a similar magnitude. Afterwards the number was much lower and it went down to 166,000 in 1997. During the most recent years, participation recovered at a level slightly above 180,000 reflecting the ongoing importance of these programs in East Germany. The share of entries into re–training in percent of training in total varies between 15% in 1991 and 28% in 1993, the share for integration subsidies declines from 15% in 1991 to 8% in 1997. No separate figures are available neither for short–term training and further vocational training for the early 1990's nor for the subprograms after 1997 due to the change in the regulation.

Stocks of participants show a similar pattern (see figure 3.2). The maximum was reached in 1992, amounting to 492,000 participants on average. Participation has been declining afterwards (2000: 139,700, 2002: 129,000 participants). The trends for the subprograms (not reported in figure 3.2) are analogous.

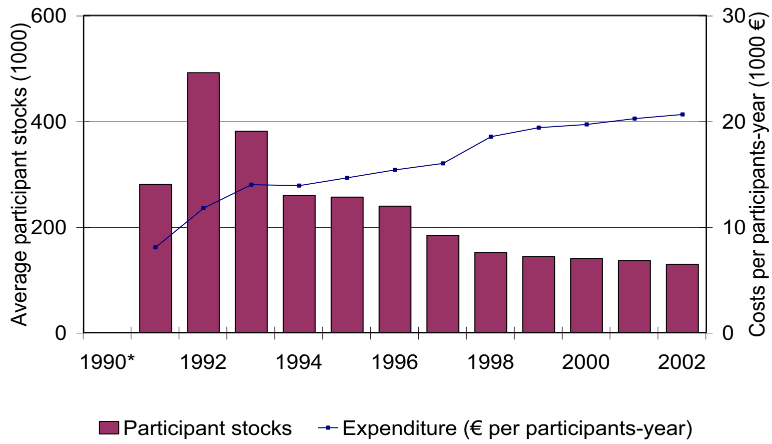
Direct costs for participation paid by the BA (see figure 3.2, right axis) – income maintenance, course fees, travel costs etc. – continuously increased over time. In 1991, when short–term training programs still existed, annual costs were at 8,000 Euro per participant. With 14,600 Euro in 1995 and the most recent number being 20,600 Euro, the programs observed became much more expensive over time.

Figure 3.1: Entries into Training in East Germany, Annual Totals



* In 1990, only in October, November and December training programs took place. Following the 1998 reform, further training can no longer be subdivided into three categories. Source: Bundesanstalt für Arbeit (1993, 1997, 2001a, 2003a), own calculations

Figure 3.2: Participation Stocks in Training and Expenditure per Participant / Year, Annual Average



* For 1990 no yearly stock can be calculated. Source: Bundesanstalt für Arbeit (1993, 1997, 2001a, 2003a), own calculations

3.3 Evaluation Approach

Our empirical analysis is based upon the potential–outcome–approach to causality (Roy, 1951, Rubin, 1974), see the survey Heckman/LaLonde/Smith (1999). We focus on estimating the average causal effect of treatment–on–the–treated (TT) in the binary treatment case.[4] TT is given by

$$E(Y^1|D=1) - E(Y^0|D=1) \;, \tag{3.1}$$

where the treatment outcome Y^1 and the nontreatment outcome Y^0 are the two potential outcomes and D denotes the treatment dummy. Our outcome variable of interest is a dummy variable for employment, possibly conditional on employment in the previous month resulting in a transition dummy. The observed outcome Y is given by $Y = DY^1 + (1-D)Y^0$. The evaluation problem consists of estimating $E(Y^0|D=1)$ since the counterfactual outcome in the nonparticipation situation is not observed for the participating individuals ($D = 1$). Thus, identifying assumptions are needed to estimate $E(Y^0|D=1)$ based on the outcomes for nonparticipants ($D = 0$).

We apply a conditional difference–in–differences (CDiD) approach which combines two widely used concepts to estimate the average nontreatment outcome for the treated $E(Y^0|D=1)$. One is to consider the situation of program participants before treatment (before–after–comparison) and the other is to consider a control group of comparable persons who did not participate. The major drawback of the before–and–after comparison lies in the assumption of a constant average nontreatment outcome over time for the treated population. This is violated, if over time labor market outcomes change irrespective of participation, i.e. formally $E(Y^0_{t0}|D=1) \neq E(Y^0_{t1}|D=1)$ where $t0$ is a point of time before treatment and $t1$ after treatment. Another issue involves participation rules and possible anticipation effects of the treatment (Ashenfelter's Dip) resulting in Y^0_{t0} already being affected by the treatment in the future. Regarding the selection of an appropriate control group, it is usually not warranted to assume that the average nonparticipation outcome of the participants is the same as for the nonparticipants, i.e. we have $E(Y^0|D=1) \neq E(Y^0|D=0)$. Thus, a readily available sample estimate for $E(Y^0|D=0)$ is not a consistent estimate for the counterfactual $E(Y^0|D=1)$.

[4] The framework can be extended to allow for multiple, exclusive discrete treatments. Lechner (1999) and Imbens (2000) show how to extend standard propensity score matching estimators for this purpose and e.g. Larsson (2003) provides an application to ALMP in Sweden based on a large and quite homogeneous treatment and comparison group. Although, it would be a natural extension in our application to explicitly allow for multiple, exclusive treatments by ALMP, we do not think that our data is sufficiently rich enough for this purpose. In addition, our analysis is much more demanding since we argue that matching on observable covariates will not suffice to control for selection bias and since we model the effects on transition rates between different labor market states. Therefore, we restrict ourselves to estimating TT for training where the comparison group is the group of all individuals who either do not participate in any program or who only participate in other programs where the latter two are weighted by their sample frequencies.

3.3.1 Selection on Observables and Matching

Assuming the Conditional Mean Independence Assumption (CIA)

$$E(Y^0|D=1,X) = E(Y^0|D=0,X) \tag{3.2}$$

implies that the nontreatment outcome of the participants and of the nonparticipants are now comparable in expectation when conditioning on X. Then, to estimate the expected nonparticipation outcome for the participants with observable characteristics X, it suffices to take the average outcome for nonparticipants with the same X. This is the basis of the popular matching approach, see Heckman/Ichimura/Todd (1998), Heckman/Ichimura/Smith/Todd (1998), Heckman/LaLonde/Smith (1999), or Lechner (1998). This approach estimates the expected nontreatment outcome for a participant i with characteristics X by the fitted value of a nonparametric regression in the sample of nonparticipants at point X. The nonparametric regression can be represented by a weight function $w_{N_0}(i,j)$ that gives the higher a weight to nonparticipants j the stronger his similarity to participant i regarding X. For each i, these weights sum up to one over j ($\sum_{j\in\{D=0\}} w_{N_0}(i,j)=1$). The estimated TT is then

$$\frac{1}{N_1}\sum_{j\in\{D=1\}}\left\{ Y_i^1 - \sum_{j\in\{D=0\}} w_{N_0}(i,j)\,Y_j^0 \right\}, \tag{3.3}$$

with N_0 the number of nonparticipants j and N_1 the number of participants i.

Matching estimators differ with respect to the weights attached to members of the comparison group. The most popular approach in the literature is nearest neighbor matching just using the outcome for the closest nonparticipant ($j(i)$) as the comparison level for participant i, see Heckman/LaLonde/Smith (1999) and Lechner (1998). In this case, $w_{N_0}(i,j(i))=1$ for the nearest neighbor $j(i)$ and $w_{N_0}(i,j)=0$ for all other nonparticipants $j \neq j(i)$. Following Heckman/Ichimura/Smith/Todd (1998), we implement a different matching approach using a nonparametric local linear kernel regression to estimate the expected nonparticipation outcome of participants with certain characteristics, see also Pagan/Ullah (1999). This amounts to specifying the weight function based on a kernel function which has as its argument the distance in terms of characteristics of the individuals.[5] This so called kernel matching has a number of theoretical advantages compared to nearest neighbor matching. The asymptotic properties of kernel based methods are straightforward to analyze and it has been shown that bootstrapping provides a consistent estimator of the sampling variability of the estimator in (3.3) even if matching is based on closeness in generated variables (this is the case with the popular method of propensity score matching which will be discussed below), see Heckman/Ichimura/Smith/Todd

[5] We also checked the sensitivity of our results by using nearest neighbor matching without and with caliper (the latter allows only for matches which are sufficiently close). For our application, it turned out that the choice of matching approach had no notable impact on the estimated treatment effects. We only report the results using kernel matching.

(1998) or Ichimura/Linton (2001) for an asymptotic analysis of kernel based treatment estimators. We are not aware of similar results for nearest neighbor matching.

It is difficult to match with respect to a high–dimensional vector of observable characteristics X ("curse–of–dimensionality"), see Pagan/Ullah (1999). Therefore, the evaluation literature uses extensively the result of Rosenbaum and Rubin (1983) that the CIA in equation (3.2) implies that participants and nonparticipants become comparable in expectation when conditioning on the treatment probability $P(X)$ (propensity score) as a function of the observable characteristics X, i.e.

$$E(Y^0|D=1,P(X)) \;=\; E(Y^0|D=0,P(X)) \tag{3.4}$$

provided $0 < P(D=1|X) < 1$. This result reduces the matching problem to one dimension effectively using the "closeness" in the propensity score as the weighting scheme. However, the propensity score has to be estimated. We implement kernel matching based on the estimated propensity score. We take account of the sampling variability in the estimated propensity score by applying a computationally quite expensive bootstrap method to construct the standard errors of the estimated treatment effects. To account for autocorrelation over time, we use the entire time path for each individual as block resampling unit. All the bootstrap results reported in this paper are based on 200 resamples.

For the local linear kernel regression in the sample of nonparticipants, we use the Gaussian kernel, see Pagan/Ullah (1999).[6] Standard bandwidth choices (e.g. rules of thumb) for pointwise estimation are not advisable here since the estimation of the treatment effect is based on the average expected nonparticipation outcome for the group of participants, possibly after conditioning on some information to capture the heterogeneity of treatment effects. Since averaging pointwise estimates reduces the variance, it is clear that the asymptotically optimal bandwidth should go to zero faster than an optimal bandwidth for a pointwise estimate, see Ichimura/Linton (2001) on such results for a different estimator of treatment effects.[7]

To choose the bandwidth, we suggest the following heuristic leave–one–out cross–validation procedure which mimics the estimation of the average expected nonparticipation outcome for each period. First, for each participant i, we identify the nearest neighbor $nn(i)$ in the sample of nonparticipants, i.e. the nonparticipant whose propensity score is closest to that of i. Second, we choose the bandwidth to

[6] A kernel function with unbounded support avoids some of the problems involved with local linear kernel regression, namely, that the variance can be extremely high in areas where there is not a lot of data, see Seifert/Gasser (1996) and Frölich (2001) for a critical assessment of local linear kernel regression.

[7] This is also the rationale for researchers using nearest neighbor matching with just the closest neighbor thus focussing on minimizing the bias.

minimize the sum of the period–wise squared prediction errors

$$\sum_{t=1}^{T}\left[\frac{1}{N_{1,t}}\sum_{i=1}^{N_{1,t}}\left(Y^0_{nn(i),t}-\sum_{j\in\{D=0\}\setminus nn(i)} w_{i,j}Y^0_{j,t}\right)\right]^2$$

where the prediction of employment status for $nn(i)$ is not based on the nearest neighbor $nn(i)$ himself and $t = 1, ..., T$ denotes the month ($T = 120$ for our data). The optimal bandwidth affecting the weights $w_{i,j}$ through the local linear regression is determined by a one–dimensional search. The resulting bandwidth is typically smaller than a rule–of–thumb value for pointwise estimation, but this is not always the case, see Ichimura/Linton (2001) for similar evidence in small samples based on simulated data. Since our method for the bandwidth choice is computationally quite expensive, it is not possible to bootstrap it. Instead, we use the bandwidth found for the sample in all resamples.

3.3.2 Employment Model and Ashenfelter's Dip

We specify the econometric model for employment in order to be clear about which treatment parameters are estimated. The dummy variable for employment Y_{it} of individual i in month t exhibits strong state dependence, i.e. holding everything else constant the probability to remain employed $P(Y_{it} = 1 \mid Y_{i,t-1} = 1)$ given that i is employed in the previous month is likely to be much higher than the reemployment probability $P(Y_{it} = 1 \mid Y_{i,t-1} = 0)$ given that i is not employed in the previous month.[8] Therefore, the dynamic employment process for individual i is specified using separate outcome equations depending on the state in the previous month as

$$(3.5)\, Y_{it} = \begin{cases} a^e(X_i,t)+\delta^e_{i,t,\tau}D_{i,t}(\tau)+c^e_i+u^e_{i,t} & \text{for} \quad Y_{i,t-1}=1 \quad \text{(employed before)} \\ a^n(X_i,t)+\delta^n_{i,t,\tau}D_{i,t}(\tau)+c^n_i+u^n_{i,t} & \qquad Y_{i,t-1}=0 \quad \text{(not empl. before)} \end{cases}$$

where $D_{i,t}(\tau)$ is a dummy variable for treatment in period τ, $a^e(X_i,t), a^n(X_i,t)$ are functions describing the state dependent employment probabilities as a flexible function of observed time invariant characteristics X_i and month t, $\delta^e_{i,t,\tau}, \delta^n_{i,t,\tau}$ are the individual specific, state dependent effects of treatment on the employment probabilities, c^e_i, c^n_i are state dependent permanent individual specific effects, and $u^e_{i,t}, u^n_{i,t}$ are the idiosyncratic, period specific effects. To simplify the notation, we only consider the effects of treatment in one period τ. Furthermore, we assume that the effect of treatment occurs after treatment, i.e. $\delta^k_{i,t,\tau} = 0$ for $t < \tau$ and $k = e, n$.[9] We will discuss below Ashenfelter's Dip as linking treatment and the idiosyncratic error term before treatment. We allow the individual treatment effect

[8] In this section, the index i denotes any individual whereas in the remainder of the paper i applies only to treated individuals.

[9] This assumption is similar to the timing–of–events approach in the literature using duration models to estimate treatment effects, see Abbring/Van den Berg (2003).

$\delta^k_{i,t,\tau}$ $(k = e, n)$ to depend upon observed characteristics X_i and the individual specific effects c^k_i. They are also allowed to vary by i, t, and τ conditional upon X_i and c^k_i. For the idiosyncratic error terms, we assume that $u^e_{i,t}, u^n_{i,t}$ are mean independent of treatment in the past.

Regarding the issue of selection bias, the evaluation approach should allow that treatment $D_{i,t}(\tau)$ is affected by the observed covariates (X_i, t), by the treatment effects $\delta^e_{i,t,\tau}, \delta^n_{i,t,\tau}$ and by the individual specific effects c^e_i, c^n_i. Furthermore, we should not impose strong functional form restrictions on the specification of $a^e(X_i, t), a^n(X_i, t)$. The evaluation approach should be as nonparametric as possible relying on the smallest plausible set of assumptions.

It is often observed, that shortly before the participation in a labor market program the employment situation of the future participants deteriorates disproportionately. A similar finding termed Ashenfelter's Dip was first discovered when evaluating the treatment effects on earnings (Ashenfelter, 1978). Later research demonstrated that the same phenomenon can also occur regarding employment, see Heckman/LaLonde/Smith (1999), Heckman/Smith (1999), and Fitzenberger/Prey (2000). We argue that in our context Ashenfelter's Dip is caused by participation rules or anticipation effects. Therefore, we allow that $D_{i,t}(\tau)$ can be correlated with $u^k_{i,\tau-s}$ $(k = e, n)$ with $s = 1, \ldots, ad$ and ad denotes the begin of Ashenfelter's Dip. Even though no tough participation rules were applied in East Germany in the early 1990's, it is clear that in most cases unemployment must have lasted some time before treatment could start. A reason for anticipation effects can be that unemployed workers or workers at the risk of becoming unemployed reduce their search effort if they know that participation in an active labor market program is an option in the near future. Analogously, unemployed individuals expecting to start a new job in the future are not likely to receive treatment.

It is conceivable to interpret Ashenfelter's Dip as a treatment effect thus violating our timing–of–events assumption. We stick to this assumption since both anticipation effects and participation rules have no bearing on the economic mechanisms at work during and after treatment. Therefore, we assume that these preprogram effects are not linked to the outcome variable once treatment has started, i.e. $u^k_{i,\tau-s}$ $(k = e, n)$ are not correlated with $u^k_{i,t}$ with $s \geq 1$ and $t \geq \tau$.[10]

In our empirical analysis, we allow for a maximum length of time (ad months) for Ashenfelter's Dip. ad is set according to institutional features of the programs under consideration. After inspection of the data, we set ad conservatively and we let it vary over time (see section 3.4.3 and 3.4.4). While it is likely that shortly after

[10] This is in contrast to Heckman/Smith (1999) who model the recovery process to be expected (based on nontreatment outcomes) after the treatment being parallel to the deterioration during Ashenfelter's Dip. The state dependence in our employment process results in a recovery process which does not have to be parallel to what happens before the treatment.

German unification the anticipation of program participation occurs only shortly before the begin of the program and participation rules were applied in a very lax way, *ad* increases with the rise of unemployment during the early 1990s.

3.3.3 Conditional Difference–in–Differences

While the matching approach addresses selection bias due to observed variables, selection bias due to unobserved characteristics has to be addressed differently. We allow the selection into treatment to be affected by the permanent unobserved effects in our employment model in equation (3.5). For instance, unobserved characteristics could be due to differences in the motivation of participants or could reflect that programs are targeted to individuals with some particular problems in the labor market.[11] The difference–in–differences estimator can be used when selection effects are additively separable and time invariant. Then, it is possible to use the framework in section 3.3.1 by merely analyzing the before–after–change in the outcome variable instead of its level. We implement a conditional difference–in–differences (CDiD) estimator using preprogram differences in the outcome variable after matching to control for remaining unobservable differences. In order to avoid the "fallacy of alignment" (Heckman/LaLonde/Smith, 1999), we have to take account of possible preprogram effects via Ashenfelter's Dip. We extend the CDiD as used in the literature to fully capture the state dependence in the employment process.

Conditional Difference–in–Differences in Employment Rate

Following the approach in Heckman/Ichimura/Smith/Todd (1998),[12] we use kernel matching based on the estimated propensity score to match participants i and nonparticipants j in the same time period t and then the simple CDiD–estimator for the treatment effect on the employment rate[13] in period $t1$ is given by

$$\frac{1}{N_1}\sum_{i=1}^{N_1}\left[Y_{i,t1}^1 - Y_{i,t0}^0 - \sum_j w_{i,j}(Y_{j,t1}^0 - Y_{j,t0}^0)\right]$$

where period $t1$ lies after and $t0$ before treatment of individual i, N_1 is the number of participants i for whom the $t1 - t0$ difference can be determined, and due to

[11] We do not pursue to estimate an econometric selection model since the scarce data do not allow for credible exclusion restrictions in the participation equation, see section 3.4.1.

[12] See also Blundell et al. (2003) for an application of the CDiD, where age and regional variation is used to take account of selection effects.

[13] Although our model is defined in discrete time we use the word 'rate', as it can be aggregated to a probability in discrete time.

Ashenfelter's Dip $t0$ must lie before $\tau - ad$.[14]

CDiD is a valid estimator if the employment process in equation (3.5) does not exhibit state dependence and if the idiosyncratic error term is conditionally mean independent of treatment status D and covariates X_i, i.e. $E(u_{i,t}|D = 1, X_i) = E(u_{i,t}|D = 0, X_i) = 0$ for $t \geq \tau$ and $t < \tau - ad$, $a^e(X_i, t) = a^n(X_i, t)$, $c_i = c_i^e = c_i^n$, and $u_{i,t} = u_{i,t}^e = u_{i,t}^n$. However, the common individual specific effect c_i does not have to be conditionally mean independent D and X_i.

Conditional Difference–in–Differences in Hazard Rates (CDiDHR)

Based on the employment model in equation (3.5), we develop the following Conditional Difference–in–Differences in Hazard Rates (CDiDHR) estimator as an extension of the CDiD estimator to a state dependent employment process. We simply estimate the treatment effect on the probability to be employed via CDiD conditional on employment status in the previous month by

$$\frac{1}{N^l} \sum_{i \in \mathcal{N}^l} g_i \left[Y_{i,t1}^1 - Y_{i,t0}^0 - \sum_j w_{i,j} (Y_{j,t1}^0 - Y_{j,t0}^0) \right] \tag{3.6}$$

where l denotes the employment status in the previous month ($l = 1$ if previously employed and $l = 0$ if previously nonemployed), $\mathcal{N}^l$ is the set of treated individuals for whom $Y_{i,t1-1} = Y_{i,t0-1} = l$, where $t1$ after and $t0$ before treatment of individual i. N^l is the number of individuals in $\mathcal{N}^l$. Also, only nonparticipants j for whom $Y_{j,t1-1} = Y_{j,t0-1} = l$. g_i is a set of weights to account for the fact that $\mathcal{N}^l$ does not include the entire treatment sample. For $l = 0$ and $l = 1$, expression (3.6) estimates the reemployment probabilities when unemployed and the probability to remain employed, respectively.

To properly account for selection bias in the nonparticipation outcome, CDiDHR only requires the idiosyncratic error terms to be conditionally mean independent of treatment status D and covariates X_i, i.e. $E(u_{i,t}^e|D = 1, X_i) = E(u_{i,t}^e|D = 0, X_i) = E(u_{i,t}^n|D = 1, X_i) = E(u_{i,t}^n|D = 0, X_i) = 0$ for $t \geq \tau$ and $t < \tau - ad$. Analogous to CDiD, the individual specific effects c_i^l do not have to be conditionally mean independent of treatment status D and covariates X_i. Also for CDiDHR, $t0$ must lie before $-ad$, i.e. before anticipation and participation rules can take effect, because of the possibility of Ashenfelter's Dip.

A disadvantage at first glance lies in the fact that using weights $g_i = 1$, CDiDHR does not identify the unconditional TT $E(\delta_{i,t1,\tau}^k|D = 1)$ but instead the TT

[14] We do not take symmetric differences ($\tau_0 - t0 = t1 - \tau_1$ with τ_0 begin of program and τ_1 end of program) as in Heckman/Ichimura/Smith/Todd (1998) or Heckman/Smith (1999). We think their approach assumes an implausible symmetry between those effects driving Ashenfelter's Dip and the recovery process after participation, see also discussion in footnote 10 above.

$E(\delta^k_{i,t1,\tau}|D=1, Y_{t1-1}=l, Y_{t0-1}=l)$ conditional on the employment status l both in the previous month ($l=0$ if $k=n$ and $l=1$ if $k=e$) and in the month before the baseline period $t0$. The latter TT is not the same as the unconditional TT with the potential treatment effects $\delta^k_{i,t,\tau}$ being defined irrespective of the employment status of individual i in the previous month. To estimate the unconditional TT, it would be necessary both to account for the differences in the distribution of the X_i characteristics and of the individual specific effects c^k_i with $k=e,n$, since the individual specific treatment effects in the employment model (3.5) as well as the observed employment status in the previous month presumably depend upon both X_i and the c^k_i's. Differences in X_i and the c^k_i's result in a sorting of high employment individuals into the group of employed individuals in the previous month and vice versa.

In section 3.4, we define the weights g_i to integrate out the distribution of X_i in the treatment sample by using a regression model where the mean effect is evaluated at the average of the X_i in the treatment sample. Effectively, we then identify the TT

$$E_{X_i,D=1}\{E(\delta^k_{i,t1,\tau}|D=1, Y_{t1-1}=l, Y_{t0-1}=l, X_i)|D=1, Y_{t1-1}=l, Y_{t0-1}=l\}$$

conditional on the employment status l in the previous month and in the month before the baseline period $t0$ where the outer expectation $E_{X_i,D=1}$ integrates out with respect to the distribution of X_i in the sample $D=1$. Thus, conditioning on $(Y_{t1-1}=l, Y_{t0-1}=l)$ only affects the distribution of the individual specific effects and the latter is partly controlled for through the correlation between X_i and the c^k_i's. Regarding the information in the c^k_i's not controlled for, our treatment effect weights the individual treatment effects by the frequencies that individuals are employed and not employed in the previous period before and after treatment, respectively.

Our approach estimates the unconditional TT under the following two stringent conditions: First, the treatment effects are conditionally mean independent of the individual specific effects when also conditioning on X_i, i.e. $E(\delta^k_{i,t1,\tau}|c^e_i, c^n_i, X_i) = E(\delta^k_{i,t1,\tau}|X_i)$. Second, we do observe each treated individual in both employment states before anticipation and participation rules take effect so that the before–after–difference can be calculated for some $t0$ in the past. The second assumption is quite innocuous in our application since we consider the preprogram situation up to 18 months in the past. The preprogram level is then the average transition rate conditional on the employment state in the previous month. For almost all treated individuals, these averages are available for both states. The first condition does not hold when the selection into treatment depends upon the treatment effects $\delta^k_{i,t1,\tau}$ conditional upon X_i via the individual specific effects. We do not think that the latter condition is likely to hold.

There is no ready procedure to estimate the unconditional TT by also integrating out the individual specific effects without imposing further stringent assumptions.

Thus, we only integrate out the X_i distribution in the treatment sample. It is quite plausible that, conditional on X_i, both treatment effects $\delta^k_{i,t1,\tau}$ are positively correlated with the individual specific effects and that the two individual specific effects are positively correlated. Then, our approach will overestimate the TT for the probabilities to remain employed and it will underestimate the TT for the reemployment probabilities. Given this, we will nevertheless be able to draw conclusions on the effectiveness of training programs based on the estimation results.

In addition, we conduct a sensitivity analysis on the basis of a different employment model (for more details see appendix 3.B). In this model the state dependence of employment is captured by a first order autoregressive component in employment (ρY). If this model holds, treatment effects can be estimated unconditional of the previous employment states with the aid of a conditional Double–Difference–in–ρ–Differences (CD2iρD) estimator. However, this model is very restrictive, notably it is assumed that the individual specific effects are constant with respect to the two previous employment states.

3.3.4 Multiple Treatments and Carousel Effects

To take into account multiple sequential treatments such that an individual participates in labor market programs more than once, we extend our evaluation approach to the analysis of a first and second treatment. We specify the TT of participation in a second program compared to the situation of not having participated in this specific treatment sequence. The treatment dummy D is defined such that $D = 1$ indicates treatment in this specific treatment sequence and $D = 0$ indicates all three other alternatives, i.e. (i) no program participation, (ii) a first training program and no further treatment or another second program not considered here, or (iii) a first treatment other than training.

The estimation of the combined effect of the sequence of the first and second treatment is a straight forward application of the single binary treatment case. Individuals with at most one training program participation $D = 0$ are matched to individuals who participate in a second program $D = 1$. For CDiD(HR), we use the differences between the period after the second treatment and the period before the first treatment.

To evaluate the incremental effect of the second program we suggest the following heuristic two step procedure. Based on the timing of events, the incremental treatment effect is estimated by CDiD(HR) using the outcome before and after the second treatment in the matched sample. Treating previous program participation as nonemployment, the average incremental effect of the second program is obtained. The matching procedure uses all nonparticipants of the second program, i.e. the estimated effect relates to the composition of this group. To properly account for selection into the second treatment, we assume that the impact of the individual specific effects enters the individual treatment effects $\delta^k_{i,t,\tau}$ for the first program as an additive constant. Unfortunately, our approach does not allow for the selection into the second program to depend directly upon the individual treatment effect of the first program.

Evaluating the combined and incremental effects of multiple program participation, it is possible to investigate whether multiple treatments occur for individuals with particularly bad labor market prospects, whether a further treatment improves the outcome, or whether it just occurs because the participants are unlikely to find a job after the first treatment and this is still the case after further program participation ("carousel effect"). In our approach, a pairwise ("data hungry") evaluation (see Lechner (1999) and footnote 4 above for the evaluation of heterogeneous treatments) can be avoided.

3.4 Empirical Analysis

3.4.1 Data

Our analysis uses the Labor Market Monitor Sachsen–Anhalt[15] (Arbeitsmarktmonitor Sachsen–Anhalt, LMM–SA) for the years 1997, 1998, and 1999. The LMM–SA is a panel survey of the working–age population of the state (*Bundesland*) of Sachsen–Anhalt with 7,100 participants in 1997, 5,800 in 1998, and 4,760 in 1999. 1999 is the last year in which the survey was conducted. Only in the three years used, retrospective questionnaires on the monthly employment status between 1990 and up to December 1999 were included. The monthly data provide all possible labor market states, i.e. employment, unemployment, or participated in a program of ALMP, as well as periods in the education system, inactivity, or in military. Individuals who did not participate in the 1998 survey are recorded until at least September 1997, those who dropped out in 1999 at least until October 1998.

Selection of Sample

Unfortunately, in the three survey years used the categories of the labor market states differ. For compatibility, the data set also includes a combined monthly calendar for the three survey years (compiled by the ZSH institute). This calendar distinguishes the following categories: Education, full–time employed, part–time employed, unemployed, job–creation scheme, training, retirement, pregnancy/maternity leave, not in active workforce.

[15] Although the data refer to the state of Sachsen–Anhalt only, the results are likely to be representative for East Germany as a whole (see Schulz, 1998). For further information on the data set, see Ketzmerik (2001).

We only consider individuals with complete information on their labor market history between January 1990 and at least until September 1997 (i.e. individuals who completed the retrospective question in 1997). The individuals are between 25 and 50 years old in January 1990 and employed before the start of the “Economic and Social Union” in June 1990. This way, only individuals are included who had belonged to the active labor force of the former GDR, who therefore were fully hit by the transformation shock, and who are not too close to retirement.[16] Individuals, who are later on in education or on maternity leave are excluded completely from the analysis. The goal is to construct a consistent data base excluding individuals who have left the labor market completely. In addition, individuals without valid information on those individual characteristics, on which we build the matching, are excluded. We aggregate the remaining labor market states to the four categories *employment*, which comprises part– and full–time employment, *nonemployment*, which comprises unemployment and out of labor force, *training* and *job creation*.

Our outcome variable employment is defined with nonemployment as alternative resulting in a binary outcome variable. Modeling transitions between unemployment and being out of labor force is here an impossible task. People move occasionally back and forth between the two states in the data and it is not obvious whether the individuals precisely distinguish between unemployment and being out of labor force, since no formal definition of unemployment is given in the questionnaire.

Table 3.1: Program Participation (number of individuals) in the LMM–SA during 1990 and 1999

One Program	JC[a]	TR[b]
At least once	689	1021
As first program	484	889

Program Sequences[c]	JC–JC	JC–TR	JC alone
First and Second	105	113	266
Program Sequences	TR–JC	TR–TR	training alone
First and Second	176	150	563

a: Training b: Job Creation Scheme
c: For instance, TR–JC indicates that a first participation in training and a second treatment in JC occurred

The resulting sample consists of 5,165 individuals and it is likely to be quite representative for the labor force in the former GDR. Table 3.1 summarizes participation in ALMP based on our data. The two programs considered, Training (TR) and

[16] Massive early retirement programs were implemented in the early 1990s in order to reduce the labor force.

Job Creation Schemes (JC), were implemented at a large scale. In total, 27% of our sample participated at least once in one of the two programs. While 13% (689 cases) participated at least once in JC, TR was the most important program with a rate of 20% (1,021 cases).[17] Our data do not distinguish between further training and retraining. Therefore, the estimated treatment effects are an average of the two programs.

After a first training program, a second treatment in training or JC occurred in 326 cases, i.e. more than 36% of the 889 cases in a first treatment in TR participated in another program at least a second time. Because of the importance of the timing of events, we differentiate between the effects of a first and a second treatment. This paper focuses on the effects of TR, thus restricting ourselves to TR being the first treatment. Using the evaluation approach described in section 3.3, we estimate first the effects of FTR (participation in training as the first program) (889 cases) and then the effect of the second treatment for the two sequences TR–TR (150 cases) and TR–JC (176 cases). No further treatment afterwards is analyzed, since the number of cases is very small.

Recall Errors

Retrospective data, which in our case covers at least 8 years, entails the danger of recall errors. In the following, we will argue that recall errors are less problematic in our analysis than it is typically the case with retrospective data.

First of all, note that the individuals were asked about their employment history starting with the year 1990. This year constitutes a turning point in the biography of East Germans, as the political and economic system changed dramatically. The connection of biographic events with historic events, as done here, typically improves the validity of recall data (Loftus/Marburger, 1983, Robinson, 1986). Additionally, starting with the salient year 1990 the individuals had to answer in *chronological* order, which is now commonly viewed as the best technique in collecting life history data in a single survey (Sudman/Bradburn, 1987). Second, our broad definition of employment states circumvents some of the recall errors which are present when analyzing more than two labor market states. It helps especially to merge the states unemployment and out of the labor force. For instance, after some time in unemployment, women tend to label this as having been out of the labor force (Dex/McCulloch, 1998). Third, our evaluation design (CDiDHR estimator) allows for recall errors occurring in the same fashion among treatment and matched control group. In particular, if both groups forget to mention transitions in a similar way then the errors simply cancel out.

Thus, recall errors in our analysis might only increase the standard errors of our

[17] The question in the LMM–SA on training does also include privately financed training. However, calculations based on the German Socioeconomic Panel for East Germany show that a very high share of training is in fact public sector sponsored training (in 1993 more than 88%).

estimates. However, if we were estimating individual labor market flows, recall errors would be more worrying (Paull, 2002) and it might be useful to change the methodological approach (e.g. following Magnac/Visser, 1999).

3.4.2 Implementation of Evaluation Approach

We estimate the following five treatment effects: (1) FTR: participation in training as the first program, (2) TR–TR: incremental effect, (3) TR–TR: combined effect, (4) TR–JC: incremental effect, and (5) TR–JC: combined effect. For FTR, TR–TR, and TR–JC,[18] the treatment probability (propensity score) is estimated by separate parametric probit models. Since the data do not provide time–varying information (except for the labor market status), the regressors are the static observable characteristics education, occupational degree, gender, age, residence (at the time of the survey) and interactions of gender and education or occupational degree (results can be found in appendix 3.A table 3.4). The probit model does not model when the participation in the program actually takes place. We do not think that the data is sufficiently rich to model the timing. Using a bootstrap estimator for the covariance matrix of the estimated treatment effects, we capture the estimation error in the propensity score.

For matching based on the propensity score, the group of "nonparticipants" $\{D = 0\}$ represents the entire sample of individuals who are not participating in the treatment sequence under consideration but who might be a participant in another program. We do not match on the employment history shortly before the program (see Lechner, 1998, for such an approach) due to the possibility of Ashenfelter's Dip. Our CDiD(HR) estimators control for remaining long–run preprogram differences after matching upon the propensity score.

Figures 3.3 – 3.6 show the high degree of overlap in the distributions of the estimated propensity score[19] between participants (Treated) and nonparticipants (Nontreated) for the treatments FTR and TR–TR (the graphs are similar in nature for TR–JC). The graphs are stratified conditional upon nonemployment and employment in the previous month, respectively. Since the employment status changes over time and since after 1997 no complete data is available for all individuals, the overlap can change over time. Here, the graphs show the overlap of the distributions for the two months 5/1993 and 5/1997, being representative for other periods. Only in rare cases, such as FTR in 5/1993 and being previously nonemployed, we find a slightly less than perfect overlap. Based on this evidence, there is no serious problem of lack of common support for matching and we match the entire treatment sample.

[18] Recall from the previous subsection that the evaluation of combined and incremental treatments only differ regarding the choice of the preprogram period for the CDiD(HR) estimators.

[19] The graphs depict the fitted values of the latent index for the probit model. The estimated treatment probability is the cdf of the standard normal applied to this index.

Figure 3.3: Overlap of Distributions of Propensity Score Index for FTR – Nonemployment in Previous Month

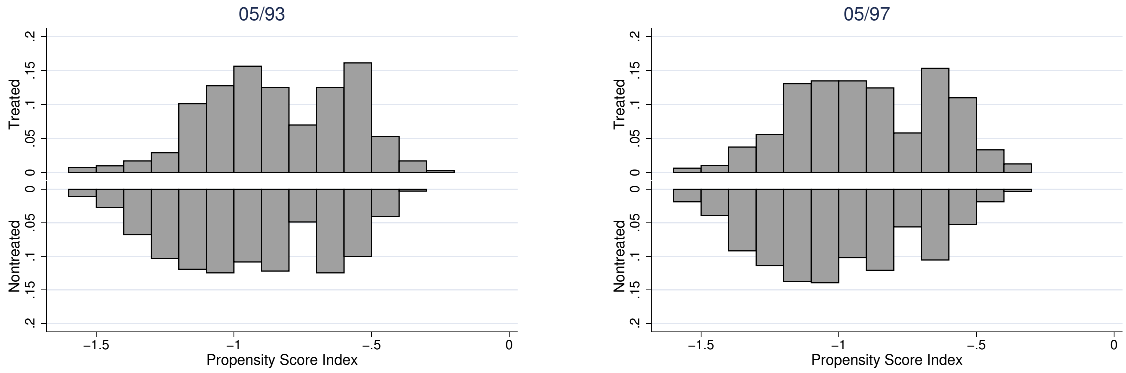
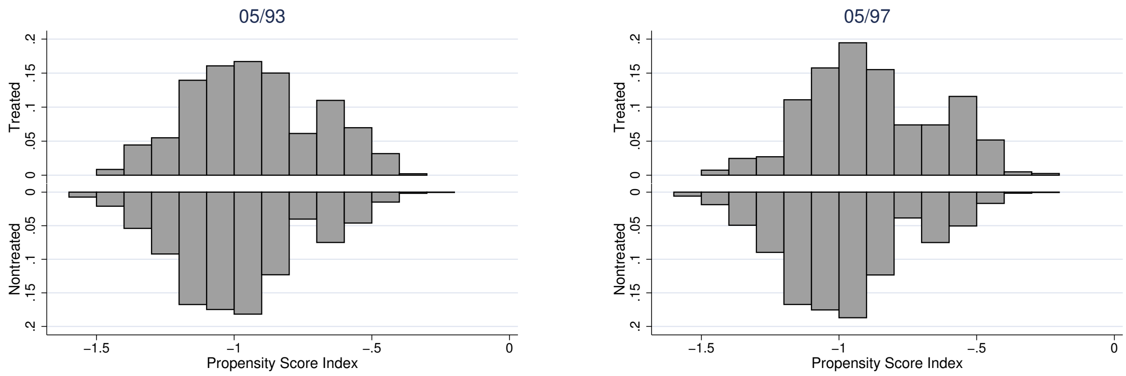


Figure 3.4: Overlap of Distributions of Propensity Score Index for FTR – Employment in Previous Month



So far, we have not been explicit about the post program evaluation period. We use two different starting points in time, which are widely used in the literature: The evaluation of treatment effects starts either after the end of the program or with the beginning of the program. The former approach excludes the treatment period from the employment history when evaluating the success of the respective treatment because treatment is viewed as time spent outside of the labor market. This exclusion is somewhat unsatisfactory since labor market history continues, especially so for the nonparticipants. In contrast, the second approach views the treatment just as a different nonemployment state while searching for a job.

The start of the evaluation period depends also upon the outcome variables considered. For employment rates and reemployment probabilities, the evaluation period starts one month after the last or the first month of the treatment depending on whether evaluation starts after or at the beginning of the treatment. For probabilities to remain employed, the evaluation period starts one month later than for

Figure 3.5: Overlap of Distributions of Propensity Score Index for TR–TR – Nonemployment in Previous Month

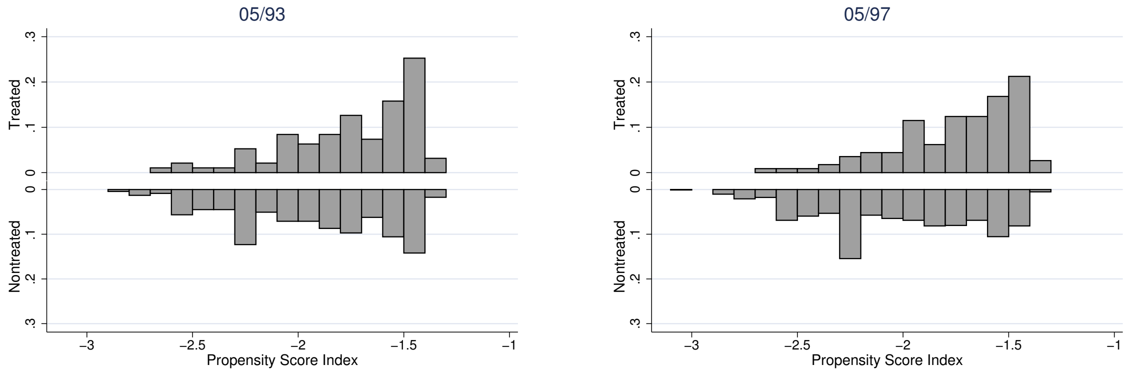
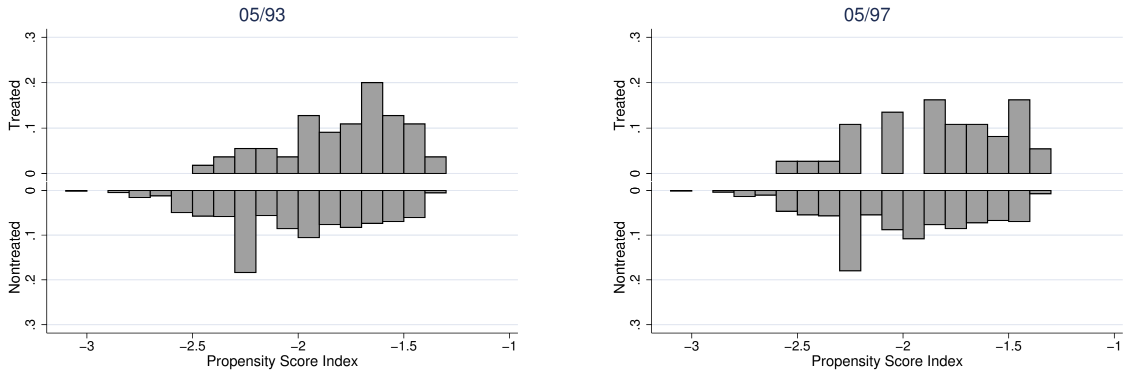


Figure 3.6: Overlap of Distributions of the Propensity Score Index for TR–TR – Employment in Previous Month



the other two outcome variables, since we first have to observe employed former participants. We choose the length of the evaluation period to be 36 months (as far as being observed in the data set – otherwise set to missing). As preprogram period, we take 18 months before the beginning of the treatment. For the incremental effect of TR–TR and TR–JC, the preprogram period is taken before the beginning of the second treatment and for the combined effect before the beginning of the first treatment.

Based on the estimated propensity scores, we construct matched samples of participants and comparable "nonparticipants" (matched nonparticipants) both during the preprogram and the evaluation period. Alignment occurs in the same calendar month. The characteristics and outcomes of matched nonparticipants are the fitted values obtained by the local linear kernel regression of characteristics and outcomes, respectively, on the estimated propensity score in the sample of nonparticipants as a whole. Table 3.2 and 3.3 give evidence on the balancing properties in the matched samples for the two cases FTR and TR–TR (combined effect). The first column shows the average characteristics in the whole sample. The remaining columns show the average characteristics conditional upon employment state in the previous month. For example, when calculating the average characteristics for the previously nonemployed, the individual contribution to the mean characteristics is weighted by the number of months the individual's state was nonemployment during the time period under consideration. For the matched nonparticipants, the average reported uses all available observations.

Table 3.2: Balancing Properties of Matching for Participation in FTR, Evaluation Starts at the Beginning of the Program

	Means of Variable in Subgroups						
Variable	All	Nonpar–ticipants	Parti–cipants	Matched Nonpart.	Nonpar–ticipants	Parti–cipants	Matched Nonpart.
		averaged over prev–iously nonemployed			averaged over prev–iously employed		
Age 25–34	0.37	0.27	0.40	0.36	0.37	0.45	0.43
Age 35–44	0.40	0.36	0.41	0.39	0.40	0.41	0.41
Age 45–50	0.23	0.37	0.19	0.26	0.23	0.14	0.16
Dessau	0.12	0.12	0.14	0.15	0.11	0.14	0.14
Halberstadt	0.09	0.07	0.10	0.06	0.10	0.08	0.09
Halle	0.19	0.17	0.17	0.15	0.19	0.16	0.18
Magdeburg	0.24	0.23	0.23	0.21	0.24	0.25	0.24
Merseburg	0.13	0.16	0.13	0.17	0.13	0.14	0.13
Sangerhausen	0.10	0.11	0.12	0.15	0.09	0.12	0.11
Stendal	0.08	0.09	0.07	0.08	0.08	0.07	0.07
Wittenberg	0.05	0.06	0.05	0.05	0.05	0.05	0.05
Un-, semi- or other skilled	0.02	0.09	0.02	0.05	0.02	0.01	0.01
Skilled worker	0.43	0.50	0.50	0.57	0.41	0.46	0.46
Craftsman	0.08	0.08	0.06	0.06	0.08	0.06	0.06
Technical college	0.19	0.16	0.19	0.16	0.20	0.20	0.20
University education	0.27	0.18	0.24	0.16	0.29	0.27	0.27
Female	0.48	0.54	0.64	0.65	0.45	0.55	0.58
Female unskilled worker	0.01	0.05	0.01	0.03	0.01	0.00	0.01
Female skilled worker	0.21	0.30	0.34	0.42	0.17	0.27	0.29
Craftswoman	0.01	0.01	0.03	0.03	0.01	0.02	0.02
Female, tech. college	0.13	0.10	0.14	0.10	0.14	0.14	0.14
Female, uni. education	0.11	0.08	0.12	0.08	0.12	0.12	0.13

Table 3.2 and 3.3 show that participants are younger than the nonparticipants and that women participate at a higher rate in training than men. There is no clear cut difference in the skill distribution. It is evident, that the matching process balances well the characteristics of the participants and the matched nonparticipants conditional upon employment status in the previous month. For example, 27% of

Table 3.3: Balancing Properties of Matching for Participation in TR–TR (Combined Effect), Evaluation Starts at the Beginning of the Program

Variable	All	Nonpar–ticipants	Parti–cipants	Matched Nonpart.	Nonpar–ticipants	Parti–cipants	Matched Nonpart.
		averaged over previously nonemployed			averaged over previously employed		
Age 25–34	0.37	0.32	0.43	0.41	0.38	0.45	0.44
Age 35–44	0.40	0.38	0.43	0.41	0.40	0.43	0.42
Age 45–50	0.23	0.30	0.14	0.18	0.23	0.12	0.15
Dessau	0.12	0.13	0.17	0.16	0.11	0.14	0.15
Halberstadt	0.09	0.08	0.08	0.06	0.10	0.06	0.07
Halle	0.19	0.16	0.19	0.16	0.19	0.17	0.20
Magdeburg	0.24	0.23	0.25	0.22	0.24	0.28	0.25
Merseburg	0.13	0.15	0.11	0.17	0.13	0.15	0.13
Sangerhausen	0.10	0.11	0.10	0.13	0.09	0.13	0.10
Stendal	0.08	0.08	0.05	0.05	0.08	0.03	0.05
Wittenberg	0.05	0.06	0.05	0.05	0.05	0.04	0.05
Un-, semi- or other skilled	0.02	0.06	0.00	0.03	0.02	0.00	0.01
Skilled worker	0.43	0.50	0.53	0.62	0.41	0.52	0.53
Craftsman	0.08	0.07	0.02	0.04	0.08	0.04	0.04
Technical college	0.19	0.17	0.21	0.16	0.20	0.22	0.21
University education	0.27	0.20	0.24	0.16	0.29	0.22	0.22
Female	0.48	0.57	0.79	0.79	0.46	0.68	0.67
Female unskilled worker	0.01	0.04	0.00	0.02	0.01	0.00	0.01
Female skilled worker	0.21	0.31	0.46	0.56	0.18	0.36	0.40
Craftswomen	0.01	0.02	0.01	0.02	0.01	0.02	0.01
Female, tech. college	0.13	0.11	0.19	0.13	0.14	0.17	0.17
Female, uni. education	0.11	0.09	0.12	0.10	0.12	0.13	0.11

Means of Variable in Subgroups

the previously nonemployed nonparticipants were aged between 25 and 34 in 1990, whereas 40% of the participants belonged to this age group. In the matched sample, 36% of the matched nonparticipants belong to this age group. The balancing works especially well for the previously employed in all cases and for the previously nonemployed in most cases. However, the labor market region does not seem perfectly balanced for the latter group.

Furthermore, table 3.2 and 3.3 shed some light on the differences in characteristics across employment states in the previous month. Previously employed participants are younger than previously nonemployed. Male participants were more often previously employed compared to females. In the case of FTR, previously employed participants more often have a university education.

3.4.3 Specification of Outcome Equation

In the matched samples, the CDiD(HR) estimators are based on a flexible semi-parametric linear probability model for the employment dummy as outcome variable. The state of nonemployment includes the participation in ALMP programs such that previous and subsequent participation in a program are both accounted for as nonemployment. We estimate an average employment effect of a program relative to all possible nonemployment states for the treated individuals thus estimating TT (with CDiDHR conditioned on the employment status in the previous month). For CDiDHR, we also control for observed, time–invariant characteristics X_i in the outcome equation. The X_i variables enter the equation as deviations from their averages in the treatment sample.

We assume treatment of individual i begins in period τ and we consider the employment outcome Y before the begin of treatment $t0 = -18, \ldots, -ad - 1$, as well as during the time of Ashenfelter's Dip and the evaluation period of 36 months $t1 = -ad, \ldots, -1, 1, \ldots, 36$. Now, $t0$ is defined relative to the start of the treatment, whereas the definition of $t1$ depends on the evaluation perspective and the success criterion. When then evaluation starts at the begin of the program then $t1$ is measured relative to τ in case the unconditional employment probability or the reemployment probability are the outcome variables, whereas $t1$ is measured relative to $\tau + 1$ in case of the probability to remain employed. However, when the evaluation starts after the end of the program τ is replaced by the last month of the program.

We estimate the following three steps both for **CDiD** (sample of all participants) and **CDiDHR** (separately depending on the employment status in the previous month):

1. We calculate the average long–run preprogram difference between participant i (treatment starts in τ) and comparable nonparticipants as
$$\hat{a}_{i,\tau} = \frac{1}{18 - ad(\tau)} \sum_{t0=-18}^{-ad(\tau)-1} (Y^0_{i,t0} - \sum_j w_{i,j} Y^0_{j,t0}) \,.$$
2. Then, $\hat{a}_{i,\tau}$ is subtracted from the difference during Ashenfelter's Dip and during the evaluation period resulting in the following model to estimate the treatment effects ($I(.)$ denotes the indicator function, $\nu_{i,t1}$ the error term)
$$Y^1_{i,t1} - \sum_j w_{i,j} Y^0_{j,t1} - \hat{a}_{i,\tau} \;=\; \sum_{s=-ad(\tau)}^{36} \delta_s I(t1 = s) \tag{3.7}$$
$$+ (\gamma_1^{ad}\tau + \gamma_2^{ad}\tau^2) I(-ad(\tau) \leq t1 < 0) \;+\; (\gamma_1^{po}\tau + \gamma_2^{po}\tau^2) I(t1 > 0) + \nu_{i,t1} \,.$$
For CDiDHR, we include deviations of the X_i characteristics from their average in the treatment sample as additional regressors in equation (3.7).

3. The average long–run preprogram differences $\hat{a}_{i,\tau}$ are regressed on a second order polynomial in the starting month of the treatment. We will report the predictions from this regression

$$\hat{\alpha}(\tau) = \alpha_0 + \alpha_1\tau + \alpha_2\tau^2 \tag{3.8}$$

to illustrate how the average long–run preprogram differences ($\equiv$ residual selection effect due to permanent individual specific effects) between participants and nonparticipants after matching depend upon the timing of the program.

The following definitions complement the description:

$\alpha_0, \alpha_1, \alpha_2$	coefficients measuring the long–run preprogram differences depending upon the month when the program starts τ,
$ad(\tau)$	month before the begin of the program when Ashenfelter's Dip starts depending upon τ,
$\delta_s, \gamma_1^{ad}, \gamma_2^{ad}, \gamma_1^{po}, \gamma_2^{po}$	coefficients modeling the DiD effect relative to the long–run preprogram differences $\hat{a}_{i,\tau}$, and
$w_{i,j}$	weights implementing local linear kernel regression on the estimated propensity score.

Estimating equation (3.7) as a linear regression, the CDiD(HR) estimators are implemented in a semiparametric way by including the employment situation before treatment in the dummy regression of outcomes and by allowing the effect of the program to depend both upon the time since treatment ($t1 > 0$) and upon the begin of the program τ. The long–run preprogram employment differences $\hat{a}_{i,\tau}$ prove critical for the alignment of the CDiD(HR) estimators. Dummy variables for the effect of Ashenfelter's Dip are included to capture the decline in the employment probability shortly before the program. The specification allows the employment differences before and after the program to depend in a flexible way upon τ. Also the length of Ashenfelter's Dip is allowed to depend upon the time when the program starts. During the period shortly after unification, it is likely that the dip is fairly short since program participation could not have been anticipated long before and participation rules were applied in a very lax way. The situation changes with the occurrence of high unemployment when people realized that labor market problems were quite severe and that ALMP at a large scale was likely to be a permanent feature of the labor market.

To capture Ashenfelter's Dip, the following heuristic approach is chosen. For the first program, a visual inspection of the average employment differences between treated and matched controls before and after the program as a function of the time when the program starts indicates that the dip occurs during one to two months in 90/91 and increases over time to something of at most six months for TR and to at most nine months for JC. In order to obtain a lower bound for the employment effect of a program (the employment of the future participants decreases during the dip), we are conservative in defining $ad(\tau)$. Before November 90, we set $ad(\tau) = -1$. Between November 1990 and July 1994, $ad(\tau)$ increases linearly in absolute value from 2 months to 6 months for TR and 9 months for JC, respectively, where $ad(\tau)$ is rounded to the nearest integer. After July 1994, $ad(\tau)$ remains constant. Our approach is conservative in the sense that taking a shorter period for Ashenfelter's Dip would effectively result a higher difference–in–differences estimate of the treatment effect.

For a program, which started in τ, the following expression captures both the estimate for the disproportionately decline in employment during Ashenfelter's Dip and the estimated TT after the program

$$DiD(t1, \tau) = \begin{cases} \delta_{t1} + \gamma_1^{ad}\tau + \gamma_2^{ad}\tau^2 & \text{for} \quad -ad(\tau) \leq t1 \leq -1 \\ \delta_{t1} + \gamma_1^{po}\tau + \gamma_2^{po}\tau^2 & \qquad t1 = 1, ..., 36 \end{cases} . \tag{3.9}$$

If we assume for CDiDHR that the linear specification of the outcome equation in the X_i characteristics holds exactly, then $DiD(t1, \tau)$ estimates the TT conditional on previous employment status while integrating out the distribution of the X_i in the treatment sample, see also section 3.3.3.

For the multiple, sequential treatments, $DiD(t1, \tau)$ estimates the incremental employment effect of the second treatment when the begin of the second program is taken as the begin of the treatment. The combined effect of the program sequence is obtained taking the begin of the first program. For the incremental effect, the effect of a first treatment is possibly included in the permanent preprogram effect for the participants. Since all TT's are estimated for the specific selection of individuals participating in a certain treatment, it is clear that the TT for FTR and the incremental TT do not have to add up to the combined effect of the treatment sequence.

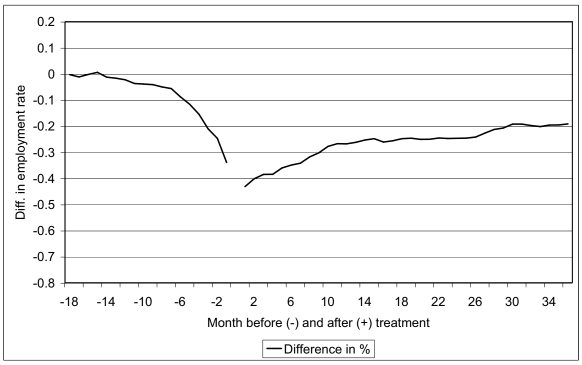
3.4.4 Estimated Treatment Effects

Before turning to the CDiD(HR) estimates for the treatments considered, we discuss the outcomes in the matched sample for the treatment FTR. Figure 3.7 reports the average differences in employment rates for the matched sample with individuals starting treatment in the two–year periods 1990/91, 1991/92, etc. If the CIA $E(Y^0|D = 1, X) = E(Y^0|D = 0, X)$ did actually hold with respect to the time invariant characteristics X_i, then the average differences in employment rates for the matched samples would be a proper estimate of TT. Right after treatment, the employment rates of the participants are between 40 and 60 percentage points (ppoints) lower that for comparable nonparticipants. There is a noticeable recovery for the participants afterwards – basically the time path reflects the changes for participants since employment rates for nonparticipants are almost constant – but the difference comes nowhere close to zero except at the end for 1997/98 (the latter can be dismissed since it is based on a very small number of cases). Even three years

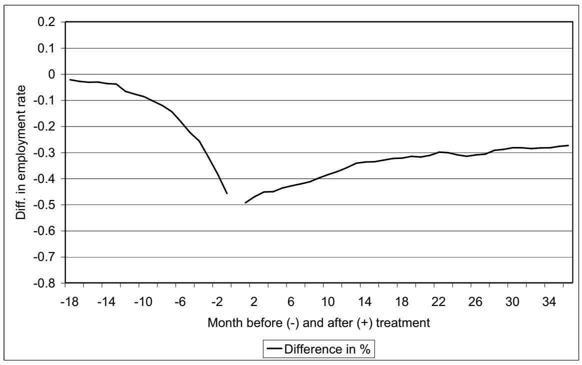
Figure 3.7: Differences in Outcome Variable (Matched Sample): First TR Beginning in Two–Year–Interval 90/91,. . . ,97/98

First Program is TR – Differences in Outcome Variable after Matching

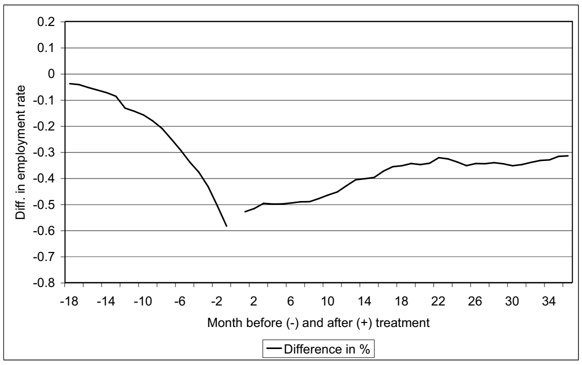
Estimated Difference 1990/91



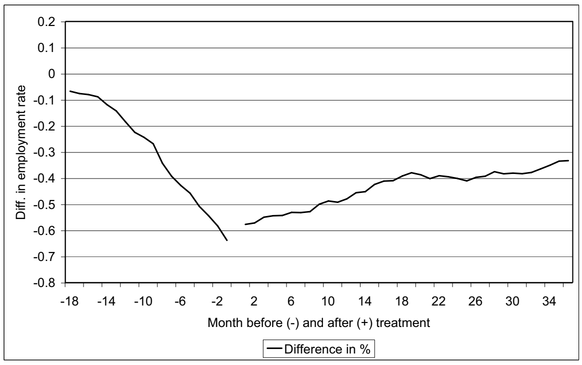
Estimated Difference 1991/92



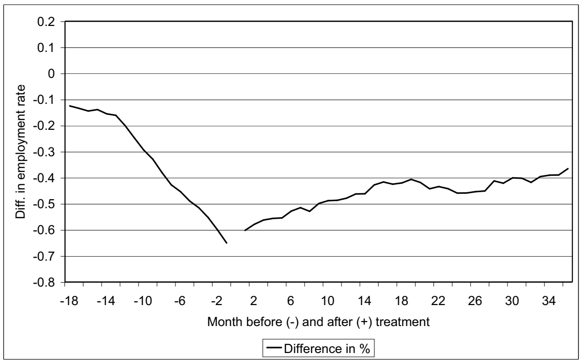
Estimated Difference 1992/93



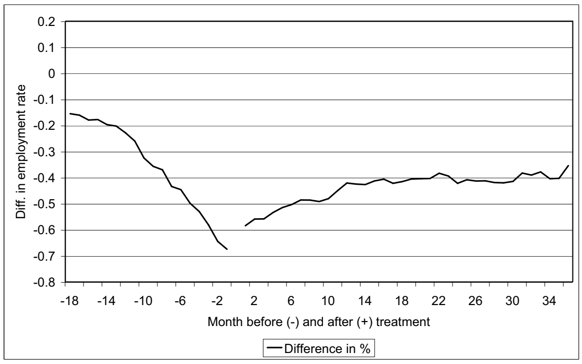
Estimated Difference 1993/94



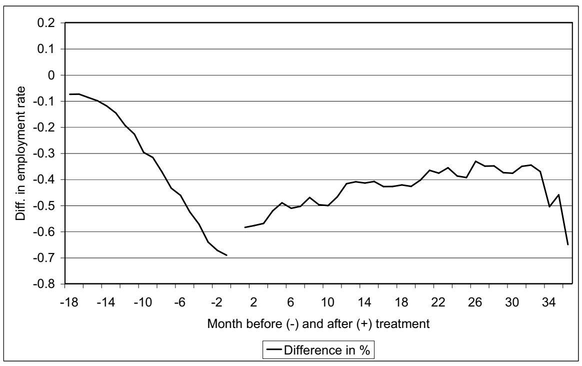
Estimated Difference 1994/95



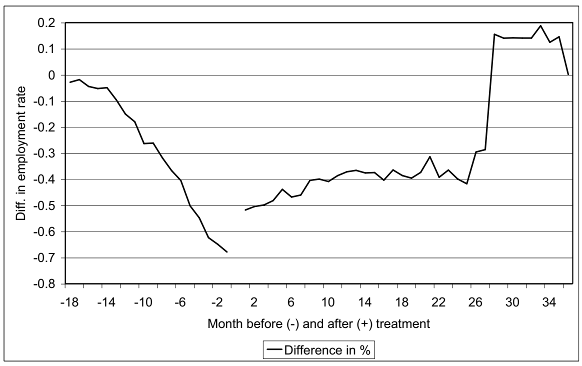
Estimated Difference 1995/96



Estimated Difference 1996/97



Estimated Difference 1997/98



after treatment, employment rates are still between 20 (90/91) and 40 (mid to late 90s) ppoints lower than for comparable nonparticipants. Thus, under the CIA as stated above one had to conclude that FTR results in a considerable reduction of employment rates, which is a common result found in the literature when matching is based on observable characteristics (see the survey in Hagen/Steiner, 2000).

Considering the preprogram effects in figure 3.7 raises a number of issues, which are addressed by our CDiD(HR) estimators, and could lead to a different conclusion. While in 1990/91 there is no preprogram difference 13 to 18 months before the treatment, long–run preprogram differences in the order of 10 to 20 ppoints exist for later years. We take this as an indication for the importance of remaining unobservable differences in the matched sample. Thus, our CDiD(HR) estimators take account of possible individual specific effects. It is also apparent here that a simple CDiD estimate based on the difference between long–run postprogram and long–run preprogram outcomes will result in a negative estimate for TT (as we will see in the following). There is also a strong decline in employment rates shortly before the program starts and the decline starts earlier in the later years. In 1990/91, the decline starts within the last six months before the treatment and the average differences immediately before the start of the program amount to 33 ppoints, whereas in 1997/98 the employment rate of the treated declines already 16 months before the treatment. We take this as an indication for Ashenfelter's Dip which a credible difference–in–differences estimator has to take account of. Basing CDiD on the difference between postprogram outcomes and preprogram outcomes shortly before the begin of the program would erroneously result in a positive estimate for TT. Finally, analyzing employment rates entails the danger that one misses the state dependence in employment. The continuous decline before the program and the recovery process after the program suggest that employment rates do not adjust instantaneously. Thus, one should check for state dependence as well.

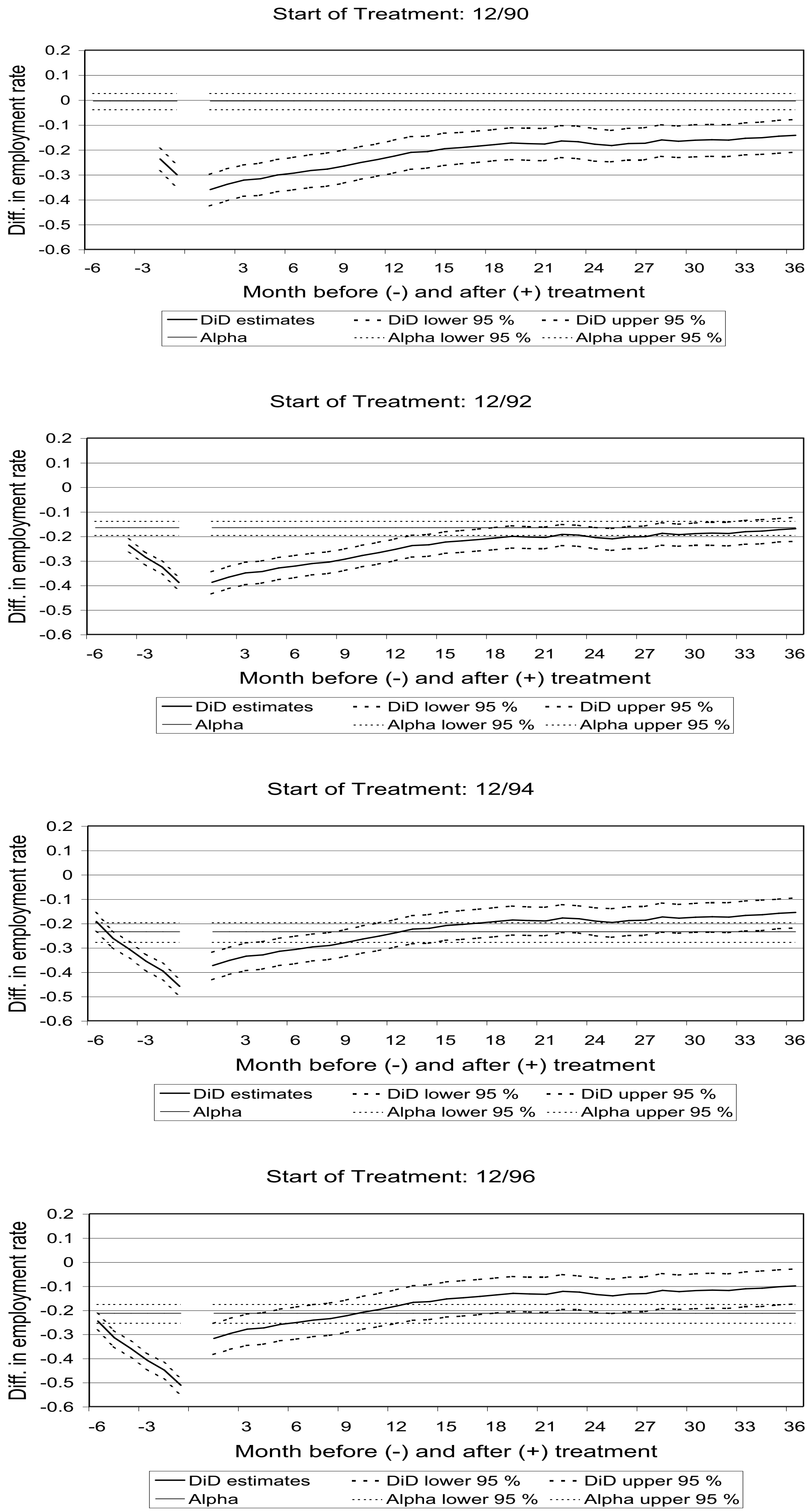
In the following we discuss the results obtained by CDiD and CDiDHR for the treatments considered. We mainly rely on graphical illustrations of the DiD estimates in equation (3.9) and the average preprogram levels $\hat{a}_{i,\tau}$. To avoid estimates which are based on the extrapolation of the parametric model in equation (3.7), our graphical illustrations only report point estimates representing at least 10 observations. Tables 3.5–3.11 in the appendix 3.A report all the estimated coefficients.

CDiD Results

Figure 3.8 depicts the estimated CDiD employment effects $DiD(t1, \tau)$ in equation (3.9) for FTR[20] during the evaluation period $t1 = 1, , \ldots, 36$ and for the period of

[20]Since we consider the CDiDHR estimates more credible, we do not report here the CDiD results for the treatments TR–TR and TR–JC to save space. Also for the same reason, we only report the FTR results for the evaluation period starting after the end of the program.

Figure 3.8: Employment Effects of FTR – CDiD – Evaluation Starts after End of Treatment



Ashenfelter's Dip $t1 = -ad(\tau), \ldots, -1$. We only report the results for the evaluation period starting after the end of the program. The effects for the evaluation period starting at the begin of the program are also similar in nature. To illustrate the changes over time, the estimates are shown in four separate graphs for the starting dates τ being the month of December in the years 1990, 1992, 1994, and 1996. The thick, changing line in the graphs represents the estimated $DiD(t1, \tau)$ for $t1 = -ad(\tau), \ldots, 36$. The dotted lines around represent the 95%–confidence interval. The constant line with dotted lines around represents the estimated long–run preprogram differences $\hat{\alpha}(\tau)$ ("alpha") with associated 95%–confidence interval. The confidence intervals are based on the bootstrap covariance estimates.[21]

For all cases, the CDiD employment effects of FTR prove significantly negative during the postprogram period, as to be expected from figure 3.7. However, the negative employment effect becomes weaker over time. For the treatment starting in 1990, we estimate an effect -14 ppoints 36 months after the treatment, the corresponding estimate for the year 1996 is -10 ppoints. Our estimates also clearly show that the employment rates become considerably lower shortly before the program starts (Ashenfelter's Dip) and this effect becomes more pronounced over time. There are also important changes in the long–run preprogram differences over time. For participants starting treatment in 1990, $\hat{\alpha}(\tau)$ is not significantly different from zero. For 1992, we find already significantly reduced long–run preprogram differences (-16 ppoints) and this feature becomes more important over time (1996: -22 ppoints). This finding corresponds to training programs becoming more focussed on groups with severe problems of finding regular employment during the course of the 1990s, see section 3.2.

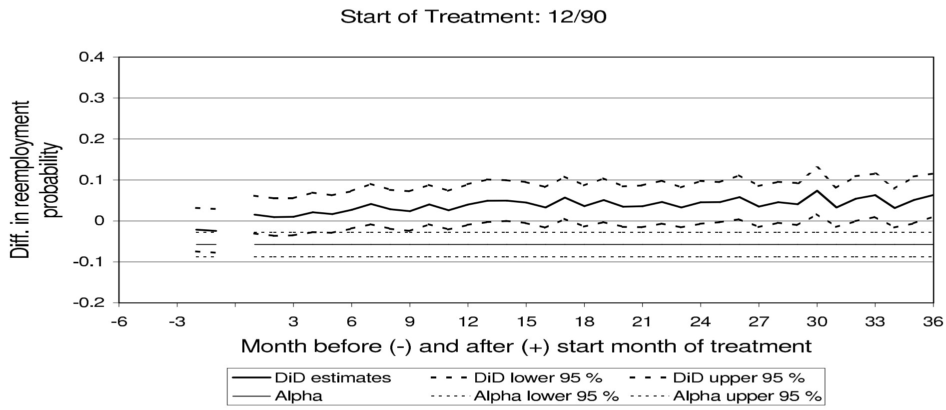
CDiDHR Results

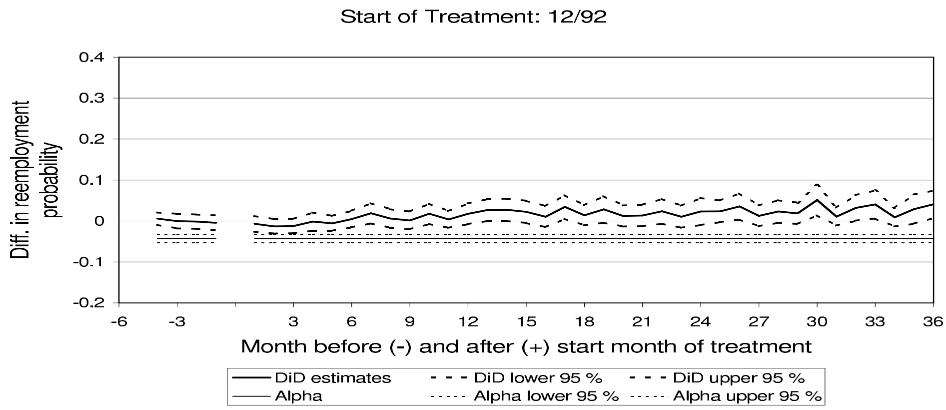
The CDiDHR esimates take the state dependence in the employment process explicitly into account. The outcome variable used is either the reemployment probability of the previously nonemployed or the probability to remain employed for the previously employed. Figures 3.9 to 3.20 display the estimated CDiDHR employment effects $DiD(t1, \tau)$ in equation (3.9). All graphs for CDiDHR are organized in the same way as described above for CDiD referring to figure 3.8.

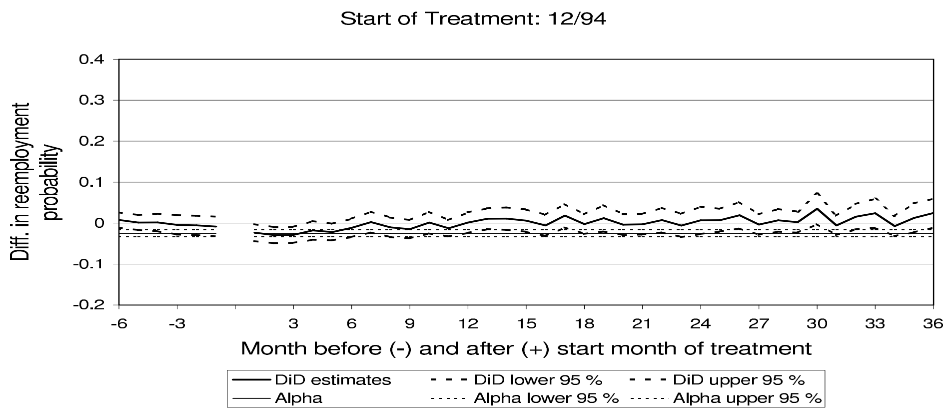
Beginning with the treatment FTR, figure 3.9 summarizes the estimated TT on the reemployment probability. Evaluation starts after the first month of the program. The first graph of figure 3.9 shows the employment effects of a FTR treatment which began in December 1990. We find positive employment effects during the evaluation period, which are, however, rarely significant. For example, one year after the

[21] When comparing the bootstrap standard errors to conventional heteroscedasticity consistent standard errors, we find that bootstrap standard errors of both $DiD(t1, \tau)$ and $\hat{\alpha}(\tau)$ are higher, the increase being stronger for the latter. This is also the case for the CDiDHR estimates.

Figure 3.9: Employment Effects of FTR – CDiDHR – Nonemployment in the Previous Month – Evaluation Starts after Begin of Treatment







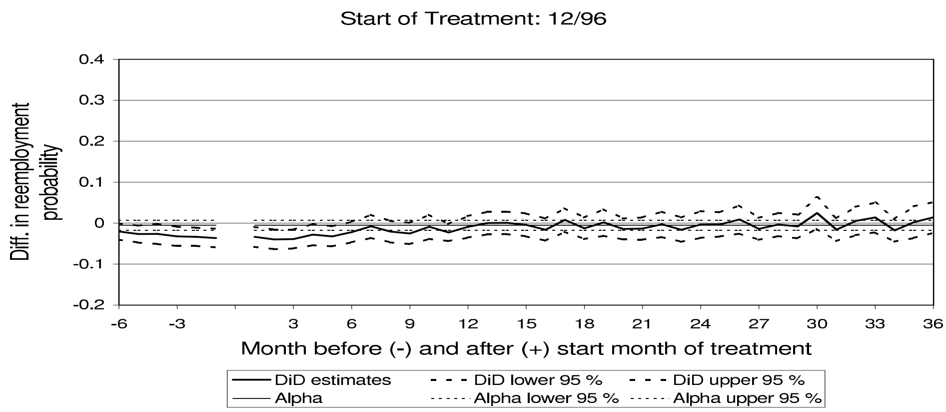
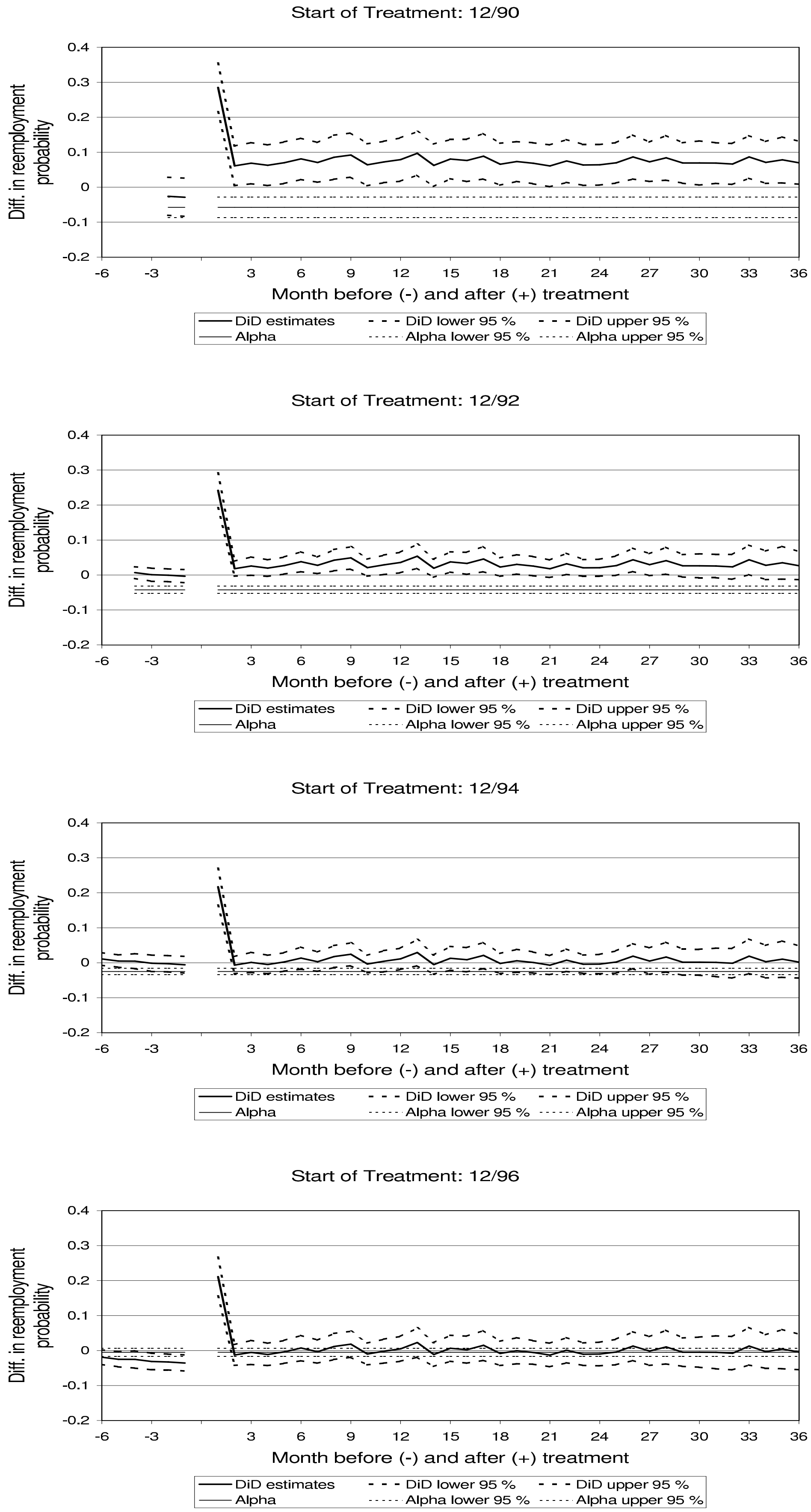


Figure 3.10: Employment Effects of FTR – CDiDHR – Nonemployment in the Previous Month – Evaluation Starts after End of Treatment



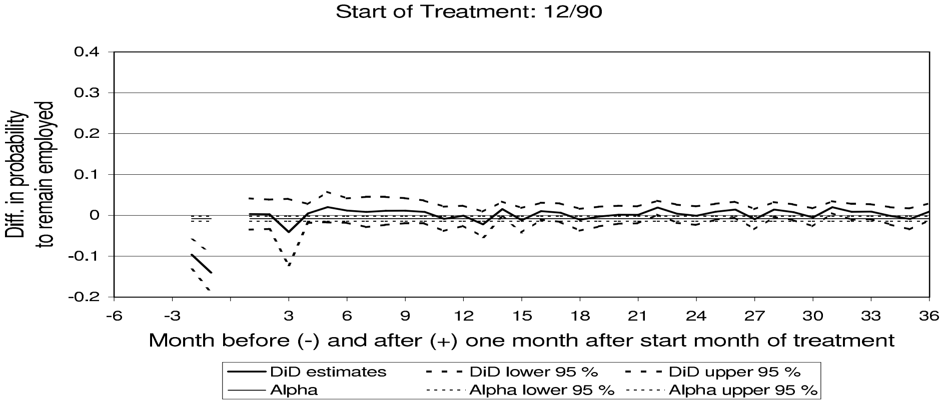
program started the participants have a 4 ppoints higher reemployment probability than in the nonparticipation case. These positive effects of a FTR vanish for programs starting later. For December 1994 and later, the effect takes sometimes negative values, which are significant shortly after the program started. This is not too surprising since one would expect a reduced search effort when the program has just started. During Ashenfelter's Dip, we find a slight decline in the reemployment probability for the group of participants. This decline is not significant in most cases and it is much less pronounced than for the CDiD employment effects. The long–run preprogram difference is significantly negative shortly after the reunification (-6 ppoints), it becomes less negative over time, and it is effectively zero for December 1996. This is in contrast to the CDiD results where the long–run preprogram difference does increase over time.

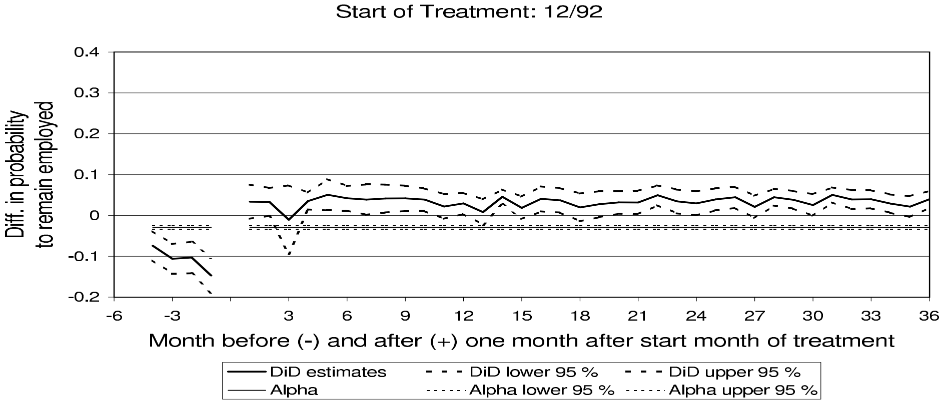
Letting the evaluation period start after the end of the program, figure 3.10 naturally shows more positive effects on the reemployment chances of former participants. Also for all cases there is a significantly positive spike in the first month after treatment. This spike can not be interpreted as pure employment effect. This is because it could reflect the endogenous, premature termination of the program due to a job offer and then the clock starts to run. However, we also observe smaller but significantly positive program effects after the first month. For example, 12 months after the program the reemployment probability increased by approximately 8 ppoints. For later starting dates, the positive effects are reduced and more often insignificant.

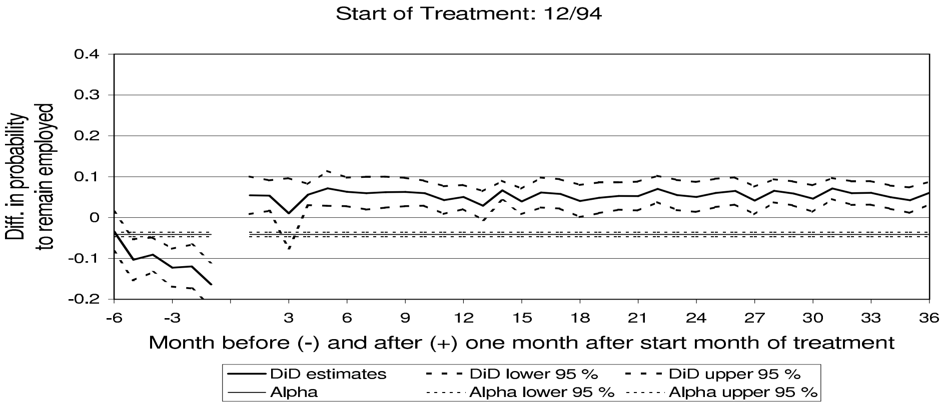
FTR can have different effects on the probability to remain employed. Figure 3.11 provides results when the evaluation period starts two months after the begin of the program. The estimated effect is close to zero for programs which start in December 1990. However, for later starting dates, the effect becomes significantly positive. For example, one year after the program started in December 1996 the probability to remain employed increases by approximately 6 ppoints. Ashenfelter's Dip is very pronounced here with strong significantly negative effects. Anticipation but also participation rules might play a role here. Shortly after the unification, the long–run preprogram difference is slightly negative and significant. It becomes more negative in later periods (-5 ppoints for programs which started in December 1996). In contrast to the results for the reemployment probability, the preprogram effects for the probability to remain employed are very similar in nature to the CDiD results above. Changing the evaluation period to start two months after the begin of the program, the results for the probability to remain employed do not change qualitatively (see figure 3.12).

Naturally the question arises, why the results differ for the two outcome variables, reemployment probability and probability to remain employed. We think that the results are driven mainly by changes in the content of the training programs over time. Shortly after unification a large part of training consisted in short courses mainly aiming at increasing the placement potential, see section 3.2.3. This could be

Figure 3.11: Employment Effects of FTR – CDiDHR – Employment in the Previous Month – Evaluation Starts after Begin of Treatment







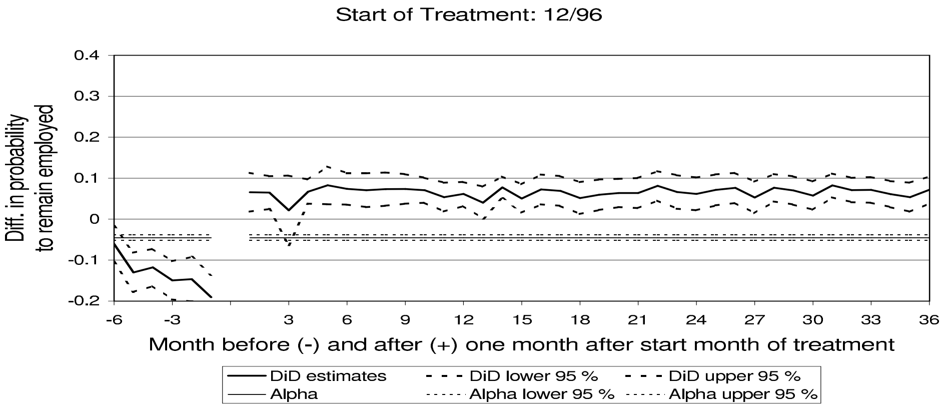
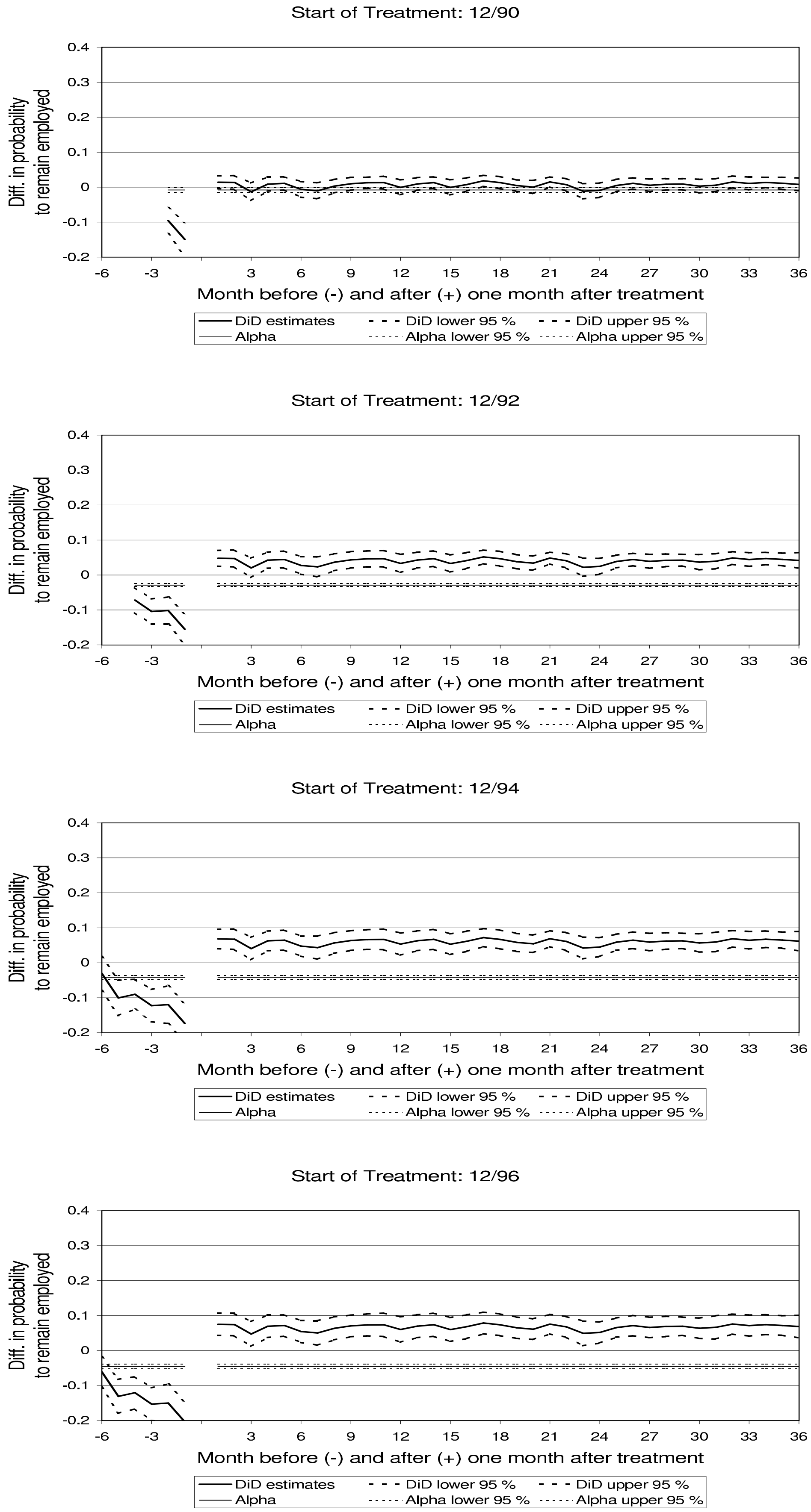


Figure 3.12: Employment Effects of FTR – CDiDHR – Employment in the Previous Month – Evaluation Starts after End of Treatment



an explanation for the small positive effect on the reemployment probability. However, later on, the composition of training courses changed towards longer courses intended to provide substantive skills. These additional skills could improve the quality of the match between participants and employers thus increasing the employment stability, once a participant finds a job. However, these additional skills do not seem to help finding a job at a faster rate.

Also, changes in the search behavior of East Germans due to a better understanding of the labor market and the benefit system in unified Germany might play a role for the differences. Shortly after unification, unemployed East Germans, not being used to a labor market in a market economy, probably tended to accept quickly a new job without focussing on wages and a high expected job duration. As a result, a positive effect of training programs might show up in an increase of their reemployment probability rather than in an increase of the probability to remain employed. Later on, individuals searching for a job became perhaps more aware of the importance of finding a 'good' job, which is not only important for their job stability, but also for the level of potential future unemployment benefits being defined by the earnings in the last job. In addition, the entitlement for transfer payments is prolonged by taking part in a training program for some time after the program, lowering the opportunity costs of job search for participants compared to other unemployed individuals. Thus, participants tended to search longer to find a 'better' job match resulting in a positive effect on the probability to remain employed.

However, an important caveat regarding the interpretation of the CDiDHR results is in order here. Since our estimated TT conditions on previous employment, it is likely that the estimates for the probability to remain employed overestimate and the estimates for the reemployment probability underestimate the true TT for the FTR treatment sample as a whole, see section 3.3.3. For this group, it might well be the case that reemployment chances increase on average and the positive effect on employment stability is smaller.

Another feature of the results which should be explained are the changes in the long–run preprogram differences. The CDiDHR estimator matches participants and nonparticipants month by month conditional upon the same employment status in the previous month. Shortly after the unification the labor market was quite turbulent. Everybody faced a high risk of becoming unemployed, resulting in a relatively small difference in the long–run preprogram difference in the probability to remain employed. However, some individuals found quickly another job and did not participate in a training program, leading to a large long–run preprogram difference in the reemployment probability at the begin of the 90's. Later on, unemployment became persistent. The difference in transitions out of nonemployment between

Figure 3.13: Combined Employment Effects of TR–TR – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Begin of Treatment

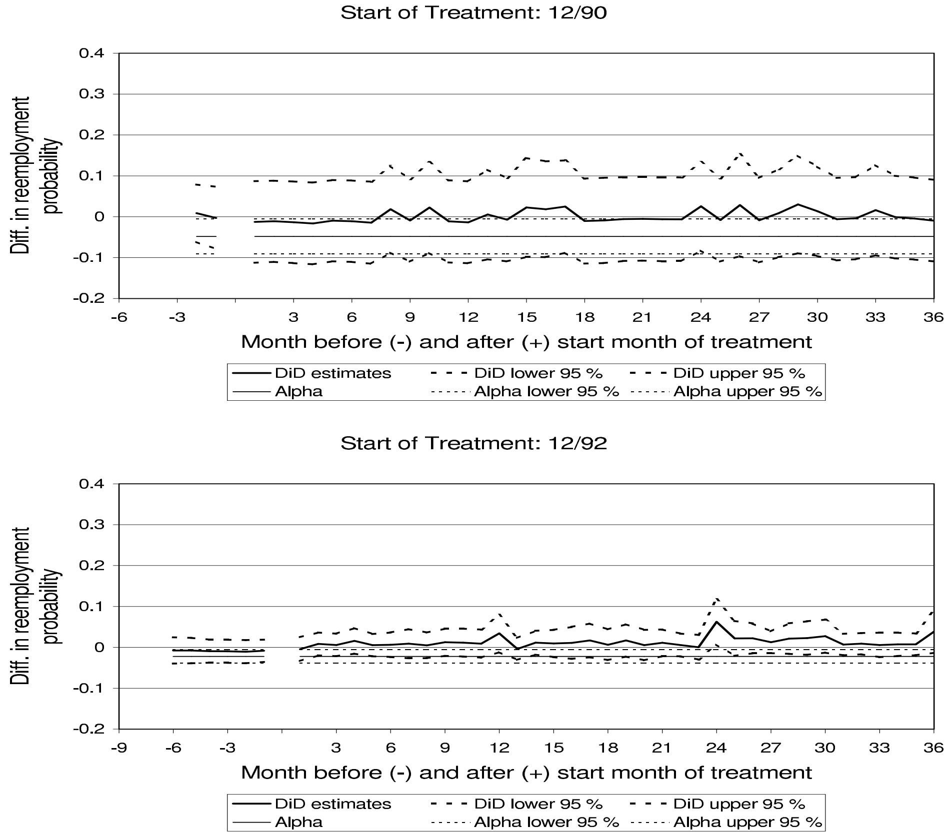


Figure 3.14: Incremental Employment Effects of TR–TR – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Begin of Treatment

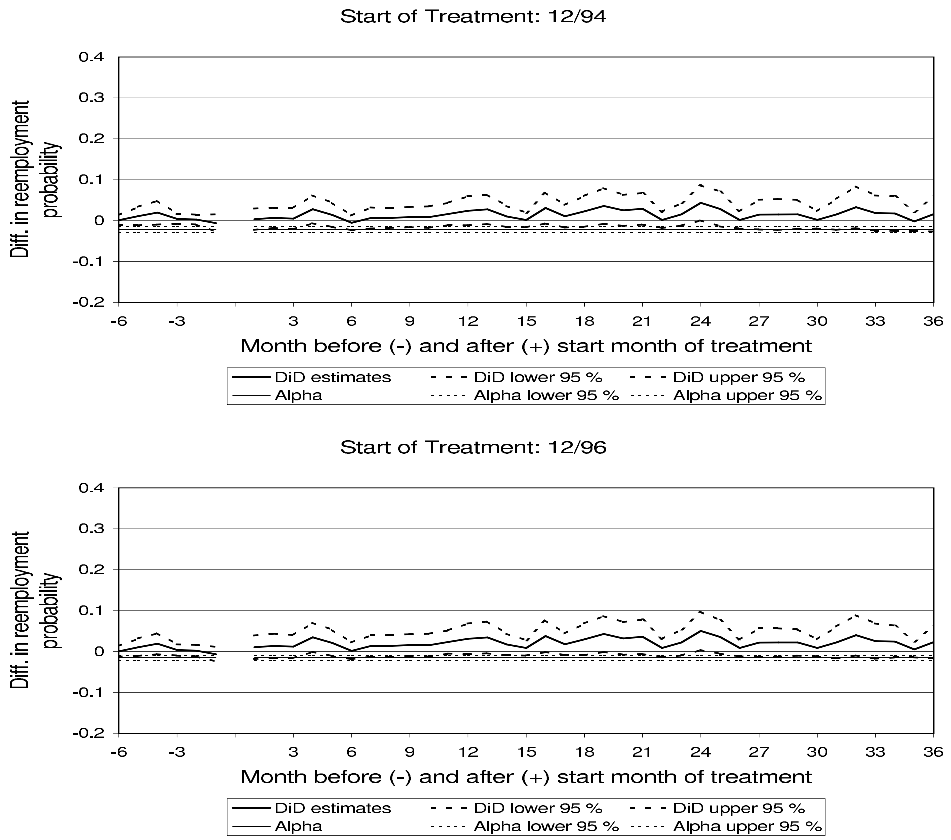
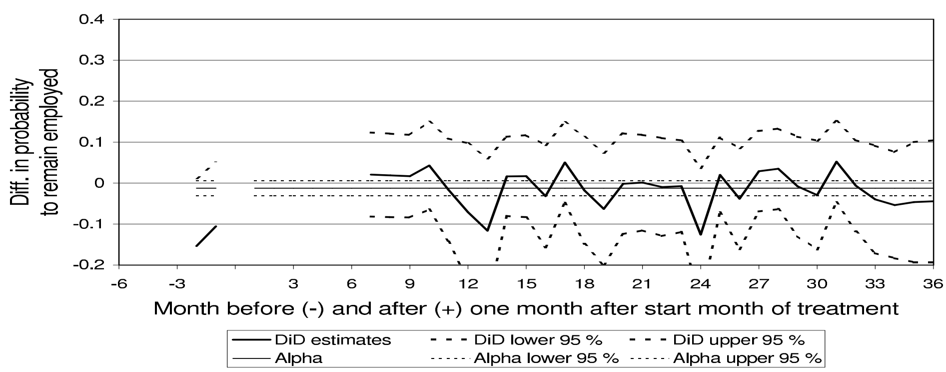


Figure 3.15: Combined Employment Effects of TR–TR – CDiDHR – Employment in Previous Month – Evaluation Starts after Begin of Treatment

Start of Treatment: 12/90



Start of Treatment: 12/92

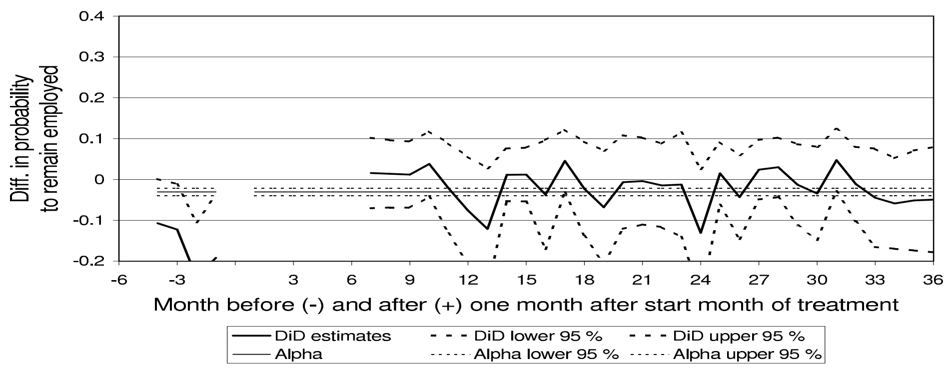
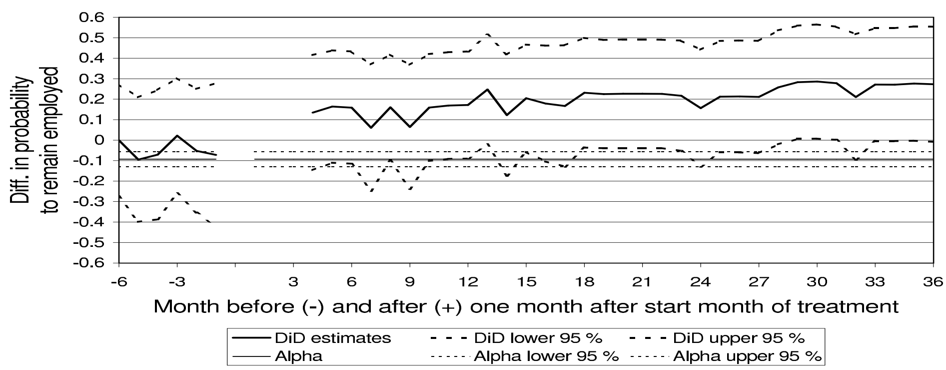


Figure 3.16: Incremental Employment Effects of TR-TR – CDiDHR – Employment in Previous Month – Evaluation Starts after Begin of Treatment

Start of Treatment: 12/94



Start of Treatment: 12/96

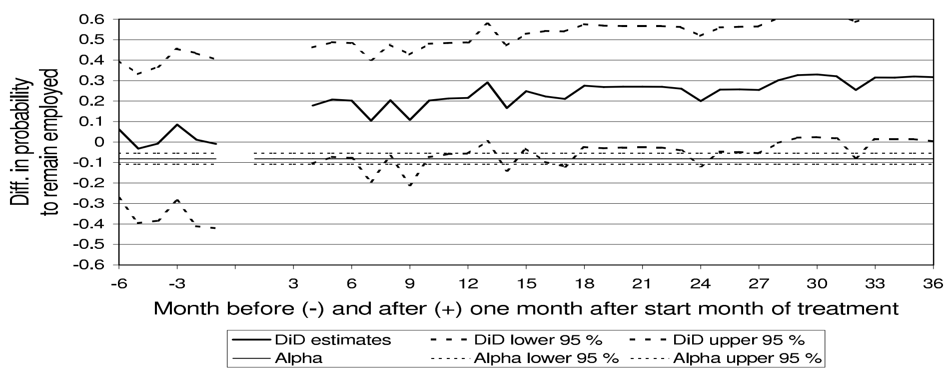
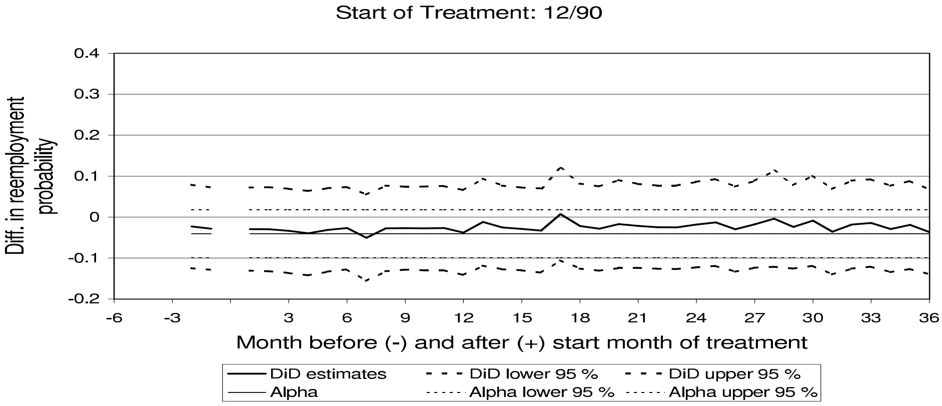


Figure 3.17: Combined Employment Effects of TR–JC – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Begin of Treatment



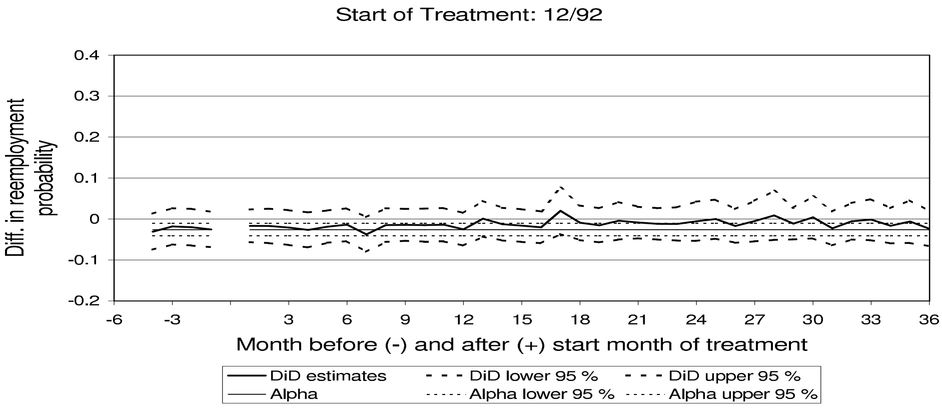
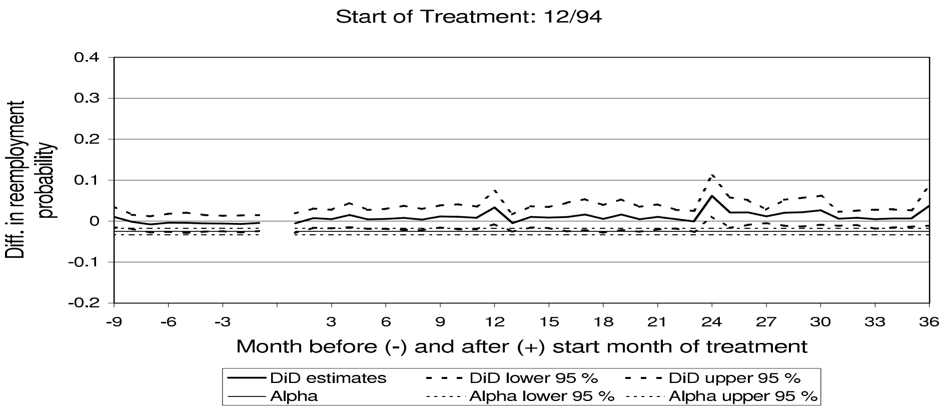


Figure 3.18: Incremental Employment Effects of TR–JC – CDiDHR – Nonemployment in Previous Month – Evaluation Starts after Begin of Treatment



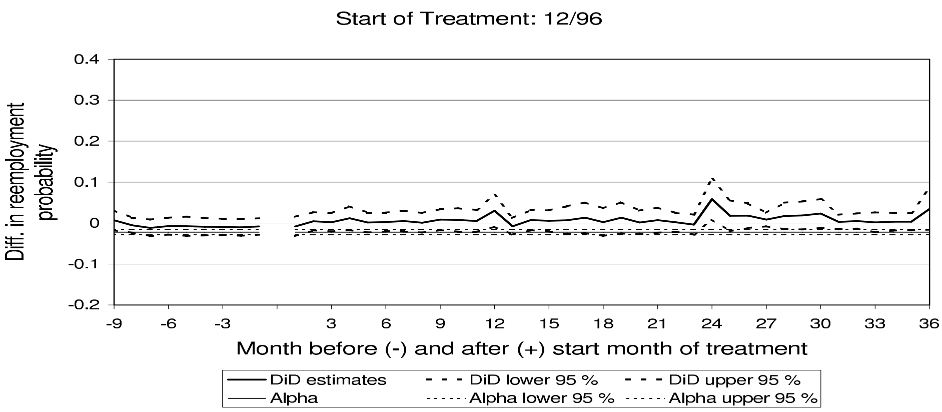
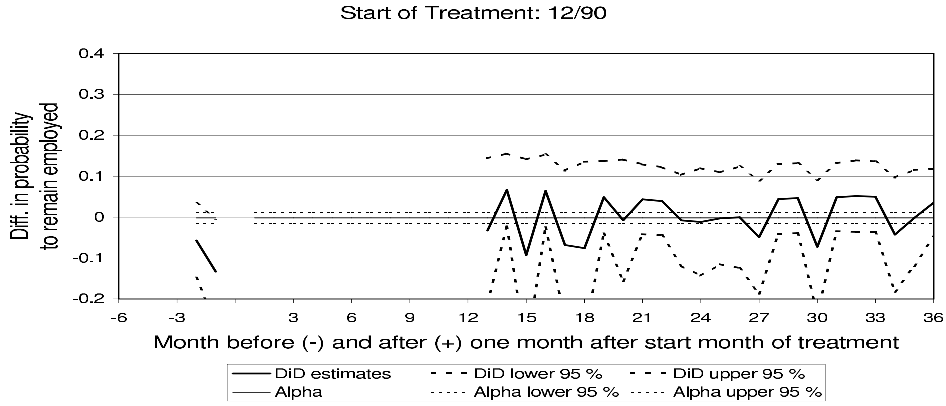


Figure 3.19: Combined Employment Effects of TR–JC – CDiDHR – Employment in Previous Month – Evaluation Starts after Begin of Treatment



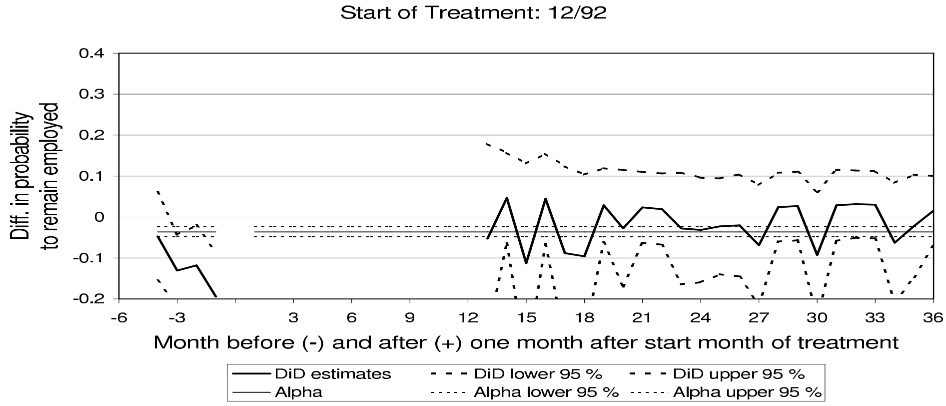
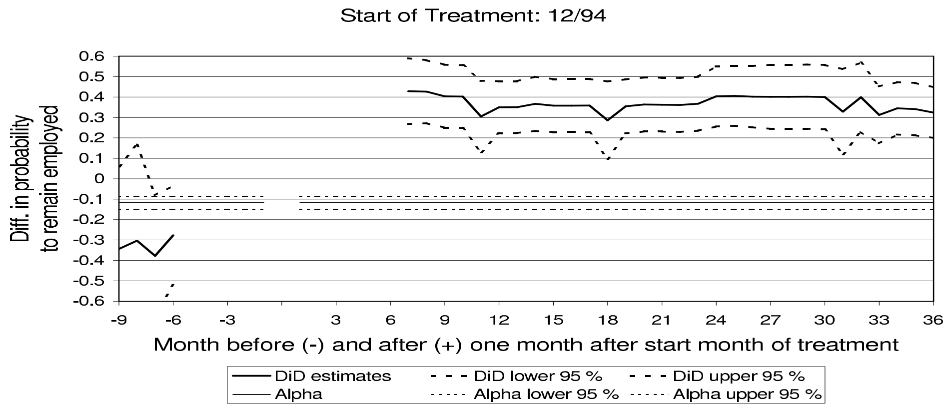
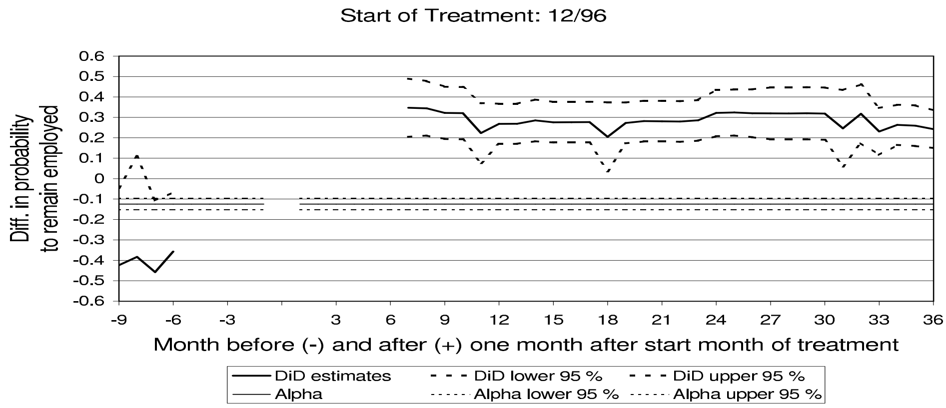


Figure 3.20: Incremental Employment Effects of TR-JC – CDiDHR – Employment in Previous Month – Evaluation Starts after Begin of Treatment





participants and nonparticipants was then less pronounced.[22] The change in the long–run preprogramm differences in the probability to remain employed most likely reflects the stricter targeting of labor market policy on unemployed individuals.

Let us now turn to the results for the multiple, sequential treatments TR–TR and TR–JC. For the combined effect, the evaluation period starts at the begin of the respective program sequence, whereas for the incremental effect it starts at the begin of the second treatment. Figures 3.13, 3.15, 3.17, and 3.19 show the estimates for the combined effects. The combined effects on the transition probabilities are mostly close to zero and always insignificant. This implies that *ex ante* it was not a successful strategy on average to assign the group of participants to the program sequences TR–TR or TR–JC. Also the incremental effects on the reemployment probability are not significantly different from zero (figures 3.14 and 3.18). However, we do find significantly positive incremental effects for the probability to remain employed (figures 3.16 and 3.20), especially for TR–JC. Note that the number of preprogram observations conditioning on being employed in the previous month is particularly small in these cases. Taken literally, the results obtained imply that the participation in a second program after a first training program improved employment stability for the group of participants. Put differently, even though the two treatments combined do not appear successful *ex ante*, *ex post* after the first training program, the second program seems partly successful.

3.5 Conclusions

This paper investigates the average employment effects for participants in Public Sponsored Training in East Germany during the time period 1990 to 1999. Modeling employment as a state–dependent outcome variable, we develop a new semiparametric conditional difference–in–differences estimator for the treatment effect. For the implementation of this estimator, we use the transition rates between employment and nonemployment as our outcome variables and we compare the results with the effects on the employment rate per se. We account for the likely occurrence of Ashenfelter's Dip caused by anticipation effects and institutional program participation rules. In addition, we develop a heuristic approach to estimate the effects of multiple sequential program participation. Thus, we estimate the effect of treatment–on–the–treated for individuals who participated in training as their first treatment. We also consider the cases where participation in a second training program or in a job creation scheme occurs afterwards. We take account of the sampling error in matching by bootstrapping.

We find negative effects of training on the employment probability. However, tak-

[22]Note that this explanation of the changes in the long–run preprogram difference does not violate the assumption of permanent fixed effects since participants change over time.

ing account of the state dependency of the employment process, the bleak picture concerning the effects of training brightens. This is especially the case for the reemployment probabilities. Concerning training programs which took place shortly after reunification, we find some positive program effects on the reemployment probability - although we have been twice conservative in modeling the effects. First, our alignment of the difference–in–differences estimation on a long–run preprogram difference is conservative (which is of course also true for the other estimates). Secondly, due to the potential positive correlation between the individual specific and the program effect when being nonemployed in the previous month, for which we do not control, we estimate a lower bound for the reemployment probability. Thus, our results indicate that modeling transition rates is more appropriate and more informative than using unconditional employment rates. Using only employment rates as success criterion might result in misleading conclusions concerning the effectiveness of ALMP programs.

Further results include that the program effects depend heavily on the time the programs took place corresponding to the institutional changes during the 1990s. Combined sequences of two programs with a first training program (e.g. a combination "training first and then job experience through a Job Creation program") are not successful from an *ex ante* point of view. In contrast, the incremental effects of the second treatment appear to have slightly positive effects on the probability to remain employed. Again, there is no positive effect on the reemployment probability when being nonemployed.

Overall, our results are not as negative as previous results in the literature and it is unlikely that training on average reduces considerably the future employment chances of participants. We also find noticeable differences among different treatment types. At the same time, it remains questionable whether on average training programs are justified in light of the large costs incurred. Our study makes some methodological progress, particularly regarding modeling the dynamic employment process in the context of program evaluation. In future research, we intend to refine the estimation of the unconditional effect of treatment–on–the–treated. Finally, our results are also of interest for other transformation countries considering the introduction of training programs as part of ALMP.

Appendix to Chapter 3

3.A Detailed Tables

Table 3.4: Propensity Score Estimations

	FTR		TR–TR		TR–JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)	Coef.	(s.e.)
Constant	-1.036	(0.161)	-2.084	(0.140)	-1.625	(0.211)
Age in 1990: Age 25–34 is omitted category						
Age 35–44	-0.094	(0.047)	-0.078	(0.081)	0.140	(0.084)
Age 45–50	-0.311	(0.058)	-0.342	(0.109)	0.224	(0.094)
Labor Market Region: Dessau is missing category						
Halberstadt	-0.109	(0.090)	-0.253	(0.164)	-0.026	(0.144)
Halle	-0.163	(0.077)	-0.126	(0.128)	-0.423	(0.137)
Magdeburg	-0.126	(0.073)	-0.121	(0.121)	-0.140	(0.117)
Merseburg	-0.110	(0.082)	-0.156	(0.140)	-0.176	(0.136)
Sangerhausen	0.009	(0.087)	-0.093	(0.149)	0.154	(0.132)
Stendal	-0.214	(0.097)	-0.414	(0.190)	-0.181	(0.159)
Wittenberg	-0.146	(0.111)	-0.183	(0.193)	0.036	(0.166)
Professional education (all): Unskilled, semi–skilled or other skills are missing category						
Skilled Worker	0.097	(0.156)	-	(-)	-0.645	(0.211)
Craftsman	-0.020	(0.176)	-0.182	(0.269)	-0.915	(0.312)
Technical college	0.271	(0.173)	0.129	(0.221)	-0.391	(0.244)
University education	0.204	(0.159)	0.288	(0.144)	-0.295	(0.204)
Professional education (women)						
Skilled worker	0.500	(0.063)	0.762	(0.119)	0.747	(0.122)
Craftsman	0.819	(0.182)	0.630	(0.397)	1.295	(0.322)
Technical college	0.035	(0.104)	0.456	(0.214)	0.074	(0.190)
University education	0.137	(0.082)	0.191	(0.143)	0.296	(0.127)

Table 3.5: Coefficient estimates for CDiD

Parameter	Coef.	FTR bootstrap – s.e.
Long–run preprogram difference		
Const	0.109538	(0.031724)
τ	-0.010506	(1.62E-03)
τ^2	7.93E-05	(1.43E-05)
Outcome–equation		
$I(t1=-6)$	0.015666	(0.051966)
$I(t1=-5)$	-0.053436	(0.051006)
$I(t1=-4)$	-0.098527	(0.049093)
$I(t1=-3)$	-0.147712	(0.047869)
$I(t1=-2)$	-0.18752	(0.046773)
$I(t1=-1)$	-0.250343	(0.047172)
$I(t1=1)$	-0.331277	(0.072718)
$I(t1=2)$	-0.310247	(0.073391)
$I(t1=3)$	-0.293056	(0.072545)
$I(t1=4)$	-0.287756	(0.07347)
$I(t1=5)$	-0.27302	(0.073619)
$I(t1=6)$	-0.265125	(0.073884)
$I(t1=7)$	-0.254978	(0.074463)
$I(t1=8)$	-0.24921	(0.074907)
$I(t1=9)$	-0.236731	(0.074903)
$I(t1=10)$	-0.222417	(0.074433)
$I(t1=11)$	-0.210578	(0.074053)
$I(t1=12)$	-0.196867	(0.074775)
$I(t1=13)$	-0.181907	(0.074531)
$I(t1=14)$	-0.178723	(0.07364)
$I(t1=15)$	-0.167247	(0.074247)
$I(t1=16)$	-0.162639	(0.073198)
$I(t1=17)$	-0.157038	(0.073196)
$I(t1=18)$	-0.150819	(0.072676)
$I(t1=19)$	-0.144263	(0.073454)
$I(t1=20)$	-0.146938	(0.073612)
$I(t1=21)$	-0.148433	(0.074218)
$I(t1=22)$	-0.136203	(0.073933)
$I(t1=23)$	-0.139326	(0.074613)
$I(t1=24)$	-0.149236	(0.074444)
$I(t1=25)$	-0.154556	(0.072969)
$I(t1=26)$	-0.146676	(0.073267)
$I(t1=27)$	-0.145388	(0.073807)
$I(t1=28)$	-0.132021	(0.0731)
$I(t1=29)$	-0.137155	(0.073106)
$I(t1=30)$	-0.133065	(0.073812)
$I(t1=31)$	-0.131043	(0.073167)

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Table 3.5: Coefficient estimates <continued>

	FTR	
Parameter	Coef.	bootstrap – s.e.
$I(t1 = 32)$	-0.132572	(0.072657)
$I(t1 = 33)$	-0.125412	(0.072885)
$I(t1 = 34)$	-0.122935	(0.073045)
$I(t1 = 35)$	-0.117068	(0.074169)
$I(t1 = 36)$	-0.113323	(0.073629)
$AD : \tau$	-4.41E-03	(2.01E-03)
$AD : \tau^2$	1.53E-05	(1.72E-05)
$PO : \tau$	-2.89E-03	(3.63E-03)
$PO : \tau^2$	3.63E-05	(3.49E-05)

AD: Ashenfelter's Dip $\equiv I(ad(\tau) \leq \tau < 0)$
PO: After end of program $\equiv I(\tau > 0)$

Table 3.6: Coefficient estimates for CDiDHR – FTR – Nonemployment in Previous Month

Start of Evaluation:	One Month after Start Month		One Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.065	(0.023)	-0.065	(0.023)
τ	5.45E-04	(8.11E-04)	5.45E-04	(8.11E-04)
τ^2	1.93E-06	(6.61E-06)	1.93E-06	(6.61E-06)
Outcome–Equation				
$I(t1=-6)$	-0.027	(0.049)	-0.035	(0.050)
$I(t1=-5)$	-0.034	(0.050)	-0.041	(0.050)
$I(t1=-4)$	-0.033	(0.049)	-0.042	(0.049)
$I(t1=-3)$	-0.039	(0.048)	-0.048	(0.049)
$I(t1=-2)$	-0.040	(0.048)	-0.049	(0.048)
$I(t1=-1)$	-0.043	(0.048)	-0.052	(0.048)
$I(t1=1)$	0.029	(0.037)	0.314	(0.050)
$I(t1=2)$	0.023	(0.037)	0.090	(0.046)
$I(t1=3)$	0.023	(0.036)	0.097	(0.047)
$I(t1=4)$	0.034	(0.038)	0.091	(0.046)
$I(t1=5)$	0.030	(0.037)	0.099	(0.047)
$I(t1=6)$	0.041	(0.036)	0.110	(0.046)
$I(t1=7)$	0.055	(0.038)	0.099	(0.046)
$I(t1=8)$	0.042	(0.037)	0.114	(0.049)
$I(t1=9)$	0.037	(0.038)	0.120	(0.048)
$I(t1=10)$	0.054	(0.037)	0.092	(0.047)
$I(t1=11)$	0.040	(0.037)	0.101	(0.047)
$I(t1=12)$	0.053	(0.038)	0.107	(0.048)
$I(t1=13)$	0.063	(0.039)	0.125	(0.048)
$I(t1=14)$	0.063	(0.038)	0.091	(0.048)
$I(t1=15)$	0.058	(0.038)	0.109	(0.045)
$I(t1=16)$	0.046	(0.038)	0.105	(0.047)
$I(t1=17)$	0.070	(0.039)	0.117	(0.049)
$I(t1=18)$	0.050	(0.038)	0.094	(0.047)
$I(t1=19)$	0.064	(0.039)	0.102	(0.045)
$I(t1=20)$	0.048	(0.038)	0.097	(0.046)
$I(t1=21)$	0.049	(0.039)	0.089	(0.047)
$I(t1=22)$	0.060	(0.039)	0.103	(0.047)
$I(t1=23)$	0.046	(0.037)	0.092	(0.047)
$I(t1=24)$	0.059	(0.038)	0.092	(0.046)
$I(t1=25)$	0.059	(0.037)	0.098	(0.046)
$I(t1=26)$	0.071	(0.039)	0.115	(0.048)
$I(t1=27)$	0.048	(0.038)	0.101	(0.045)

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Table 3.6: Coefficient estimates <continued>

Start of Evaluation:	One Month after Start Month		One Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 28)$	0.059	(0.038)	0.113	(0.048)
$I(t1 = 29)$	0.054	(0.039)	0.098	(0.045)
$I(t1 = 30)$	0.087	(0.040)	0.098	(0.048)
$I(t1 = 31)$	0.046	(0.037)	0.097	(0.045)
$I(t1 = 32)$	0.068	(0.040)	0.095	(0.045)
$I(t1 = 33)$	0.076	(0.038)	0.115	(0.046)
$I(t1 = 34)$	0.045	(0.037)	0.099	(0.045)
$I(t1 = 35)$	0.065	(0.041)	0.107	(0.048)
$I(t1 = 36)$	0.077	(0.039)	0.098	(0.046)
AD:τ	1.83E-03	(1.98E-03)	2.21E-03	(1.98E-03)
AD:τ^2	-2.08E-05	(1.76E-05)	-2.40E-05	(1.76E-05)
PO:τ	-1.19E-03	(1.31E-03)	-2.57E-03	(1.65E-03)
PO:τ^2	5.28E-06	(1.05E-05)	1.61E-05	(1.35E-05)
Variables as deviation from their mean value over all treated:				
Age 35–44	6.54E-03	(1.65E-02)	-7.19E-03	(1.69E-02)
Age 45–50	-1.40E-02	(1.62E-02)	-2.81E-02	(1.81E-02)
Halberstadt	5.19E-04	(1.74E-02)	-1.90E-02	(1.90E-02)
Halle	-2.33E-02	(2.48E-02)	-3.49E-02	(3.17E-02)
Magdeburg	5.95E-03	(1.53E-02)	-4.47E-03	(1.70E-02)
Merseburg	-5.56E-03	(1.82E-02)	-2.23E-03	(1.98E-02)
Sangerhausen	1.42E-02	(1.72E-02)	2.92E-03	(1.96E-02)
Stendal	-2.51E-02	(2.95E-02)	-4.48E-03	(1.98E-02)
Wittenberg	-8.81E-02	(7.32E-02)	-1.15E-01	(8.92E-02)
Skilled Worker	-2.78E-02	(3.32E-02)	2.46E-02	(2.79E-02)
Craftsman	5.39E-05	(2.38E-02)	3.03E-02	(3.50E-02)
Technical college	-2.69E-02	(3.04E-02)	1.11E-02	(4.19E-02)
University education	-3.35E-02	(3.40E-02)	-1.89E-02	(4.15E-02)
Female skilled worker	1.86E-02	(2.60E-02)	-2.01E-02	(2.09E-02)
Craftswoman	2.37E-02	(4.28E-02)	1.74E-02	(4.94E-02)
Female and technical college	2.12E-02	(2.88E-02)	-5.28E-03	(3.83E-02)
Female and university education	2.70E-02	(3.25E-02)	3.04E-02	(4.04E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table 3.7: Coefficient estimates for CDiDHR – FTR – Employment in Previous Month

Start of Evaluation:	Two Month after Start Month		Two Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	0.005	(0.006)	0.005	(0.006)
τ	-1.22E-03	(2.69E-04)	-1.22E-03	(2.69E-04)
τ^2	7.33E-06	(2.48E-06)	7.33E-06	(2.48E-06)
Outcome–Equation				
$I(t1 = -6)$	-0.011	(0.050)	-0.008	(0.051)
$I(t1 = -5)$	-0.080	(0.048)	-0.078	(0.048)
$I(t1 = -4)$	-0.068	(0.043)	-0.068	(0.044)
$I(t1 = -3)$	-0.099	(0.039)	-0.100	(0.040)
$I(t1 = -2)$	-0.096	(0.033)	-0.097	(0.033)
$I(t1 = -1)$	-0.141	(0.038)	-0.151	(0.038)
$I(t1 = 1)$	-0.016	(0.024)	-0.007	(0.016)
$I(t1 = 2)$	-0.016	(0.025)	-0.008	(0.016)
$I(t1 = 3)$	-0.060	(0.043)	-0.035	(0.018)
$I(t1 = 4)$	-0.014	(0.020)	-0.013	(0.016)
$I(t1 = 5)$	0.001	(0.025)	-0.011	(0.015)
$I(t1 = 6)$	-0.007	(0.022)	-0.028	(0.017)
$I(t1 = 7)$	-0.010	(0.025)	-0.032	(0.016)
$I(t1 = 8)$	-0.008	(0.024)	-0.019	(0.016)
$I(t1 = 9)$	-0.007	(0.022)	-0.012	(0.015)
$I(t1 = 10)$	-0.010	(0.021)	-0.009	(0.014)
$I(t1 = 11)$	-0.027	(0.022)	-0.009	(0.015)
$I(t1 = 12)$	-0.020	(0.020)	-0.022	(0.016)
$I(t1 = 13)$	-0.041	(0.023)	-0.013	(0.016)
$I(t1 = 14)$	-0.004	(0.019)	-0.009	(0.015)
$I(t1 = 15)$	-0.031	(0.023)	-0.022	(0.017)
$I(t1 = 16)$	-0.009	(0.016)	-0.014	(0.016)
$I(t1 = 17)$	-0.012	(0.018)	-0.004	(0.015)
$I(t1 = 18)$	-0.030	(0.019)	-0.009	(0.015)
$I(t1 = 19)$	-0.022	(0.018)	-0.017	(0.015)
$I(t1 = 20)$	-0.017	(0.018)	-0.021	(0.016)
$I(t1 = 21)$	-0.018	(0.017)	-0.007	(0.015)
$I(t1 = 22)$	0.000	(0.017)	-0.015	(0.016)
$I(t1 = 23)$	-0.015	(0.018)	-0.033	(0.017)
$I(t1 = 24)$	-0.020	(0.018)	-0.031	(0.017)
$I(t1 = 25)$	-0.010	(0.017)	-0.017	(0.016)
$I(t1 = 26)$	-0.005	(0.017)	-0.011	(0.016)
$I(t1 = 27)$	-0.028	(0.020)	-0.016	(0.016)

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Table 3.7: Coefficient estimates <continued>

Start of Evaluation:	Two Month after Start Month		Two Month after End	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 28)$	-0.005	(0.018)	-0.013	(0.016)
$I(t1 = 29)$	-0.011	(0.018)	-0.013	(0.016)
$I(t1 = 30)$	-0.024	(0.018)	-0.019	(0.016)
$I(t1 = 31)$	0.001	(0.017)	-0.016	(0.016)
$I(t1 = 32)$	-0.010	(0.018)	-0.007	(0.016)
$I(t1 = 33)$	-0.010	(0.018)	-0.011	(0.016)
$I(t1 = 34)$	-0.020	(0.019)	-0.008	(0.015)
$I(t1 = 35)$	-0.028	(0.021)	-0.011	(0.016)
$I(t1 = 36)$	-0.010	(0.019)	-0.014	(0.016)
AD:τ	1.27E-04	(1.66E-03)	2.62E-04	(1.67E-03)
AD:τ^2	-8.62E-06	(1.38E-05)	-1.06E-05	(1.38E-05)
PO:τ	1.67E-03	(1.00E-03)	1.95E-03	(8.69E-04)
PO:τ^2	-8.36E-06	(9.53E-06)	-1.15E-05	(8.49E-06)
Variables as deviation from their mean value over all treated:				
Age 35–44	-3.48E-03	(9.25E-03)	-6.46E-03	(8.75E-03)
Age 45–50	2.68E-02	(2.66E-02)	2.41E-02	(2.21E-02)
Halberstadt	2.14E-02	(1.99E-02)	1.81E-02	(1.79E-02)
Halle	2.78E-02	(2.55E-02)	2.27E-02	(2.05E-02)
Magdeburg	1.29E-02	(1.48E-02)	1.05E-02	(1.22E-02)
Merseburg	2.01E-02	(1.54E-02)	3.12E-02	(1.44E-02)
Sangerhausen	9.00E-03	(1.60E-02)	5.61E-03	(1.39E-02)
Stendal	1.61E-02	(1.73E-02)	1.52E-02	(1.81E-02)
Wittenberg	8.23E-03	(1.97E-02)	5.98E-03	(1.68E-02)
Skilled Worker	-3.27E-02	(3.50E-02)	-1.22E-02	(3.92E-02)
Craftsman	-2.92E-02	(3.61E-02)	-1.65E-02	(3.95E-02)
Technical college	-3.31E-02	(3.57E-02)	-2.07E-03	(4.08E-02)
University education	-3.84E-02	(3.43E-02)	-2.12E-02	(3.89E-02)
Female skilled worker	4.50E-04	(1.84E-02)	-2.71E-04	(1.55E-02)
Craftswoman	-2.15E-02	(3.59E-02)	-2.87E-03	(2.85E-02)
Female and technical college	-2.11E-03	(2.15E-02)	-8.30E-03	(2.28E-02)
Female and university education	2.12E-03	(1.64E-02)	7.07E-03	(1.40E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table 3.8: Coefficient estimates for CDiDHR – TR–TR – Nonemployment in Previous Month

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.066	(0.037)	-0.011	(0.029)
τ	1.62E-03	(1.46E-03)	-5.12E-04	(7.82E-04)
τ^2	-1.28E-05	(1.31E-05)	5.53E-06	(5.35E-06)
Outcome–Equation				
$I(t1 = -6)$	0.013	(0.067)	-0.062	(0.059)
$I(t1 = -5)$	0.008	(0.066)	-0.052	(0.059)
$I(t1 = -4)$	0.013	(0.066)	-0.043	(0.059)
$I(t1 = -3)$	0.012	(0.065)	-0.059	(0.058)
$I(t1 = -2)$	0.009	(0.065)	-0.060	(0.058)
$I(t1 = -1)$	-0.003	(0.068)	-0.069	(0.061)
$I(t1 = 1)$	-0.017	(0.089)	-0.235	(0.204)
$I(t1 = 2)$	-0.015	(0.088)	-0.232	(0.204)
$I(t1 = 3)$	-0.017	(0.089)	-0.234	(0.206)
$I(t1 = 4)$	-0.020	(0.089)	-0.211	(0.206)
$I(t1 = 5)$	-0.014	(0.089)	-0.225	(0.207)
$I(t1 = 6)$	-0.015	(0.089)	-0.244	(0.208)
$I(t1 = 7)$	-0.019	(0.089)	-0.232	(0.208)
$I(t1 = 8)$	0.014	(0.090)	-0.232	(0.207)
$I(t1 = 9)$	-0.013	(0.089)	-0.230	(0.207)
$I(t1 = 10)$	0.018	(0.093)	-0.230	(0.207)
$I(t1 = 11)$	-0.015	(0.089)	-0.222	(0.208)
$I(t1 = 12)$	-0.018	(0.089)	-0.215	(0.208)
$I(t1 = 13)$	0.002	(0.093)	-0.211	(0.208)
$I(t1 = 14)$	-0.011	(0.090)	-0.229	(0.209)
$I(t1 = 15)$	0.019	(0.098)	-0.237	(0.209)
$I(t1 = 16)$	0.014	(0.096)	-0.208	(0.209)
$I(t1 = 17)$	0.021	(0.094)	-0.228	(0.209)
$I(t1 = 18)$	-0.014	(0.091)	-0.216	(0.209)
$I(t1 = 19)$	-0.013	(0.091)	-0.203	(0.208)
$I(t1 = 20)$	-0.010	(0.090)	-0.214	(0.206)
$I(t1 = 21)$	-0.009	(0.090)	-0.210	(0.207)
$I(t1 = 22)$	-0.010	(0.090)	-0.237	(0.208)
$I(t1 = 23)$	-0.010	(0.090)	-0.223	(0.208)
$I(t1 = 24)$	0.022	(0.090)	-0.195	(0.209)
$I(t1 = 25)$	-0.012	(0.090)	-0.210	(0.211)

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Table 3.8: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1=26)$	0.025	(0.099)	-0.237	(0.210)
$I(t1=27)$	-0.012	(0.090)	-0.224	(0.211)
$I(t1=28)$	0.005	(0.092)	-0.224	(0.211)
$I(t1=29)$	0.026	(0.097)	-0.223	(0.212)
$I(t1=30)$	0.009	(0.092)	-0.237	(0.209)
$I(t1=31)$	-0.010	(0.089)	-0.223	(0.210)
$I(t1=32)$	-0.007	(0.089)	-0.206	(0.211)
$I(t1=33)$	0.012	(0.093)	-0.220	(0.210)
$I(t1=34)$	-0.005	(0.089)	-0.221	(0.213)
$I(t1=35)$	-0.008	(0.089)	-0.241	(0.211)
$I(t1=36)$	-0.013	(0.089)	-0.223	(0.210)
AD:τ	1.34E-05	(2.95E-03)	1.82E-03	(1.57E-03)
AD:τ^2	-2.12E-06	(2.80E-05)	-1.28E-05	(1.03E-05)
PO:τ	3.62E-04	(3.68E-03)	6.61E-03	(5.77E-03)
PO:τ^2	-3.23E-06	(3.36E-05)	-4.39E-05	(3.84E-05)
Variables as deviation from their mean value over all treated:				
Age 35–44	-1.56E-02	(4.56E-02)	-1.29E-02	(2.32E-02)
Age 45–50	5.54E-03	(5.43E-02)	8.92E-03	(2.95E-02)
Halberstadt	3.16E-02	(4.70E-02)	5.70E-02	(5.30E-02)
Halle	2.90E-02	(4.80E-02)	3.84E-02	(4.32E-02)
Magdeburg	3.22E-02	(4.53E-02)	4.28E-02	(5.30E-02)
Merseburg	2.60E-02	(4.53E-02)	5.02E-02	(5.05E-02)
Sangerhausen	2.95E-02	(5.16E-02)	5.49E-02	(4.33E-02)
Stendal	2.79E-02	(4.62E-02)	3.58E-02	(5.61E-02)
Wittenberg	-1.74E-01	(2.02E-01)	2.84E-02	(3.81E-02)
Skilled Worker	-	(-)	-	(-)
Craftsman	-8.01E-02	(1.34E-01)	-1.52E-02	(5.37E-02)
Technical college	-7.34E-02	(1.19E-01)	-4.57E-02	(5.38E-02)
University education	-7.28E-02	(1.24E-01)	-1.35E-01	(9.07E-02)
Female skilled worker	-	(-)	-3.95E-02	(3.19E-02)
Craftswoman	-	(-)	-7.93E-03	(5.48E-02)
Female and technical college	-9.49E-02	(1.26E-01)	9.53E-03	(3.35E-02)
Female and university education	3.06E-02	(6.58E-02)	1.20E-01	(6.71E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table 3.9: Coefficient estimates for CDiDHR – TR–TR – Employment in Previous Month

Start of Evaluation:	Combined Effect Two Month after Start Month of Sequence		Incremental Effect of Second TR Two Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	0.000	(0.017)	-0.159	(0.055)
τ	-1.19E-03	(7.88E-04)	1.58E-03	(2.02E-03)
τ^2	9.24E-06	(8.27E-06)	-7.81E-06	(1.61E-05)
Outcome–Equation				
$I(t1=-6)$	0.134	(0.179)	-0.280	(1.371)
$I(t1=-5)$	0.111	(0.184)	-0.374	(1.368)
$I(t1=-4)$	0.027	(0.169)	-0.350	(1.367)
$I(t1=-3)$	0.011	(0.157)	-0.257	(1.364)
$I(t1=-2)$	-0.106	(0.146)	-0.331	(1.343)
$I(t1=-1)$	-0.058	(0.137)	-0.351	(1.364)
$I(t1=1)$	0.111	(0.130)	0.207	(0.442)
$I(t1=2)$	0.108	(0.125)	0.257	(0.443)
$I(t1=3)$	-0.147	(0.233)	0.243	(0.442)
$I(t1=4)$	0.075	(0.109)	0.254	(0.439)
$I(t1=5)$	0.039	(0.103)	0.283	(0.440)
$I(t1=6)$	0.028	(0.103)	0.278	(0.440)
$I(t1=7)$	0.034	(0.101)	0.180	(0.449)
$I(t1=8)$	0.032	(0.102)	0.279	(0.446)
$I(t1=9)$	0.030	(0.101)	0.184	(0.460)
$I(t1=10)$	0.056	(0.104)	0.278	(0.451)
$I(t1=11)$	-0.004	(0.107)	0.288	(0.452)
$I(t1=12)$	-0.058	(0.129)	0.291	(0.446)
$I(t1=13)$	-0.103	(0.131)	0.366	(0.447)
$I(t1=14)$	0.030	(0.102)	0.241	(0.454)
$I(t1=15)$	0.030	(0.103)	0.324	(0.441)
$I(t1=16)$	-0.019	(0.102)	0.298	(0.431)
$I(t1=17)$	0.064	(0.102)	0.286	(0.427)
$I(t1=18)$	-0.005	(0.110)	0.351	(0.434)
$I(t1=19)$	-0.050	(0.108)	0.344	(0.435)
$I(t1=20)$	0.012	(0.106)	0.345	(0.436)
$I(t1=21)$	0.014	(0.105)	0.345	(0.436)
$I(t1=22)$	0.004	(0.109)	0.345	(0.436)
$I(t1=23)$	0.006	(0.095)	0.336	(0.437)
$I(t1=24)$	-0.113	(0.119)	0.275	(0.436)
$I(t1=25)$	0.033	(0.098)	0.332	(0.434)

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Table 3.9: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of Second TR	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of Second TR	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 26)$	-0.025	(0.109)	0.333	(0.435)
$I(t1 = 27)$	0.042	(0.102)	0.330	(0.433)
$I(t1 = 28)$	0.048	(0.102)	0.377	(0.448)
$I(t1 = 29)$	0.005	(0.110)	0.402	(0.447)
$I(t1 = 30)$	-0.016	(0.112)	0.405	(0.448)
$I(t1 = 31)$	0.066	(0.103)	0.397	(0.445)
$I(t1 = 32)$	0.007	(0.106)	0.330	(0.462)
$I(t1 = 33)$	-0.027	(0.111)	0.391	(0.443)
$I(t1 = 34)$	-0.041	(0.109)	0.390	(0.443)
$I(t1 = 35)$	-0.033	(0.119)	0.395	(0.443)
$I(t1 = 36)$	-0.031	(0.117)	0.392	(0.448)
AD:τ	-4.09E-03	(6.84E-03)	6.09E-03	(3.77E-02)
AD:τ^2	1.04E-05	(6.51E-05)	-2.41E-05	(2.40E-04)
PO:τ	-1.40E-03	(5.62E-03)	-4.71E-03	(1.35E-02)
PO:τ^2	2.49E-05	(6.86E-05)	4.54E-05	(1.02E-04)
Variables as deviation from their mean value over all treated:				
Age 35–44	1.83E-02	(4.14E-02)	-7.00E-02	(1.77E-01)
Age 45–50	2.07E-01	(1.94E-01)	-	(-)
Halberstadt	3.78E-02	(8.53E-02)	-4.41E-01	(6.82E-01)
Halle	1.75E-01	(1.07E-01)	-3.28E-01	(6.53E-01)
Magdeburg	2.40E-02	(6.62E-02)	-1.88E-01	(6.70E-01)
Merseburg	4.97E-02	(7.21E-02)	-3.11E-01	(6.69E-01)
Sangerhausen	-1.47E-02	(7.70E-02)	-3.23E-01	(6.57E-01)
Stendal	-1.17E-02	(1.06E-01)	-4.14E-01	(6.63E-01)
Wittenberg	1.51E-01	(1.78E-01)	-4.84E-01	(7.57E-01)
Skilled Worker	-	(-)	-	(-)
Craftsman	-7.76E-02	(1.12E-01)	-	(-)
Technical college	-1.96E-01	(2.16E-01)	-	(-)
University education	-2.74E-01	(1.67E-01)	3.53E-01	(3.29E-01)
Female skilled worker	-1.05E-01	(7.43E-02)	2.11E-01	(1.65E-01)
Craftswoman	-1.11E-01	(1.00E-01)	-	(-)
Female and technical college	1.21E-01	(1.55E-01)	1.07E-01	(1.66E-01)
Female and university education	1.72E-01	(1.07E-01)	-1.51E-01	(3.64E-01)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table 3.10: Coefficient estimates for CDiDHR – TR–JC – Nonemployment in Previous Month

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.050	(0.048)	-0.008	(0.031)
τ	8.72E-04	(1.74E-03)	-5.70E-04	(9.03E-04)
τ^2	-5.37E-06	(1.44E-05)	4.76E-06	(6.27E-06)
Outcome–Equation				
$I(t1 = -9)$	-	(-)	-0.013	(0.049)
$I(t1 = -8)$	-	(-)	-0.025	(0.049)
$I(t1 = -7)$	-	(-)	-0.032	(0.048)
$I(t1 = -6)$	0.007	(0.080)	-0.027	(0.049)
$I(t1 = -5)$	-0.003	(0.081)	-0.028	(0.047)
$I(t1 = -4)$	-0.036	(0.079)	-0.029	(0.048)
$I(t1 = -3)$	-0.023	(0.081)	-0.029	(0.048)
$I(t1 = -2)$	-0.025	(0.081)	-0.030	(0.047)
$I(t1 = -1)$	-0.031	(0.081)	-0.028	(0.047)
$I(t1 = 1)$	-0.041	(0.084)	-0.007	(0.053)
$I(t1 = 2)$	-0.042	(0.085)	0.005	(0.052)
$I(t1 = 3)$	-0.046	(0.085)	0.003	(0.052)
$I(t1 = 4)$	-0.051	(0.085)	0.013	(0.052)
$I(t1 = 5)$	-0.043	(0.084)	0.002	(0.052)
$I(t1 = 6)$	-0.039	(0.084)	0.003	(0.053)
$I(t1 = 7)$	-0.062	(0.086)	0.006	(0.054)
$I(t1 = 8)$	-0.040	(0.086)	0.002	(0.053)
$I(t1 = 9)$	-0.039	(0.084)	0.010	(0.054)
$I(t1 = 10)$	-0.040	(0.085)	0.009	(0.053)
$I(t1 = 11)$	-0.039	(0.085)	0.006	(0.055)
$I(t1 = 12)$	-0.050	(0.085)	0.031	(0.057)
$I(t1 = 13)$	-0.024	(0.087)	-0.007	(0.052)
$I(t1 = 14)$	-0.037	(0.085)	0.008	(0.052)
$I(t1 = 15)$	-0.041	(0.084)	0.007	(0.055)
$I(t1 = 16)$	-0.045	(0.085)	0.008	(0.055)
$I(t1 = 17)$	-0.005	(0.089)	0.014	(0.055)
$I(t1 = 18)$	-0.034	(0.085)	0.003	(0.054)
$I(t1 = 19)$	-0.040	(0.085)	0.014	(0.054)
$I(t1 = 20)$	-0.029	(0.087)	0.003	(0.055)
$I(t1 = 21)$	-0.033	(0.085)	0.008	(0.052)
$I(t1 = 22)$	-0.037	(0.084)	0.003	(0.053)

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Table 3.10: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	One Month after Start Month of Sequence		One Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1=23)$	-0.037	(0.084)	-0.003	(0.052)
$I(t1=24)$	-0.030	(0.085)	0.059	(0.060)
$I(t1=25)$	-0.025	(0.086)	0.019	(0.057)
$I(t1=26)$	-0.042	(0.085)	0.019	(0.055)
$I(t1=27)$	-0.030	(0.086)	0.010	(0.054)
$I(t1=28)$	-0.016	(0.091)	0.018	(0.055)
$I(t1=29)$	-0.036	(0.085)	0.020	(0.056)
$I(t1=30)$	-0.021	(0.088)	0.024	(0.056)
$I(t1=31)$	-0.048	(0.086)	0.004	(0.054)
$I(t1=32)$	-0.030	(0.087)	0.006	(0.053)
$I(t1=33)$	-0.026	(0.086)	0.003	(0.054)
$I(t1=34)$	-0.041	(0.086)	0.004	(0.053)
$I(t1=35)$	-0.031	(0.086)	0.004	(0.053)
$I(t1=36)$	-0.049	(0.085)	0.035	(0.058)
AD:τ	2.07E-04	(2.96E-03)	7.82E-04	(1.39E-03)
AD:τ^2	-1.96E-06	(2.63E-05)	-6.50E-06	(9.68E-06)
PO:τ	1.16E-03	(3.25E-03)	1.53E-04	(1.62E-03)
PO:τ^2	-1.31E-05	(3.00E-05)	-1.98E-06	(1.16E-05)
Variables as deviation from their mean value over all treated:				
Age 35–44	-1.96E-03	(3.09E-02)	-1.85E-02	(1.33E-02)
Age 45–50	-3.19E-02	(3.97E-02)	3.59E-03	(1.78E-02)
Halberstadt	-5.33E-02	(4.62E-02)	5.34E-03	(2.69E-02)
Halle	-1.63E-01	(1.47E-01)	-4.34E-03	(1.97E-02)
Magdeburg	-5.78E-02	(4.96E-02)	-2.13E-02	(2.54E-02)
Merseburg	-7.08E-02	(5.43E-02)	-1.21E-02	(2.18E-02)
Sangerhausen	-6.33E-02	(4.99E-02)	1.08E-03	(1.79E-02)
Stendal	-3.89E-02	(5.44E-02)	1.63E-02	(3.07E-02)
Wittenberg	-1.09E-01	(7.63E-02)	2.36E-02	(3.27E-02)
Skilled Worker	1.98E-02	(6.64E-02)	1.24E-02	(1.96E-02)
Craftsman	7.19E-02	(9.11E-02)	2.55E-02	(4.87E-02)
Technical college	-6.51E-02	(8.33E-02)	-1.71E-01	(1.75E-01)
University education	-1.21E-01	(1.22E-01)	9.95E-03	(1.98E-02)
Female skilled worker	-2.02E-02	(6.82E-02)	-1.39E-02	(1.53E-02)
Craftswoman	-7.82E-02	(1.07E-01)	-1.68E-02	(4.82E-02)
Female and technical college	7.32E-02	(8.06E-02)	1.69E-01	(1.74E-01)
Female and university education	1.10E-01	(1.09E-01)	2.40E-03	(1.33E-02)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$

PO: After end of program $\equiv I(t1 > 0)$

Table 3.11: Coefficient estimates for CDiDHR – TR–JC – Employment in Previous Month

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	0.021	(0.016)	-0.031	(0.026)
τ	-2.03E-03	(8.81E-04)	-2.23E-03	(1.18E-03)
τ^2	1.24E-05	(9.29E-06)	1.34E-05	(9.27E-06)
Outcome–Equation				
$I(t1 = -9)$	-	(-)	1.194	(0.639)
$I(t1 = -8)$	-	(-)	1.234	(0.590)
$I(t1 = -7)$	-	(-)	1.160	(0.571)
$I(t1 = -6)$	0.170	(0.138)	1.261	(0.568)
$I(t1 = -5)$	-0.025	(0.150)	0.858	(0.597)
$I(t1 = -4)$	0.031	(0.127)	0.883	(0.366)
$I(t1 = -3)$	-0.053	(0.110)	0.635	(0.423)
$I(t1 = -2)$	-0.040	(0.090)	-0.106	(0.337)
$I(t1 = -1)$	-0.116	(0.107)	-	(-)
$I(t1 = 1)$	0.076	(0.118)	-0.468	(0.162)
$I(t1 = 2)$	0.075	(0.118)	-0.594	(0.208)
$I(t1 = 3)$	0.073	(0.118)	-0.569	(0.176)
$I(t1 = 4)$	0.059	(0.114)	-0.449	(0.190)
$I(t1 = 5)$	0.057	(0.114)	-0.464	(0.177)
$I(t1 = 6)$	0.075	(0.114)	-0.476	(0.172)
$I(t1 = 7)$	0.059	(0.110)	-0.510	(0.167)
$I(t1 = 8)$	0.065	(0.116)	-0.512	(0.163)
$I(t1 = 9)$	0.062	(0.112)	-0.535	(0.159)
$I(t1 = 10)$	0.060	(0.112)	-0.536	(0.159)
$I(t1 = 11)$	-0.074	(0.149)	-0.634	(0.159)
$I(t1 = 12)$	0.055	(0.107)	-0.589	(0.154)
$I(t1 = 13)$	-0.019	(0.111)	-0.588	(0.154)
$I(t1 = 14)$	0.081	(0.104)	-0.572	(0.153)
$I(t1 = 15)$	-0.078	(0.149)	-0.581	(0.153)
$I(t1 = 16)$	0.079	(0.103)	-0.581	(0.153)
$I(t1 = 17)$	-0.054	(0.125)	-0.580	(0.154)
$I(t1 = 18)$	-0.062	(0.149)	-0.652	(0.165)
$I(t1 = 19)$	0.063	(0.108)	-0.584	(0.152)
$I(t1 = 20)$	0.007	(0.127)	-0.575	(0.152)
$I(t1 = 21)$	0.058	(0.108)	-0.576	(0.152)
$I(t1 = 22)$	0.053	(0.107)	-0.577	(0.152)

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Table 3.11: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$I(t1 = 23)$	0.006	(0.108)	-0.571	(0.152)
$I(t1 = 24)$	0.003	(0.120)	-0.535	(0.153)
$I(t1 = 25)$	0.011	(0.114)	-0.533	(0.153)
$I(t1 = 26)$	0.014	(0.118)	-0.537	(0.154)
$I(t1 = 27)$	-0.035	(0.121)	-0.537	(0.153)
$I(t1 = 28)$	0.058	(0.109)	-0.538	(0.154)
$I(t1 = 29)$	0.061	(0.109)	-0.537	(0.154)
$I(t1 = 30)$	-0.058	(0.133)	-0.538	(0.154)
$I(t1 = 31)$	0.063	(0.107)	-0.610	(0.165)
$I(t1 = 32)$	0.066	(0.110)	-0.539	(0.156)
$I(t1 = 33)$	0.064	(0.109)	-0.626	(0.158)
$I(t1 = 34)$	-0.028	(0.121)	-0.594	(0.154)
$I(t1 = 35)$	0.012	(0.115)	-0.598	(0.155)
$I(t1 = 36)$	0.050	(0.108)	-0.615	(0.156)
AD:τ	-1.04E-03	(4.97E-03)	-4.16E-02	(1.61E-02)
AD:τ^2	-3.15E-05	(5.08E-05)	2.65E-04	(1.04E-04)
PO:τ	-1.33E-03	(6.70E-03)	2.92E-02	(7.02E-03)
PO:τ^2	1.04E-05	(7.64E-05)	-2.27E-01	(3 .602536E-04)
Variables as deviation from their mean value over all treated:				
Age 35–44	-3.26E-02	(4.70E-02)	-	(-)
Age 45–50	-1.31E-02	(5.81E-02)	-2.13E-01	(8.14E-02)
Halberstadt	-5.15E-02	(7.68E-02)	4.53E-01	(1.11E-01)
Halle	-5.17E-02	(1.16E-01)	4.29E-01	(1.37E-01)
Magdeburg	-2.09E-02	(4.68E-02)	3.89E-01	(8.11E-02)
Merseburg	-7.97E-02	(6.90E-02)	1.61E-01	(7.59E-02)
Sangerhausen	-4.35E-03	(5.11E-02)	3.15E-01	(7.61E-02)
Stendal	-8.06E-02	(8.23E-02)	3.47E-01	(1.56E-01)
Wittenberg	-9.75E-02	(7.19E-02)	5.02E-01	(1.14E-01)
Skilled Worker	-1.34E-01	(7.43E-02)	-4.80E-01	(1.09E-01)
Craftsman	-9.86E-02	(1.25E-01)	-	(-)
Technical college	-2.07E-01	(1.12E-01)	-	(-)
University education	-1.29E-01	(8.24E-02)	-	(-)
Female skilled worker	-3.71E-02	(5.94E-02)	5.56E-01	(1.50E-01)
Craftswoman	-	(-)	-	(-)

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Table 3.11: Coefficient estimates <continued>

	Combined Effect		Incremental Effect of JC	
Start of Evaluation:	Two Month after Start Month of Sequence		Two Month after Start Month of JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Female and technical college	-2.24E-03	(9.76E-02)	-	(-)
Female and university education	-5.84E-03	(5.37E-02)	-1.29E-01	(5.93E-02)

Incremental Effect of JC with conventional, heteroscedasticity consistent standard errors due to insufficient number of observations.
AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$
PO: After end of program $\equiv I(t1 > 0)$

3.B Sensitivity Analysis for the Evaluation Approach Using Conditional–Double–Difference–in–ρ–Differences

3.B.1 Employment Dynamics and Conditional Double–Difference–in–ρ–Differences (CD2iρD)

The conditional Double–Difference–in–ρ–Differences (CD2iρD) method, with which we conduct a sensitivity analysis for the estimation of treatment effects in the context of a state dependent employment process, builds upon the following employment model for any individual i:

$$Y_{i,t} = \rho(X_i, t)Y_{i,t-1} + a(X_i, t) + \delta_{i,t,\tau}D_{i,t}(\tau) + c_i + u_{i,t} \tag{3.10}$$

This autoregressive model is a more restrictive version of our econometric model in equation 3.5. The differences in the transitions from the two different employment states are here captured alone by the autoregressive component $\rho(X_i, t)Y_{i,t}$ and not as in 3.5 by separate components for each previous employment state.

After matching on observables, the following Conditional Double–Difference–in–ρ–Differences (CD2iρD) estimator

$$\frac{1}{N_1}\sum_{i=1}^{N_1}\left[(Y^1_{i,t1} - \rho(X_i, t1)Y^1_{i,t1-1}) - (Y^0_{i,t0} - \rho(X_i, t0)Y^0_{i,t0-1})\right]$$
$$-\frac{1}{N_1}\sum_{i=1}^{N_1}\left[\sum_j w_{i,j}[(Y^0_{j,t1} - \rho(X_j, t1)Y^0_{j,t1-1}) - (Y^0_{j,t0} - \rho(X_j, t0)Y^0_{i,t0-1})]\right]$$

based in non–parametric matching estimates consistently the average treatment–on–the–treated effect where i now denotes participants and j nonparticipants. Furthermore, $t0$ denotes some time before and $t1$ some time after treatment. We suggest to estimate the unknown parameters $\rho(.)$ in a prior stage based on the sample of non–participants.

Our CD2iρD estimator is related to the unconditional difference–in–ρ–differences estimator by Heckman and Robb (1985, p. 171) who suggest to use such an estimator when a treatment and a control group is available and the outcome variable exhibits a first order autoregressive component. Our extension is to take the long-run before–after–difference (to eliminate the permanent component c_i) and to use a conditional version of this estimator based on a matched sample.

For implementation of the CD2iρD we also apply local linear kernel matching on the estimated propensity score using the Gaussion kernel as for the CDiDHR. However, we do not use cross validation procedure to choose the bandwidth due to its computational complexity. We choose Silverman's Rule of Thumb (ROT) for the selection of the bandwidth instead, see Silverman (1986, 47f.).

3.B.2 Implementation Details

3.B.2.1 Data

The data used are the same as described in section 3.4.1 with one exception. Treatment sequences are determined differently due to the inclusion of wage subsidies as a third treatment type. In the questionnaire of 1999 individuals were asked whether they ever had an employer that received a wage subsidy in connecting with their employment relationship. The respondents in 1999 could only give one time period as an answer. In our main analysis we discharge this information on wage subsidies due to two considerations. First wage subsidies are a very heterogeneous group and are not reported in a consistent way in this data source, thus making them difficult to evaluate. Secondly, part of the wage subsidies might actually already be covered by the category job creation schemes, as the question on wage subsidies is not precisely formulated and timing differences are just due to differences in recording their start date in the two different interview modes, direct question on dates and chronological calendar.

Table 3.12: Program Participation (number of individuals) in the LMM–SA during 1990 and 1999 when including wage subsidies

One Program	JC[a]	FT[b]	WS[c]
At least once	689	1021	222
As first program	455	883	80

Program Sequences[d]	JC–JC	JC–FT
First and Second	97	105
Program Sequences	FT–JC	FT–FT
First and Second	163	146

a: Further Training b: Job Creation Scheme c: Wage Subsidy
d: For instance, FT–JC indicates that a first participation in FT and a second treatment in JC occurred

3.B.2.2 Specification of Outcome Equation

We estimate the following outcome equation for CD2iρD. The employment outcome Y (treatment of individual i begins in period τ, time before begin of treatment $t = -18, \ldots, -1$ and after end of treatment $t = 1, \ldots, 36$) is modelled as:

$$(3.11) \qquad (Y_{i,t} - \rho_i Y_{i,t-1}) - \sum_j w_{i,j}(Y_{j,t} - \rho_j Y_{j,t-1}) \; = \; \alpha_0 + \alpha_1 \tau + \alpha_2 \tau^2$$

$$+ \sum_{j=ad(\tau)}^{36} \beta_j D(t=j) + (\gamma_1^{ad}\tau + \gamma_2^{ad}\tau^2) D(-ad(\tau) < t < 0) + (\gamma_1^{po}\tau + \gamma_2^{po}\tau^2) D(t > 0)$$

where

t	month before $t < 0$ and after treatment $t > 0$
τ	month when the treatment begins (calendar time)
$\alpha_0, \alpha_1, \alpha_2$	coefficients measuring the long–run preprogram differences depending upon the month when the program starts (τ)
$ad(\tau)$	month before the begin of the program when Ashenfelter's Dip starts depending upon τ
$\beta_j, \gamma_j^{ad}, \gamma_j^{po}$	coefficients modeling the DiD effect relative to the long–run preprogram differences

Besides the inclusion of ρ–differences instead of estimating the outcome equation separably for the different employment states in the previous months, the outcome equation (3.11) differs from the equation 3.7 in the main analysis in two aspects. Here we estimate the long–run preprogram difference simultaneously with the treatment effects. In the main analysis we first subtracted the average long–run preprogram difference between participant i and comparable nonparticipants from the difference during Ashenfelter's Dip and during the evaluation period, in order to avoid to capture a potential correlation between the individual specific effect and the treatment effect with respect to the starting date. However a comparison of the empirical results, produced by these two different approaches on the basis of CDiDHR, showed no remarkable differences.

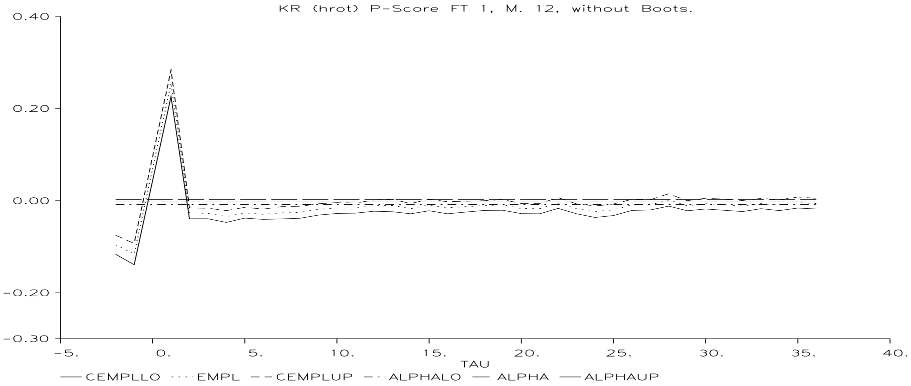
3.B.3 Evaluation Results

For the CD2iρD approach, the ρ–estimation results based upon the sample of non–participants for each treatment are given in table 3.13 in section 3.B.4. These are first step GMM–estimates using the Arellano–Bond estimation approach in a non-linear context. The set of instruments involves second and third lags of employment interacted with the other strictly exogenous (time–invariant) variables.[23] The estimated participation probits, which propensity score matching is based upon, can

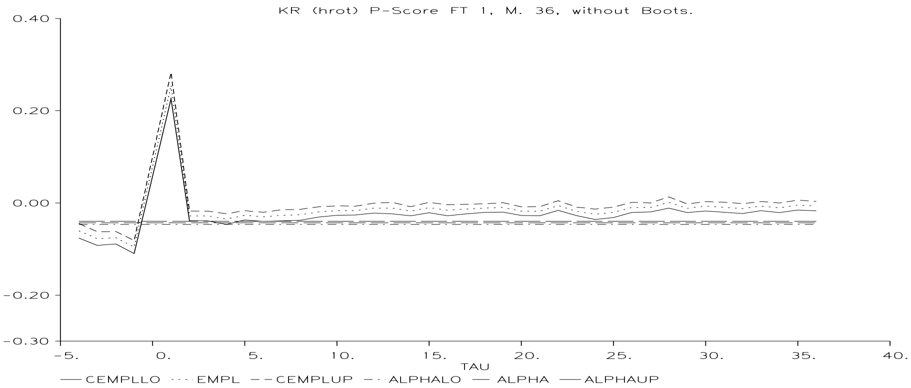
[23] Since we have a long panel of 120 monthly observations, we do not use all conceivable instruments.

Figure 3.21: Employment Effects of FTR – CD2iρD – – Evaluation Starts after End of Treatment

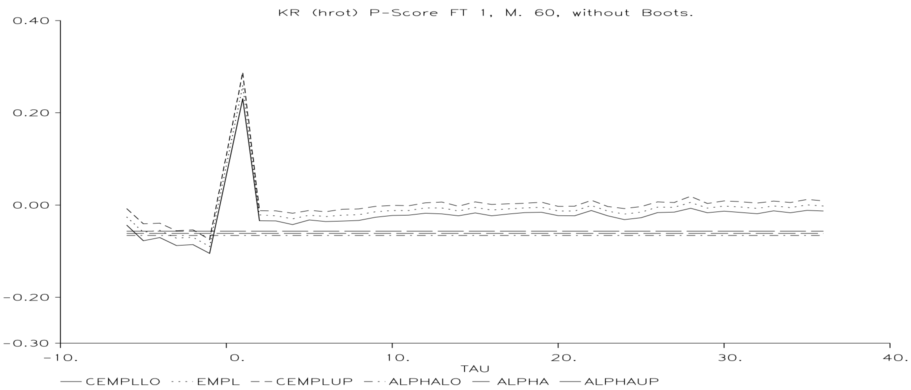
Start of Treatment: 12/90



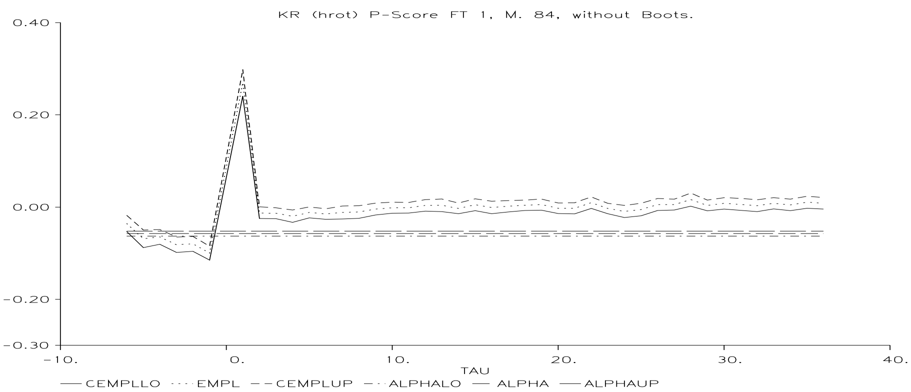
Start of Treatment: 12/92



Start of Treatment: 12/94



Start of Treatment: 12/96



be found in table 3.14 in section 3.B.4. The coefficient estimates for the CD2iρD outcome equations are reported in table 3.16 in section 3.B.4. In the following, we rather discuss these estimation results by means of graphical illustrations. However, a caveat applies as we have not calculated bootstrap standard errors for the CD2iρD estimates.

Figures 3.21 to 3.25 display graphically the estimated C2iρD–effect of treatment on the treated depending on τ and t as well as the long–run preprogram difference $\alpha_0 + \alpha_1 t + \alpha_2 t^2$ depending upon the begin of the program. We put 90%–confidence interval around the estimates.

The thick line shows the CD2iρD–estimator for the time period during Ashenfelter's Dip and until 36 months after the end of the program. Participation takes place in time 0. This should be taken as an interruption of the curves. The postprogram period here shows the average treatment effect on the treated. The horizontal estimate displays the estimated long–run preprogram differences between the treated individual and the nonparametrically estimated non–treatment outcome. A program here can only be considered as successful for the participants if the confidence–intervals lie in the positive region.

Figure 3.21 shows the CD2iρD estimates of a first training program (FTR). Most notably, we find a strong positive spike in the first period after treatment and effects which are close to zero from period 2 to 36. For early starting dates of the program (12/90 and 12/92), the effects are significantly negative during the first part of the postprogram period (except for the first month). In contrast, for later starting dates of the program (12/96), the effects turn significantly positive during the later part of the postprogram period. The long–run preprogram difference is not significantly different from zero for early starting dates and turns significantly negative for later starting dates. We also find evidence for Ashenfelter's Dip in the employment dynamics during the months shortly before the begin of the program.[24]

Figures 3.22 and 3.25 display the CD2iρD estimates for the program sequences TR–TR and TR–JC. In contrast to our main analysis we start the evaluation after the treatment sequence considered.

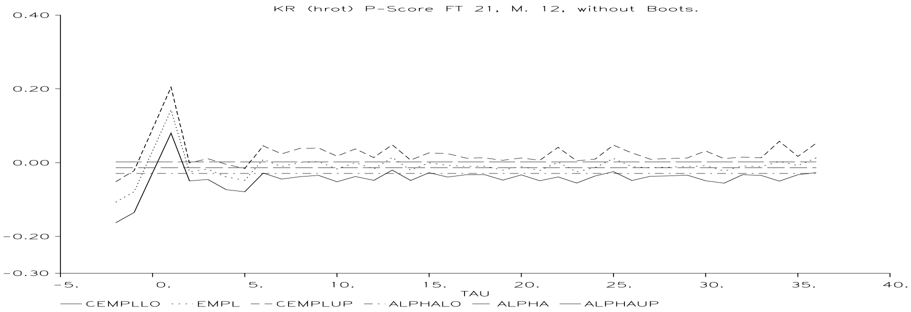
Apart from the initial spikes, we find postprogram effects for both combined treatments which are basically zero and which even turn significantly negative with early starting dates. In contrast, the incremental effects are somewhat different. For both program sequences, we find slightly positive postprogram effects during month 2 to 36 after the end of the program and these effects are even significantly positive (especially for the later starting period).

Naturally, a comparison of the results of CD2iρD and CDiDHR needs undertaken under the caveat that we did not calculated bootstrap standard errors, we put 10 % confidence intervals around the estimates and that we started the evaluation period generally after the end of the program. Nevertheless, we can say that the results

[24]This corresponds to the results obtained in Fitzenberger and Prey (2000) based on a completely parametric model.

Figure 3.22: Combined Employment Effects of TR–TR – CD2iρD – – Evaluation Starts after End of Treatment

Start of Treatment: 12/90



Start of Treatment: 12/92

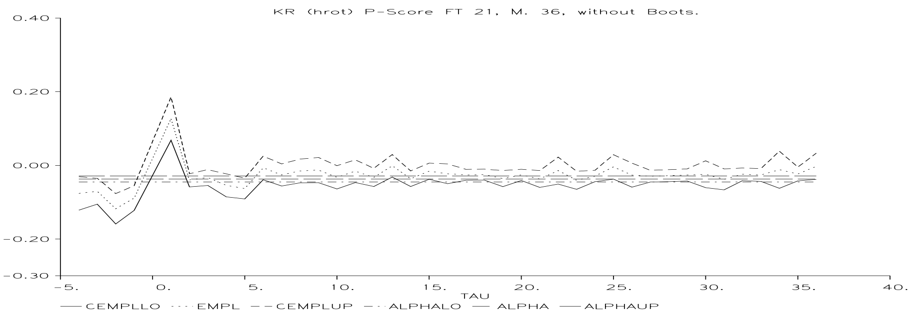
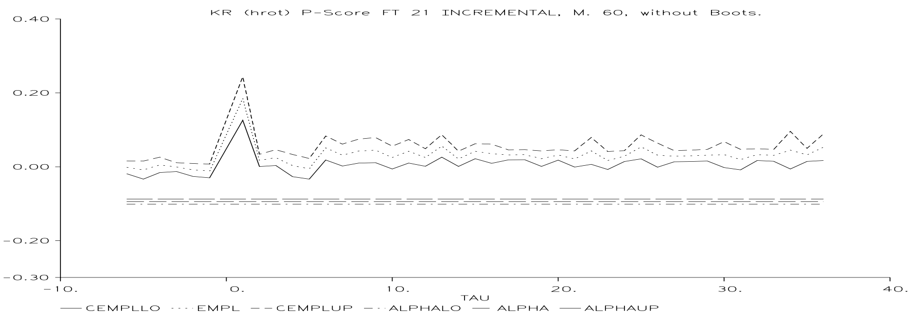


Figure 3.23: Combined Employment Effects of TR–TR – CD2iρD – – Evaluation Starts after End of Treatment

Start of Treatment: 12/94



Start of Treatment: 12/96

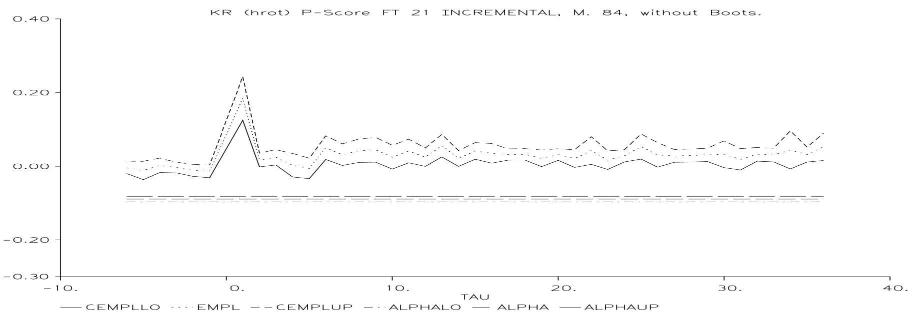
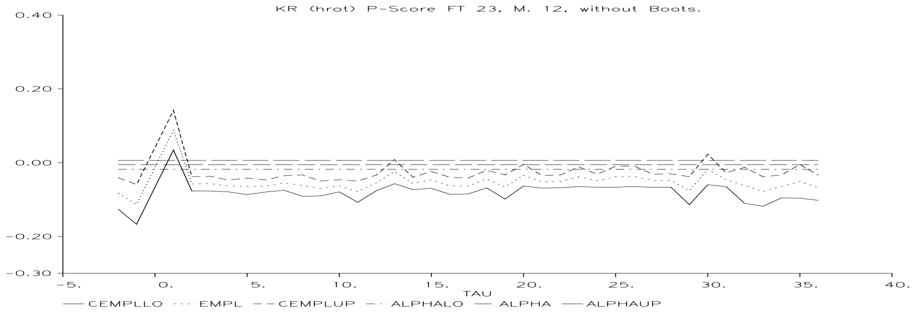


Figure 3.24: Combined Employment Effects of TR–JC – CD2iρD – – Evaluation Starts after End of Treatment

Start of Treatment: 12/90



Start of Treatment: 12/92

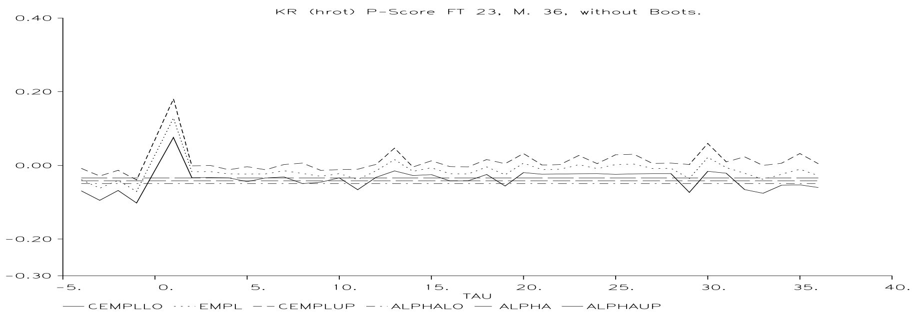
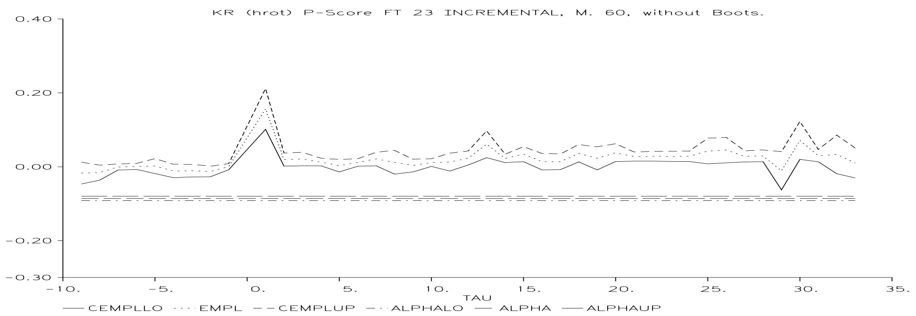
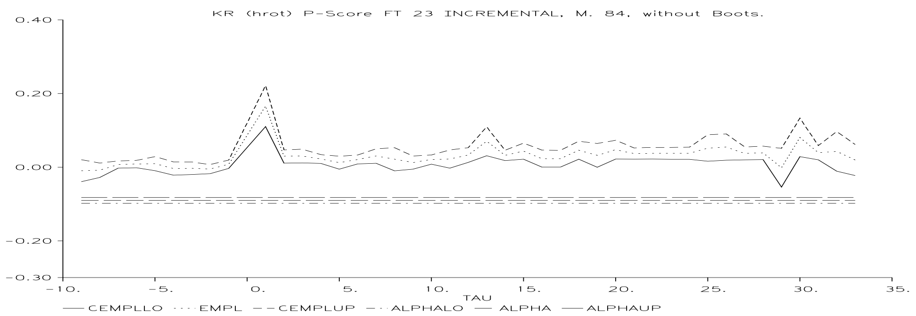


Figure 3.25: Combined Employment Effects of TR–JC – CD2iρD – – Evaluation Starts after End of Treatment

Start of Treatment: 12/94



Start of Treatment: 12/96



of CD2iρD confirm our results of the CDiDHR. Training as a first treatment shows effects which are mainly close to zero and for some selected start dates we find positive effects. The combined effect of treatment sequences is also close to zero, whereas the incremental effect (for CDiρD also for the sequence TR–TR) shows slightly positive effects.

3.B.4 Tables

Table 3.13: Coefficient estimates of employment equation[a]

Variable	TR as first prog Coef.	(s.e.)		
Constant	-.227601E-03	(.181104E-03)		
ΔY_{T-1}	.609992	(.078171)		
ΔY_{T-1}·AGE90	.661628E-02	(.224870E-02)		
ΔY_{T-1}·University	-.048539	(.034373)		
	TR-TR		**TR-JC**	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Constant	-.236920E-03	(.189910E-03)	-.236648E-03	(.191229E-03)
ΔY_{T-1}	.709074	(.077189)	.714254	(.076709)
ΔY_{T-1}·AGE90	.483554E-02	(.219154E-02)	.462583E-02	(.218730E-02)
ΔY_{T-1}·University	-.030825	(.033946)	-.021289	(.033035)

a: Included Instruments: Y_{T-2}, Y_{T-3}, Y_{T-2}·SEX, Y_{T-3}·SEX, Y_{T-2}·AGE90, Y_{T-3}·AGE90, Y_{T-2}·University, Y_{T-3}·University, Y_{T-2}·T, Y_{T-3}·T, SEX, AGE90, University, T

Table 3.14: Propensity Score Estimations for TR as First Program

Variable	TR as first prog Coef.	(s.e.)
Constant	-1.07184	(.1635)
Age 35 – 44	-.094331	(.0471)
Age 45 and older	-.315686	(.0583)
Region		
Halberstadt	-.117374	(.0906)
Halle	-.176224	(.0766)
Magdeburg	-.126768	(.0726)
Merseburg	-.109939	(.0821)
Sangerhausen	.217639E-02	(.0873)
Stendal	-.224979	(.0970)
Wittenberg	-.145804	(.1109)
Professional education (all)		
Skilled worker	.133797	(.1588)
Craftsman	.021812	(.1782)
Technical college	.312798	(.1753)
University education	.245768	(.1612)
Professional education (women)		
Skilled worker	.495313	(.0630)
Craftswoman	.819274	(.1823)
Technical college	.034995	(.1043)
University education	.129868	(.0818)

Table 3.15: Propensity Score Estimations for Treatment Sequences

	TR–TR		TR–JC	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Constant	-2.09211	(.141)	-1.77539	(.231)
Age 35 – 44	-.100332	(.081)	.124494	(.085)
Age 45 and older	-.367851	(.111)	.213575	(.095)
Region				
Halberstadt	-.255556	(.164)	.010844	(.145)
Halle	-.163950	(.129)	-.440414	(.141)
Magdeburg	-.146524	(.121)	-.128937	(.119)
Merseburg	-.157698	(.140)	-.184040	(.140)
Sangerhausen	-.094592	(.148)	.135846	(.135)
Stendal	-.417601	(.190)	-.221477	(.166)
Wittenberg	-.183282	(.193)	.035717	(.169)
Professional education (all)				
Skilled worker	–	(–)	-.484547	(.229)
Craftsman	-.149133	(.270)	-.751420	(.324)
Technical college	.165301	(.222)	-.229241	(.260)
University education	.301976	(.147)	-.193344	(.225)
Professional education (women)				
Skilled worker	.789295	(.122)	.708492	(.122)
Craftswoman	.637046	(.397)	1.22173	(.326)
Technical college	.454056	(.213)	.022239	(.192)
University education	.190879	(.146)	.338782	(.130)

Table 3.16: Coefficient estimates for CD2iρD outcome equation

	TR as first prog		TR–TR (comb.)	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$Const$	.025501	(.611E-02)	.476685E-02	(.017)
τ	-.260476E-02	(.277E-03)	-.173280E-02	(.819E-03)
τ^2	.192363E-04	(.247E-05)	.157837E-04	(.812E-05)
$D(t=-6)$	-.067718	(.023)	-.035629	(.073)
$D(t=-5)$	-.101119	(.024)	-.027104	(.066)
$D(t=-4)$	-.096935	(.021)	-.062193	(.062)
$D(t=-3)$	-.114022	(.020)	-.056385	(.057)
$D(t=-2)$	-.111974	(.018)	-.103914	(.054)
$D(t=-1)$	-.132194	(.020)	-.075035	(.056)
$D(t=1)$	.257086	(.018)	.158988	(.043)
$D(t=2)$	-.025359	(.953E-02)	-.851691E-02	(.024)
$D(t=3)$	-.025649	(.932E-02)	-.971927E-03	(.026)
$D(t=4)$	-.032286	(.010)	-.022399	(.028)
$D(t=5)$	-.024135	(.952E-02)	-.031060	(.026)
$D(t=6)$	-.027396	(.907E-02)	.025162	(.030)
$D(t=7)$	-.024160	(.010)	.608648E-02	(.028)
$D(t=8)$	-.023151	(.010)	.016993	(.030)
$D(t=9)$	-.016805	(.969E-02)	.019329	(.029)
$D(t=10)$	-.013924	(.950E-02)	-.556241E-03	(.028)
$D(t=11)$	-.013945	(.910E-02)	.016333	(.030)
$D(t=12)$	-.863352E-02	(.969E-02)	-.593212E-03	(.027)
$D(t=13)$	-.847889E-02	(.010)	.030753	(.028)
$D(t=14)$	-.015228	(.918E-02)	-.422234E-02	(.025)
$D(t=15)$	-.736931E-02	(.965E-02)	.016124	(.025)
$D(t=16)$	-.013186	(.010)	.925676E-02	(.027)
$D(t=17)$	-.010642	(.950E-02)	.628532E-02	(.023)
$D(t=18)$	-.853118E-02	(.896E-02)	.691260E-02	(.023)
$D(t=19)$	-.698236E-02	(.943E-02)	-.383921E-02	(.025)
$D(t=20)$	-.015029	(.911E-02)	.625561E-02	(.023)
$D(t=21)$	-.015174	(.937E-02)	-.481961E-02	(.025)
$D(t=22)$	-.295277E-02	(.946E-02)	.017932	(.031)
$D(t=23)$	-.015509	(.910E-02)	-.856228E-02	(.026)
$D(t=24)$	-.021898	(.978E-02)	.307831E-02	(.023)
$D(t=25)$	-.017676	(.994E-02)	.028006	(.028)
$D(t=26)$	-.695260E-02	(.976E-02)	.547960E-02	(.029)
$D(t=27)$	-.703906E-02	(.929E-02)	.277416E-02	(.023)
$D(t=28)$	.366746E-02	(.010)	.411275E-02	(.024)
$D(t=29)$	-.891265E-02	(.925E-02)	.598257E-02	(.024)
$D(t=30)$	-.436543E-02	(.942E-02)	.799699E-02	(.031)
$D(t=31)$	-.660747E-02	(.964E-02)	-.596325E-02	(.028)
$D(t=32)$	-.943684E-02	(.965E-02)	.781956E-02	(.024)

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Table 3.16: Coefficient estimates <continued>

	TR as first prog		TR–TR (comb.)	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$D(t=33)$	-.411337E-02	(.931E-02)	.580269E-02	(.024)
$D(t=34)$	-.775484E-02	(.938E-02)	.020363	(.038)
$D(t=35)$	-.190543E-02	(.951E-02)	.852553E-02	(.024)
$D(t=36)$	-.420573E-02	(.949E-02)	.029503	(.031)
AD:τ	.148725E-02	(.714E-03)	-.240851E-03	(.238E-02)
AD:τ^2	-.131374E-04	(.576E-05)	-.407514E-05	(.234E-04)
PO:τ	-.246218E-03	(.354E-03)	-.162851E-02	(.112E-02)
PO:τ^2	.469095E-05	(.338E-05)	.204997E-04	(.121E-04)

	TR-JC (comb.)		TR–TR (incr.)		TR–JC (incr.)	
Variable	Coef.	(s.e.)	Coef.	(s.e.)	Coef.	(s.e.)
$Const$	.016426	(.013)	-.081200	(.038)	-.052980	(.034)
τ	-.197837E-02	(.630E-03)	-.538051E-03	(.103E-02)	-.813732E-03	(.945E-03)
τ^2	.999392E-05	(.613E-05)	.523305E-05	(.687E-05)	.440899E-05	(.635E-05)
$D(t=-9)$	–	(–)	–	(–)	-.124324	(.064)
$D(t=-8)$	–	(–)	–	(–)	-.123017	(.060)
$D(t=-7)$	–	(–)	–	(–)	-.107747	(.059)
$D(t=-6)$	-.087147	(.050)	-.396631E-02	(.046)	-.106373	(.058)
$D(t=-5)$	-.129171	(.059)	-.011228	(.048)	-.105200	(.060)
$D(t=-4)$	-.111006	(.051)	.269935E-02	(.046)	-.118560	(.059)
$D(t=-3)$	-.134399	(.049)	-.330333E-02	(.047)	-.117680	(.059)
$D(t=-2)$	-.113087	(.043)	-.010978	(.047)	-.119923	(.063)
$D(t=-1)$	-.143205	(.049)	-.013837	(.045)	-.106905	(.057)
$D(t=1)$	.062343	(.035)	.320118	(.057)	.211972	(.054)
$D(t=2)$	-.083359	(.018)	.152757	(.045)	.075065	(.040)
$D(t=3)$	-.082730	(.019)	.160186	(.045)	.076344	(.040)
$D(t=4)$	-.089005	(.017)	.138687	(.048)	.068360	(.040)
$D(t=5)$	-.089833	(.019)	.130026	(.045)	.058372	(.040)
$D(t=6)$	-.089002	(.017)	.186340	(.047)	.067216	(.040)
$D(t=7)$	-.080631	(.019)	.167150	(.046)	.076444	(.040)
$D(t=8)$	-.087783	(.022)	.177978	(.048)	.067730	(.042)
$D(t=9)$	-.095731	(.019)	.180582	(.047)	.058724	(.041)
$D(t=10)$	-.088520	(.017)	.160426	(.047)	.066880	(.040)
$D(t=11)$	-.104385	(.022)	.177386	(.048)	.068132	(.041)
$D(t=12)$	-.080784	(.019)	.160520	(.046)	.079175	(.042)
$D(t=13)$	-.050157	(.025)	.191798	(.046)	.116396	(.045)
$D(t=14)$	-.082049	(.018)	.157069	(.044)	.078150	(.040)
$D(t=15)$	-.072454	(.020)	.177551	(.045)	.089669	(.042)
$D(t=16)$	-.088574	(.020)	.170822	(.046)	.069367	(.041)
$D(t=17)$	-.088985	(.019)	.167828	(.044)	.069148	(.041)
$D(t=18)$	-.070390	(.020)	.168319	(.044)	.092016	(.042)
$D(t=19)$	-.091617	(.025)	.157405	(.045)	.078355	(.043)

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Table 3.16: Coefficient estimates <continued>

	TR–JC (comb.)		TR–TR (incr.)		TR–JC (incr.)	
Variable	Coef.	(s.e.)	Coef.	(s.e.)	Coef.	(s.e.)
$D(t = 20)$	-.059711	(.023)	.167491	(.044)	.093649	(.041)
$D(t = 21)$	-.077627	(.018)	.156573	(.045)	.083126	(.040)
$D(t = 22)$	-.076532	(.018)	.178522	(.049)	.083936	(.040)
$D(t = 23)$	-.063903	(.021)	.152409	(.046)	.083639	(.040)
$D(t = 24)$	-.074740	(.018)	.164323	(.044)	.083868	(.040)
$D(t = 25)$	-.063750	(.022)	.189379	(.047)	.098172	(.043)
$D(t = 26)$	-.062509	(.022)	.166852	(.047)	.100813	(.043)
$D(t = 27)$	-.074511	(.018)	.163997	(.044)	.083615	(.040)
$D(t = 28)$	-.074088	(.018)	.165104	(.044)	.085096	(.040)
$D(t = 29)$	-.101618	(.026)	.166804	(.044)	.044702	(.048)
$D(t = 30)$	-.044225	(.029)	.168373	(.049)	.126984	(.052)
$D(t = 31)$	-.071983	(.018)	.154760	(.046)	.085634	(.040)
$D(t = 32)$	-.087333	(.034)	.168363	(.044)	.088862	(.054)
$D(t = 33)$	-.103977	(.028)	.166315	(.044)	.065690	(.048)
$D(t = 34)$	-.090151	(.023)	.180371	(.051)	.062871	(.045)
$D(t = 35)$	-.076411	(.032)	.167633	(.044)	.057984	(.048)
$D(t = 36)$	-.093931	(.025)	.188480	(.047)	.053768	(.048)
AD:τ	.272967E-02	(.187E-02)	.146001E-03	(.129E-02)	.282863E-02	(.163E-02)
AD:τ^2	-.199939E-04	(.168E-04)	-.179350E-05	(.893E-05)	-.174040E-04	(.110E-04)
PO:τ	.229227E-02	(.880E-03)	-.385941E-02	(.126E-02)	-.187046E-02	(.117E-02)
PO:τ^2	-.127798E-04	(.941E-05)	.266662E-04	(.925E-05)	.157306E-04	(.861E-05)

AD: Ashenfelter's Dip $\equiv D(ad(\tau) \leq t < 0)$
PO: After end of program $\equiv D(t > 0)$
comb.: combined effect of first and second program
incr.: incremental effect of second program
Heteroscedasticity robust standard errors in brackets.

Chapter 4

Do Job Creation Schemes Initiate Positive Dynamic Employment Effects? Evidence from the East German State of Sachsen–Anhalt

4.1 Introduction

Since the German unification in 1990 a significant amount of resources has been spent on job creation schemes (*Beschäftigungsschaffende Maßnahmen*) in East Germany. For example, in 1995 the budget for job creation schemes was the highest among all programs of Active Labor Market Policies (ALMP). The German Federal Government and the German Federal Employment Service (*Bundesanstalt für Arbeit, BA*) devoted 4.6 Billion Euro on job creation schemes, which was approximately 18 % of the budget for Active and Passive Labor Market Policies in East Germany (25.6 Billion Euro) paid for by these two institutions.[1] Although the money spent on job creation schemes was reduced over the years, it is still on quite a high level. In 2002 the expenditures for job creation schemes added up to 2.2 Billion Euro, which constituted a share of 8.9 % of the budget for Labor Market Policies.

Job creation schemes, as any other ALMP, seek to fight unemployment by raising the employment prospects of participants who are unemployed or threatened to become unemployed. In particular, the German regulations envisage that participation in job creation schemes should not only increase the rate for participants of leaving unemployment and but it should also stabilize the future employment situation of participants.

There are at least two potential channels of how job creation schemes might accomplish these goals.[2] By providing work experience job creation schemes can increase

[1] Compare Wolfinger/Brinkmann (1996) and Bundesanstalt für Arbeit (1995).

[2] For a analysis of the effects of ALMP in a theoretical framework see for example Calm-

the attachment of the participants to the labor market. This stronger bond might motivate the participants to intensify their search effort for a regular job and increase their ability to stay on a regular job.

The second channel consists of the potential ability of job creation scheme to increase the rate at which participants receive a job offer and to decrease the rate at which they are laid off. Naturally, job seekers become more attractive for employers if their human capital is raised and job creation schemes offer a number of possibilities to achieve this. First of all, participation in a job creation scheme, as any other program of ALMP, might stop the accelerated depreciation of human capital which would occur if the participants were unemployed. By providing work experience, job creation schemes also foster noncognitive skills such as motivation, ability to work regular hours, and communication skills - skills which importance has been emphasized recently (Heckman, 1999). Participants also acquire cognitive skills by learning–on–the–job and short training courses, which are sometimes offered in combination with a job creation scheme. Participation in a job creation scheme might also offer the possibility to participants to signal their positive work attitude to potential employers.

This paper evaluates whether job creation schemes indeed display these intended employment effects. Our time horizon starts in 1990, shortly before the reunification, and ends in 1999. The data stem from the Labor Market Monitor Sachsen–Anhalt (LMM–SA), which is a survey on the working age population of the East German state of Sachsen–Anhalt. This paper uses the last three waves (1997, 1998, 1999) of the survey which include retrospective calendars on the complete labor market history including participation in ALMP since the reunification. This calendar offers unique possibilities for the empirical analysis of program participation, which no other survey data, at least for Germany provides. Unfortunately, we can not rely on administrative data in order to evaluate job creation scheme as yet no such data set is publicly available. Also data from experiments can not be used, as experiments with job creation schemes have not yet been conducted in Germany.

In the presence of nonexperimental data, microeconomic evaluation of treatment effects faces methodological challenges. At a specific point in time, neither the situation of nontreatment is observable for the participants nor the situation of treatment is observable for the nonparticipants, i.e. the evaluation problem is a problem of missing data. As individuals differ with respect to observable and unobservable characteristics one can not use simple comparison groups to construct the counterfactuals. For example, in order to estimate the nontreatment outcome of the participants usually one can not use the outcome of the nonparticipants as the nonparticipants can differ from the participants, e.g. with respect to education and motivation. Also the situation of participants before the treatment is usually not a good approximation of the nontreatment outcome after treatment as the potential nontreatment outcome might change over time, e.g. due to changes of the overall economy. Consequently, it is necessary to make identifying assumption with respect

fors/Forslund/Hemström (2001). For a discussion of the theoretical effects of training course as ALMP, see for example Fitzenberger/Speckesser (2004).

to the adequate comparison level for the participants and nonparticipants in order to solve this problem of selection bias.

As Heckman/LaLonde/Smith (1999, p. 1868) conclude, there is no method of choice for estimating treatment effects. In fact, the choice of method should depend on the economics underlying the problem, the data availability and the evaluation question.

At present, the literature suggests four main solutions for the microeconomic evaluation problem.[3] They all concentrate on the individual employment effect of (potential) participants by which automatically substitution and displacement effects which might occur by way of market interactions are ignored. As a first estimator for treatment effect the difference–in–differences estimator should be mentioned which is often applied in the context of a policy reform. It contrasts the change of the outcome variable for the participants with the change of the outcome variable in the same time period for a group of nonparticipants. This approach is able to remove unobservable individual effects and common macro effect. However, the main underlying assumptions are that the selection bias is time invariant and that there are no changes in the composition of the groups.

A second approach consists of matching method which relies on the assumption that conditional on observables the (non)treatment outcome of the participants is equal to the (non)treatment outcome of the nonparticipants. Building on this assumption, the relevant (non)treatment outcome can be estimated nonparametrically.

Starting with Heckman (1979), the class of selection models has been developed. In contrast to matching methods, they allow for selection on unobservable. However, here often either very critical assumption concerning the functional form of the model and/or exclusion restriction have to be made. The exclusion restriction requires a variable that determines participation in the program but not the outcome of the program itself.

The fourth method is the timing–to–events–approach which has recently become popular. This method models jointly the unemployment duration until finding a job and the unemployment duration until program participation where selection on unobservable is taken into account by including heterogeneity terms which are allowed to be correlated. The timing of events conveys the information that helps to identify the program effects (Abbring/Van den Berg, 2002). The identifying assumption, however, lies in the independence between the observables and the unobservables.

This study uses a combination of the matching and the difference–in–differences approach. This so called conditional difference-in-differences (CDiD) method has been proposed by Heckman/Ichmura/Smith/Todd (1998). With this method one is able to control for selection on observables together with a time invariant, additive linear selection bias on nonobservables. In particular, we apply the in Bergemann/Fitzenberger/Speckesser (2004) developed extension of the CDiD estimator.

[3]For an overviews see for example Heckman/LaLonde/Smith (1999) or Blundell/Costa Dias (2000).

Their estimator emphasizes on the one hand the importance of aligning the estimator on a long–run preprogram difference, as anticipation effects regarding future participation or eligibility criteria (Ashenfelter's Dip), which require a certain elapsed duration of unemployment in order to participate, are likely to affect the results of any difference–in–differences estimator. On the other hand their CDiD estimator extends the traditional CDiD estimator to use transition rates between different employment states (Conditional Difference–in–Differences in Hazard Rates, CDiDHR). They show that the use of transition rates is more appropriate compared to the use of unconditional employment rates as often done in the literature. The use of unconditional employment rates can lead to misleading conclusions concerning the effectiveness of program participation. Furthermore, by estimating the effects on transition rates the following questions can be answered: Does the program help the (potential) participant to find regular employment? Does the program prolongs employment once the (potential) program participant found a regular job?

We do not use selection models as our data is not informative enough for credible exclusion restrictions. Furthermore, as we are not only interested in the effects of the program on the transition rate out of unemployment but also on the rate to stay in employment, we exclude the timing of events approach as a possible evaluation approach.

This paper also advances on the CDiDHR–estimator. The CDiDHR estimator was developed to estimate program effects for the actual participants (treatment–on–the–treated effect), as usually done in evaluation studies. With this parameter important but restricted conclusions on the effectiveness of programs can be drawn. Here we will extend the CDiDHR estimator in order to be able to estimate also population average treatment effects. By also assessing this second parameter we go beyond existing evaluation studies for Germany. We are able to make statements on the employment effects for participants in case the participation group would have been changed into the direction of the average active workforce at the time of the reunification. By doing so, we will also ignore potential substitution and displacement effects as usually done in the literature.

Estimation of treatment effects of ALMP different from the treatment–on–the–treated effect are rarely conducted. The main reasons might be that the timing of potential treatment of nonparticipants is difficult to set. At the same time it is important to take into account that programs can start at different times. For example, changes in institutional regulations of job creation schemes, which took place in the time period of 1990–1999 in East Germany, might influence the treatment effects significantly. We suggest a solution for distributing potential starting dates over the whole time range from 1990–1999 and for correcting that participants with different starting dates are used to estimate the predicted treatment outcome of nonparticipants.

There are only few studies on the microeconomic evaluation of job creation schemes in East Germany.[4] They all concentrate on the treatment–on–the–treated effect and

[4]For an overview of evaluation studies of ALMP in Germany, including East Germany see for example Fitzenberger/Speckesser (2000).

mainly estimate the effects on unconditional employment or unemployment rates. They differ in the method and the data set used. The conclusion which the studies draw concerning the effectiveness of job creation schemes are quite mixed.

Hübler (1997) and Kraus/Puhani/Steiner (2000) for example use the Labor Market Monitor for East Germany as a data set. This data set is restricted to the early 1990's. Hübler (1997) uses a whole range of parametric models as for example random effects probit and multinomial logit, but also constructs matched samples with a distance measure. The last approach is relatively close to the matching method. His results differ with the different methods used. He concludes, however, that participation in job creation schemes does not display positive effects. Steiner/Puhani/Kraus (2000) use a duration model for unemployment where also program participation is modeled, ignoring unobserved heterogeneity, contrary to the suggestions of the timing–of–events approach. Their results show negative effects of job creation schemes on the reemployment rate of participants.

Eichler and Lechner (2002) use more recent data. Their empirical analysis builds as the present study on data of the LMM–SA. However, they do not make use of the monthly retrospective calendar as it is done here. Instead, they use the panel structure of the data set by using the waves from 1992 to 1997. With the aid of this data they can only identify labor market states on a yearly basis. Due to the design of the LMM–SA which involves one complete redrawing of the sample, attrition and refreshment samples they have only a small number of participants in job creation schemes which they can observe before and after participation in a job creation scheme, which is necessary for their evaluation approach, as they also use a CDiD estimator. In their man analysis they align the CDiD on the labor market state observed directly before the participation. By way of this they can not avoid that a temporary deterioration in the employment situation affects their estimates. Their results show a significant decline in the unemployment probability of participants due to participation in a job creation scheme.

The empirical analysis of Bergemann/Fitzenberger/Schultz/Speckesser (2000) is closest to this study. They use the retrospective calendar of the LMM-SA of the wave 1997 and 1998 for the time 1990-1998 and evaluate the employment effects of job creation schemes with a CDiD estimator which is aligned on the long–run preprogramm difference. The outcome variable is in contrast to this study the unconditional employment probability. They find significant negative effects for participants in a first job creation scheme in the first 1 1/2 years after the participation. Two years after the participation the effects become significantly positive, indicating that there are positive employment dynamics at work.

Hujer/Caliendo/Thomsen (2003) is the only study for Germany on job creation schemes which is conducted with the aid of an administrative data set to which they have exclusively access. They use a pure matching framework and evaluate the treatment–on–the–treated effects of participation in a job creation scheme which started in February 2000 on two differently defined nonemployment probabilities. Their general finding is that participation in a job creation scheme does not significantly lower the nonemployment probability within a two years period after the starting date. However, over time the effects evolve from significantly negative to close to zero.

As mentioned, estimation of treatment effects differently from the treatment–on–the–treated effects have until now been rarely conducted. One exception is Brand/Habaly (2003) for the US. They estimate the population average treatment effects for attending elite colleges for all college students by applying matching methods.

The rest of the paper is organized as follows: Section 4.2 lays out the institutional set up and participation figures of job creation schemes in East Germany. Section 4.3 discusses the evaluation approach. Section 4.4 contains the implementation details including a description of the data and section 4.5 presents the results. Section 4.6 concludes. The appendix contains detailed results.

4.2 Job Creation Schemes in East Germany

4.2.1 Background and Aims

With the German social and economic union, the West German Labor Promotion Act (*Arbeitsförderungsgesetz*, AFG) was transferred to East Germany. To take into account the special situation of the East German economy a number of additional regulations were added. These regulations essentially meant less restrictive rules for participation in programs of ALMP shortly after reunification. Moreover, some additional instruments like special early retirement schemes were introduced for a limited number of years.[5] Until the replacement of the AFG by the new Social Law Book III (SGB III) in 1998 a number of changes in the regulations took place. We will describe regulations and some implementation details of the three main types of employment subsidies for temporary jobs for the time the AFG was in force.[6] This covers the main part of our observation period. The first two consist of traditional job creation schemes (*Arbeitsbeschaffungsmaßnahmen*, ABM, see §§ 91 – 96 AFG) and ordinary productive wage subsidies (*trad. produktive Lohnkostenzuschüsse*, LKZ, see §§ 249h AFG),[7] which were introduced in January 1993. According to the common use of the word, both programs can be called job creation scheme, as both programs intend to create additional temporary jobs mainly in the public or non–profit sector for the time of the subsidy (*Beschäftigungsschaffende Maßnahmen*).[8] They differ, however, with respect to the level of subsidy and the activity areas. The subsidy of the traditional job creation scheme covered 30-100% of the wage costs and was only granted for jobs which are beneficial for the soci-

[5]For an overview on ALMP in East Germany shortly after the reunification see Ehlers (1996).

[6]Note, however, that only few additional changes concerning employment subsidies took place with the introduction of SGB III.

[7]Since 1998, these subsidies are called structural adjustment measures (*Strukturanpassungsmaßnahmen*, SAM).

[8]See Martin and Grubb (2001) for an classification and overview on the ALMP in the OECD countries.

ety and would otherwise be not performed. No further constrains were imposed on the activity area. Ordinary productive wage subsidies were more restrictive. These subsidies amounted to the average allowance which the unemployment offices paid to the unemployed and were typically smaller than the subsidies for traditional job creation schemes. Furthermore, the activities of ordinary productive wage subsidies which should also not occur without the subsidies were restricted to specific areas, such as environmental redevelopment.

The third subsidy type which we will consider is the program of productive wage subsidies for private firms (*Produktive Lohnkostenzuschüsse Ost für Wirtschaftsunternehmen* , LKZ OfW, see §§ 249h). The program was introduced in 1997 exclusively in East Germany and offered generous employment subsidies to private firms in order to employ unemployed individuals for the time of the subsidy. After the end of the subsidy, there was no requirement for a continuation of the employment relationship. Thus, although the program subsidized private sector jobs, it resembled relatively closely a job creation scheme.

There was a number of other small scale employment subsidies programs in East Germany mainly aiming at the permanent integration of the unemployed into the firm, such as integration subsidies or recruitment subsidies for business start ups (see Fitzenberger/Speckesser (2000)). As we can rarely identify these program in our data, we will not consider them here (see section 4.4.1) .

The government pursued several aims by supporting employment subsidies for temporary jobs at a large scale in East Germany (see e.g. Wolfinger/Brinkmann, 1996). One aim was simply to provide jobs and income during the time of the employment subsidy for unemployed individuals and those who were at risk to become unemployed. In this way the social consequences of the transformation process for individuals could be eased and the official unemployment rate could be lowered. Another aim was to improve the infrastructure of East Germany. Typical areas of the activities of job creation schemes were environmental redevelopment, landscape building and social services. Especially in the time of 1993–1996, this aim was emphasized by introducing in 1993 the ordinary productive wage subsidies on a large scale, where the activity areas consisted primarily of these three areas.

The third aim which gained more and more importance over the years is the traditional aim of ALMP. The employment subsidies should help the participants to find regular jobs. In addition, the *AFG* emphasized, that especially those employment subsidies scheme should be supported, which help to find or create *stable* employment relationships.

Naturally, the first two aims of employment subsidies, alleviation of the social consequences of the transformation process and the improvement of the infrastructure, were already reached by purely implementing job creation schemes in specific activity areas - although, it is an open question, whether they were reached efficiently. However, whether the third aim was achieved at all is far from settled. This paper focuses on this important aspect and evaluates whether employment subsidies helped to find and retain regular employment. To achieve this, we will evaluate the described three program together, as we can only distinguish between them for a subset of cases (see section 4.4.1). The basic set up and aims of these schemes are however quite similar, which justifies a joint evaluation. Our evaluation period will consist of the time between 1990 to 1999 such that the main emphasis will lie on job creation schemes, and productive wage subsidies for private firms will only be considered at the margin.

4.2.2 Institutional Provisions 1990–1997

The implementation of the two job creation schemes involved the following steps. A project organizing institution, which could be a public authority or a charity organization, had to create one or more jobs within a project which needed to be beneficial for the community and had to be additional in the sense that it would not be carried out without the subsidy. In East Germany so called "Societies for Employment Promotion and Structural Development" (*ABS-Gesellschaften*) often acted as large scale organizers of job creation schemes. Formally, after approval of a project, the local labor office alone should choose participants (i.e. employees) for this project. However, evidence exists that in the early 90's large scale ABS-Societies had significant influence on the selection of participants. The ABS-Societies preferred young, educated men (Brinkmann/Vökel, 1992). The subsidy given to the employer covered part of (or fully) the wage costs, but also part of the material and capital costs could be taken over by the labor offices. Costs which were not covered by the labor offices, although connected to the project, needed to be covered by the project organizing institution. Often local governments acted as co–financiers. Finally, participants and the organizer of the job creation scheme (or delegated firms by the organizing institution) signed a fixed term work contract, which induced regular social security contributions. As a consequence the participant renewed or prolonged his or her eligibility period for unemployment benefits. During the job creation program, the local labor office and the participant should continue their search for a regular job. The job creation program ended in case a regular job or a suitable training program was found.

The implementation details depended on the type of subsidy program and the time it took place. Formally, a participant in a traditional job creation scheme (ABM) had to be unemployed, with 6 months of unemployment in the last 12 months and s/he needed to be eligible for unemployment benefits (*Arbeitslosengeld*) or unemployment assistance (*Arbeitslosenhilfe*), since 1994 also social assistance (*Sozialhilfe*). The criteria for eligibility of ordinary productive wage subsidies (LKZ) were less strict. Next to be eligible to one of the three allowances, a participant needed to have been unemployed with 3 months of unemployment in the last 12 months, or needed to have had finished a traditional job creation scheme, or enter from short time work zero.[9]

The length of traditional job creation schemes (ABM) was typically 12 months, in

[9]Short time work zero was a benefit based on the last wage which was only given until 1992 in East Germany to workers for a short period of time, whose firm stopped running.

some cases of up to 24 months, with possible extensions of up to 36 months, if a permanent job was offered subsequently by the organizer of the job creation scheme. The ordinary productive wage subsidy (LKZ) could be granted even longer, up to 48 months if a permanent job was offered.

For traditional job creation schemes (ABM) the organizers received 30-100 % of the wages costs, whereas for the ordinary productive wage subsidies (LKZ) the local labor offices paid a fixed lump sum, which is equal to the average unemployment allowance the labor office would have needed to pay in case the individual had been unemployed.

4.2.3 Changes in Implementation Rules

The local labor offices could depart from the above mentioned participation criteria. Especially shortly after the reunification, it was common practice after plants closure to collectively put the work force of the plant into a so called *Mega-ABM's* and let the workers for example close down the obsolete plant, or clean–up the environmental damage produced by the plant.

This practice and the influence of the large scale ABS-Societies on the selection of participants was the main reason for the deviations from the original target group for job creation schemes. For traditional job creation schemes the group consisted of disabled individuals, long–term unemployed, unemployed over 50 years, individuals below 25 years and women. For the ordinary productive wage subsidy it consisted only of older unemployed individuals. It should be mentioned that for older participants a small scale program similar to traditional job creation schemes in order to bridge the time until retirement existed (*Maßnahmen zur Arbeitsbeschaffung für ältere Arbeitslose* §§97-99 AFG). In order to avoid to evaluate this program, we will exclude older people from our analysis (see section 4.4.1). Additional subsidies were given by the East German state governments to the organizing institutions in case a participant was either youth unemployed, single parent, long–term unemployed or disabled. In the mid 90's, the local labor offices started to focus more on the original target groups of the job creation schemes.

Participation in a job creation scheme was often financially attractive for unemployed individuals. Until 1993, the wage paid in job creation schemes was equal to the wage set by collective wage agreements between the unions and employers organizations for similar but unsubsidized work (*Tariflohn*). In East Germany, however, employers often paid wages below the union contract wage. Thus, the wage paid in a job creation scheme was not only higher than the benefit payment, which participants would receive without participating, but also it could be higher than the wage in an unsubsidized job, providing additional incentive to participate and stay in a job creation scheme. In 1994, the new regulation was that either the wage had to amount to at most 90% of the union contract wage or the working time to reduced to at most 80% of the union set working hours.

The most problematic part for the implementation of the regulations of job creation schemes was the requirement that the activities of the job would otherwise not have occurred. For example, explorative studies find displacement effects in the gardening and landscaping sector (Schneider et. al., 2000). In this sector ABS-Societies and private firms competed with each other. As ABS-Societies mainly worked with subsidized workers and private firms rarely had access to these subsidies, ABS-Societies could often offer lower prices .

The prevention of some of the macroeconomic displacement effects was one reason to introduce productive wage subsidies for private firms (*Lohnkostenzuschüsse Ost für Wirtschaftsunternehmen*, LKZ OfW) in April 1997. With this program private firms had access to subsidized work. The subsidy was designed in such a way that it should be given for regular jobs. A firm was only eligible for this subsidy if it did not reduce its workforce during the last half year before the start of the subsidy and during the time of the subsidy. The number of employees who could be subsidized depended on the size of the firm, the maximum amounts to 10 employees. Each employee could be subsidized for at most 12 months. The only requirements for an employee to be subsidized were that s/he was eligible for unemployment benefits or assistance and be unemployed or threatened to become unemployed. At the time the productive wage subsidies for private firms were introduced no specific target group was spelled out. This changed in the year 1999. Since then, the subsidies targeted at similar groups as traditional job creation schemes. Also in 1999, the level of productive wage subsidies for private firms was reduced to 70% of the average unemployment benefit.

Naturally, when looking at employment subsidies, especially at subsidies such as the productive wage subsidy for private firms, the question of direct substitution effects arises. And indeed, first explorative studies find that part of the jobs would have been filled also without the subsidy (see Jaenichen 1999; Schneider and Schultz, 2001). However, this strand of research is still in its fledgling stage due to insufficient data quality such that reliable estimates of the substitution effects could not yet been calculated. Here we will not estimated the macroeconomic substitution effects and also not the displacement effects but the individual employment effects of job creation schemes, including productive wage subsidies for private firms, which were introduced at the end of our observation period.

4.2.4 Participation and Costs

Entries in traditional job creation schemes already peaked in 1991 (see figure 4.1). In this year around 422,000 individuals entered in traditional job creation schemes in East Germany. This was mainly realized by the *Mega-ABM's*, where whole firms were collectively put into job creation schemes. From 1992 until 1997 entries in the two different job creation schemes together fluctuated around 290,000. After the introduction of the productive wage subsidies for private firms the maximum of entries in these three wage subsidy programs was reached in 1998 with over 500,000 entries. Alone 200,000 individuals were granted the new kind of subsidy in 1998. After defining target groups and reducing the subsidy level, entries into the new

Figure 4.1: Entries into Job Creation Schemes*

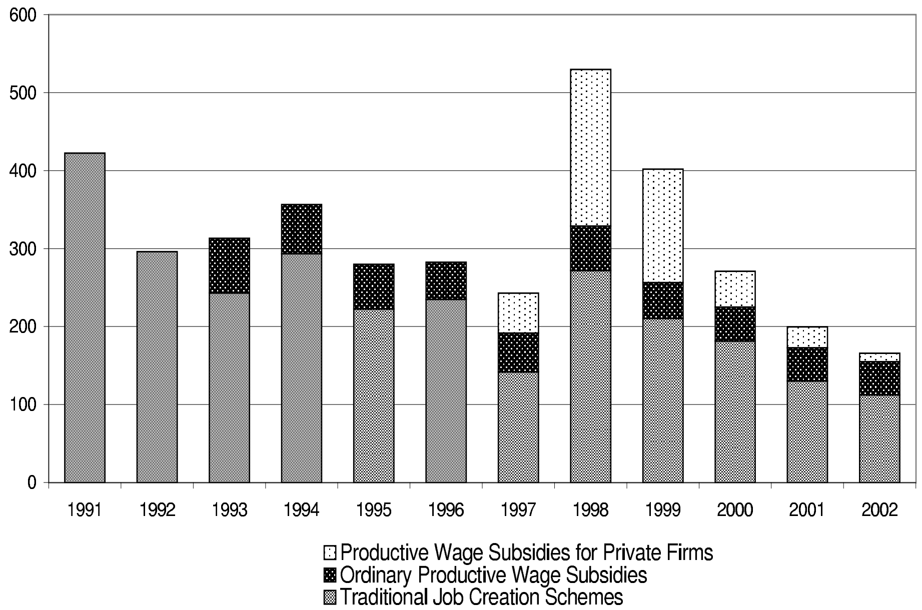
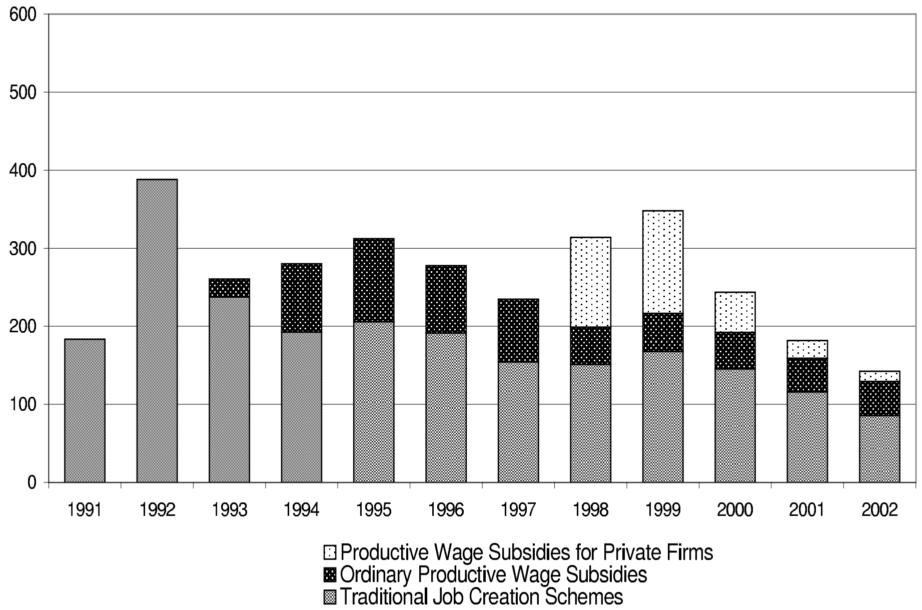


Figure 4.2: Participation Stocks for Job Creation Schemes, Annual Average*



* Source: Bundesanstalt für Arbeit (1995, 2001b, 2003b), own calculations.

scheme declined dramatically. At the same time the entries into the two old job creations schemes were also reduced. In 2002 entries were with around 170,000 on the lowest level since reunification.

The yearly stocks follow with a time lag a similar but not completely equal development as entries (see figure 4.2). The maximum stock was reached in 1992, with on

Figure 4.3: Participation Stock and Expenditure for Traditional Job Creation Schemes*

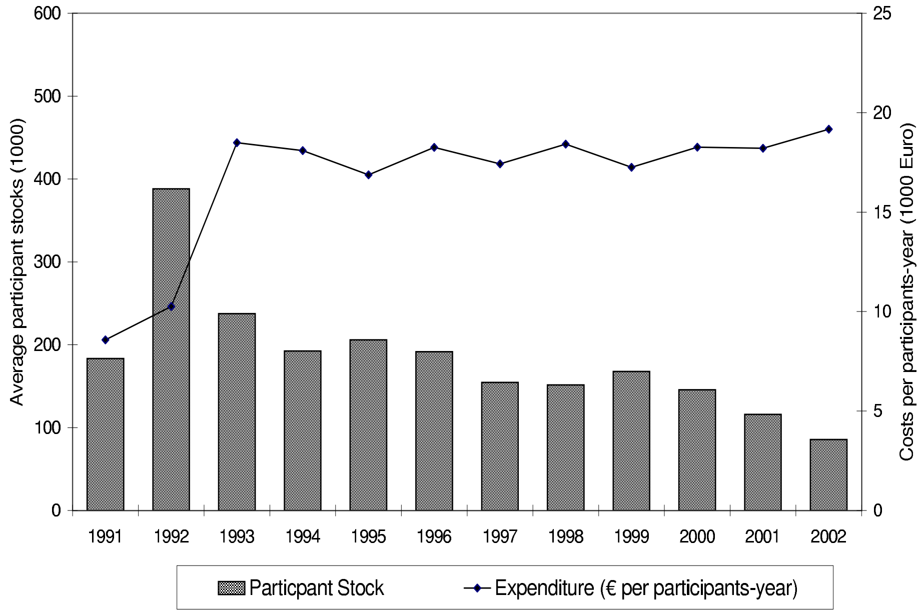
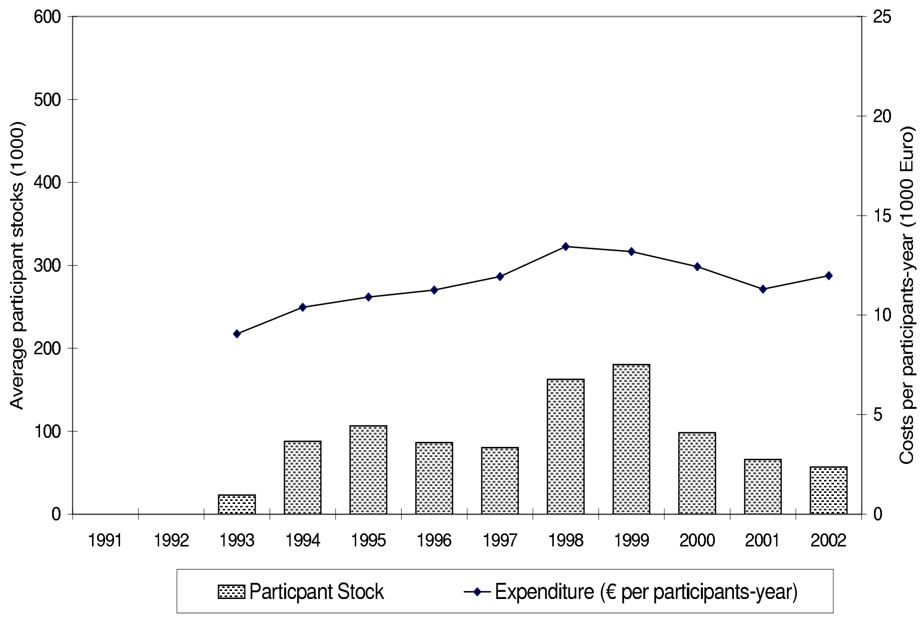


Figure 4.4: Participation Stocks and Expenditure for Productive Wage Subsidies*



* For 1990 no figures available. Source: Bundesanstalt für Arbeit (1997, 2001b, 2003b) and Wolfinger/Brinkmann (1996), own calculations.

average nearly 400,000 individuals participating in a traditional job creation scheme. Between 1993 and 1997 participation fluctuated around 250,000 individuals. As the maximum length of the productive wage subsidy for private firms is shorter than for the other two schemes, the increase in entries in the new subsidy scheme does not reciprocate so strongly in the average participation. In 1999 participation increased to around 350,000 and fell again thereafter.

The costs per participant–year of traditional job creation schemes for the labor office (without the costs for co–financiers) increased until 1993 to around 20,000 Euro and staid approximately constant thereafter (see figure 4.3). The average costs of the two productive wage subsidies are clearly lower than the costs for ABM (see figure 4.4).[10] In 1993 costs per participant–year amounted to close to 9,000 Euro. Until 1998 the costs per participant–year were increasing to over 13,000 Euro. Since then they ares slightly declining with an exception in 2002.

4.3 Evaluation Approach

4.3.1 Econometric Model

When evaluating the treatment effects of job creation schemes we want to take into consideration that employment is a dynamic process which can exhibit strong state dependence. For an individual, holding everything else constant, the probability to stay employed is usually higher than the reemployment probability. The evaluation approach builds upon the following econometric model which was developed in Section 3.3.2, which takes state dependence into account by allowing employment of individual i (Y_{it}) to be determined separately depending on the employment state in the previous period. Thus, the model allows employment to be determined by a first order Markov Process where the current employment depends on the employment state in the previous period but not on periods further in the past. In the rest of the paper we will consider two different employment situations: employment $Y_{it} = 1$ and nonemployment $Y_{it} = 0$. Therefore, the model consists of two separate employment equations one for each possible employment states in the previous period, where e denotes employment in the previous period and n denotes nonemployment in the previous period.

$$(4.1) Y_{it} = \begin{cases} a^e(X_i, t) + \delta^e_{i,t,\tau} D_{i,t}(\tau) + c^e_i + u^e_{i,t} & \text{for} \quad Y_{i,t-1} = 1 \quad \text{(employed before)} \\ a^n(X_i, t) + \delta^n_{i,t,\tau} D_{i,t}(\tau) + c^n_i + u^n_{i,t} & \qquad Y_{i,t-1} = 0 \quad \text{(not empl. before)} \end{cases}$$

[10]Here the part of the costs of productive wage subsidies is included which are covered by transfer payments from the Federal Government to the Labor Offices.

The model uses the following notation:

$a^e(X_i, t), a^n(X_i, t)$	systematic part of state dependent employment probabilities as a flexible function of observed time invariant characteristics X_i and month t
τ	time of treatment
$D_{i,t}(\tau)$	dummy variable for treatment in period τ
$\delta^e_{i,t,\tau}, \delta^n_{i,t,\tau}$	individual specific, state dependent effects of treatment on the employment probabilities
c^e_i, c^n_i	state dependent, permanent individual specific effects
$u^e_{i,t}, u^n_{i,t}$	idiosyncratic, period specific effects.

For expositional reason, treatment in this model only takes place in one period τ. The model allows the individual treatment effects $\delta^k_{i,t,\tau}$ $(k = e, n)$ to be correlated with a number of other model determinants. They are allowed to depend upon observed characteristics X_i and the individual specific effects c^k_i. Furthermore, $\delta^k_{i,t,\tau}$ is allowed to vary by i, t, and τ if we condition upon X_i and c^k_i.

The idiosyncratic error terms $u^e_{i,t}, u^n_{i,t}$, however, are assumed to be conditionally mean independent of treatment in the past and of the covariates X_i, i.e. $E(u^e_{i,t}|D = 1, X_i) = E(u^e_{i,t}|D = 0, X_i) = E(u^n_{i,t}|D = 1, X_i) = E(u^n_{i,t}|D = 0, X_i) = 0$ for $t \geq \tau$. It is also assumed that treatment affects the outcome only after treatment, such that $\delta^k_{i,t,\tau} = 0$ for $t < \tau$ and $k = e, n$, although it is widely found that shortly before the treatment the employment situation of the participants deteriorates for example due to anticipation effects or participation rules. This phenomenon is called Ashenfelter's Dip following Ashenfelter (1978), who discovered this relationship first in connection with earnings. We interpret Ashenfelter's Dip as temporary phenomenon and not as treatment effects. Therefore, we allow for a correlation of $D_{i,t}(\tau)$ with $u^k_{i,\tau-s}$ $(k = e, n)$ with $s = 1, \ldots, ad$ and ad is the begin of Ashenfelter's Dip. Before the begin of Ashenfelter's Dip we also assume that $u^e_{i,t}, u^n_{i,t}$ are conditionally mean independent of the treatment. Furthermore, the temporary deterioration of the employment situation of participants is not connected to the outcome variable after treatment took place i.e. $u^k_{i,\tau-s}$ $(k = e, n)$ are not correlated with $u^k_{i,t}$ with $s \geq 1$ and $t \geq \tau$. In our implementation we set the begin of Ashenfelter's Dip (ad) according to institutional features of the program (see section 4.4.5).

Naturally, the econometric model should take into account the basic evaluation problem, i.e. is that participants and nonparticipants are different with respect to observables and unobservables. Expressed in this model, treatment $D_{i,t}(\tau)$ is influenced by the observed covariates (X_i, t) and by the individual specific effects c^e_i, c^n_i. Furthermore, we also want to allow for $D_{i,t}(\tau)$ to be influenced by the size of the treatment effects $\delta^e_{i,t,\tau}, \delta^n_{i,t,\tau}$ and that X_i is correlated with c^k_i for $k = e, n$. Finally, we do not want to put strong functional form restrictions on the specification of $a^e(X_i, t), a^n(X_i, t)$.

This econometric model can be estimated by the conditional difference–in–differences in hazard rates estimator (CDiDHR) as developed in section 3.3.3 and will be laid out again in the following sections. This CDiDHR advances on the previous CDiD estimators by taking into account the state dependence of the employment process by conditioning on the employment status in the previous period.[11] We do not want to take employment states of periods further in the past into account (i.e the duration dependence) in contrast to the timing–of–events approach (Abbring/Van den Berg, 2003). In order to evaluate one treatment per individual, as done here, and taking account of duration dependence in the employment process, the timing–of–events approach needs to assume that X_i and c_i^k with $k = e, n$ are independent. We do not want to impose such a restriction here.

4.3.2 Potential–Outcome–Approach to Causality

The empirical analysis is based upon the potential–outcome–approach to causality (Roy, 1951; Rubin, 1974). We focus on two treatment effects which are interesting from a policy point of view. We not only estimate the widely used average treatment–on–the–treated effect (TT), but also the population average treatment effect (ATE). Although extensively treated in the theoretical literature (see e.g. Imbens (2003)), ATE is rarely estimated.

Commonly treatment effects are defined without referring to the employment state in the previous period. In this case TT is given by

$$E(Y^1 - Y^0 | D = 1) \; , \tag{4.2}$$

and ATE is given by

$$E(Y^1) - E(Y^0) \tag{4.3}$$

where Y^1 denotes potential treatment outcome, Y^0 potential nontreatment outcome for some time after treatment $D = 1$. With the aid of these definitions of the treatment parameters, it becomes obvious that an evaluation method needs to solve the problem of estimating $E(Y^0|D = 1)$ and $E(Y^1|D = 0)$ as the nontreatment outcome for the participants and the treatment outcome for the nonparticipants can not be observed.

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[11] The CDiD approach has recently become popular, compare e.g. Heckman/ Ichimura/Smith/Todd (1998); Blundell/Costa-Dias/Meghir/Van Reenen (2003), Bergemann/ Fitzenberger/Schultz/Speckesser (2000).

However, we want to estimate each treatment parameter with respect to each possible employment state in the previous period. In this case TT^k, where index k indicates the employment state in the previous period, with $k = e, n$, is given by

$$E(Y^{1,k} - Y^{0,k}|D = 1) \ , \tag{4.4}$$

and ATE^k is given by

$$E(Y^{1,k}) - E(Y^{0,k}) \tag{4.5}$$

When estimating the treatment parameters with respect to the employment states in the previous period in the presence of an employment process which follows a state dependent process as formulated in equation 4.1, then the evaluation method has not only to solve the above mentioned evaluation problem that the nontreatment outcome for the participants and the treatment outcome for the nonparticipants can not be observed. It also has to address that the same person can not be observed in both employment states in the previous period. Individuals sort in specific employment groups according to X_i and c_i^k. But X_i and c_i^k also influence the treatment effect. Thus, when only considering the individuals in observed employment states we would get biased estimates for the treatment effects TT^k and ATE^k.

We apply the CDiDHR approach to estimate the above defined parameters. The CDiD method combines matching to control for selection on observables with the difference–in–differences approach to control for time invariant, additive separable selection effects on unobservables. In our econometric model (see equation 4.1) selection on observables is captured by $a^e(X_i, t), a^n(X_i, t)$ and the time invariant selection effect on unobservables is represented by c_i^e, c_i^n.

4.3.3 Matching under the Conditional Mean Independence Assumption

For clarity reasons, let us first consider the case that selection only occurs due to observables. In the next section we will widen the scope and discuss how we will take account of selection on unobservables.

If selection purely occurs due to observables, one can estimate TT^k and ATE^k by solely applying the matching method. Formally, the following Conditional Mean Independence Assumptions (CIA) has to be imposed.[12]

$$E(Y^{\omega,k}|D = 1, X) = E(Y^{\omega,k}|D = 0, X) \ , \quad \text{with } \ \omega = 0, 1 \ \text{ and } \ k = e, n \tag{4.6}$$

[12]Usually, the CIA is formulated without reference to the employment state in the previous period (see e.g. Imbens (2003)). This is due to the fact that usually also the treatment parameters do not refer to the employment state in the previous period.

It implies that the nontreatment (respective treatment) outcome of the participants and of the nonparticipants with respect to the same employment state in the previous period are comparable in expectation when conditioning on X.

Under the CIA the expected potential nontreatment outcome for participants with the observable characteristics X can be estimated by averaging over the nontreatment outcome for the nonparticipants with the same X and with the same employment state in the previous period. Similar, the expected potential treatment outcome for the nonparticipants with characteristics X can be estimated by averaging over the treatment outcome for the participants with the same X and the same previous employment state. The different matching estimators differ with respect to how the averaging is conducted.

We make use of the result of Rosenbaum and Rubin (1983) which states that CIA is also valid when conditioning on the treatment probability $P(X)$ as a function of observable characteristics X given that $0 < P(X) < 1$:

$$E(Y^{\omega,k}|D = 1, P(X)) = E(Y^{\omega,k}|D = 0, P(X)) \; , \tag{4.7}$$

again with $\omega = 0, 1$ and $k = e, n$.

In order to construct estimators for the treatment effects, it is necessary to define who is a participant and who is a nonparticipant. Are for example participants only those who are treated at a specific point in time or those who received treatment at some point during a predefined time period? Due to the small sample size we choose to define participants as those who participated during the observation period where the treatment itself consist of the first participation of the participant in the program considered, and nonparticipants as those who never participated during the observation period.

Although defining treatment with respect to a time period we will take into consideration that treatment effects can depend on the time of treatment. Thus, we take account that job creation schemes shortly after reunification might have other effects on the employment process than job creation schemes later in the 90's. We expect for example that changes in the implementation of job creation schemes (e.g. set–up problems shortly after reunification), changes in the regulations, introduction of new program types, and changes in the economic environment can influence the treatment parameters. Furthermore we will investigate whether the treatment effects are temporary, long–lasting or wether they need time to evolve.

To capture that treatment effects can vary with respect to these different aspects of time, we need to be clear about the time concepts used. We capture with τ the calendar time of treatment (here for expositional reasons treatment occurs only for one time period). $T1$ denotes in calendar time points in time after treatment and $t1$ denotes time elapsed since treatment. Consequently, the following relationship is valid: $T1 = t1 + \tau$. When estimating the treatment we take account of the variation with respect to different treatment times and with respect to elapsed time since treatment, i.e. we want to estimate $\mathrm{TT}^k(\tau, t1)$ and $\mathrm{ATE}^k(\tau, t1)$.

Average Treatment–on–the–Treated Effect

If the CIA holds we can formulate the matching estimator of $\mathrm{TT}^k(\tau, t1)$ in the following way:

$$(4.8) \qquad \frac{1}{N^k_{1,\tau}} \sum_{i \in \mathcal{N}^k_{1,\tau}} g_i \left[Y_{i,T1} - \sum_{j \in \mathcal{N}^k_0} w_{N^k_0}(i,j)\, Y_{j,T1} \right]$$

where from now on i always refers to treated individuals and j always to nontreated individuals, contrary for the econometric model in section 4.3.1 where i denoted a random individual. $N^k_{1,\tau}$ is the number of individuals in the set of treated individuals at time τ ($\mathcal{N}^k_{1,\tau}$) for whom $Y^k_{i,T1}$ can be observed, i.e. for whom $Y_{i,T1-1} = l$ where $l = 0$ if $k = n$ and $l = 1$ if $k = e$. N^l_0 is the number of individuals in the set of never treated individuals ($\mathcal{N}^k_0$) for whom $Y^k_{j,T1}$ can be observed i.e. for whom $Y_{j,T1-1} = l$ where $l = 0$ if $k = n$ and $l = 1$ if $k = e$. For expositional reasons we suppressed the subscript $T1$ for $\mathcal{N}$ and N although they depend on $T1$. g_i is a set of weights with respect to the observable characteristic X to account for the fact that $\mathcal{N}^l_{1,\tau}$ does not include the entire treatment sample (for details compare section 4.3.4). For each participant i in his/her previous employment state k, the weights $w^k_{N_0}(i,j)$ sum up to one over j ($\sum_j w_{N_0}(i,j) = 1$). Note that when estimating $\mathrm{TT}^k(\tau, t1)$ the outcomes of participants $Y_{i,T1}$ are always contrasted with outcomes of nonparticipants $Y_{j,T1}$ with respect to the same calendar time $T1$.

Equation (4.8) gives an estimate for TT for treatment in τ and elapsed time $t1$ since treatment with $t1 = T1 - \tau$ with respect to the reemployment probability if $k = n$ and with respect to the probability to remain employed if $k = e$.

The different matching estimator differ with respect to how the weights $w^k_{N_0}$ are constructed (Heckman/Ichimura/Smith/Todd, 1998). Matching estimators give a higher weight to individuals which are closer in their characteristics to the individual whose expected potential outcome should be estimated. The most intuitive matching estimator is the nearest neighbor matching, where only the outcome of the closest counterpart in terms of X, $p(X)$ respectively, is used. Here we apply a kernel matching approach, which uses a nonparametric local linear kernel regression on the estimated propensity score to estimate the unobservable outcomes.[13]

The basic idea of this kernel matching method should be presented exemplarily for the case of estimating TT (see equation 4.8). Concerning TT the challenging task is to estimate $\sum_{j \in \mathcal{N}^k_0} w_{N^k_0}(i,j)\, Y_{j,T1}$. The local linear regression estimator obtains a solution for this problem with the aid of the following minimization problem:

$$(4.9) \quad (m^k(i,T1), \beta^k_i) = \underset{\{m,\beta\}}{argmin} \sum_{j \in \mathcal{N}^k_0} \{Y_{j,T1} - m - \beta'(p_j - p_i)\}^2 K\left(\frac{p_j - p_i}{h}\right)$$

[13] We do not bootstrap our result as for the estimation of the ATE it would be computationally too demanding.

where p denotes the estimated propensity score, $K((p_j - p_i)/h)$ is a kernel that downweights distant observations from p_i and h is the bandwidth parameter. The minimization problem consists in minimizing locally the weighted sum of squares with respect to the location parameter m and the slope parameters β. The local linear estimator is equivalent to a weighted least squares regression including a constant and the deviation $(p_j - p_i)$. The expected nontreatment outcome for participant i is then estimated by the value of the estimated intercept $m(p_i)$.

Formally, $m(i, T1)$ is a weighted average of $Y_{j,T1}$, for all $j \in \mathcal{N}_0^k$, where the weights depend both on the kernel weights and the differences $(p_j - p_i)$ i.e. the resulting estimator $m^k(i, T1)$ represents then $\sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j)\, Y_{j,T1}$.

In contrast to nearest neighbor matching which would only use the nearest neighbor among those who have the same employment state in the previous month, kernel matching uses the outcomes of all nonparticipant (with the same employment state in the previous months) to estimate the expected nontreatment outcome of the participants. The Nadaraya–Watson kernel regression estimator is another common matching estimator (see e.g. Pagan/Ullah 1999). Compared to the Nadaraya–Watson our estimator differs by including the differences $(p_j - p_i)$ into the minimization problem. In this way it takes stronger the mismatch into consideration which occurs between the estimated propensity score between participant and nonparticipants. The local linear kernel regression estimator corrects for this mismatch locally.

For the local linear kernel regression, we use the Gaussian kernel and a bandwidth choice according to Silverman's rule of thumb (ROT). In order to minimize the mean squared error of the kernel estimate, Silverman (1986, 47f) recommends the following bandwidth in case there is only one explanatory variable.

$$h_{ROT} = 0.9 \cdot A \cdot n^{-1/5} \tag{4.10}$$

where $A = min(s, iqr/1.34)$, in which s is the standard deviation and iqr the interquartile range of the explanatory variable, here the estimated propensity score p, in the sample (n is the sample size). For the prediction of $Y^0_{i,T1}$ by $\sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j)\, Y_{j,T1}$ we base our choice of h_{ROT} on the sample of nonparticipants (also as is the kernel regression).

Population Average Treatment Effect

As made already explicit in formulating the matching estimator for TT, we want to allow for the possibility that the treatment effects depend on the time the treatment took place. Therefore, in order to estimate the population average treatment effects we first have to set potential treatment times τ in the time path of the nonparticipants. We take random draws from the empirical distribution of treatment times of the participants conditional on time invariant characteristics (for more details compare section 4.4.4).

The matching estimator of $\mathrm{ATE}^k(\tau, t1)$ under the CIA is given by:

$$
\begin{aligned}
(4.11) \qquad & \frac{1}{N_\tau^k} \sum_{i \in \mathcal{N}_{1,\tau}^k} h_i \left[Y_{i,T1} - \sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j)\, Y_{j,T1} \right] \\
+ \; & \frac{1}{N_\tau^k} \sum_{j \in \mathcal{N}_{0,\tau}^k} h_j \left[\sum_{i \in \mathcal{N}_1^k} w_{N_1^k}(i,j) Y_{i,t1} - (Y_{j,t1}) \right]
\end{aligned}
$$

where N_τ^k is the number of individuals $m = i, j$ with (partly imputed) treatment time τ for whom $Y_{m,t1}^k$ can be observed, i.e. for whom $Y_{m,t1-1} = l$ where $l = 0$ if $k = n$, $l = 1$ if $k = e$ and $T1 = t1 + \tau$. For each participant i, the weights $w_{N_0}^k(i,j)$ sum up to one over j ($\sum_j w_{N_0}^k(i,j) = 1$) and similarly for each nonparticipant j over i ($\sum_i w_{N_1}^k(i,j) = 1$).

The first part of equation 4.11 resembles TT. Here we also calculate for each participant with treatment in τ and previous employment state k the difference between his or her outcome variable at time $T1$ ($Y_{i,T1}$) and a weighted average of the nontreatment outcome at time $T1$ ($Y_{j,T1}$) of all nonparticipants with the same previous employment state. Now, however, the weights h_i do not correct for the fact that $\mathcal{N}_{1,\tau}^k$ does not include the entire sample of participants with treatment in τ, as done with g_i in the estimator for TT, but that participants in $\mathcal{N}_{1,\tau}^k$ are not representative for the whole population.

In the second part of equation 4.11 we calculate for each nonparticipant with imputed treatment in τ and employment state k in the previous period the difference between a weighted average of all the treatment outcome of participants at time $t1$ ($Y_{i,t1}$) with the same employment state in the previous period and the his or her nontreatment outcome at time t1 ($Y_{j,t1}$). We also attach weights h_j to this difference as nonparticipant being part of $\mathcal{N}_{0,\tau}^k$ are not representative for the whole population.

A remark is in order here, why in the second part of equation 4.11 the outcomes at $t1$, i.e. outcomes at times relative to the treatment at τ, are used. In our application we have only few participants starting the program at the same time τ. Therefore, for a given employment state in the previous period, we can not exclusively use outcomes at calendar time $T1$ of participants with treatment in τ in order to estimate the expected treatment outcome at $T1$ of nonparticipants with the same, but imputed treatment time τ. Instead, we have to use all participant with the respective employment state in the previous period, although starting at different τ. This also implies that we can not use the outcomes for the participants at $T1$ to estimate the expected treatment outcomes of the nonparticipants at the same $T1$ as participant's time distance to the treatment time is in most cases different than to the nonparticipant imputed treatment time. The time difference naturally matters as we should not use e.g. outcomes before treatment to estimate the expected treatment outcomes. Furthermore, we also want to allow elapsed time since treatment to play a role with respect to treatment effects. Thus, to estimate the expected treatment outcome of nonparticipants with the employment state k in the previous period, we match on the time $t1$ relative to treatment τ, while taking the discrepancy between the treatment times into account when calculation the weights $w_{N_1^k}(i,j)$, see equation 4.12.

We apply the following local linear kernel regression to receive estimates of the expected treatment outcomes of the nonparticipants ($\sum_i w_{N_1^k}(i,j)Y_{i,t1}$) separately for all different i, k, and $t1$. This estimator is obtained as the result of the following minimization problem:

$$(4.12) \quad (m^k(j,t1),\beta_k^l) = \underset{\{m,\beta\}}{argmin} \sum_{i\in\mathcal{N}_1^k} \{Y_{i,t1} - m - \beta_1'(p_i - p_j) - \beta_2'(\tau_i - \tau_j) \\ - \beta_3'(\tau_i - \tau_j)^2 - \beta_4'(\tau_i - \tau_j)(p_i - p_j)\}^2 K\left(\frac{p_i - p_j}{h}\right)$$

Note that for calculating $m^k(j,t1)$ we use the outcomes of participants at the same elapsed time since treatment ($t1$) measured with respect to their individual treatment time τ_i. To take account that treatment times τ_i of participants are in most cases different from the imputed treatment time of the nonparticipant j, τ_j, we include correction terms resembling a Taylor series expansion of second order around the imputed treatment time τ_j of the nonparticipants. By doing so we follow the idea of the basic local linear regression estimator (see equation 4.9) where the difference between the estimated propensity score between nonparticipants and the participant is include. With the aid of these correction terms the estimates for the expected treatment outcome for the nonparticipants with a given employment state k in the previous month is corrected locally from the influence of the deviation of treatment times of the participants and the imputed treatment time for the nonparticipants.

Formally, $m^k(j,t1)$ is a weighted average of $Y_{i,t1}$ for all $i \in \mathcal{N}_1^k$, where the weights depend not only on the kernel weights and the differences $(p_j - p_i)$ as for $m^k(i,t1)$ in equation 4.9 but also on $\tau_i - \tau_j$. The resulting estimator $m^k(j,t1)$ of equation 4.12 represents then $\sum_{i\in\mathcal{N}_1^k} w_{N_1^k}(i,j)(Y_{i,t1})$.

Also in this case, we follow Silverman's Rule of Thumb in order to choose the bandwidth. Naturally, for the estimation of the expected treatment outcome of the nonparticipants with the aid of observations on the outcome of the participants, the choice of the bandwidth h_{ROT} should be based on the sample of participants.

4.3.4 Conditional Difference–in–Differences in Hazard Rates

Additionally to selection bias due to observables, for which matching estimators can take account of, we want to allow for selection bias due to unobservables. We build on the conditional difference–in–differences in hazard rates estimator (CDiDHR) of section 3.3.3 which is developed in order to estimate the treatment–on–the–treated effect.[14] This CDiDHR estimator is is able to take account of permanent unobserved selection effects as represented by c_i^e, c_i^n in equation 4.1, in case they are time invariant and additively separable.

The basic idea of CDiD estimators is to extend matching estimators by analyzing before–after changes in the outcome variable instead of its level. In this way preprogram differences in the outcome variable after matching are used to control for remaining unobservable differences.

The CDiDHR estimator for $\mathrm{TT}^k(\tau, t1)$ as proposed in section 3.3.3 takes the following form (notation is adjusted):

$$\frac{1}{N_{1,\tau}^k} \sum_{i \in \mathcal{N}_{1,\tau}^k} g_i \left[Y_{i,T1} - Y_{i,T0} - \sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j)(Y_{j,T1} - Y_{j,T0}) \right] \tag{4.13}$$

where $T0$ is some time before possible preprogram effects can take effect (Ashenfelter's Dip, see section 4.3.1). Now, $N_{1,\tau}^k$ is the number of individuals in the set of participants with treatment at time τ ($\mathcal{N}_{1,\tau}^k$) for whom $Y_{i,T1-1} = Y_{i,T0-1} = l$ is valid, where $l = 0$ if $k = n$, $l = 1$ if $k = e$ and N_0^k is the number of individuals in the set of nonparticipants ($\mathcal{N}_0^k$) for whom $Y_{j,T1-1} = Y_{j,T0-1} = l$ is valid.

Under the assumption of the econometric model (see equation 4.1) the CDiDHR estimator properly accounts for the selection bias in the nonparticipation outcome. In particular, with respect to the idiosyncratic error term the only two necessary assumptions are that they need to be conditionally mean independent of treatment status D and they need to be conditionally mean independent of the covariates X_m with $m = i, j$, i.e. $E(u_{m,T}^e | D = 1, X_m) = E(u_{m,T}^e | D = 0, X_m) = E(u_{m,T}^n | D = 1, X_m) = E(u_{m,T}^n | D = 0, X_m) = 0$ for $T \geq \tau$ and $T < \tau - ad$. Note that for example the individual specific effects c_m^k do not have to be conditionally mean independent of treatment status D_m and covariates X_m.

The following estimator for $\mathrm{ATE}^k(\tau, t1)$ is an extension of the idea of the CDiDHR estimator for $\mathrm{TT}^k(\tau, t1)$:

$$\begin{aligned} &\frac{1}{N_\tau^k} \sum_{i \in \mathcal{N}_{1,\tau}^k} h_i \left[Y_{i,T1} - Y_{i,T0} - \sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j) \left(Y_{j,T1} - Y_{j,T0} \right) \right] \\ + \; &\frac{1}{N_\tau^k} \sum_{j \in \mathcal{N}_{0,\tau}^k} h_j \left[\sum_{i \in \mathcal{N}_1^k} w_{N_1^k}(i,j)(Y_{i,t1} - Y_{i,t0}) - (Y_{j,t1} - Y_{j,t0}) \right] \end{aligned} \tag{4.14}$$

where $t0$ is some time before treatment, but in contrast to $T0$ measured relative time to treatment. Thus $t0$ always takes on negative values, i.e. $T0 = \tau + t0$ with $t0 < 0$,

[14]Note that here the the word 'rate' is used, although the model is defined in discrete time, as it can be aggregated to probabilities

whereas $t1$ always takes positive values with $T1 = \tau + t1$ and $t1 > 0$. Naturally, with respect to choosing $t0$ the same caveat as for choosing $T0$ is in order. For aligning the estimator, time $t0$ should be chosen in the same way as $T0$. It should also lie before the start of Ashenfelter's Dip, i.e. $t0 < -ad$.

At this point it is necessary to discuss the role of the weights g_i, h_i and h_j. As mentioned in section 4.3.2 individuals sort according to their characteristics into specific employment states. Expressed in terms of the econometric model, equation 4.1, individuals sort with respect to X_m, c^e_m, c^n_m into the different employment states. As we allow the treatment effect $\delta^e_{m,t1,\tau}, \delta^n_{m,t1,\tau}$ also to depend on these parameters, the unconditional TT $E(\delta^k_{m,t1,\tau}|D = 1)$ is most likely different from the conditional TT $E(\delta^k_{m,t1,\tau}|D = 1, Y_{t1-1} = l, Y_{t0-1} = l)$ and similar the unconditional ATE is most likely different $E(\delta^k_{m,t1,\tau})$ different from the conditional TT $E(\delta^k_{m,t1,\tau}|Y_{t1-1} = l, Y_{t0-1} = l)$.

We therefore define the weights to integrate out the distribution of X_m with respect to the population for which we want to estimate the treatment parameters. With the proposed estimators we are able to estimate the unconditional TT or ATE under two conditions: First, the treatment effects $\delta^k_{m,t1,\tau}$ are conditionally mean independent of the individual specific effects c^e_m, c^n_m, when also conditioning on X_m. Second, we observe each treated individual in both employment states before the start of Ashenfelter's Dip, so that the before–and–after difference can be calculated for some $t0$ in the past. The second assumption is nearly always fulfilled, as we consider the preprogram situation of up to 18 months in the past.

The first condition is violated if the selection into treatment still depends upon the treatment effect $\delta^k_{m,t1,\tau}$ conditional upon X_m by way of the individual specific effects c^e_m, c^n_m. It is not possible to rule out such a relationship. However, the following relationship seems to be plausible. Conditional on X_m, the individual specific effects of the reemployment probability and the probability to stay employed are positively correlated. Furthermore, the treatment effects are positively correlated with the individual specific effects. In this case, we will overestimate the effect on the probability to remain employed and underestimate the effect on the reemployment probability.

4.4 Implementation

4.4.1 Data

The data used stem from the last three years (1997-1999) of the Labor Market Monitor Sachsen–Anhalt (LMM–SA). The LMM–SA is a survey of the working–age population of the state (*Bundesland*) of Sachsen–Anhalt with around 6.000 participants each year. The LMM–SA is a unique data set in the sense that in the last three waves of its existence it provides a retrospective monthly employment calendar that goes back until 1990. The labor market states, which are asked for in this calendar, include information on participation in the two main programs of ALMP in East Germany, job creation schemes (comprising traditional job creation schemes and productive wage subsidies including private firms) and training. In our empirical analysis we make use of this calendar information and combine it with time invariant individual characteristics asked in the cross–sectional dimension of the questionnaire. We do not include waves before 1997 in our analysis as they do not offer information on employment states on a monthly basis which is crucial for our approach.

Of course, it would be interesting to include additional types of wage subsidies in the analysis. However, only in 1999 an explicit question concerning wage subsidies was posed. Individuals were asked whether they ever had an employer that received a wage subsidy in connecting with their employment relationship. The respondents in 1999 could only give one time period as an answer. With this new data source only few additional time periods of wage subsidies could be identified, potentially encompassing very heterogeneous types of wage subsidies. We therefore decided to discard this additional information and evaluate a relatively homogeneous treatment which consists of traditional job creation schemes, ordinary productive wage subsidies together and since April 1997 also productive wage subsidy for private firms. In the following we will call them jointly job creation schemes (JC), by keeping in mind that at the end of our observation period also productive wage subsidies for private firms are included, which are elsewhere sometimes defined differently.

We only include individuals who gave complete information on their labor market history starting with January 1990 until the time the fist interview was conducted in September 1997. The last interview took place in December 1999 thus giving us an observation period of up to 120 months.

Table 4.1: Program Participation (number of individuals) in the LMM–SA during 1990 and 1999

One Program	JC[a]	TR[b]
At least once	689	1021
As first program	484	889

Program Sequences[c]	JC–JC	JC–TR	JC alone
First and Second	105	113	266
Program Sequences	TR–JC	TR–TR	TR alone
First and Second	176	150	563

a: Training b: Job Creation Scheme
c: For instance, TR–JC indicates that a first participation in training and a second treatment in JC occurred

In order to avoid to evaluate programs which had the aim to bridge the time until retirement we choose individuals aged between 25 and 50 years in January 1990. With the aim to receive a sample which is representative for the active labor force of the former GDR, we only include individuals in the analysis who were employed in June 1990, the months before the Social and Economic Union came into effect. Furthermore, we exclude individuals completely from our analysis who went into education and maternity leave, as well as individuals with missing values on those individual characteristics on which we base the matching. Our sample is likely to be representative for the active labor force of the former GDR which was fully hit by the transformation shock. In the following this group will be called population.

In the empirical analysis we will analyze three labor market states. *Employment* which comprises part–time and full–time employment, *nonemployment* which comprises unemployment, out of the labor force and participation in training programs, and *participation in job creation schemes.*

After our selection process we are left with 5,165 individuals of whom 689 participated at least once in an job creation scheme (see table 4.1). For 484 individuals of these 689 job creation scheme was the first participation in a program of ALMP. As mentioned above, training programs is the second program of ALMP which can be identified in the data. We observe 1,021 individuals with at least one training spell for 889 individuals this was a first participation in a program of ALMP.[15]

4.4.2 Choice of Program Type

Here, we will consider participation in a first job creation scheme irrespectively of whether it was observed to be the first, second, etc. participation in a program of ALMP. Thus, we analyze 689 participating individuals and 4476 nonparticipation individuals.

Distinguishing for example first and second participation in a program of ALMP is appealing when one intends to evaluate multiple participation in a programs of ALMP (see e.g. chapter 3). Here, however, it is not possible to estimate a separate population average treatment effect of JC for more than the first participation in a program of ALMP. This is due to the fact that the individuals whom we observe to participate in more than one program are very selective. As a consequent, we do not find fitting counterparts in terms of estimated propensity score for a non–negligible number nonparticipants in the group of participants (see next section).

In order to evaluate a large part of program participation in JC, despite not being able to estimate the separate effects of JC as second, etc. participation in a program of ALMP, we choose to evaluate participation in a first JC irrespectively in which position in the sequence of participation in ALMP it took place. This approach offers another advantage. Except for the random draws from the conditional distribution of treatment time of the participants we do not have to make additional assumptions on the start of potential participation. Whereas additional assumption would be needed in case we evaluated JC as a first participation in ALMP. Then the treatment time would have to be set before or during any other participation in ALMP.

[15]For more details on the data set see Ketzmerik (2001) and section 3.4.1. See also the latter for a discussion of the relative small risk of recall errors in this data set compared with other studies.

4.4.3 Propensity Score Estimation and Common Support

The propensity to participate in a first JC is estimated with the aid of a probit estimation. We include all relevant time invariant information that is available in the data set as covariates into the estimation. These variables include age in 1990, professional education, local labor market area at the time of the interview, and gender interacted with professional education (see table 4.2 in the appendix 4.A).

The validity of propensity score matching rests on the existence of a common support of the propensity score for the participants and the nonparticipants. If the common support condition is fulfilled we can estimate TT and ATE without reducing the groups that the estimates should represent (Heckman/Ichimura/Smith/Todd, 1998).

Figure 4.5 shows that a high degree of overlap of the estimated propensity score of participants and nonparticipants is given with respect to the first JC. The overlap of the distribution of the estimated propensity score index for four representative points of time and for the two different possible employment states in the previous months is calculated. To illustrate, why we can not estimate population average effects for treatment sequences, figure 4.6 shows the overlaps for the treatment sequence JC–JC. The values of the propensity score index of this treatment sequence is also estimated on the basis of a probit estimation with the same covariates as in table 4.2, appendix 3.A. The group of nonparticipants of the treatment sequence JC–JC consists of all individuals who did not follow this specific treatment sequence. Figure 4.6 displays an insufficient overlap of the estimated propensity score index for JC–JC. We can not find treated counterparts for a non–negligible part of the nonparticipants with low values of estimated the propensity score index.

4.4.4 Imputation of Treatment Times and Further Settings

For the imputation of the potential start dates of participants in a first JC, we estimate the start dates for the group of participants by way of Ordinary Least Squares (OLS) depending on time invariant individual characteristics (see table 4.3, appendix 4.A). We choose OLS due to its insensitivity concerning distributional assumptions. With the incorporation of time invariant characteristics in the estimation of the start dates it is possible to take account of the changing selection rules for individuals to participate in a JC over time. The potential start date for each nonparticipant is calculated by using the predicted start date of the OLS model depending on his/her time invariant characteristics and by adding a random draw from the residuals of the OLS estimation. This procedure is justified under the assumption of exchangeability of residuals, which requires that the residuals are homoscedastic. Homoscedasticity seems to be a plausible assumption as all groups of the labor market in East Germany were exposed to the same randomness concerning the selection process into programs of ALMP. An additional formal requirement for the assumption of exchangeability of residuals consists in an unbounded range of possible values of the dependent variable, which is not fulfilled in this application. We think, however, that the assumption of exchangeability of the residuals is

Figure 4.5: Overlap of Distributions of Propensity Score Index for First Participation in JC

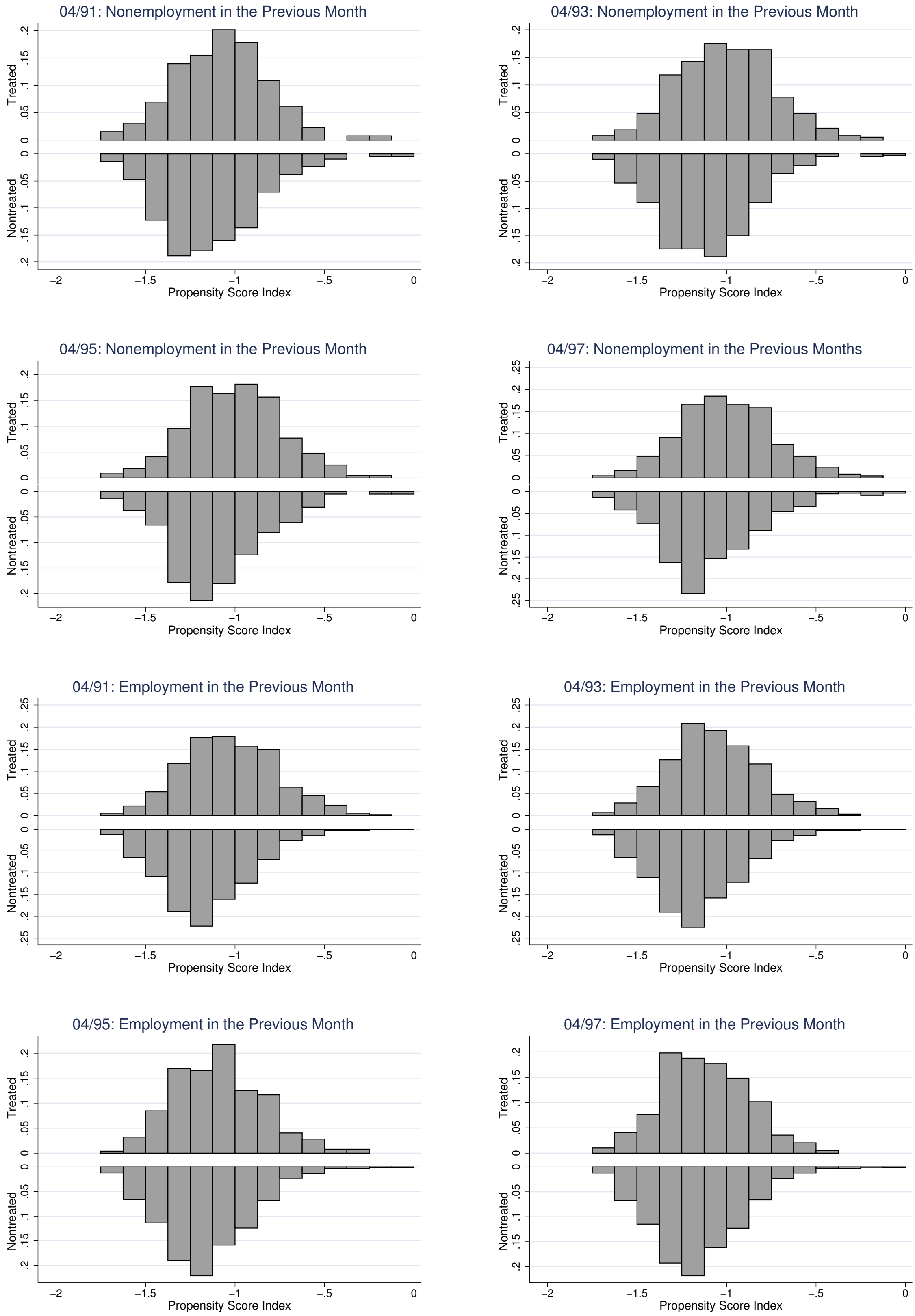
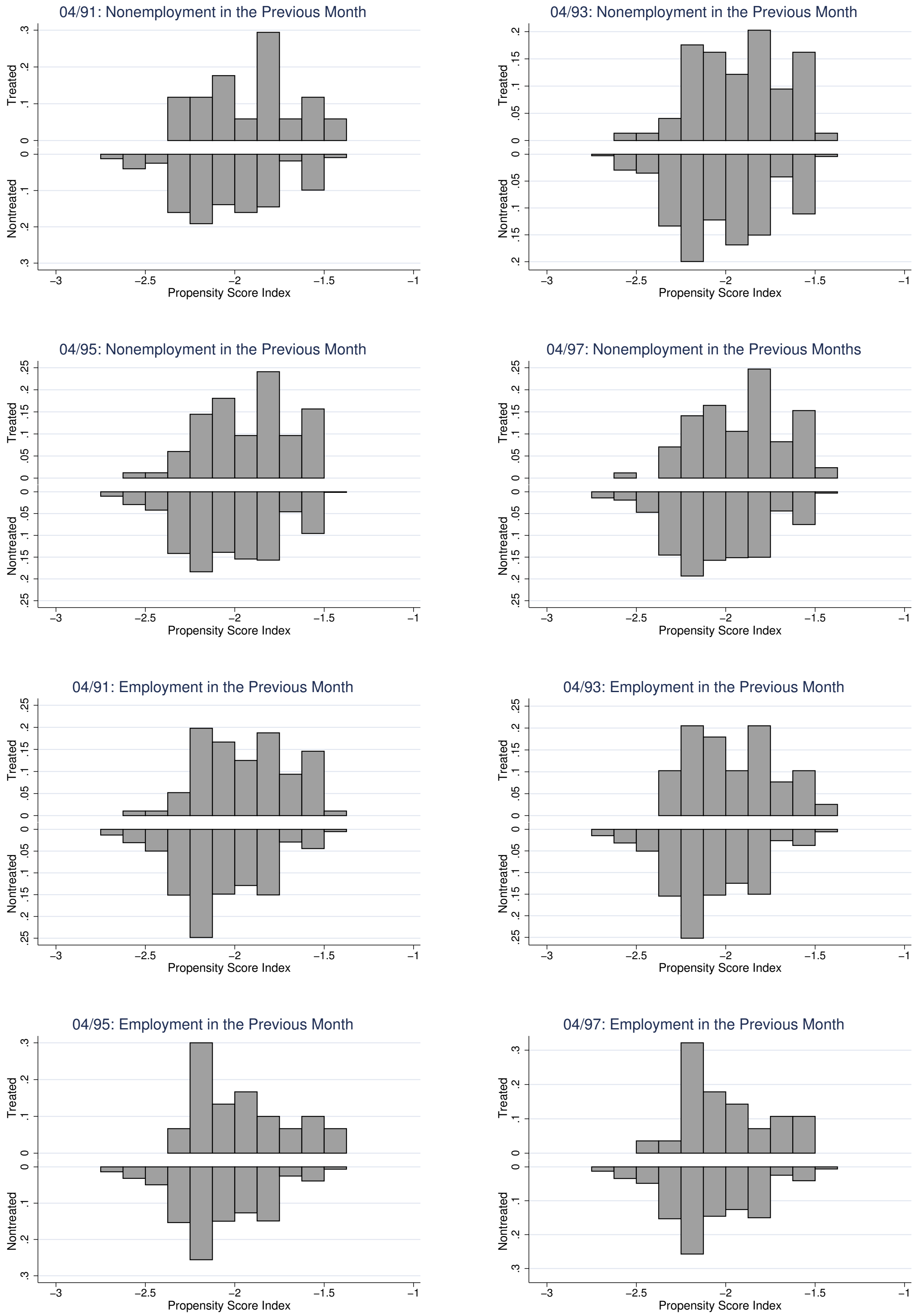


Figure 4.6: Overlap of Distributions of Propensity Score Index for Sequence JC–JC



approximately justified.

Accordingly, in the rare cases that a calculated start date lies outside the observation period of the nonparticipant, we set it to the closest border value of the observation period.

Following the institutional rules of JC, program participation is viewed as time searching for a job. Therefore, the evaluation period starts directly after the start date of program. With respect to the evaluation criterion 'reemployment probability' ('probability to remain employed') the evaluation period starts one months (two months) after the treatment started.

Theoretically, also a less strict point of view with respect to the evaluation period would be possible. If treatment is understood as time spend outside the labor market, the evaluation should start after the treatment ended. We do not consider this option here, as it is opposite to the intention of job creation schemes in East Germany. Furthermore, this perspective seems somewhat unrealistic as labor market history continues during participation. To contrast these two points of view, the data offers not sufficient information. Is is not possible to model the program duration for the nonparticipants in a satisfactory way, especially as program duration itself is partly a treatment effect. Some participants might choose to end a program because they found a job.

The evaluation period comprises 36 months and the preprogram period 18 months. Time spend in any program of ALMP is understood as nonemployment.

4.4.5 Specification of Outcome Equation

In the matched samples, the CDiDHR estimators of TT and ATE are based on a flexible semiparametric linear probability model. Employment dummies serve as outcome variables.[16] The average employment effects of a program are estimated relative to all possible nonemployment states for either the treated resulting in TT or jointly for the treated and the nontreated individuals resulting in ATE, where we condition on the employment state in the previous month. We also control for observed, time–invariant characteristics X_m in the outcome equation. The X_m variables enter the equation for TT as deviations from their averages in the treatment sample and for ATE as deviation from their sample averages. Treatment takes place in period τ. With respect to the treatment effects on the reemployment probability (probability to remain employed) τ stands for the start month of the program (start months of the program and the following month). We consider the employment outcome Y before the begin of treatment $t0 = -18, \ldots, -ad - 1$, as well as during the time of Ashenfelter's Dip and the evaluation period of 36 months which consists of $t1 = -ad, \ldots, -1, 1, \ldots, 36$. Note that $t1$ is here differently defined as in section 4.3.4, where $t1$ did not include the time of Ashenfelter's Dip.

[16]The basic idea for this outcome equation and the specification of Ashenfelter's Dip was developed in the context of estimating treatment–on–the–treated effects, see chapter 3.

We estimate TT and ATE with CDiDHR in the following steps (separately depending on the employment status in the previous month):

1. The average long–run preprogram difference is calculated between participant i (treatment starts in τ) and comparable nonparticipants ($\hat{a}_{i,\tau}$) as well as the average long-run preprogram difference between nonparticipant j and comparable participants ($\hat{a}_{j,\tau}$).

$$\hat{a}_{i,\tau} = \frac{1}{18 - ad(\tau)} \sum_{T0-\tau=-18}^{-ad(\tau)-1} \left(Y_{i,T0} - \sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j) Y_{j,T0} \right)$$

$$\hat{a}_{j,\tau} = \frac{1}{18 - ad(\tau)} \sum_{t0=-18}^{-ad(\tau)-1} \left(\sum_{i \in \mathcal{N}_{1,t0}^k} w_{N_1^k}(i,j) Y_{i,t0} - Y_{j,t0} \right)$$

2. Then, a 'raw treatment effect' is calculated by subtracting the relevant long–run preprogram difference from the employment difference during Ashenfelter's Dip and during the evaluation period .

$$\Delta_{m,t1}(\tau) = \begin{cases} Y_{i,T1} - \sum_{j \in \mathcal{N}_0^k} w_{N_0^k}(i,j) Y_{j,T1} - \hat{a}_{i,\tau} & \text{for} \quad m = i \in \mathcal{N}_{1,\tau}^k,\ T1 = t1 + \tau \\ \sum_{i \in \mathcal{N}_1^k} w_{N_0^k}(i,j) Y_{i,t1} - Y_{j,t1} - \hat{a}_{j,\tau} & \text{for} \quad m = j \in \mathcal{N}_{0,\tau}^k \end{cases}$$

3. With the following model the treatment effects are estimated. For TT $m \in \mathcal{N}_{1,\tau}^k$ and for ATE $m \in \mathcal{N}_{1,\tau}^k \cup m \in \mathcal{N}_{0,\tau}^k$ ($I(.)$ denotes the indicator function)

$$\begin{aligned} (4.15)\quad \Delta_{m,t1}(\tau) &= \sum_{r=-ad(t)}^{36} \delta_k I(t1 = r) \\ &+ (\gamma_1^{ad}\tau + \gamma_2^{ad}\tau^2 + \gamma_3^{ad}\tau\, t1 + \gamma_4^{ad}\tau^2\, t1) I(-ad(\tau) \leq t1 < 0) \\ &+ (\gamma_1^{po}\tau + \gamma_2^{po}\tau^2 + \gamma_3^{po}\tau\, t1 + \gamma_4^{po}\tau^2\, t1) I(t1 > 0) + \nu_{m,t1} \end{aligned}$$

For TT, we also include deviations of the X_m characteristics from their averages in the treatment sample as additional regressors in equation (4.15) and for ATE the deviations of the X_m characteristics from their sample averages.

4. The average long–run preprogram differences $\hat{a}_{m,\tau}$ are regressed on a second order polynomial in the start month of the treatment. We will report the predictions from this regression

$$(4.16)\qquad \hat{\alpha}(\tau) = \alpha_0 + \alpha_1\tau + \alpha_2\tau^2$$

where $m \in \mathcal{N}_{1,\tau}^l$ for TT and $m \in \mathcal{N}_{1,\tau}^l \cup m \in \mathcal{N}_{0,\tau}^l$ for ATE.

The average long–run preprogram differences are reported to illustrate how the average long–run preprogram differences ($\equiv$ residual selection effect due to permanent individual specific effects) between participants and nonparticipants after matching depend upon the timing of the program.

In this linear probability model we let the effects of the program depend in a very flexible way on the time since treatment ($t1 > 0$) and upon the begin of the program τ. We also take account of the decline in the employment perspectives shortly before Ashenfelter's Dip by including dummies for the time of Ashenfelter's Dip $-ad \leq t1 < 0$ and interacting this time period with the start of treatment τ. It should also be noted, that when estimating the treatment outcome of the nonparticipants before (during) [after] Ashenfelter's Dip, we made sure that we only used outcomes of participants which were observed also before (during) [after] Ashenfelter's Dip.

Naturally, the long–run preprogram employment differences $\hat{a}_{m,\tau}$ are crucial when aligning the CDiDHR estimator. Therefore, we model the start of Ashenfelter's Dip conservatively in order to obtain a lower bound for the program effects. On the basis of the following observation we let the time period of Ashenfelter's Dip vary with the program start date: Shortly after the reunification participation could not have been anticipated a long time in advance. Therefore, the dip should be very short here. Later with a more widespread knowledge of the regulation of ALMP and the occurrence of high unemployment, participation could be anticipated a longer time in advance. Also, more strictly enforced participation rules made unemployment more often to a requirement for participation. With the aid of these considerations we set the begin of Ashenfelter's Dip in the following heuristic way. Before November 90, we set $ad(\tau) = 1$. Between November 1990 and July 1994, $ad(\tau)$ increases linearly from 2 months to 9 months, where $ad(\tau)$ is rounded to the nearest integer. After July 1994, $ad(\tau)$ remains constant. When taking the program structure and participation rules of job creation schemes into consideration, we think that we are generous in allowing for a these anticipation periods. As using shorter time periods for the difference–in–differences estimates would deliver higher estimates, we estimate a lower bound for the program effects.

4.5 Results

The estimation results are discussed by way of graphical illustrations, see figures 4.7 to 4.12. The coefficient estimates for the CDiDHR outcome equations are reported in tables 4.4 to 4.5 in the appendix 4.A.

The basic set up of the figures is the following: The thick curved line displays the CDiDHR–estimates for the time period during Ashenfelter's Dip and the evaluation period of a maximum of 36 months. The success criterion differs with respect to the type of conditional probability, we either use the reemployment probability or the probability to remain employed.

Depending on the success criterion, time 0 stands for the first month of the program (reemployment probability) or the first and the following month (probability to remain employed). To illustrate program participation the curves are interrupted at time 0. Before time 0 the development of the conditional probabilities in the time period of Ashenfelters' Dip is displayed. After time 0 the thick line displays either the estimates of TT or ATE, that is, the effects of interest. The figures also show the estimated long–run preprogram difference in the matched samples depending upon the begin of the program τ. We put 95%–confidence intervals around the estimates.

For TT, ATE, and each success criterion we show a maximum of five different starting dates (December 1990, 1992, 1994, 1996, 1997). These five points in time are exemplary for the development of JC (see also section 4.2). December 1990 was a year in which the infrastructure for JC was still being built up. In December 1992 the program organizing institutions, being mostly ABS-Societies, had gathered experience with organizing JC. At that time JC still only consisted of traditional JC. In December 1994, ordinary productive wage subsidies were already a significant part of newly started JC. Subsequently, participation became financially less attractive and the focus turned more to the problem groups of the labor market. December 1996 represents these changes. In April 1997 the productive wage subsidies for private firms were introduced and participation increased rapidly in the first months after their introduction. Therefore, we choose December 1997 as the fifth representative starting date. For this starting date we will only show an evaluation period of 2 years as our observation period ends in December 1999.

Let us first discuss the estimates for TT for a first JC on the reemployment probability of formerly nonemployed (figures 4.7 and 4.9). For programs starting in December 1990 the effect is significantly negative shortly after the program started (-5 percentage points). After one year the effect fluctuates around zero. Two years later the effect becomes negative again, although not significant. For programs starting later, the program effect seems to improve continuously. The negative level of the effect shortly after the program started disappears. In addition, the effect displays some increasing tendency the more time elapsed since the program started. At the end of our observation window these two developments lead to a significantly positive program effect on the reemployment probability for the actual participants in a first JC. For example, participating in a first JC in December 1997 increases the reemployment probability of the actual participants by five percentage points two years later.

Figures 4.8 and 4.9 display the estimated TT on the probability to remain employed. Programs in December 1990 have a positive although mainly insignificant effect for the actual participants on the probability to remain employed. With later start dates, this effect increases and becomes significant. Already programs in December 1992 show a permanently significant effect during the third year after the program started. For example, at the end of the evaluation period the probability to remain employed increases by 8 percentage points for the actual participants. It seems, however, that for later programs this positive effect on the probability to remain employed relocates to times which are closer to the start dates. For example, the effect on the probability to remain employed is significantly positive throughout 6 to 33 months after the program started, but it vanishes after that.

Figure 4.7: Treatment–on-the–Treated Employment Effects of First Participation in JC – Nonemployment in the Previous Month

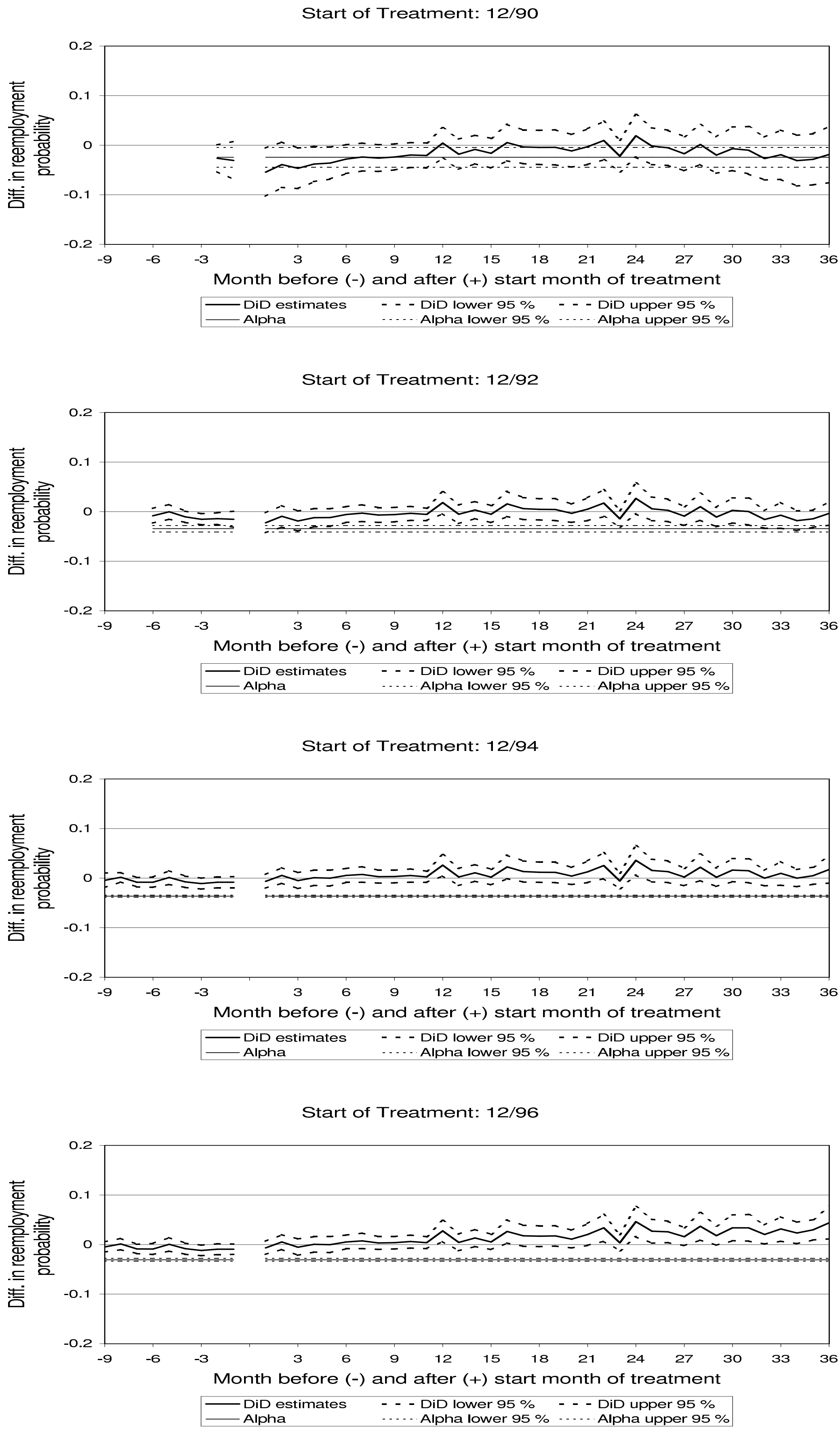


Figure 4.8: Treatment–on–the–Treated Employment Effects of First Participation in JC – Employment in the Previous Month

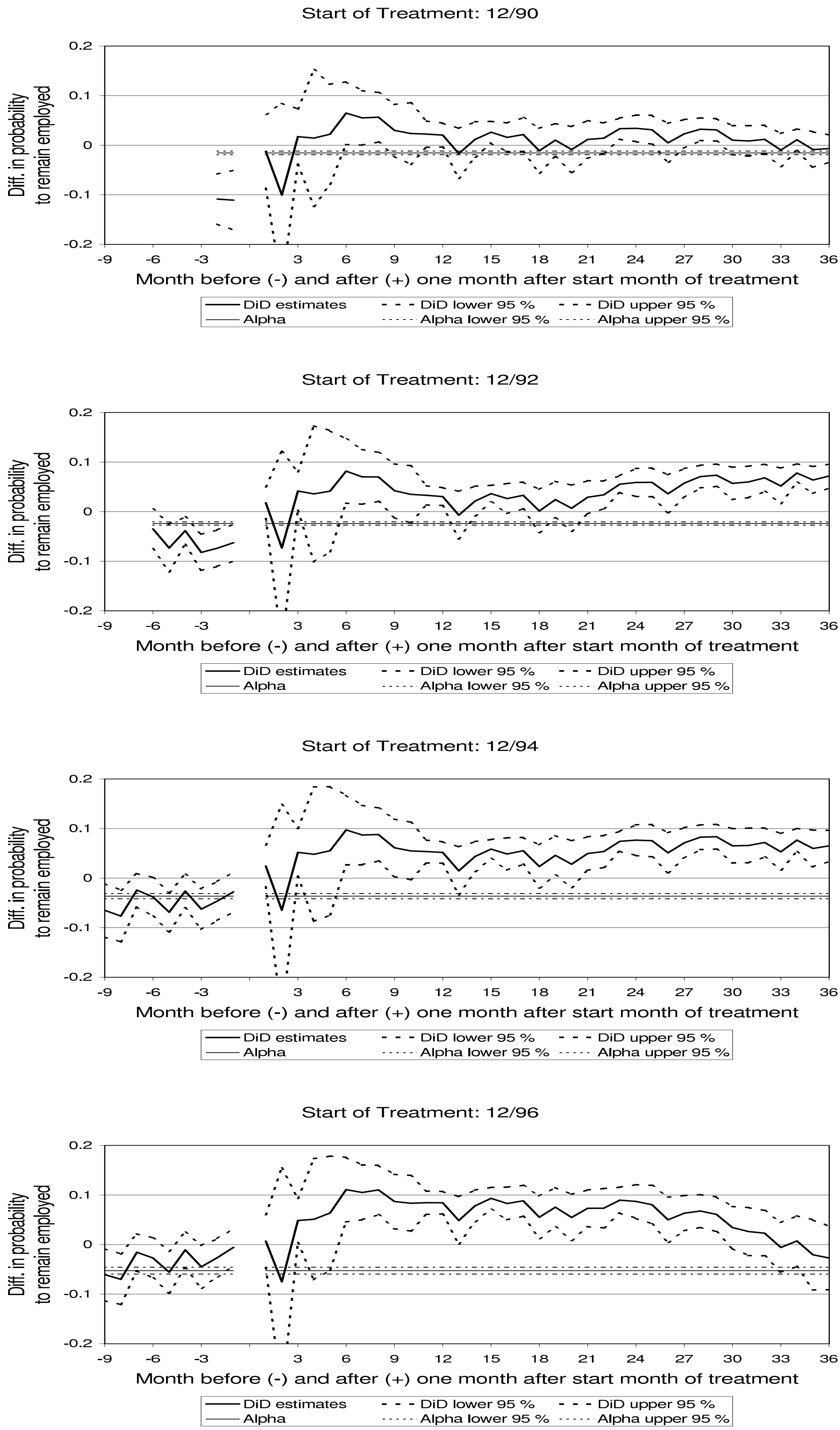
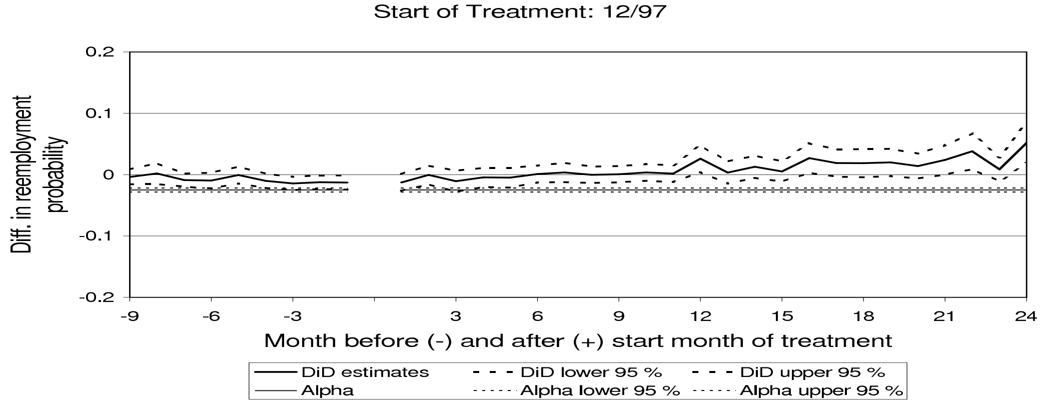
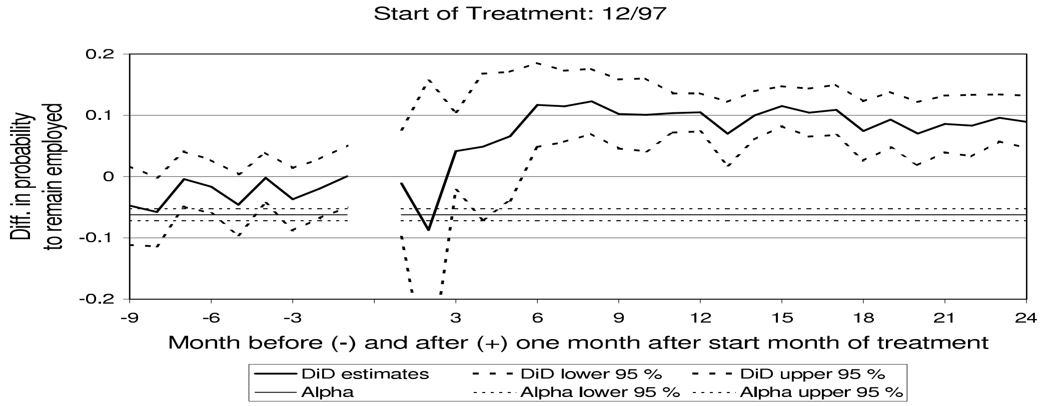


Figure 4.9: Treatment–on–the–Treated Employment Effect of First Participation in JC in December 1997





We find negative long–run preprogram differences between the participants and the matched nonparticipants. These differences are relatively constant with respect to the different program start dates for the success criterion reemployment probability. It is increasing in absolute value for the success criterion probability to remain employed, which might reflect a stricter targeting on problem groups of the labor market over time.

Let us now turn to estimates for ATE. Figures 4.10 and 4.12 summarize the results for ATE on the reemployment probability. With respect to this success criterion, the estimated ATE resembles very closely the estimated TT. For example, the ATE of programs in December 1990 are negative and partly significant in the first few months. One year after the program started, the effect increases to close to zero. After another year the effect become again negative. Also for later starting dates, the ATE changes in a similar fashion as the TT. Thus, for programs in December 1997, the estimated ATE on the reemployment probability is significantly positive two years after the program started. A similar result can be found for TT.

The estimated ATE for the evaluation criterion probability to remain employed are displayed in figures 4.11 and 4.12. To be able to display the the tight confidence intervals we choose a broader scaling for these graphs.

Figure 4.10: Population Average Employment Effects of First Participation in JC – Nonemployment in the Previous Month

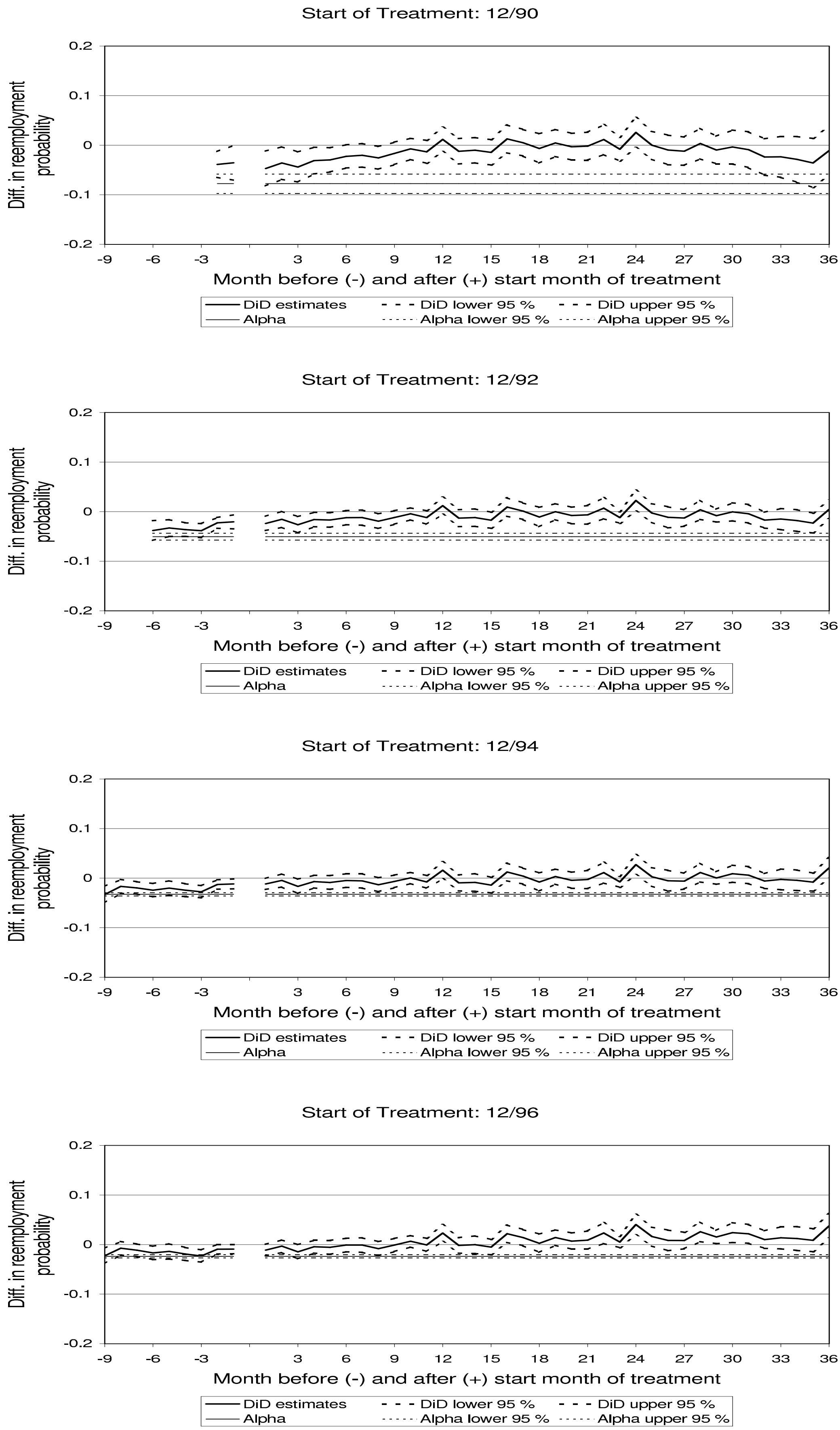


Figure 4.11: Population Average Employment Effects of First Participation in JC – Employment in the Previous Month

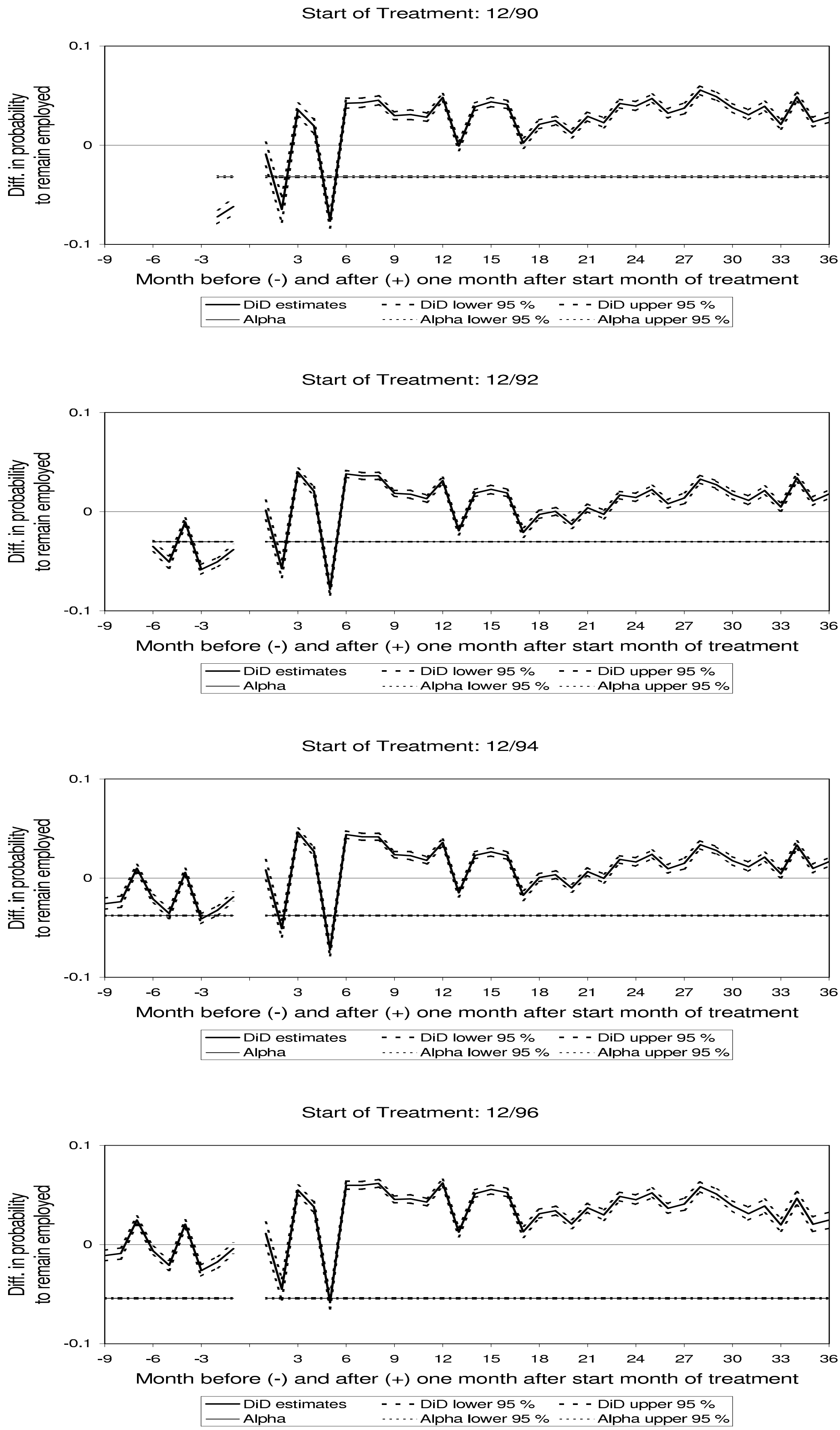
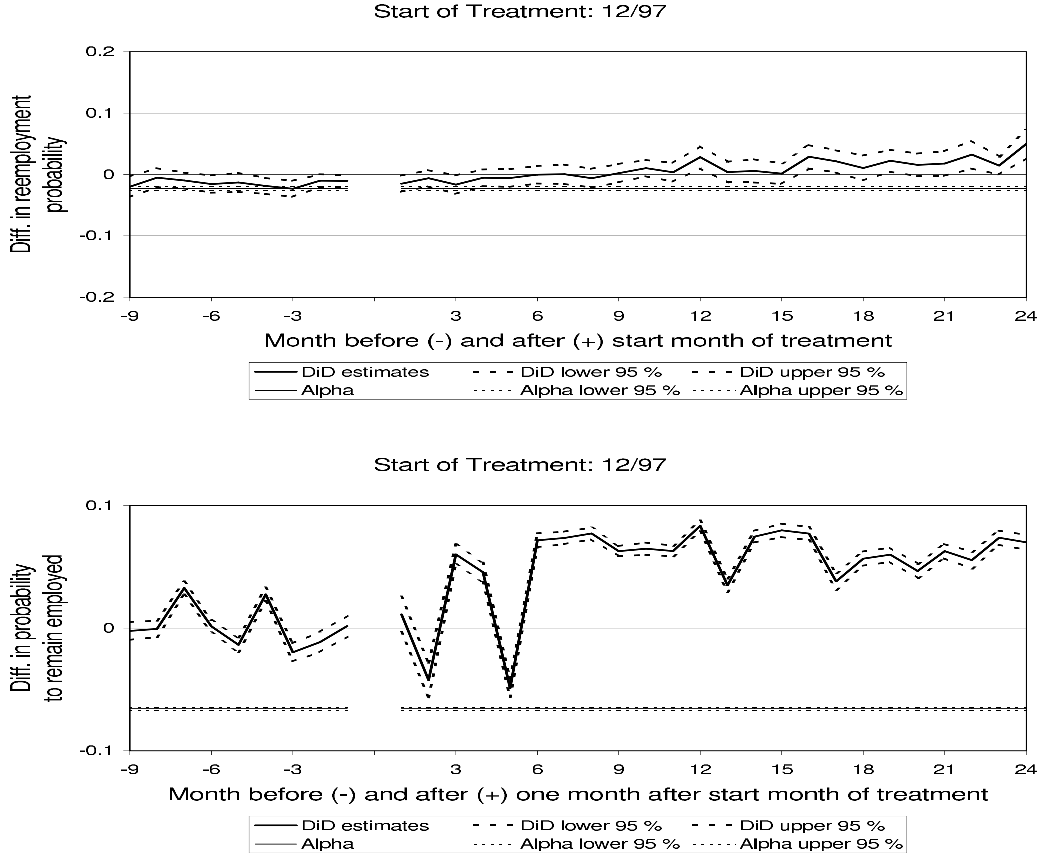


Figure 4.12: Population Average Employment Effect of First Participation in JC in December 1997



ATE with respect to the probability to remain employed seem to be slightly lower compared to the TT, except for the very early starting dates. But note that we do not conduct a formal test here. Nevertheless, the effect is in most cases positive and also significant, due to confidence intervals which lie quite tight around the estimates.

As one would expect, the long–run preprogram differences when estimating ATE are negative, similarly as they were when estimating TT.

When comparing the effects on the two different success criteria (irrespectively whether for TT or for ATE) a certain disparity becomes obvious. The effect on the probability to remain employed is on average higher than the effect on the reemployment probability.

It is conceivable that the relative small success of JC to increase the reemployment probability arises from the feeling of participants to be already reintegrated into the labor market by purely participating in a JC. Especially, the relative high wage which participants received in the early 90's might have supported this view. As a consequence, participants might decide to reduced their search effort for a regular job during participation.

That former participants in JC schemes are attractive for employers could be a conclusion from the positive effects of JC on the probability to remain employed which in turn might give some evidence for the ability of JC to raise human capital. Here, especially the noncognitive skills which JC mainly provides might play a role. That the estimates for ATE on the reemployment probability seem to be lower than for TT suggests that the actual participants gain most from this increase in human capital.

However, these interpretations are under the provision that the following two limitations do not influence the results qualitatively: First, it should be noted that the results are still conditioned on the employment state in the previous month (see also section 4.3.4). Given the assumptions concerning the correlation of the program effects and the individual specific effects, we might overestimate the effects on the probability to remain employed and we might underestimate the effects on the reemployment probability. Furthermore, we did not take into account the estimation error from the estimation of the propensity score, which might especially widen the confidence interval on the ATE for the probability to remain employed. Here we had to base the estimates for the predicted treatment outcome of nonparticipants on a small number of observations of participants.

Even if these limitations would influence the results qualitatively, we can conclude, that it is unlikely that a first JC has negative effects for the actual participants. Furthermore, it can be noted that the most favorable effect occurs for programs which started in the end of the 90's. Thus, the change towards a stricter targeting on problem groups of the labor market, reduced financial incentives to participate in the programs and changes toward subsidizing jobs which are closer or equal to jobs in the private sector seem to improve the effects of job creation schemes.

4.6 Conclusions

This study estimates average treatment–on–the–treated and population average treatment effects for participation in a first job creation scheme in East Germany. It uses a recently developed approach for the treatment–on–the–treated effect which extends the conditional difference–in–differences method for estimating transition rates. The paper advances this approach in order to be able to estimate the population average treatment effect. We propose a solution for setting hypothetical starting dates for the nonparticipants and for taking account that starting dates vary when estimating the predicted treatment outcome of nonparticipants on the basis of outcomes of participants.

We find a zero to positive effect on the reemployment probability and often significantly positive effect on the probability to remain employed for the actual participants. We therefore conclude that is unlikely that job creation schemes reduce the employment chances for the participants. We also find that the effects improve for programs which started later. These programs were financially less attractive for the participants, partly stricter targeted towards problems groups and subsidized more often jobs which were closer or equal to jobs in the private sector.

The estimated population average treatment effect does not deviate strongly from the results for the treatment–on–the–treated effect. Thus, a change of the group of participants towards the population average would not have had negative consequences from the view point of the individual employment chances of the (potential) participants. However, with respect to the probability to remain employed there are indications that the population average treatment effect is not as positive as the treatment–on–the–treated effect. This suggests that the human capital, which is provided by JC, is especially relevant for the actual participants.

Former results of evaluation studies of job creation schemes should be contrasted to ours, by keeping in mind that those did not take into account employment dynamics, which, however, can be very informative and more appropriate as chapter 3 shows. In chapter 3 we found for the case of training programs on the one hand a zero to positive effect on the reemployment probability and on the probability to remain employed. On the other hand, chapter 3 shows that an evaluation approach which does not take account of state dependency can find a negative effect on in the employment chances. Under the light of these considerations, the negative treatment–on–the–treated effect of job creation schemes in Bergemann/Fitzenberger/Schultz/Speckesser (2000) and Hujer/Caliendo/Thomsen (2003) on the employment probability shortly after participation which is later improving can be interpreted as positive dynamics which could be attributed to JC. The positive effect of Eichler/Lechner (2002) can be interpreted as a kind of upper bound for the treatment–on–the–treated effect of job creation schemes on the unemployment probability due to the aligment of their estimates.

For a complete evaluation of job creation schemes a number of further questions need to be answered. Before a cost–benefit analysis can be conducted, it is necessary to know the extend of the macroeconomic displacement and substitution effects. Only if these effects are not too large, the individual benefits of participating can outweigh the costs of the program. To take account of these macroeconomic effects seems especially relevant as we find the highest individual employment effects at a time where our variable for job creation schemes also include wage subsidies for private sector jobs. Naturally, here we would also expect the largest substitution effects. Unfortunately, macroeconomic studies which could shed some light on this issue have not yet come to clear conclusions. (Speckesser 2004, Hujer/Blien/Caliendo/Zeiss, 2002).

Appendix to Chapter 4

4.A Detailed Tables

Table 4.2: Propensity Score Estimation for a First Participation in a Job Creation Scheme

Variable	Coef.	(s.e.)
Constant	10.326	(2.693)
Age in 1990: Age 25–34 is omitted category		
Age 35–44	2.098	(1.244)
Age 45–50	3.092	(1.325)
Labor Market Region: Dessau is omitted category		
Halberstadt	-0.866	(2.334)
Halle	6.319	(1.968)
Magdeburg	2.743	(1.744)
Merseburg	4.339	(1.879)
Sangerhausen	1.410	(2.001)
Stendal	0.926	(2.379)
Wittenberg	2.350	(2.416)
Professional Education (All):Unskilled, semi–skilled and other skills is omitted category		
Skilled Worker	1.700	(2.398)
Craftsman	1.941	(3.143)
Technical college	3.663	(2.939)
University education	2.042	(2.500)
Professional Education (Women):		
Female skilled worker	0.807	(1.506)
Female Craftsman	-5.474	(4.457)
Female and technical college	-4.178	(2.645)
Female and university education	-1.784	(2.004)

Table 4.3: OLS Estimation of the Start Date of Job Creation Scheme

Variable	Coef.	(s.e.)
Constant	57.294	(5.795)
Age in 1990: Age 25–34 is omitted category		
Age 35–44	6.243	(2.678)
Age 45–50	7.822	(2.852)
Labor Market Region: Dessau is omitted category		
Halberstadt	-3.525	(5.023)
Halle	-1.377	(4.234)
Magdeburg	-8.187	(3.754)
Merseburg	-3.032	(4.043)
Sangerhausen	-8.226	(4.305)
Stendal	-7.183	(5.121)
Wittenberg	-8.573	(5.200)
Professional Education (All):Unskilled, semi–skilled and other skills is omitted category		
Skilled Worker	-4.698	(5.162)
Craftsman	-8.765	(6.765)
Technical college	-4.794	(6.325)
University education	2.020	(5.380)
Professional Education (Women):		
Female skilled worker	4.641	(3.240)
Female Craftsman	10.620	(9.592)
Female and technical college	3.774	(5.693)
Female and university education	-1.746	(4.312)

Table 4.4: Coefficient Estimates for the Average Treatment–on–the–Treated Effect of a First Job Creation Scheme

Employment Status in the Previous Month:	Nonemployment		Employment	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Constant	-0.016	(0.015)	-0.013	(0.004)
τ	-7.37E-04	(4.24E-04)	-1.81E-04	(2.30E-04)
τ^2	6.76E-06	(2.87E-06)	-3.48E-06	(2.24E-06)
Outcome–Equation				
$D(t1=-9)$	0.013	(0.053)	0.057	(0.142)
$D(t1=-8)$	0.013	(0.045)	0.016	(0.116)
$D(t1=-7)$	-0.003	(0.034)	0.039	(0.095)
$D(t1=-6)$	-0.010	(0.027)	-0.004	(0.073)
$D(t1=-5)$	-0.006	(0.020)	-0.064	(0.065)
$D(t1=-4)$	-0.022	(0.016)	-0.051	(0.040)
$D(t1=-3)$	-0.031	(0.016)	-0.116	(0.035)
$D(t1=-2)$	-0.035	(0.020)	-0.129	(0.038)
$D(t1=-1)$	-0.041	(0.028)	-0.140	(0.048)
$D(t1=1)$	-0.076	(0.036)	-0.037	(0.067)
$D(t1=2)$	-0.060	(0.033)	-0.121	(0.102)
$D(t1=3)$	-0.066	(0.029)	0.000	(0.050)
$D(t1=4)$	-0.056	(0.025)	0.000	(0.077)
$D(t1=5)$	-0.053	(0.023)	0.010	(0.051)
$D(t1=6)$	-0.043	(0.021)	0.056	(0.042)
$D(t1=7)$	-0.038	(0.020)	0.048	(0.037)
$D(t1=8)$	-0.039	(0.019)	0.051	(0.034)
$D(t1=9)$	-0.036	(0.019)	0.026	(0.032)
$D(t1=10)$	-0.031	(0.018)	0.021	(0.038)
$D(t1=11)$	-0.031	(0.019)	0.021	(0.022)
$D(t1=12)$	-0.005	(0.021)	0.020	(0.021)
$D(t1=13)$	-0.027	(0.021)	-0.017	(0.030)
$D(t1=14)$	-0.016	(0.021)	0.011	(0.024)
$D(t1=15)$	-0.023	(0.022)	0.026	(0.019)
$D(t1=16)$	-0.001	(0.025)	0.015	(0.020)
$D(t1=17)$	-0.009	(0.023)	0.020	(0.024)
$D(t1=18)$	-0.010	(0.024)	-0.014	(0.027)
$D(t1=19)$	-0.009	(0.025)	0.006	(0.021)
$D(t1=20)$	-0.016	(0.024)	-0.014	(0.027)

<continued on next page>

Table 4.4: Coefficient Estimates <continued>

Employment Status in the Previous Month:	Nonemployment		Employment	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$D(t1=21)$	-0.007	(0.025)	0.004	(0.025)
$D(t1=22)$	0.006	(0.026)	0.004	(0.022)
$D(t1=23)$	-0.025	(0.024)	0.021	(0.018)
$D(t1=24)$	0.016	(0.028)	0.019	(0.019)
$D(t1=25)$	-0.005	(0.026)	0.013	(0.020)
$D(t1=26)$	-0.008	(0.025)	-0.017	(0.024)
$D(t1=27)$	-0.020	(0.026)	-0.002	(0.019)
$D(t1=28)$	-0.002	(0.028)	0.003	(0.017)
$D(t1=29)$	-0.023	(0.028)	-0.003	(0.017)
$D(t1=30)$	-0.010	(0.032)	-0.028	(0.019)
$D(t1=31)$	-0.014	(0.035)	-0.034	(0.021)
$D(t1=32)$	-0.031	(0.034)	-0.036	(0.021)
$D(t1=33)$	-0.023	(0.038)	-0.063	(0.022)
$D(t1=34)$	-0.036	(0.041)	-0.048	(0.022)
$D(t1=35)$	-0.034	(0.043)	-0.074	(0.030)
$D(t1=36)$	-0.024	(0.046)	-0.078	(0.027)
AD:τ	1.16E-03	(1.07E-03)	3.32E-03	(2.41E-03)
AD:τ^2	-8.36E-06	(7.50E-06)	-1.60E-05	(2.02E-05)
AD:$\tau * t1$	1.84E-04	(2.62E-04)	7.69E-04	(6.52E-04)
AD:$\tau^2 * t1$	-1.30E-06	(1.81E-06)	-4.69E-06	(4.85E-06)
PO:τ	2.13E-03	(1.18E-03)	2.65E-03	(3.17E-03)
PO:τ^2	-1.48E-05	(8.15E-06)	-2.58E-05	(2.88E-05)
PO:$\tau * t1$	-1.38E-04	(1.63E-04)	-3.96E-04	(3.17E-04)
PO:$\tau^2 * t1$	9.07E-07	(1.19E-06)	5.12E-06	(2.99E-06)
PO:$\tau * t1^2$	2.48E-06	(4.54E-06)	1.42E-05	(7.27E-06)
PO:$\tau^2 * t1^2$	-9.98E-09	(3.45E-08)	-1.80E-07	(7.10E-08)
Variables as deviation from their mean value over all treated:				
Age 35–44	-0.011	(0.003)	-0.003	(0.007)
Age 45–50	0.006	(0.004)	0.007	(0.008)
Halberstadt	-0.014	(0.004)	0.019	(0.012)
Halle	-0.012	(0.004)	-0.016	(0.011)
Magdeburg	-0.048	(0.005)	0.003	(0.012)
Merseburg	-0.002	(0.004)	-0.004	(0.013)
Sangerhausen	0.010	(0.004)	-0.004	(0.013)
Stendal	-0.004	(0.004)	-0.040	(0.018)
Wittenberg	0.006	(0.005)	0.016	(0.015)

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Table 4.4: Coefficient Estimates <continued>

Employment Status in the Previous Month:	Nonemployment		Employment	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Skilled Worker	0.022	(0.005)	-0.053	(0.020)
Craftsman	0.002	(0.006)	-0.037	(0.022)
Technical college	-0.109	(0.013)	-0.036	(0.023)
University education	0.000	(0.005)	-0.027	(0.020)
Female skilled worker	-0.019	(0.004)	0.038	(0.010)
Craftswoman	0.028	(0.008)	0.045	(0.045)
Female and technical college	0.089	(0.014)	-0.008	(0.018)
Female and university education	0.012	(0.005)	-0.014	(0.010)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$
PO: After end of program $\equiv I(t1 > 0)$
Heteroscedasticity robust standard errors in brackets.

Table 4.5: Coefficient Estimates for Average Treatment Effect of a First Job Creation Scheme

Employment Status in the Previous Month:	Nonemployment		Employment	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Long–run preprogram difference				
Const	-0.095	(0.015)	-0.036	(0.001)
τ	1.52E-03	(4.25E-04)	4.38E-04	(3.92E-05)
τ^2	-8.01E-06	(2.90E-06)	-7.80E-06	(3.40E-07)
Outcome–Equation				
$D(t1 = -9)$	-0.088	(0.055)	-0.046	(0.020)
$D(t1 = -8)$	-0.069	(0.047)	-0.049	(0.016)
$D(t1 = -7)$	-0.069	(0.037)	-0.019	(0.013)
$D(t1 = -6)$	-0.071	(0.030)	-0.055	(0.011)
$D(t1 = -5)$	-0.064	(0.022)	-0.074	(0.009)
$D(t1 = -4)$	-0.066	(0.017)	-0.037	(0.006)
$D(t1 = -3)$	-0.066	(0.018)	-0.088	(0.006)
$D(t1 = -2)$	-0.049	(0.020)	-0.084	(0.005)
$D(t1 = -1)$	-0.045	(0.026)	-0.075	(0.007)
$D(t1 = 1)$	-0.063	(0.026)	-0.016	(0.008)
$D(t1 = 2)$	-0.050	(0.024)	-0.069	(0.009)
$D(t1 = 3)$	-0.056	(0.022)	0.035	(0.006)
$D(t1 = 4)$	-0.041	(0.019)	0.021	(0.005)
$D(t1 = 5)$	-0.038	(0.017)	-0.073	(0.005)
$D(t1 = 6)$	-0.029	(0.016)	0.048	(0.004)
$D(t1 = 7)$	-0.025	(0.016)	0.051	(0.003)
$D(t1 = 8)$	-0.029	(0.016)	0.055	(0.003)
$D(t1 = 9)$	-0.019	(0.016)	0.042	(0.003)
$D(t1 = 10)$	-0.008	(0.015)	0.044	(0.003)
$D(t1 = 11)$	-0.013	(0.016)	0.043	(0.003)
$D(t1 = 12)$	0.013	(0.017)	0.065	(0.002)
$D(t1 = 13)$	-0.010	(0.018)	0.017	(0.003)
$D(t1 = 14)$	-0.007	(0.018)	0.058	(0.003)
$D(t1 = 15)$	-0.011	(0.018)	0.064	(0.003)
$D(t1 = 16)$	0.017	(0.019)	0.062	(0.003)
$D(t1 = 17)$	0.009	(0.019)	0.024	(0.003)
$D(t1 = 18)$	-0.002	(0.020)	0.043	(0.003)
$D(t1 = 19)$	0.010	(0.020)	0.047	(0.003)
$D(t1 = 20)$	0.002	(0.019)	0.035	(0.003)
$D(t1 = 21)$	0.003	(0.020)	0.052	(0.003)

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Table 4.5: Coefficient Estimates <continued>

Employment Status in the Previous Month:	Nonemployment		Employment	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
$D(t1 = 22)$	0.017	(0.020)	0.046	(0.003)
$D(t1 = 23)$	-0.003	(0.018)	0.065	(0.003)
$D(t1 = 24)$	0.030	(0.021)	0.062	(0.003)
$D(t1 = 25)$	0.004	(0.020)	0.069	(0.003)
$D(t1 = 26)$	-0.006	(0.021)	0.054	(0.003)
$D(t1 = 27)$	-0.009	(0.021)	0.058	(0.003)
$D(t1 = 28)$	0.006	(0.022)	0.076	(0.003)
$D(t1 = 29)$	-0.008	(0.022)	0.068	(0.003)
$D(t1 = 30)$	-0.003	(0.026)	0.056	(0.003)
$D(t1 = 31)$	-0.009	(0.028)	0.048	(0.003)
$D(t1 = 32)$	-0.025	(0.029)	0.055	(0.003)
$D(t1 = 33)$	-0.026	(0.032)	0.035	(0.003)
$D(t1 = 34)$	-0.033	(0.036)	0.061	(0.004)
$D(t1 = 35)$	-0.042	(0.039)	0.034	(0.004)
$D(t1 = 36)$	-0.018	(0.041)	0.037	(0.004)
AD:τ	8.46E-04	(9.86E-04)	1.30E-03	(3.22E-04)
AD:τ^2	-5.45E-06	(6.88E-06)	-4.69E-06	(2.83E-06)
AD:$\tau * t1$	-4.44E-05	(2.48E-04)	1.28E-04	(8.58E-05)
AD:$\tau^2 * t1$	2.23E-08	(1.65E-06)	-8.82E-07	(6.41E-07)
PO:τ	1.64E-03	(8.94E-04)	8.81E-04	(3.36E-04)
PO:τ^2	-1.14E-05	(6.41E-06)	-6.18E-06	(3.20E-06)
PO:$\tau * t1$	-2.08E-04	(1.33E-04)	-2.87E-04	(3.88E-05)
PO:$\tau^2 * t1$	1.64E-06	(1.01E-06)	2.91E-06	(3.86E-07)
PO:$\tau * t1^2$	4.98E-06	(3.91E-06)	6.65E-06	(9.56E-07)
PO:$\tau^2 * t1^2$	-3.63E-08	(3.09E-08)	-7.00E-08	(9.79E-09)
Variables as deviation from their mean value over all treated:				
Age 35–44	-0.006	(0.003)	-0.005	(0.001)
Age 45–50	0.007	(0.003)	-0.008	(0.001)
Halberstadt	-0.012	(0.004)	0.010	(0.001)
Halle	-0.019	(0.004)	0.012	(0.001)
Magdeburg	-0.029	(0.004)	0.006	(0.001)
Merseburg	-0.007	(0.004)	-0.001	(0.002)
Sangerhausen	0.003	(0.004)	0.010	(0.002)
Stendal	-0.014	(0.004)	0.007	(0.002)
Wittenberg	0.003	(0.005)	0.001	(0.002)

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Table 4.5: Coefficient Estimates <continued>

Employment Status in the Previous Month:	Nonemployment		Employment	
Variable	Coef.	(s.e.)	Coef.	(s.e.)
Skilled Worker	0.004	(0.004)	0.017	(0.004)
Craftsman	-0.013	(0.008)	0.012	(0.004)
Technical college	-0.088	(0.010)	0.010	(0.005)
University education	-0.003	(0.005)	0.006	(0.004)
Female skilled worker	-0.005	(0.004)	-0.018	(0.001)
Craftswoman	0.019	(0.010)	-0.048	(0.005)
Female and technical college	0.068	(0.011)	0.001	(0.002)
Female and university education	0.001	(0.005)	-0.001	(0.001)

AD: Ashenfelter's Dip $\equiv I(-ad(\tau) \leq t1 < 0)$
PO: After end of program $\equiv I(t1 > 0)$
Heteroscedasticity robust standard errors in brackets.

Chapter 5

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Ehrenwörtliche Erklärung

Diese Dissertation habe ich selbständig angefertig und habe mich anderer als der in ihr angegebenen Hilfsmittel nicht bedient. Insbesondere haben aus anderen Schriften Entlehnungen, soweit sie in der Dissertation nicht ausdrücklich als solche gekennzeichnet und mit Quellenangaben versehen sind, nicht stattgefunden.

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